PROCEEDINGS

OF THE

LITERARY AND PHILOSOPHICAL SOCIETY

OF

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VOL. III.

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INDEX.

ERRATA.

p. 105, line 7 from bottom, for "Jouffray" read *Jouffroy;* line 6 from bottom, for "Titch" read *Fitch;* also dele the comma after "Miller."

p. 113, 1st line, for "1862" read 1863.

p. 192, line 6, for "glass" read *pair.*

p. 270, line 4 from bottom, for "Vanquelin" read *Vauquelin.*

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.

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

Ordinary Meeting, October 7th, 1862.

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

A Paper, by Robert Rawson, Esq., Hon. Mem., was read, entitled, "Memoir of the late Professor E. Hodgkinson. Part 2nd."

Ordinary Meeting, October 21st, 1862.

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

The Rev. W. N. Molesworth introduced to the Society M. Jules Gérard, the celebrated African traveller and lion hunter, officer of the French Army in Africa.

M. Gerard delivered an address on Africa, in reference to its future relations with Europe, and on the journey of exploration which he proposes to undertake in the former continent.

The following is a translation by the Rev. W. N. Molesworth, from a summary made by M. Gérard, who spoke in the French language.

M. Gérard discussed—1st, the question of climate; 2nd, that of the races of men by whom the African continent is peopled; 3rd, the means by which he hopes to avoid the difficulties and the dangers which have led to the failure or the death of many previous explorers. M. Gérard observed that Africa is unhealthy only on the southern, eastern, and western coasts, in the neighbourhood of the equator; but he maintained that in other latitudes, and especially in the interior, the country is healthy and very habitable for Europeans; and in proof of this he referred to the English colony at the Cape of Good Hope, at the southern extremity of the continent; to Algeria, at the northern extremity; and to all the countries in the centre which are situated at a distance from the great lakes and from the rivers which overflow their banks, that is to say, all the countries which are at a sufficient elevation above the level of the sea, and remote from the miasma produced by stagnant water.

With regard to races, M. Gérard has found that there exist various peoples who are both morally and physically degraded; but he has also found that Africa contains populations which are deserving of interest, and which are susceptible of progress and civilisation. With regard to the former, he expressed his regret at the utter neglect with which they are treated, inasmuch as the more weak and ignorant a being is, the greater is its claim on the indulgence and support of the strong; and he called the attention of the Society to the people of Dahomey, stating that their barbarous customs have not originated with themselves, but from the sovereign and his favorites. As a proof of this assertion M. Gérard said that the victims in their human sacrifices are sometimes prisoners of war, but more frequently men, women, and children belonging to the nation of Dahomey. It is incredible that it can be agreeable or even indifferent to the people of Dahomey to sacrifice their families to the pleasure of the king. M. Gérard is of opinion that the people endure this calamity because imposed on them by despotic violence; but that, however brutalised, they would bless the hands that would bring them deliverance. As a means of promoting a result so desirable in the interests of humanity, M. Gérard proposes to arm the peoples who dwell on the northern borders of the country of Dahomey. They are the natural enemies of the abominable government of Dahomey for many reasons, of which the principal are that the troops of the king make continual incursions into their territories to procure slaves, and that he prevents them from trading with the European establishments on the coast. The king of Dahomey closes all the routes in order to prevent these peoples from obtaining any other arms than the primitive ones which they use at present, viz., the lance and the arrow, thus enabling him to preserve the advantages he possesses over them in consequence of the supply of fire-arms which he can command. M. Gérard pointed out that arms and ammunition might be distributed among these peoples by two channels over which the king of Dahomey has no command. With these arms we might introduce men to instruct and direct the natives. This done, the interests of the people of Dahomey would have to be separated from the interests of the dynasty, by means of agents recruited in the country, and thus the axe would be laid to the very root of this dynasty. Then the abominations which have made us groan with horror will be succeeded by a natural state of things; the labours of peace will take the place of war, carnage and fire; and Europe, as well as Africa, will profit by the happy change. In the journey of exploration which he is going to undertake, M. Gérard proposes to study *on the spot* this great question, in the solution of which he will be happy to participate actively. His point of departure will be Sierra Leone, or some other English possession a little further to the west. He will endeavour first to discover the *sources of the Niger*, which are still unknown. Afterwards he will visit the peoples who dwell at the northern extremity of the republic of Liberia and the country of Dahomey. He will then return towards the upper basin of the great river, where, according to the information he has obtained, he expects to find a vast agricultural country in which the natives produce cotton and indigo; and in which he hopes that an establishment may be founded for encouraging the growth of these productions, and extending commercial relations with Europe. After having studied these countries and their inhabitants, M. Gérard will visit Timbuctoo, in order to obtain the papers of the unfortunate Major Laing, who was murdered near this town in 1822. From Timbuctoo he will return to Europe by Ainselah, Goleah, and Algiers. The length of this journey is about three thousand miles; for the purpose of accomplishing it M. Gérard reckons on the destruction which he will make of leopards, lions, and panthers, which depopulate whole villages in these countries; and on the relations he has formed with the Arabs. M. Gérard concluded by thanking the president and the members of the Society for the great attention with which he had been heard, although addressing them in a foreign language.

A vote of thanks to M. Gérard having been passed, and some discussion having taken place on his Paper,

The Rev. W. N. Molesworth remarked that M. Gérard had visited Manchester in the hope of obtaining pecuniary assistance towards carrying out his proposed explorations. His object was of great importance both in a scientific and commercial point of view. He was opening up to them new sources whence they might be supplied with good and cheap cotton, and he also expected to make geographical, ethnological, and zoological discoveries. It was to be hoped, therefore, that gentlemen present who felt an interest in either of these two objects would make his purpose as widely known as possible, and render to him, or obtain for him, all the assistance they could.

Some remarks having been made respecting the sudden fall of the barometer, which occurred on Sunday the 19th inst., Mr. Baxendell read the following extract of a letter from Mr. Vernon, F.R.A.S. :—"At 8h. a.m. on Sunday, the reading of the barometer was 29·483 inches, and at 9h. 50m. p.m. 28·502 inches, a fall of 0·981 inches in a little over twelve hours: the entire fall in ten days was 1·8 inches, a very unusual amount. The depression accompanying the Royal Charter storm was only about one-half that accompanying the past storm."

Mr. Alfred Fryer, who observed the movements of an aneroid barometer under the microscope during the storm, stated that the minimum pressure occurred at 10h. 10m. p.m., and that for some minutes before and after this time the index of the instrument was in a state of rapid vibration.

A Paper was read by Dr. J. P. Joule, entitled, "Notice of a Compressing Air Pump."

The Author referred to the difficulties of realising in practice the theoretical advantages of the air, or the superheated steam engine. The abrasion which takes place when metal rubs against metal, without an intermediate lubricator, speedily destroys the cylinder. He believed that the necessity of using elastic packing would not exist if the length of the channel along which the elastic fluid must pass, in order to arrive at the opposite side of the cylinder, were sufficiently increased. This might be accomplished by increasing the depth of the piston, or by placing on the rim of the piston concentric rings to enter, at the beginning and end of each stroke, corresponding concentric grooves in the covers of the cylinder.

The principle of great depth of piston, as a substitute for packing, had been successfully carried out in the pump which was the subject of this communication. The cylinders, two in number, are twenty inches long and two inches in diameter. The pistons are solid cylinders of iron, ten inches long, fitting as accurately to the cylinders as is consistent with freedom of motion. The depth of each piston, as compared with its diameter, renders the usual guide or parallel motion unnecessary, so that the connecting rod is simply jointed to the top of the piston. Air is readily compressed to sixteen atmospheres, the quantity passing the sides of the cylinders being very trifling.

MICROSCOPICAL SECTION.

Fourth Annual Meeting, 19th May, 1862.

E. W. Binney, F.R.S., F.G.S., V.P. of the Section, in the Chair.

Mr. David Joy, and Mr. John Leigh, M.R.C.S., were elected members of the Section.

The Secretary then read the Annual Report of the Council, which was agreed to and ordered to be printed for circulation amongst the members.

The following gentlemen were then elected officers for the ensuing Session :—

President.
WILLIAM C. WILLIAMSON, F.R.S.

Vice-Presidents.
EDWARD W. BINNEY, F.R.S., F.G.S.
JOSEPH SIDEBOTHAM.
ARTHUR G. LATHAM.

Secretary.
GEORGE MOSLEY.

Treasurer.
JAMES G. LYNDE, M. Inst. C.E., F.G.S.

Of the Council.
THOMAS ALCOCK, M.D.
JOSEPH BAXENDELL, F.R.A.S.
JOHN DALE, F.C.S.
J. W. MACLURE, F.R.G.S.
THOMAS H. NEVILL.
JOHN PARRY.
WILLIAM ROBERTS, M.D.
JOHN WATSON.

It was proposed by Mr. Lynde, seconded by Mr. Nevill, and carried unanimously, that the following alterations be made in the Rules :—

No person shall be eligible for the office of President for more than two years in succession.

The Vice-President who has attended the least number of meetings shall be ineligible for re-election as Vice-President for at least one year.

The two members of the Council who have attended the least number of meetings shall be ineligible for re-election for at least one year.

October 20th, 1862.

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

Mr. George Venables Vernon, F.R.A.S., was elected a member of the Section.

Mrs. BURY, of Croft Lodge, Ambleside, presented, through Professor Williamson, a series of twelve photographic plates, with seventy-two figures of *Polycistins* from drawings by that lady; also seventeen mounted slides of Barbadoes earth.

Mr. HORATIO J. FREMBLY, of Gibraltar, presented, through Mr. H. A. Hurst, thirty-four slides of tongues of mollusca, collected and mounted by himself. An elaborate report upon their scientific classification was read by Dr. THOMAS ALCOCK.

Mr. H. A. HURST presented a collection of tongues of mollusca, from Bengal, made by him during his residence in India. They have been placed in the hands of Dr. Alcock, who has kindly undertaken to examine and report thereupon.

Captain J. C. GALES, of the ship "Quito," presented soundings taken off the coast of Chili and the Falkland Islands; two specimens of anchor mud, from the Falkland Islands; and some of the *Fucus Natans* from the Sargossa Sea, with specimens of its inhabitants, dried in the sun. Captain CURLING, of the P. and O. S. S. "China," presented four soundings from the coasts of Malabar, Yemen, Malacca, &c.; Captain VICKERS, of the "Rosina Claypole," anchor mud from Port Royal and Black River, Jamaica, and a sounding taken off the south coast of Ireland; and Captain SAMUEL FLOOD, of the ship "Pantoleon," anchor mud from Sumatra and Malacca. Two deep Atlantic soundings in 1,730 and 2,220 fathoms, were also thankfully acknowledged.

Mr. Thomas Heelis presented many interesting specimens, collected during his late voyage, amongst which may be named two soundings from the Agulhas Bank, two specimens of Hoogly mud, scales of flying fish, and gulf weed, with minute crustacea and other animals preserved in spirits.

Mr. Joseph Sidebotham presented to every member of the Section a photographed finder for high powers, in case inscribed with the member's name.

Mr. John Dale presented a quantity of desiccated balsam dissolved in chloroform, to be divided amongst the members.

Mr. A. G. Latham presented a mounted spiracle of a mole cricket from Africa, and pointed out the difference from that found in this country.

Mr. E. W. Binney presented a copy of his Paper "On some Fossil Plants showing structure, from the Lower Coal Measures of Lancashire."

A Letter was read from Mr. Thomas D. Toase, of Jamaica, with reference to the animalcule previously described.

Mr. Brothers exhibited some fine specimens of *Stephanoceros.*

Mr. Parry exhibited some photographs of magnified sections of wood.

Mr. W. H. Heys exhibited the peculiar oil glands on the leaf of the *Procranthera Violacea;* stellate hairs on the calyx of the *Deutzia Scabra,* which differ from those on the leaves by having a greater number of rays and a central disc; spines of the *Loasa Coccinea;* and two kinds of spangles upon oak leaves from Beddgelert.

Dr. William Roberts exhibited a mounted specimen of crystals of *Cystin.*

Ordinary Meeting, November 4th, 1862.

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

Mr. Peter Hart was elected an Ordinary Member of the Society.

Dr. Roberts exhibited some microscopic preparations illustrating the effect of a solution of magenta on the blood. The red blood-disks were tinted of a faint rose; and one portion of their outline, in a majority of the corpuscles, appeared more deeply tinted than the remainder. The pale corpuscles were more strongly tinted than the red; and their nuclei were displayed with great clearness, dyed of a magnificent carbuncle-red. A number of the nuclei were seen in the process of division, more or less advanced, and in some cells the partition had issued in the production of two, three, or four distinct secondary nuclei. There was evidence that these secondary nuclei were set free in the blood, and, by subsequent enlargement and change of form and chemical constitution, developed into red blood disks, which would therefore appear to be, as Mr. Wharton Jones first conceived, free cellæform nuclei, and not, according to the current belief, enlarged and altered pale corpuscles.

Mr. Dyer, Vice-President, exhibited a broken screw bolt, $1\frac{1}{8}$in. square (used to fasten a cart body to the axle). The fracture, near the head end, appeared very much like one of cast iron; imbedded in the centre of the bar was a smooth egg-shaped mass about ½in. diameter, crossing the fracture, and leaving a cavity as its mould in the metal on one side. He assumed that faults like this were probably owing to the rapid processes in use for reducing masses from the puddle into bars of wrought iron, whilst the metal was only partially converted to the malleable state, as appeared in this sample of bad iron. The iron in a semi-fluid state, is passed from the furnace through a succession of rollers, without re-heating or faggoting, as were formerly practised, and at once reduced to the sizes required. The improved rolling mills could not, it seemed, insure improved qualities of wrought iron, whilst they afforded temptations to make it far inferior to any that could have been made fifty years ago. Considering the many hazards to which life and property are exposed in travelling by railway and otherwise, from the iron "shuffled off in haste," and found in use in engineering constructions, it becomes important that previous tests should be employed to ascertain the real nature of the iron, so as to leave no question of its being in a safe condition for the purpose intended, and not like this specimen, and like much now-a-days made by pressing the half-converted puddle into marketable shapes.

In connection with the subject of the slow changes which iron undergoes, M. Breguet, of Paris, stated that in their furnace for preparing soft iron, he had observed a remarkable case of crystallisation of wrought iron. One of the furnace bars became brittle, and on breaking a portion of it, he found it to contain a large cubical crystal of iron, each of whose sides measured five millimetres in length. This singular specimen is now in the possession of M. Balard.

A Paper was read "On a certain class of Linear Differential Equations," by the Rev. ROBERT HARLEY, F.R.A.S.

In the *Philosophical Magazine* for May of last year, Mr. Cockle showed, in a Paper entitled, "On Transcendental and Algebraic Solution," that from any algebraic equation of the degree n, whereof the coefficients are functions of a variable, there may be derived a linear differential equation of the order $n-1$, which will be satisfied by any one of the roots of the given algebraic equation. The connexion of this theorem with a certain general process for the solution of algebraic equations led me to consider its application to the form

$$y^n - ny + (n-1)x = 0 \ldots \text{(I)}$$

to which it is known that any equation of the nth degree, when n is not greater than 5, can, by the aid of equations of inferior degrees, be reduced.

In the course of my investigations I was conducted to the conclusion that for all integral values of n between the limits

$$n=2,\ n=5,$$

both inclusive, the linear differential equation, or, as it is proposed to call it, the "differential resolvent," is of the form

$$\left\{a_0 + a_1 x\frac{d}{dx} + a_2 x^2\left(\frac{d}{dx}\right)^2 \cdots + a_{n-1}x^{n-1}\left(\frac{d}{dx}\right)^{n-1}\right\} y$$

$$= a_{n-1}\left(\frac{d}{ax}\right)^{n-1} y,$$

and I completely determined the constants $a_0, a_1, \ldots a_{n-1}$ for all the cases up to and including $n=5$.

I found, moreover, that this result, in itself sufficiently remarkable, might be put under a still more simple and striking form by following a process of transformation proposed by Professor Boole in his Memoir on a General Method in Analysis, which appeared in the *Philosophical Transactions* for 1844, part II. I found in fact that writing ϵ^{θ} for x, and D for $x\frac{d}{dx}$ or $\frac{d}{d\theta}$, the differential resolvent of the trinomial equation (I) may be made to take the form

$$D(D-1)(D-2)\ldots(D-n+2)y-\left(D-\frac{2n-1}{n}\right)\left(D-\frac{3n-2}{n}\right)$$

$$\left(D-\frac{4n-3}{n}\right)\ldots\left(D-\frac{n^2-n+1}{n}\right)\epsilon^{(n-1)\theta}y=0\ \ldots\ (A)$$

the only exception being the case $n=2$, in which the resolvent contains a term independent of y.

Using the ordinary factorial notation, that is to say, representing

$$(u)(u-1)(u-2)\ldots(u-r+1)$$

by $[u]^r$, the form (A) may be written

$$n^{n-1}\left[x\frac{d}{dx}\right]^{n-1}y-(n-1)^{n-1}\left[\frac{n}{n-1}x\frac{d}{dx}-\frac{2n-1}{n-1}\right]^{n-1}x^{n-1}y=0\ \ldots\ (B)$$

In the *Proceedings* of this Society (vol. II., pp. 181—184) for the 4th of February last, I gave, without the details of calculation, the several differential resolvents for the successive cases $n=2, 3, 4, 5$; and these results Mr. Rawson, of Portsmouth, has kindly verified. I gave, also, in the same Paper, the Boolian (symbolical) form of the resolvent for the biquadratic, and this seems to have suggested to Mr. Cayley an investigation, in which he showed, by the aid of Lagrange's theorem, that the equation (B) holds for all values of n. I had the honour of communicating Mr. Cayley's investigation to the Society on the ensuing 18th of February, and an abstract of it appeared at p. 193, vol. II., of the *Proceedings*. The Paper itself will be printed in the forthcoming volume of Memoirs. Before receiving Mr. Cayley's remarkable analysis, I had calculated, and I believe I had also communicated to Mr. Cockle, the Boolian forms of the resolvents for the cases $n=2$ to $n=5$, both inclusive; and these suggested to me the general form (A). (See *Proceedings*, vol. II., pp. 199—201, and pp. 237—241.)

The singular simplicity of these results for the form (I) had an effect inducing me to consider the corresponding form.

$$y^n - ny^{n-1} + (n-1)x = 0 \ldots \text{(II)}$$

to which also any algebraic equation of the nth degree, n being not greater than 5, can, as Mr. Jerrard has shown, be reduced by means of equations of inferior degrees; and by induction I was led to the following general expression for its resolvent, viz.

$$n^{n-1}[(n-1)\mathrm{D}]^{n-1}\,y - (n-1)(n\mathrm{D}-n-1)[n\mathrm{D}-2]^{n-2}\epsilon^{\theta}\,y$$
$$=[n-1]^{n-1}\,\epsilon^{\theta} \ldots \text{(C.)}$$

or, what is the same thing,

$$n^{n-1}\left[(n-1)x\frac{d}{dx}\right]^{n-1}y - (n-1)\left(nx\frac{d}{dx}-n-1\right)\left[nx\frac{d}{dx}-2\right]^{n-2}xy$$
$$=[n-1]^{n-1}\,x \ldots \text{(D)}$$

The particular cases on which these inductions were founded are given in the present Memoir.

Every differential resolvent may be regarded under two distinct aspects. It may be considered either, first, as giving in its complete integration the solution of the algebraic equation from which it has been derived; or, secondly, as itself solvable by means of that equation. In the first aspect I have considered the differential equation (A) in a Paper entitled "On the Theory of the Transcendental Solution of Algebraic Equations," just published in the *Quarterly Journal of Pure and Applied Mathematics*, No. 20. In the second aspect every differential resolvent of an order higher than the second gives us, at least when the dexter of its defining equation vanishes, a new primary form, that is to say, a form not recognised as primary in Professor Boole's theory. And in certain cases in which the dexter does not vanish, a comparatively easy transformation will rid the equation of the dexter term, and the resulting differential equation will be of a new primary form.

A Paper was read, entitled, "Note on Differential Resolvents," by William Spottiswoode, Esq., M.A., F.R.S., &c. Communicated by the Rev. ROBERT HARLEY, F.R.A.S.

The object of this note is, first, to show how the differential resolvent of an equation of any degree may be found by elimination from systems of linear equations. The method is exemplified in the cases of the complete quadratic and the complete cubic; and in the latter case the results of some investigations by Mr. Harley are introduced, whereby the resolvent is freed from an extraneous factor, and reduced to its proper degree (the 10th) in the coefficients of the original cubic.

Ordinary Meeting, November 18th, 1862.

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

It was proposed by Mr. David Morris, and seconded by Mr. Joseph Sidebotham:—That the words "three-fourths" in Rule 6, Section iv., be altered to "two thirds." The motion being put to the meeting, there were 36 for it and 33 against: it was therefore declared to be carried.

Ordinary Meeting, December 2nd, 1862.

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

E. W. Binney, F.R.S., the President, said that in a paper published in Vol. X. (Second Series) of the Society's Memoirs, "On the Drift Deposits found near Blackpool," he had stated, in a note at page 122, that since the paper was written Mr. J. F. Bateman, C.E., F.G.S., had informed him that in making the Hollingworth reservoir near Mottram-in-Longdendale, he had met with the common cockspur shell (*Turritella terebra*) in considerable abundance. During the past summer he had visited the locality alluded to by Mr. Bateman, in company with Mr. Prestwich, F.R.S. After going up to the uppermost part of the reservoir, which is one of those belonging to the Corporation of the City of Manchester, to the point where the goit conveys the water on the east side of the valley, we saw a deposit of brown sandy clay, or till, which had been cut through to the depth of between three and four feet for the purpose of forming the goit. This deposit contained small granite and greenstone pebbles, some rounded and others angular. In it he found a considerable number of shells, some quite entire and others in fragments. He procured and showed to the meeting specimens of *Turritella terebra, Fusus Bamfius, Purpura* *lapillus*, two specimens of *Tellina* and *Cardium edule*. The City Engineer, Mr. J. G. Lynde, F.G.S., had given him the exact height of the spot where the fossils were found at as 568 feet above the level of the Irish sea.

Shells, identical with recent sea shells, have been found at much greater elevations on the mountains of North Wales, but very few so far inland; for the locality where the specimens were met with is full fifty miles in a straight line from the Irish sea, and a greater distance if the water courses of the Etherow and Mersey are followed. Mr. John Taylor has found recent marine shells in the sands of Bredbury and Hyde, which he has described in the Transactions of the Manchester Geological Society, and Mr. Prestwich informed him that he has found similar fossils on the Buxton Road, about three miles from Macclesfield, but the specimens herein described are the first that have been noticed in the deep vallies running up into the sides of the pennine chain.

He further stated that he had found a large mass of greenstone, evidently a travelled rock of the drift period, at the extreme end of one of the tributary vallies of the Tame, in Saddleworth, as high up as New Year's Bridge, near Denshaw Vale. All these facts prove the former presence of the sea (in some cases containing inhabitants similar to those found on our present coasts) high up on the sides of the Cheshire, Yorkshire, and Derbyshire hills at a recent period geologically speaking, and show that many of our deep vallies have not been formed by the streams of water now traversing them, but are chiefly due to the more powerful action of the waters of the ocean, most probably assisted by ice.

A communication "On certain Linear Differential Equations," by James Cockle, Esq., M.A., F.R.A.S., F.C.P.S., &c., was read by the Rev. Robert Harley, F.R.A.S.

At pages 11 to 14 of the Society's *Proceedings* for the 4th of November last, Mr. Harley has made an important contribution to the subject of linear differential equations. I am not in a condition to make a substantial addition to Mr. Harley's formulæ, but the following, derived by a mere change of notation from his, seem to me to give a new interest to his valuable results. Write

$$y^n - ny^{n-r} + rx = 0 \;\ldots\; \text{(III)}$$

$$n^{n-1}\left[(n-r)\mathrm{D}\right]^{n-1} y$$
$$-r^{n-1}\left[n\mathrm{D}-r-1\right]^{n-r-1}\left[\frac{n}{r}\mathrm{D}-\frac{n+r}{r}\right]^{r} e^{r\theta} y = \mathrm{X} \;\ldots\ldots\; \text{(E)}$$

or, which is the same thing,

$$n^{n-1}\left[(n-r)\,x\frac{d}{dx}\right]^{n-1} y - r^{n-1}\left[nx\frac{d}{dx}-r-1\right]^{n-r-1}\left[\frac{n}{r}\,x\frac{d}{dx} - \frac{n+r}{r}\right]^{r} x^{r} y = \mathrm{X} \;\ldots\ldots\; \text{(F)}$$

and, further, suppose that X is equal to ρx^r, where

$$\rho = \frac{1}{n-r}\,\frac{d^{n-1}}{dx^{n-1}}\,x^{n-r}, \text{ or } \rho = (1-1)^{r-1}[n-2]^{n-2}, \text{ or } \rho = \frac{1-1}{r-1}[n-2]^{n-2}$$

or any other discontinuous function of the like nature. Then (E) or (F) is, in the cases $r=n-1$ or $r=1$, the differential resolvent of (III).

Mr. Harley added the following remarks:—

Mr. Cockle's formula, (E) or (F), is interesting and useful, as comprehending the differential resolvents of both the trinomial algebraic equations

$$y^n - ny + (n-1)x = 0$$

and

$$y^n - ny^{n-1} + x = 0$$

under one and the same general expression. For these cases, ($r=1$ and $r=n-1$,) to which the author confines it by express statement, the formula holds good; but I do not find that it holds for other cases. If (*ex. gr.*) we assume

$$n=4 \text{ and } r=2,$$

the equation which in Mr. Cockle's communication is marked (III), becomes

$$y^4 - 4y^2 + 2x = 0 \ldots (a)$$

and the formula (E), after some slight reduction, gives

$$2^4(D)(2D-1)(D-1)y=(4D-3)(2D-3)(D-2)\epsilon^{2\theta}y$$
$$=\epsilon^{2\theta}(4D+5)(2D+1)(D)y;$$

whence

$$2^3(2^2-x^2)x\frac{d^3y}{dx^3}+2(2^3.3-19x^3)\frac{d^2y}{dx^2}-3^3x\frac{dy}{dx}=0,$$

an equation which I find, on trial, is not satisfied by the roots of (α). In fact, the differential resolvent of (α), obtained by direct calculation, is

$$2^4(2-x)x\frac{d^3y}{dx^3}+2^4.3(1-x)\frac{d^2y}{dx^2}-3.5\frac{dy}{dx}=0\ldots(\beta)$$

and the Boolian or symbolical form is

$$2^4[2D]^3y-[4D-3]^3\epsilon^{\theta}y=0,$$

which, expunging a common symbolical factor, may be reduced to the form

$$2^4D(2D-1)y-(4D-3)(4D-5)\epsilon^{\theta}y=0,$$

and returning to the ordinary form of a differential equation, the last result may be written thus:

$$2^4(2-x)x\frac{d^2y}{dx^2}+2^4(1-x)\frac{dy}{dx}+y=0\ldots(\gamma)$$

This equation I have also obtained by direct calculation. It is worth while noticing that (β) and (γ) may be combined in one equation

$$2^4(2-x)x\frac{d^3y}{dx^3}+2^4\left\{3(1-x)+\mu(2-x)x\right\}\frac{d^2y}{dx^2}$$
$$-\left\{3.5-2^4\mu(1-x)\right\}\frac{dy}{dx}+\mu y=0\ldots(\delta)$$

in which μ is a perfectly arbitrary function, either constant or otherwise.

I content myself at present with simply recording these results, without attempting a discussion of them; but I believe they will be considered, by cultivators of the calculus, as not wanting in interest.

In my third communication to the Society, entitled "On the Theory of the Transcendental Solution of Algebraic Equations" (see pp. 237—241 of Vol. II. of the *Proceedings*), I gave Mr. W. H. L. Russell's solution by definite integrals of a certain quartic resolvent, and in connexion therewith I remarked that Professor Boole had pointed out to me a method of solution which did not differ essentially from Mr. Russell's. I have since found that there is an essential difference, inasmuch as Professor Boole's method gives the solution in forms involving single, not (as in Mr. Russell's solution) triple integrals.

Soon after the publication of the Paper above quoted, I received from Mr. Cayley a letter (dated April 29, 1862.) containing some remarks on the subject, which I have pleasure in appending to this communication.

Mr. Cayley to Mr. Harley. "The series for the solution of your resolvent equation should I think be assimilated to the form of the hypergeometric series. If instead of the ordinary notation

$$F(a, \beta, \gamma, x) = 1 + \frac{a.\beta}{1.\gamma}x + \frac{a.a+1.\beta.\beta+1}{1.2.\gamma.\gamma+1}x^2 + \&c.,$$

we write

$$F\begin{pmatrix} a, & \beta \\ 1, & \gamma \end{pmatrix}; x$$

then naturally

$$F\begin{pmatrix} a, & \beta, & \gamma \\ 1, & \delta, & \epsilon \end{pmatrix}; x$$

will denote

$$1 + \frac{a.\beta.\gamma}{1.\delta.\epsilon}x + \frac{a.a+1.\beta.\beta+1.\gamma.\gamma+1}{1.2.\delta.\delta+1.\epsilon.\epsilon+1}x^2 + \&c.$$

and Mr. Russell's series, writing them under the forms

$$1 + \frac{\frac{5}{12}\cdot\frac{2}{12}\cdot\frac{-1}{12}}{1\cdot\frac{2}{3}\cdot\frac{1}{3}}x^3 + \frac{\left(\frac{17}{12}\cdot\frac{5}{12}\right)\left(\frac{14}{12}\cdot\frac{2}{12}\right)\left(\frac{11}{12}\cdot\frac{-1}{12}\right)}{1\cdot 2\cdot\frac{2}{3}\cdot\frac{5}{3}\cdot\frac{4}{3}\cdot\frac{1}{3}}x^6 + \&c.$$

will be

$$F\left[\begin{matrix} -\frac{1}{12}, \frac{2}{12}, \frac{5}{12} \\ \frac{1}{3}, \frac{2}{3}, 1 \end{matrix} ; x^3\right] \qquad xF\left[\begin{matrix} \frac{3}{12}, \frac{6}{12}, \frac{9}{12} \\ \frac{2}{3}, 1, \frac{4}{3} \end{matrix} ; x^3\right]$$

$$x^2F\left[\begin{matrix} \frac{7}{12}, \frac{10}{12}, \frac{13}{12} \\ 1, \frac{4}{3}, \frac{5}{3} \end{matrix} ; x^3\right]$$

There is a little (incidentally) on these ultra-hypergeometric series in Krummer's valuable Memoir on the hypergeometric series, Crelle, t. xv. I attach *no value whatever* to the transformation of the series into definite integrals. As to your own remark 'what is wanted is the solution without the aid of definite integrals'—it seems to me that the *series* is *the solution*, and that what is wanted is a study of these series, so as that they may be considered as known transcendents; which, considering the great extent of the theory of the simplest case, that of the hypergeometric series, is likely to remain a desideratum."

Ordinary Meeting, December 16th, 1862.

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

Dr. JOULE read the following letter addressed to himself:—

"Varagnes, near Annonay, Ardèche, France,
"November 25, 1862.

"Dear Sir,

"I am much obliged to you for the disinterestedness you have maintained in reference to the discovery of the great principle that my uncle, the celebrated Montgolfier, revealed to me some sixty years ago. Since that time I have passed my whole life in studying this question.

"Much merit is due to you, sir, for your defence of another's rights, especially after the compositions which you have published, and are, without doubt, the best explanations yet written of the great principle so continually occupying you, myself, M. Grove, M. Mayer, and many others. Among the latter there are many who endeavour to persuade the public (every one in his interest) that the world owes the great advancement of the dynamical theory of heat solely to their works; but the public do not like being led in this kind of logic, and look with indifference upon the pretended authors of these discoveries, at the same time raising the merit of those, like yourself, who take a pleasure in rendering justice to those who wait patiently until just and impartial men judge them by their works and not by their words.

"For more than sixty years I have studied the question of identity of caloric and movement, and yet have perceived no advancement of this subject; nothing that either has been said or written has helped me towards the conviction I have arrived at. Indeed, almost all the experiments since then have proved this a fact (which formerly no one endeavoured to contest), viz., that immediately movement disappears, heat is produced, reciprocally. We cannot, however, praise too much your studies, and those of the physicists who have followed you, having witnessed the details of this great principle and calculated the different circumstances with the utmost accuracy. All the attempts in giving a plausible explanation of the phenomenon in question have generally conveyed this opinion, that the molecules of bodies, as well as those of ordinary matter, are liable to Newton's attraction, and, consequently, must be separated from each other by spaces infinitely larger than they occupied before; then, they must keep their distances from each other by virtue of laws, if not identical, at least analogous with those which govern the celestial bodies and maintain in fixed boundaries the distances between the planets and the stars, which determine their course.

"Such were the ideas I had, when addressing a letter on this subject to the celebrated and most learned Sir John Herschel, when President of the Royal Astronomical Society, the most distinguished man of our time for his philosophical genius and his wise and judicious manner of treating sciences, well known to me through the connection I have had with him.

"We are indebted, sir, there is not a doubt, to the solid and enlightened spirit that characterises the English learned, for the development made in your country on this interesting question, which occupies the most learned men in the world. I have endeavoured to engage, by numerous papers, the attention of the members of the French Academy of Sciences, my colleagues, upon this important subject, but without avail. But I take the liberty of asking you to communicate this letter to the members of the Institution of which you are a member, and, should I obtain its publication, I hope my studies will be known by all your learned colleagues, and contribute to enlighten the opinion of those who pursue this branch of science.

"The letter I addressed, on the 2nd of April, 1824, to Sir John Herschel has been translated into English, and was sent to Sir David Brewster, who inserted it, on the 24th of

April, 1824, in the 20th number of the *Edinburgh Review* (page 280), in which I expressed an opinion that the cohesion of the bodies constituted on the surface of the earth may be liable to attraction, which will tend to join in their centre of gravity the composed molecules. I also explained the great difficulty I had in attaining this result. Indeed, if the particles of invisible and invaluable bodies could maintain their distances by the same laws as the celestial bodies, the former would necessarily be perceived, for they would be obliged to collect themselves in a symmetrical and regular manner about their centres of gravity, and make their movements apparent to our senses. I have understood since that a law exists, unknown to me, determinating the organised parts of bodies to keep a distance and to preserve the permanent forms they exhibit. Therefore I studied incessantly to solve a question of so great an interest, and in 1836, after twelve years passed in study and meditation, I was fortunate enough to succeed. Indeed, an attentive examination of the manner of action exercised reciprocally by the particles and generally by all the bodies brought this result: If systems of two or several molecules, being comparatively in repose, traverse other systems comparatively in movement, the former separate the latter from another, losing a part of their own movement. Consequently, the question of cohesion was for me implicitly resolved; and, after eleven years' further reflection, I began to publish (in 1848) the series of papers I have read in the Institute, and in which I proved, under the name of Distension, the new result of Newton's attraction applied by myself.

"Then, making myself better acquainted with this law, I published, in 1855, a large volume upon cohesion, and in 1859 another upon the origin and propagation of force, in which I have perfectly explained these principles, trusting to my own proofs and experience. Moreover, I published in 1861, in the form of a letter addressed to M. Tramblay, director of the *Cosmos*, a third paper in reference to the same subject. But yourself, sir, a physicist of the greatest reputation, and who have taken quite a different direction in science not attempted by any other, must know very well how difficult it is, even for those who sincerely wish the triumph of truth, to leave ideas and old practices in which they have grown up. To attain this result we must have confidence in ourselves. New ideas, however clear and judicious they may be, are never adopted immediately, but by degrees, so that those who are occupied with the same question, already explained by their predecessors, fancy themselves the discoverers and authors. After several generations, however, when the tomb is closed above all, historians and commentators will re-establish the truth of the facts and render them due justice.

"Mr. Faraday, your fellow-countryman, gave me that great satisfaction, and his powerful testimony has paid me more than I ever could hope, for the oblivion with which several of my contemporaries wished to cover me, who endeavoured to uphold one another without attaining anything but ridicule, the public always being just and free from prejudice, even if the question is above their intelligence.

"Let us console ourselves, sir, should our efforts rest in obscurity and not be valued as we suppose just; let us be assured that those who will follow us will know how to sift the true from the false, that they will adopt the one and reject the other; and that, if the opinions we have exhibited are just and exact, they will be duly valued one day. But we shall not complain for the present to see our reputation injured by errors we have incurred, perhaps at the first moment, for these errors will have the definitive result of classing us according to merit in the history of science.

"I have the honour to be, with the greatest respect,

"Sir,

"Your most obedient humble Servant,

"SEGUIN, Aîné."

In connection with a conversation on the solutions of different salts with which almost all the waters in England are contaminated, and the practical evils resulting from these impurities to steam-boilers, Mr. Spence stated that various means had been adopted, with only partial success, for preventing incrustation on boilers caused by these impurities. For some years he had adopted a simple plan, which is perfectly successful, both at Goole, where the Aire and Calder Canal has a considerable amount of lime salts, and also at his Newton works, where the water from the Rochdale Canal is not only impregnated with lime salts, but apparently also with sulphate or chloride of iron. The plan is as follows: On the suction part of the water-pipe by which the boiler is supplied, that is, between the force-pump and the canal, a small vessel is placed, capable of containing about two gallons of water; a pipe of half an inch diameter, with a stop cock, communicates from the water-pipe with this vessel; every day the boiler man puts into this vessel 1lb. or 1½lb., as found sufficient, of soda ash, and dissolves it in water in the vessel, and then, when pumping into the boiler, turns the small cock, and in three or four minutes all the solution is taken up and passed through the force-pump into the boiler, and this is daily repeated; the consequence is that not the slightest crust forms on the boilers and no chipping is ever required, the salts being all decomposed and the earthy and metallic bases thrown down as mud, which may be blown off or cleaned out periodically, if the boiler is a flued one. These two boilers are cleaned monthly, and a broom and shovel are the only tools required. The plan is easily adopted, perfectly efficient, and very generally applicable.

Dr. Joule drew attention to the great sacrifice of life by steam-boiler explosions. He believed that, in nearly every instance, rupture took place simply because the iron, by wear or otherwise, had become unable to withstand the ordinary working pressure. Various hypotheses set up to account for explosions were worse than useless, because they diverted attention from the real source of danger. He believed that one of these hypotheses—that which attributed explosions to the introduction of water into a boiler the plates of which are heated in consequence of deficiency of water, was quite inadequate to account for the facts; although weak boilers might be exploded at the moment of starting the engine, in consequence of the swelling of the water through renewed ebullition throwing hot water over the heated plates. The absolute necessity of employing the hydraulic test periodically had been pointed out so frequently that he considered that the neglect of it was highly criminal.

A Paper was read by Mr. THOMAS HOPKINS, M.B.M.S., "On the Influence of the Earth's Rotation on Winds."

In this Paper the Author treated of the opinion generally entertained by writers on the influence of the rotation of the earth on winds that are passing over its surface, and proposed to show that the atmosphere, in passing from polar towards tropical latitudes, is not left behind by the more rapidly revolving parts, as has been generally maintained by meteorologists. In doing this he showed that air, going from certain slowly revolving polar localities to others that revolve more swiftly, is not left behind in the way that has been generally supposed.

Extracts were given from Kaemtz and Herschel's Meteorology, to show that these writers attribute the western direction of the trade winds within the tropics to the increasing rapidity of the rotating surface of the globe over which they successively pass; and the Author maintained that it was a mere assumption, unsupported by evidence, and was proved by numerous facts to be erroneous.

On the eastern side of the tropical Atlantic, in the Gulf of Guinea, air passes towards the east, not only without being left behind by the great velocity of the surface, but with additional speed, as it constitutes a wind blowing briskly towards the east. In like manner western winds blow on the eastern side of the Pacific ocean within both the northern and southern tropics, and must therefore rotate eastward more swiftly than the surface on which the air presses. It was contended that the cause which determined the air constituting these winds to rotate faster than the surface of the globe that was beneath them, ought to be considered that which makes other winds pass over the surface when they do not move with equal rotatory velocity, and are therefore said to be left behind. We cannot presume that there are two separate causes in operation to create these winds — one making them move faster than the surface which supports them, and the other slower. In the Indian ocean also, as well as among the islands of the great East Indian Archipelago, strong evidence on the subject is to be found. Thus the north-western monsoon, and other similar winds, are not left behind by the more rapidly rotating equatorial surface; and there does not appear to be any reason to doubt that they are made to rotate faster than the surface over which they are passing by the same cause which produces all other winds, including those which are said to be produced by being left behind. It was also pointed out that in both the Pacific and Indian oceans, regular season winds blow; in one season eastward, making the air pass in that direction more rapidly than the surface of the globe, and at another season westward, when the air must move less rapidly than the surface revolves, and is therefore left behind; but the rotation of the surface can no more be supposed to retard one of these winds than that it can accelerate the speed of the other. It was contended that the cause of all such winds is to be found in atmospheric vacua, created by condensation of aqueous vapours and consequent heating and expansion of the gases where the heating takes place. This expansion creates a partial vacuum in the atmosphere, into which neighbouring air rushes to restore the disturbed equilibrium of pressure; and it is this rush of air that constitutes all winds, whether they move in a pirection contrary to the earth's rotation and appear to be left behind, or go with it and rotate faster. The atmosphere appears to press on the surface of the globe with a force that causes the air to move with the surface in its rotation, unless its own uniformity of pressure is disturbed, when gravitation comes into action to restore the equilibrium.

PHYSICAL AND MATHEMATICAL SECTION.

November 13th, 1862.

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

A Paper by Mr. THOMAS HEELIS, F.R.A.S., entitled "Notes of Observations on η Argûs," was read.

In this the Author states that from a series of comparisons of this interesting star with others near it, carefully made with the naked eye and with a first-rate opera glass, during a voyage in the South Atlantic and Indian oceans, he constructed the following table: —

DATE.	SHIP'S PLACE.		MAG.	COMPARISON.
1861.				
Decr. 7, 4 a.m.	2°45′N	15°10′W	3	Equal to δ Crucis.
Decr. 9, 4 a.m.	0°44′N	19°58′W	3·5	
Decr. 24, 9.30 p.m.	35°54′S	4°27′W	3.5	
1862.				
Mar. 21, 9.30 p.m.	19°15′N	112°15′E	3	Equal to δ but less than γ Crucis.
June 18, 7 p.m.	7° 1′S	79°20′E	Less than 3	Less than δ Crucis.
June 22	14°48′S	70° 5′E	3	
June 29, 7.30 p.m.	24°47′S	53°56′E	Greater than 3	
July 11, 9 p.m.	35° 3′S	25°14′E	Less than 3	Less than δ Crucis.
July 20, 6.37 p.m.	28°48′S	9°54′E	3	
July 21. 7 p.m.	27°14′S	8°26′E	Less than 3	Less than δ Crucis.
July 24, 8 p.m.	22°21′S	3°17′E	2·5	
July 27, 8 p.m.	19°44′S	0°18′W	3	

A Paper by Mr. THOMAS HEELIS, "On Hydrometric Observations of the Water of the Mediterranean," was read.

From a series of observations, (affected by the observed temperature of the water) made by himself in the Mediterranean, the Author concluded—

That, from the gut of Gibraltar the specific gravity of the water decreases towards the area lying between Sicily and the Morea.

From Cape Malea the specific gravity increases as one proceeds up the Archipelago, the influence of the current from the Black Sea being hardly felt at all to the south of Tenedos.

The creeks and bays of the Greek Islands give water of very high specific gravity, such gravity increasing with the distance from open water and being independent of depth.

In the Mediterranean, water taken from a point 16 feet below the surface always, except in one instance, gave higher hydrometric readings than surface water, and the temperature of water drawn from the lower station was generally higher than that at the surface. In the Atlantic no difference in specific gravity was observed in water taken from the surface and the lower station.

Mr. Baxendell, F.R.A.S., mentioned the following observations made by himself and Mr. Richard Dale, on October 24th, 1862, at Mr. Worthington's Observatory:—

The night was remarkably clear; there was no moon, nor any decided indication of aurora in the north; but there was so much light in the atmosphere that the country around could be distinctly seen, and houses and hedgerows at a distance clearly perceived. This apparent luminosity of the atmosphere continued for two hours, and the night then became very dark, but still remained very clear. Mr. Baxendell afterwards noticed that objects at a distance were seen much more distinctly on this occasion than when the moon was seven days old.

MICROSCOPICAL SECTION.

November 17th, 1862.

J. G. Lynde, F.G.S., M. Inst. C.E., in the Chair.

Capt. Randall, late of the barque "Brazil," forwarded eight soundings, taken on the north coast of the Brazils.

Mr. Thos. Heelis presented a specimen of the *Echeneis Remora*, or sucking fish.

Mr. J. Parry presented a number of cells and rings in cardboard; they were very smooth and sharply cut, without the bur usually produced by punching out cells. Mr. Parry explained they were cut in a lathe, twenty to thirty together, the outside cuttings only presenting an appreciable bur.

Dr. Roberts called attention to the aid that might be received in the examination of the structure of animal and vegetable tissue by the use of colouring materials. Magenta is peculiarly adapted for this purpose, in consequence of its solubility in simple water and its inert chemical character. The nuclear structures of animal cells are deeply tinted by magenta, and by its use the nuclei of the pale blood corpuscles, of pus globules, of the renal and hepatic cells, and of all epithelial structures, are brought out in great beauty, tinted of a bright carbuncle red. The *red* blood discs are tinted of a faint rose colour, and a darker red speck, not hitherto noticed, is to be observed on the periphery of the corpuscle; it undergoes some changes when treated with tannin and subsequently with caustic potash, but this point is still under investigation.

Dr. Roberts exhibited drawings and mounted specimens to illustrate his views.

Mr. John Leigh, M.R.C.S., exhibited a case of microscopical dissecting instruments, by Messrs. Wood, of Manchester, which were highly approved of for completeness and finish.

Mr. Thos. H. Nevill exhibited, with dark ground illumination, some fine specimens of *Conochilus volvox*.

Ordinary Meeting, December 30th, 1862.

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

Mr. Samuel Ogden was elected an Ordinary Member of the Society.

Mr. T. T. Wilkinson, F.R.A.S., said that, in a Paper "On the Burnley Coal Field," prepared by Mr. Joseph Whitaker, of Burnley, and himself, and read before the Geological Section of the British Association, at Manchester, they gave a sketch of the principal mines in this district, but they purposely omitted all mention of those whose thickness did not exceed one foot. There are, however, one or two of these thinner mines which may hereafter become worthy of notice, and hence he offered the present note as an addition to Mr. Hull's valuable synopsis in pp. 133-4 of his "Coal Fields of Great Britain."

SECTION OF STRATA NEAR WORSTHORNE, BURNLEY.

Strata, composed principally of blue clay, followed by light metals	102	feet.
1. *Coal*, the "China Bed"	2	,,
Strata, consisting chiefly of grey rag and metals	39	,,
2. *Coal*, the bed not named, and overlaid by about *three inches* of cannel, together.............	$1\frac{1}{3}$	,,
Strata, composed mainly of dark rag and metal seating................................	73	,,
3. *Coal*, the "Danby Bed"	$2\frac{3}{4}$	,,
Strata, consisting of rag, *light blue rock*, metals, and black shales.........................	126	,,
4. *Coal*, the Arley, or Habergham mine	$4\frac{1}{2}$	,,

The bed (2) is the one which has been hitherto omitted; and, if of no other value at present, it may be useful for co-ordination and identification with the seams of coal in other localities.

A Paper was read by EDWARD HULL, B.A., F.G.S., "On the New Red Sandstone and Permian Formations as Sources of Water Supply for Towns."

The Paper commenced by pointing to the advantage enjoyed by most of the large towns of the central counties from their geological position, when built on the new red sandstone. First, from being in proximity to coal; second, from having a dry foundation; third, from having easy access to building stone; and, fourth, from the fact of their resting upon natural reservoirs of water stored up in the sandstone itself. The Author considered that many of these towns had not taken full advantage of this last-named source of water supply, partly from a distrust of the resources of the rock, and partly from the failure sometimes sustained in consequence of the positions for the wells having been selected without a proper regard to the geological structure of the country.

The excellence of the new red sandstone and the lower permian sandstone as sources of water supply, was shown to depend upon three qualities. 1. Their porousness; 2. Homogeneity, or uniformity of structure and composition; 3. Filtering powers. Each of these were treated of in detail, and with examples from several wells in Lancashire and Cheshire. The Author then referred to the failure of the attempts to obtain fresh water at Rugby, and a sufficient supply at Wolverhampton; and he maintained that an abundant supply might have been found at the latter town had the position for the well been selected with due regard to the geological structure of the country.

The Author then proceeded to lay down certain rules of general application for the selection of proper sites for wells, and illustrated the subject by reference to a well now being sunk under his direction at Whitmore, for the supply of the railway works and town of Crewe. The position of the well is in a *trough*, both geographically and topographically, and on a four-inch bore hole being sunk to a depth of 148 feet the water ascended to the surface with a head of four feet, and has continued to flow without diminution for the last six months. The well, which is being made within 100 yards of the bore hole, has only reached a depth of 60 feet, but already yields 250,000 gallons per day.

In conclusion, the Author expressed his conviction that the question of water supply from the strata was becoming every day more a geological one, just as was the discovery of coal beneath the formations which overlie the coal-measures.

Ordinary Meeting, January 13, 1863.

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

The President said:—In a very valuable work lately published by that eminent geologist Dr. Geinitz, of Dresden, entitled, "Dyas, or the Magnesian Limestone Formation and the Lower New Red Sandstone," the Author, whom I have had the pleasure of accompanying over some of the permian deposits of South Lancashire, has done me the honour to allude to two Papers of mine, "On the Permian Deposits of the North-West of England," printed in Vols. XII. and XIV., Second Series, of the Society's Memoirs.

At page 313 of the above work the Author says, "Through Mr. Binney we have become acquainted with true Rothliegende, and indeed of its upper portion in the region of the lower red sandstone of the north-west of England. That accurate observer mentioned to me that the reddish gray sandstone underlying it, and which is very similar to the lower red sandstone of the north-east of England, contains plants of the coal measures, and that it occupies even a lower position than certain limestones of the coal measures which are rich in ichthyolites. I have myself seen from the reddish sandstone of Astley, near Manchester, *Calamites approximatus* Schl., and *Calamites Suckowi* Brongn.; from the red shales at Ardwick, Manchester, *Sagenaria dichotoma* Sternb., *Calamites Suckowi* Brng., *Sphenopteris irregularis* Sternb., *Sphenopteris coralloides* Gutb., *Dictyopteris neuropteroides* Gutb., *Cyatheites villosus* Brmg., *Cyatheites oreopteroides* Göpp., and *Alethopteris lonchitides* Sternb., all true coal plants."*

No one more than the learned Author, I am sure, would like any mistake to be corrected. Now, in the statement that "the reddish gray sandstone underlying it (the true Rothliegende), and which is very similar to the lower red sandstone of the north-east of England, contains plants of the coal measures," the Doctor is quite correct; but when he proceeds to state "that it (the true Rothliegende) occupies even a lower position than certain limestones of the coal measures which are rich in ichthyolites," the Author has misunderstood me by apparently confounding the ribbon beds of limestone in red marls lying above the pebbly beds at Astley with the upper carboniferous limestones at Ardwick, and then stating that such pebble beds occupy an inferior position to the latter. It is certain that the Astley pebble beds containing coal plants occupy a higher geological position than the red shales which the Doctor truly states also contain true coal plants. How much higher it is impossible to say, as it is most probable there are higher carboniferous strata than those yet seen at Ardwick, and there may be permian strata lower than those up to this time met with at Astley — at present we cannot tell.

As the permian group of strata is now occupying the attention of geologists both in England and on the Continent, the following table of the beds in the north-west of England, with their approximate thicknesses, as seen at Shawk, west of Carlisle, Westhouse, south of Kirkby Lonsdale, and Manchester, in the descending order, may be acceptable.

* For the translation of this extract I am indebted to the kindness of Mr. Ekman.

	SHAWK.	WESTHOUSE.	MANCHESTER.
	Feet.	Feet.	Feet.
1.*Laminated and fine grained red sandstones	300	not seen.	not seen.
2. Red and variegated marls containing sometimes, but not always, beds of limestone and gypsum, with fossil shells of the genera *Schizodus*, *Bakevellia*, &c, ...	150	traces of them seen.	300
3. Conglomerate......	4	300	50
4. Lower new red sandstone, generally soft and incoherent......	7	500	500
5. Red shaly clays	not seen	250	not seen.
6. Astley pebble beds, containing common coal plants, termed by me lower permian	not seen.	not seen.	60

Mr. SPENCE brought under the notice of the society a specimen of the metal magnesium. This metal has hitherto been produced only in small quantities, by means of sodium. The specimen shown derived its interest from the fact that it is stated to have been produced by direct reduction, either from the oxide or some of its salts.

If this information, which has been obtained through a friend of the inventor, proves correct, it is a matter of considerable importance, as the metal has most valuable qualities.

Professor ROSCOE read the following extract from a letter which he had just received from Professor Bunsen, respecting the properties of metallic rubidium:—"I have prepared metallic rubidium by reduction with carbon in an iron vessel; from 75 grammes of the bitartrate of rubidium I obtained 5 grammes of the metal in coherent masses, one of which

* The first four strata of the above series, Professor Harkness, F.R.S., in a fine natural section seen at Hilton Beck, north of Brough, estimates to be of 3000 feet in thickness.—*Quarterly Journal of the Geological Society* for August, 1862.

weighed 3 grammes. Rubidium closely resembles potassium in its properties, its vapour possesses a greenish blue colour, the specific gravity of the metal is 1·65, it is more electro-positive than potassium, and it melts at the very low temperature of 38·5° C."

In reference to the age of the New South Wales coalfield, Mr. EDWARD HULL stated that he had received letters from the Rev. W. B. Clarke, who has for many years been engaged in its exploration, and from Mr. John Mackenzie, who has had considerable experience as a mining surveyor in Wigan and North Wales. It is well known that Mr. Clarke maintains the Palæozoic age of the carbonaceous deposits of New South Wales, in opposition to Professor McCoy, who holds that they are of more recent formation (Mesozoic). As Professor McCoy has never actually visited the New South Wales coalfield, and derives his information from cabinet specimens, men of science will probably prefer the evidence of one who has spent years in personally exploring and collecting from the beds themselves. In Mr. Clarke's Memoir on the "Recent Geological Discoveries in Australasia" (2nd edit.), the author defends his view of the Palæozoic age of the coal-bearing strata; and in the letter from Mr. Mackenzie, the writer gives the following series of fossiliferous strata *overlying* the coal and cannel belonging to the Hon. B. Russell, which, if correct (as there is every reason for supposing), ought to set the question at rest in favour of the true Carboniferous age of the coal-measures. He states—"In a pit above this coal are strata with *Fenestella, Stenopora, Orthonota costata, Spirifer, Producta, Terebratula,* &c. In a pit about 100 feet below the same coal occur *Spirifer, Producta, Conularia,* and vegetable impressions; about 60 feet lower, *Spirifer, Terebratula, Pleurotomaria, Stenopora*; and similar shells, accompanied by vegetable remains, are stated to occur still lower. Mr. Mackenzie promises to send specimens of *Lepidodendron* and *Sigillaria* from the same beds.

Dr. Joule described a peculiar kind of mirage which he had witnessed from the northern extremity of Douglas Bay, Isle of Man, and at an elevation of 20 feet above the surface of the water. At about 9 a.m. a number of chimneys in the town were fired, and the products of combustion were driven out to sea by a very gentle breeze. Presently the wind changed and drove the smoke at right angles to its former track. The steamer for Liverpool had in the meantime attained a distance of about three miles, and, although somewhat obscured, was distinctly visible through the smoke. Noticing something extraordinary in her appearance, he viewed her through a telescope, and then observed that nearly the whole of the hull was obscured by the horizon, the uppermost part of the paddle-boxes, the bowsprit, and tafferel being alone visible, whilst at the same time the masts and funnel appeared considerably elongated. Quarter of an hour afterwards, when the smoke had cleared away and the steamer was about seven miles distant, the hull was seen as usual, quite unobscured by the horizon. Dr. Joule attributed the phenomenon to a stratum of highly refractive gases, lying about 20 feet above the level of the sea and gradually diluted and diminished in extent on its upper and lower sides, so as to produce the effect of a convex lens.

Dr. Angus Smith said: I saw once a very remarkable instance of the diversion of the rays of light from the straight line. Although fogs are common on our hills, I have met no instance of similar exaggeration of effect among us. I went with some friends up Skiddaw, and near the top entered a cloud which prevented us from seeing many yards before us, although it was not extremely dark. When moving over those loose stones which form a highway to the summit for a considerable distance, we observed a building which appeared to us about fourteen feet high. The side towards us seemed to be a wall nearly square, and we took it for granted that the top of the hill was attained and that the foremost of the party would be found there; but as we approached the building sank, and in the course of a few steps it went downwards until we found that there were only three layers of stones not very thick, and the whole under two feet high. The disappearance must have occurred within the space of twenty feet; my present impression is that it was less. Further on we attained the man at the summit, and the cloud became denser; we could not see many feet, and I did not think it safe to move from the spot for some time, not knowing the locality. Sitting there, I saw four perpendicular lines on the cloud; I looked up and found that they terminated in the body of an animal which was moving slowly towards us. The distance could not be measured, except by the number of steps taken by the animal, and from these I concluded that when first seen it was from twelve to twenty feet from us; I looked on twelve as being most probable. I do not exaggerate when I say that the height was in appearance thirty feet. Wondering what was to be the end of this strange vision, I called the attention of all, but the animal diminished in size so rapidly that only one or two, who instantly attended to my call, could perceive the monstrousness of the exaggeration of form now presented by a moderately-sized pointer. On coming up to the more advanced portion of our party, they were enlarged and distorted, as we often hear described; but such effects are comparatively common, and they are not to be compared with the two instances mentioned. The first was seen with my face towards the sun nearly. It was about midday. The second phenomenon was seen twenty minutes later, when looking nearly north.

Although I have lived much in sight of Morven, where one might expect to see the "spirit of the mist on the hills," I had not seen anything similar before, nor have I gained from the shepherds that such things are common. Perhaps if they were common, Ossian's ghosts would be less imposing.

MICROSCOPICAL SECTION.

15th December, 1862.

Mr. J. G. Lynde, F.G.S., M. Inst. C. E., in the chair.

Mr. Alfred Fryer was elected a member of the Section.

Captain Moodie, of the R.M.S.S. "Canada," presented two soundings taken off Nova Scotia. Captain Thomas Millard, of the barque "First of May," presented specimens of anchor mud from Montego Bay and the harbours of Kingston and Port Royal, Jamaica; also a sounding from the banks of Newfoundland.

The Chairman stated that he was led to pursue Dr. Roberts's suggestions on the use of magenta dye in examining tissues. From experiments made since the last meeting of the Section, he finds that the dye has no power to colour living tissue, whether animal or vegetable, but that as soon as life is extinct the action of the dye commences. He is continuing the experiments, which are of a most interesting character, and he hopes to lay the result before the next meeting of the Section.

Mr. Leigh considered it probable that so long as vital action continued, ordinary endosmosis could not take place.

Mr. Mosley said that Mr. Hepworth had frequently tried magenta for injections, and the results were not satisfactory, as the colouring matter diffuses itself through the whole of the tissues, giving an appearance of dyed flesh rather than that of injected preparations. This appears to confirm the preceding observations, and to account for the accumulation of colour where the integument is thickest, by means of which Dr. Roberts discovered the spot on the red blood discs, as announced at the previous meeting.

Mr. Leigh drew the attention of the Section to the adulteration of size as a cause of mildew in cotton goods.

Mr. Watson named the investigations made by Mr. Thompson about twenty-five years ago, as to the cause of mildew in madder purple-printed cottons shipped to hot climates. It was attributed to the starch employed in finishing the goods, which, acted upon by moisture, heat, and pressure, had given rise to an organic acid which discharged the colour.

Mr. HURST described his experience of mildew on printed cottons and upon dyed fustians, at Gibraltar and Calcutta. In most cases it appeared in spots and round patches, which affected the colours. On the fustians, he had no doubt it was caused by the growth of a fungus, as the surface of the spots was sensibly raised.

Mr. MOSLEY considered there might be several kinds of mildew; that upon the fustians might be attributed to the bone size with which those goods were generally finished, and known by the characteristic smell. Mr. Mosley also exhibited a pattern of grey calico, which had become discoloured and quite rotten in irregular patches, from mildew; it had lain for some time in a damp place, under pressure; there was this peculiarity about it, that the coloured patches whilst damp were quite tender, but on exposure to the air and drying, the cloth had recovered its strength.

Mr. HEYS remarked that twenty years ago he was engaged in the manufacture of fine muslins; it was usual to soak the weft in soap suds to facilitate the weaving; and it was found the cloth was most liable to mildew during hot, close, summer weather, and the greater the quantity of goods heaped together the more rapidly would mildew set in. The flour from which the size was made was always the best that could be purchased.

Mr. HEYS exhibited mounted specimens of the fibres of the Zostera Marina, and stated that, as the fibre is considerably finer than the finest Sea Island cotton, it might probably be of use in the manufacture of fine muslins for ladies' dresses, if possible to obtain a supply, and separate the fibre from the plant by machinery. Mr. Heys also exhibited mounted specimens of Queensland cotton, lately sold at five shillings per pound; the fibre is very regular in size and much more cylindrical than other cottons; also several specimens from ripe and unripe pods, and Mr. Heys expressed the opinion that great advantage would arise from a regular and careful examination of cotton fibre taken fresh from the plant through every stage of growth.

Ordinary Meeting, January 27th, 1863.

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

The President said that during the last few years the MS. papers of the late Mr. Sturgeon, the electrician, and Professor Eaton Hodgkinson, F.R.S., had been presented to the Society, and were now deposited in its archives. The Council had resolved to collect, as far as possible, all MSS. by the late illustrious President of the Society, Dr. Dalton. He thought that this Society was the most proper depository for the scientific papers or correspondence of any of its deceased members. As it was probable that members and other parties might possess papers and correspondence of the late Dr. Dalton, he hoped that they would be induced to present them to the Society. He was sure that their excellent Librarian would devote great care in cataloguing and arranging the papers, and that they could not be in better custody than in the Society with which that illustrious philosopher had been connected for so long a period of his life.

Mr. Baxendell, referring to the nebula discovered by Mr. Hind, in 1852, in the constellation *Taurus*, but which has since disappeared; and to the variable star near its north following edge, stated that he had lately found that another star, distant only about nineteen minutes of arc from the place of the nebula, was also variable. His observations of the brightness of this star were as follow :—

1862.—March 19.	=	10·4	magnitude.
October 18.	=	10·1	———
November 15.	=	9·9	———
December 26.	=	9·6	———
1863.—January 23.	=	9·2	———

These numbers show an apparently progressive increase of brightness; but it is not improbable that a minimum occurred during the summer months, at which time no observations could be made, owing to the star's proximity to the sun; and therefore the actual range of variation may have been greater than that indicated by these observations. This star is No. 705, Zone + 19°, of Professor Argelander's *Bonner Sternverzeichniss*, where its magnitude is stated to be 9·4. Its approximate mean place for 1860·0 is

R.A. 4h. 13m. 40s. Declination north, 19° 28′·9.

According to Professor Argelander's system of nomenclature this variable will be called U *Tauri*.

Mr. Baxendell also communicated the following observations of the variable star near the edge of the nebula (T *Tauri*) :—

1862.—March 19.	T =	10·3	magnitude.
October 18.	below	12·5	———
November 15.	=	12·6	———
" 16.	=	12·6	———
December 26.	=	12·7	———
1863.—January 23.	=	12·1	———

From the last observation it would appear that T *Tauri*, after being at a minimum for some time, is now brightening up again; and judging from the course of its changes in the early part of last year, a maximum may be expected to occur within the next two months.

A Paper, by Mr. Thos. Heelis, "On Swell Observed at Sea, particularly in the Regions of the South-east Trade Wind," was then read, in which the Author, after examining the causes of swell and giving instances and especially alluding to the rollers at Ascension, St. Helena, and Tristan d'Acunha, gave some instances of the observed succession and order of the undulations in altitude and of the speed of translation of the crests, which in one instance he had found to attain twenty-two miles per hour; and, after setting forth a table of different instances of swell chiefly in the Atlantic and Indian Oceans, and Bay of Bengal, addressed himself to the consideration of the swell which is frequently observed in tropical latitudes in the southern hemisphere to roll up from the southward. This he attributed to the action of currents setting especially in the Indian Ocean from the neighbourhood of the southern tropic, towards the great connecting current which extends from the Cape of Good Hope to Australia. The Author gave instances of these currents experienced in

35°47′ S.	79°10′ E.	running	south	27	miles in	24 hours.
28°22′ S.	81°13′ E.	,,	,,	27	,,	,,
26°42′ S.	81°45′ E.	,,	,,	23	,,	,,
23°38′ S.	83°10′ E.	,,	S. 50° E.	42	,,	,,

and called attention to the difference between them as actually experienced, and the set of the water as laid down upon general current charts, the peculiar circulation which they shewed to exist at times in the Indian Ocean, and the meteorological disturbances which the causes which set such currents in motion cannot fail to originate.

Ordinary Meeting, February 10th, 1863.

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

Mr. G. Stanley Darbishire was elected an Ordinary Member of the Society.

Among other donations which received the thanks of the Society, Mr. Parry presented a volume of MS. Lectures on Chemistry, by Mr. Thomas Henry, one of the founders of the Society, and the father of the late Dr. William Henry, the celebrated chemist.

Dr. Joule described a barometer for measuring small atmospheric disturbances. It consists of a large carboy connected by a glass tube with a miniature gasometer formed by inverting a small platinum crucible over a small vessel of water. The crucible is attached to the short end of a finely suspended lever multiplying its motion six times. When the apparatus was raised two feet the index moved through one inch; hence he was able in serene weather to observe the effect corresponding to the elevation of less than one inch. The barometer is placed in a building, the slated roof of which affords, without perceptible draught, free communication with the external atmosphere. In this situation it was found that the slightest wind caused the index to oscillate, a gale occasioning oscillations of two inches, an increase of pressure being generally observed when the gusts took place.

Dr. Clay exhibited a specimen of the Snake Nut, the fruit of the *Ophiocaryon paradoxum (Schomb.)*. Nat. ord.

Sapindaceæ, from British Guiana. For a description of the Snake-Nut Tree, see Ann. Nat. Hist., vol. v., p. 202 (1840).

Mr. Joseph Sidebotham exhibited a cornelian pebble with a cavity containing a globule of water, brought from the coast of Tuscany. These pebbles are often picked up on the shore, and when broken exhibit a crystalline cavity about one-third filled with water.

Mr. Dyer read a Paper entitled "Notes on the Introduction of Steam Navigation."

Mr. Dyer stated that this subject, being of great importance, had engaged many able pens in tracing the origin of the several inventions and experiments that preceded the final triumph of steam power over that of wind for navigating ships; each writer claiming the honor of priority for his own country. It may be useful to state the order in which, and the parties by whom the principal attempts were made to realise that object. Several letters lately appeared in the *Times*, and were thence transferred to the pages of the *Engineer*, giving a graphic account of the "first steamer in English waters," the "*Margery*, built at Dumbarton, by the late William Denny, for William Anderson, of Glasgow, and passed through the canal to the Forth and thence to the Thames, where she arrived on the 23rd January, 1815." On the authority of Mr. Anderson, then, this date is fixed when the first steamboat was seen on English waters. The first steamboat, the *Claremont*, was started as a regular packet on the Hudson River, in the spring of 1807; so that the first steamer seen on the American waters was fifty-five years ago, a lapse of time that should now insure a calm view of the steps that led to this first actual success in steam navigation. It will be shown that, by a long course of persevering labours, the honor of that success must be conceded to Robert Fulton, by whom it was achieved.

Whilst admitting the merits of other ingenious men long engaged in the same pursuit, it is clearly proved that, either from good fortune, or by the exercise of superior judgment and skill, the race was won by eight years' priority of steam navigation, by Fulton, on the Hudson River.

In 1793, Mr. Fulton sent his plan for a steamboat to Lord Stanhope, who approved of, and thanked him for the communication. Shortly after, Fulton went to Paris, and made experiments on the French waters, with the chain floats, the duck's-foot paddles, the screw or smoke-jack propellers, and with the paddle wheels, to which latter he gave the preference, and constructed a boat with them in 1803, which was the model adopted in building the *Claremont* in 1806.

Mr. Dyer had sailed in the *Claremont*, and remembers the sensation created by her appearance, and the high admiration bestowed on the author of so great an enterprise. That sensation in 1807 was precisely the same as the *Margery* created among the vessels on the Thames in 1815.

All attempts at steam navigation were fruitless before the invention of Mr. Watt's steam-engine, his engine being the *first* that could be usefully applied to rotative machines on *land*, and therefore for propelling ships. The principal claims put forth by other inventors of steamboats, are the following:—

In France, the Marquis de Jauffroy constructed a steamboat at Lyons, in 1782, "with paddle wheels," but that this boat did not succeed is obvious, because she was not heard of until 1816, when the first Fulton boat was started to run on the Seine.

In 1783, Daniel Bernoulli proposed a plan which consisted of forcing water through a tube, out at the stern of the boat. This scheme has been tried many times since, but fails on account of the defective principle of applying the force. *Endless chains,* with float propellers, have been many times tried and have failed on the same ground.

In 1795, Lord Stanhope made experiments with a boat on the Thames, using the reciprocating or "duck's foot" paddles, which also failed, from the loss of time and power by the return stroke.

In 1785, James Rumsey, of Virginia, tried a boat on the Potomac, and afterwards in London, both without success; and about the same time Mr. Fitch, of Philadelphia, tried one with paddle wheels, on the Delaware, but this boat also did not succeed and was given up as a failure. J. C. Stephens, of New York, made experiments in 1804, with a "boat 25 feet long and 5 feet wide," which of course did no good and was stopped as a failure, though again brought to notice as preceding Mr. Fulton's.

In 1788 and 1789, William Symington, in conjunction with Patrick Millar and James Taylor, made experiments with their patents for navigating by steam, and in 1802 commenced running a boat on the canal at Glasgow which made three miles an hour, but after many changes of her propellers and trials, the scheme was given up and no more was heard of the steamboat of Mr. Symington until long after those of Fulton were widely spread over the American waters.

In 1816, the Marquis de Jauffroy complained that the Fulton steamboat on the Seine had taken the "paddle wheels" invented by him and used at Lyons thirty-four years before, but also abandoned by him. To this charge Mons. Royou replied in the *Journal des Debats,* thus:—"It is not concerning an invention, but the means of applying a power already known. Fulton never pretended to be an inventor in regard to steamboats in any other sense. The application of steam to navigation had been thought of by all artists, but the means of applying it were wanting, and Fulton furnished them."

The first ocean steamer was *The Fulton*, of 327 tons, built in 1813, and the first steamer for harbour defence was built under Fulton's direction, 2,470 tons, launched in 1814. This became the model ship for the iron-clad batteries and rams since constructed with many changes. It will be seen by the drawings of Fulton's plans, that he had tried the several other kinds of propellers—the chain float, duck's foot, and the screw fan—before adopting the paddle wheel, for though the *screw* was good in principle, it was many years before it could be constructed to act efficiently. The *James Watt,* was the first boat with the screw running between London and Havre, about ten years after the advent of *The Margery.*

In 1811 I endeavoured to introduce steam navigation into England, but I found a strong conviction that it would not answer in this country, our most eminent engineers saying, "We don't doubt the success of steam-boats in the wide rivers and harbours of America, but in our comparatively small rivers and crowded harbours they will never answer." Even such scientific engineers as the late John Rennie, sen., and Peter Ewart, a Vice-President of this Society, both advised me to relinquish the attempt to introduce steamboats, as sure to prove a waste of time and money to no purpose. However, when conviction came over the public mind that steam navigation would answer here—but not until after more than 5,000 tons of steamboats had been launched on the Hudson in 1816, did it so come—then began the spread of steam navigation, since extended with such marvellous rapidity and perfection as to atone for the sluggish beginning. Since nations are indebted to the genius of Watt for success in using steam power, to that of Fulton for its successful application to navigation, to Stephenson for the like success on railways, the meed of praise due to each of their names should be cheerfully awarded by all who are so largely benefited by the result of their labours. In doing this we should bear in mind, that *inventions* do not spring into existence perfect from their birth, like Pallas from the brain of Jupiter, but they come from the prior labours of many brains, and he is the true inventor who first collects the essence of and gives the stamp of vitality to those labours. In this sense the invention of steam navigation will for ever illustrate the name of Robert Fulton.

MICROSCOPICAL SECTION.

19th January, 1863.

Mr. Joseph Sidebotham, Vice-President of the Section, in the Chair.

Captain Isaac Tessyman, of the ship "Ann Mary," presented four soundings off the coasts of Patagonia, Singin, Malay Peninsula, and at Algoa Bay, taken during his late voyage to San Francisco, Singapore, &c.

Captain J. B. Husband, of the ship "Mutlah," presented three soundings taken off Orissa, and the Black Pagoda on the coast of Bengal.

Mr. Latham presented mounted slides of the skin of the Muræna Guttata, cocoon of silk worm, and cuticle of Gladiolus.

Mr. John Slagg, jun., presented mounted specimens of floats and ovaries of Ianthina; shells of ditto; berry of Fucus natans covered with Membranifera; and Cresia aciculata, a Pteropod.

Mr. JOHN HEPWORTH referred to the mildew mentioned in the proceedings of the previous meeting, which had been observed at Gibraltar on fustians stiffened with bone size, and stated that he had noticed a peculiar fungoid growth upon mounted sections of bone, and that it would be desirable if possible to compare them.

Mr. JOHN LEIGH, M.R.C.S., read a Paper "On the Use of Dialysis in Microscopical Investigations."

After describing the researches of Professor Graham, the present Master of the Mint, and explaining the nature of the division into two classes of all natural bodies, namely, into crystalloids and colloids, their affinities and means of separation, the Author proceeded to describe some curious bodies found in cleaning out a steam boiler, which have almost exactly the external form and internal structure of the concretions formed by Mr. Rainey, and figured in Dr. Carpenter's work on the Microscope, third edition, page 769. They are composed chiefly of carbonate of lime and organic matter aggregated in the presence of the aluminous colloidal mud in the boiler, and are, on a large scale, a singular illustration of Mr. Rainey's experiments. The Author then enlarged upon the advantages of this method of investigation to the microscopist and chemist, who may go hand in hand in the examination of the crystalloidal constituents of organic bodies. "The microscopist (says the Author) will often be able to direct his fellow worker into new channels of research. A careful study of minute crystals, with accurate measurements of their angles and observations on the effects of polarised light, may, to speak medically, lead to an accurate diagnosis of them, as is afforded to the tests of the chemist, to whose larger operations they may be referred for further analysis. Dialysis affords us the means of separating the saline constituents of the juices of plants from the salts fixed in their tissues, and similarly in regard to animal bodies. By one or more dialytic operations on a limited scale the crystalloids of any vegetable juice may be obtained in solution of great limpidity, and by careful evaporation over a water bath they will crystallise out in a state fit for examination." Mr. Leigh concluded his Paper by the quotation of some apposite remarks by the late Professor Johnstone, of Durham, and exhibited the small trays he uses in his experiments, consisting of a double rim of gutta percha securing a disc of parchment paper in the form of a sieve; also specimens of the mulberry-shaped nodules found in a steam boiler as before named.

The Chairman said that for minute experiments he had used the parchment paper in the form of a filter.

Mr. Dancer stated that porous earthenware could be advantageously used as a dialyser.

Professor Williamson indicated a number of subjects upon which dialysis would probably throw light, both in vegetable and animal physiology. He especially dwelt upon the phenomena of calcification and silification, illustrating his remarks by reference to what occurs in the formation of calcareous and silicious growths in the colloid sarcode of sponges and polypifera, in the development of the dental plates of the teeth of Echinus, in the calcification of the derms of the crustacea, the shells of mollusca, the scales of fishes, and in the chondriform and membraniform bones and teeth of the vertebrate animals. The Professor suggested that a natural process of dialysis probably underlay all these formations. He specially called attention to the close resemblance subsisting between the primary spherical and concentric granules seen in the derms of the crustacea, in the scales of cycloid and ctenoid fishes, in the outermost layers of many teeth, and the artificial concretions produced by Mr. Rainey, to which Mr. Leigh alluded in his Paper. Professor Williamson further suggested for inquiry, how far the structureless basement membrane seen underlying the calcareous layer of many calcified structures (*e.g.* the pulp membrane of the tooth) played some part equivalent to the parchment dialyser of the Master of the Mint.

Mr. Mosley read extracts from a Report to the Cotton Supply Association, of a microscopical examination of a sample of cotton supposed to have some peculiarities. On comparison with good American cotton it was found to contain a greater proportion of round and partially flattened filaments, all more or less twisted, but full and well developed; the polarised colours were more bright and vivid, all indicative, he considered, of strong and vigorous growth in a congenial soil, and careful gathering when the pod was at its highest stage of development. The fibres varied in size, from flattened ribbons of $\frac{1}{80}$ of an inch broad to cylindrical fibres of $\frac{1}{1500}$ of an inch in diameter; the variation being due mainly to the amount of compression of the cylinder rather than to actual difference in bulk. The staple measured from 1 inch to $1\frac{1}{4}$ inch in length. The contrast with some inferior cottons was strongly marked, as regards their twisted, flat, tape-like appearance, and faint polariscopic colouring, which he attributed either to weakly growth or to having been picked from over-ripe pods, when the fibre had become dry and sapless. Too little is, however, known to form an exact opinion; dissection of buds and pods in all stages of growth would be necessary for a full and exhaustive investigation of the subject.

In reply to a question from Dr. Roberts, Professor Williamson stated that, like all vegetable hairs, the cotton fibre in its early stage is unquestionably cylindrical.

Mr. Sidebotham exhibited a convenient and effective form of binocular microscope, by Mr. Dancer, suitable for naturalists and others.

Mr. Brothers exhibited a mounted slide of Foraminifera, and a drawing of Colochœte Scutata, a minute fresh-water alga.

Mr. Whalley exhibited Trichoda. lynceus, marine infusoria; also an objective, $\frac{1}{25}$ of an inch focus, by Messrs. Powell and Lealand.

Ordinary Meeting, February 24th, 1863.

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

Among other donations for which the thanks of the Society were voted were an extensive collection of Dr. Dalton's manuscript correspondence, presented by Dr. W. C. Henry; and a framed photograph of Professors Bunsen and Kirchhoff, presented by Mr. H. Petschler.

Professor Roscoe stated that he had been for some little time, and is still, engaged in an interesting examination of the spectrum produced by the flame evolved in the manufacture of cast steel by the Bessemer process, on the works of Messrs. John Brown and Co., of Sheffield.

The spectrum of this highly luminous and peculiar flame exhibits during a certain phase of its existence, a complicated but most characteristic series of bright lines and dark absorption bands. Amongst the former the sodium, lithium, and potassium lines are most conspicuous; but these are accompanied by a number of other, and as yet undetermined bright lines; whilst among the absorption bands those formed by sodium vapour and carbonic oxide can be readily distinguished. Professor Roscoe expressed his belief that this first practical application of the Spectrum Analysis will prove of the highest importance in the manufacture of cast steel by the Bessemer process, and he hoped on a future occasion to be in a position to bring the subject before the Society in a more extended form than he was at present able to do.

A communication was likewise made by Professor Roscoe, concerning "The Existence of a Crystallizable Carbon Compound and Free Sulphur in the Alais Meteorite."

Through the kindness of R. P. Greg, Esq., of Manchester, I was placed in possession of about a gramme and a half of this peculiar meteorite, which fell near Alais, in France, on the 15th March, 1806, and was examined by Berzelius in

1834. This distinguished chemist states* that the Alais meteorite is remarkable as containing an organic carbon compound, soluble in water, which turns brown on heating, deposits a black carbonaceous mass, and burns without residue. In the year 1860, Wöhler† discovered the presence of small traces of a crystallizable hydrocarbon, soluble in alcohol and ether, in two meteorites, one of which fell at Kaba, in Hungary, on the 15th April, 1857, and the other at Bokkevelde, in South Africa, on October 13, 1838. The fact thus undoubtedly proved of the existence in these two meteorites of crystallizable carbon compounds, which in terrestrial matter are solely the results of vital action, rendered a further confirmation of the existence of organic matter in the Alais meteorite of special interest.

In general appearance the small fragments of the meteorite experimented upon coincided exactly with the minute description of the substance given by Berzelius; the white efflorescence which covers the surface of the mineral was found to consist mainly of small crystals of sulphate of magnesium; the only other bodies which could be detected by spectrum analysis were soda and lime. Iron was not contained in the soluble salts. On extracting 1·0583 gramme of the meteorite with water, 0·1155 gramme of soluble salts was dissolved, corresponding to 10·91 per cent, and thus closely agreeing with Berzelius's estimation of 10·3 per cent.

Ether was found to dissolve from the residue 1·94 per cent on the original meteorite, a substance which on evaporation was deposited in distinct crystals. The crystals possessed a peculiar aromatic odour, and melted at 114° C., subliming on heating, and leaving a slight carbonaceous residue. Under the microscope the crystals were seen to be of two forms, one acicular, the other rhombic. The acicular crystals were difficultly soluble in absolute alcohol, but easily soluble in ether, bisulphide of carbon, turpentine, and cold nitric acid,

* Pogg. Ann. xxxiii. p. 113.

† Wien. Acad. Ber. xxxv. 5., ditto xli. 565.

and dissolved in sulphuric acid with formation of a brown colouring matter; the rhombic crystals likewise dissolved in ether and bisulphide of carbon, but were unaltered by cold nitric and sulphuric acids, or turpentine. The etherial extract gave no reaction for sulphuric acid, but, after boiling with nitric acid, a copious precipitate of sulphate of barium was deposited. When burnt in a stream of dry oxygen gas, 0·0078 gramme of the extract, dried at 100° C., yielded 0·010 gramme of sulphurous acid, 0·008 gramme of carbonic acid, and 0·003 gramme of water. Hence the meteorite contained 1·24 per cent of free sulphur, 0·54 per cent of carbon, and 0·1 per cent of hydrogen, in a form soluble in ether. The meteorite contains a considerable quantity of carbon (probably as graphite) which is insoluble in ether. The total percentage of carbon found on igniting the meteorite in oxygen amounted to 3·36 per cent; this closely corresponds with the amount found by Berzelius, viz., 3·05 per cent.

From the above it is evident that the Alais meteorite contains at least a half per cent of a hydrocarbon, which is deposited in acicular crystals when the mass is treated with ether, together with considerable quantities (more than one per cent) of free sulphur, crystallizing from the ethereal solution in rhombic octohedra. To judge by the melting point, the hydrocarbon may be analogous to a mineral wax called Könlite, discovered by Kraus in the lignite of Uznach, which contains an equal number of atoms of carbon and hydrogen, and melts at 114° C.

The Rev. T. P. Kirkman, M.A., F.R.S., read a Paper "On Maximum Groups."

A substitution is positive or negative, as it is made by an even or by an odd number of transpositions of two letters.

To every group G of positive substitutions (a *positive* group G), derangements GP, GQ, ... can be added, so as to complete the entire group of $\frac{1}{2}$ ΠN substitutions, N being the number of letters, which is always a positive group.

A positive group G is a *maximum positive group*, when no derangements of G or of any factor of G can be added to it so as to complete a positive group of an order inferior to $\frac{1}{2}\,\Pi N$.

To every *mixed* group J, *i.e.* a group containing positive and negative substitutions, derangements JP, JQ, . . . can be added so as to complete the entire group of the order ΠN.

A mixed group J is a *maximum mixed group* when no derangements of J or of any factor of J can be added to it to complete a group of an order inferior to ΠN.

To every group H, whether mixed or positive, derangements can be added so as to complete the entire group of ΠN, which is always a mixed group if $N > 2$.

The suspicion expressed in the final paragraph of my memoir *On the Theory of Groups* (Memoirs of the Literary and Philosophical Society of Manchester, 1861), that the effect of variation of exponents in the auxiliary group g (Art. 34) on the number of equivalent groups constructible, would have to be reconsidered, turns out to be correct. The groups enumerated in theorem H (Art. 37) always exist, but they are not always the whole of the equivalents. If the partition of N is

$$N = A \cdot a = A \cdot A a',$$

A being a prime number, my estimate of the effects of variation of exponents is correct for $N = A \cdot A = A^2$, whatever prime A may be, and also for $N = A \cdot A^2 = 2 \cdot 2^2 = 8$; but it is defective for higher values of N. I took it for granted that if the auxiliary be

$$\begin{matrix} 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \\ 2 & 1 & 4 & 3 & 6 & 5 & 8 & 7 \\ 3 & 4 & 1 & 2 & 7 & 8 & 5 & 6 \\ 4 & 3 & 2 & 1 & 8 & 7 & 6 & 5 \\ 5 & 6 & 7 & 8 & 1 & 2 & 3 & 4 \\ 6 & 5 & 8 & 7 & 2 & 1 & 4 & 3 \\ 7 & 8 & 5 & 6 & 3 & 4 & 1 & 2 \\ 8 & 7 & 6 & 5 & 4 & 3 & 2 & 1, \end{matrix} \qquad (g)$$

no variation of exponents would give different equivalent groups of H, by the method of Art. 39, except by taking for unity in (g) one of the seven forms following,

$1\ 2^2 3\ 4\ 5\ 6\ 7\ 8$, $1\ 2\ 3\ 4^2 5\ 6\ 7\ 8$, $1\ 2\ 3\ 4\ 5\ 6^2 7\ 8$,
$1\ 2\ 3\ 4\ 5\ 6\ 7\ 8^2$, $1\ 2^2 3\ 4^2 5\ 6\ 7\ 8$, $1\ 2^2 3\ 4\ 5\ 6^2 7\ 8$,
$1\ 2^2 3\ 4\ 5\ 6\ 7\ 8^2$,

and then affecting every element in (g) with the exponent read over it in unity. The truth is that (g) is an effective auxiliary if we write it thus:

$$\begin{array}{cccccccc} 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 \\ 2 & 1 & 4 & 3 & 6 & 5 & 8 & 7 \\ 3 & 4 & 1 & 2 & 7^2 & 8^2 & 5^2 & 6^2 \\ 4 & 3 & 2 & 1 & 8^2 & 7^2 & 6^2 & 5^2 \\ 5 & 6 & 7^2 & 8^2 & 1 & 2 & 3^2 & 4^2 \\ 6 & 5 & 8^2 & 7^2 & 2 & 1 & 4^2 & 3^2 \\ 7^2 & 8^2 & 5 & 6 & 3 & 4 & 1^2 & 2^2 \\ 8^2 & 7^2 & 6 & 5 & 4 & 3 & 2^2 & 1^2, \end{array} \qquad (g')$$

which can be proved, as in Art. 39, to be a group equivalent to (g), and different as an auxiliary from the preceding eight. Seven mo.e auxiliaries can be formed as above from (g'), and thus we can construct on (g), not eight only, but sixteen equivalent groups K of the 16th order with 16 letters, all having 15 principal substitutions of the second order, and all beginning with

$$\begin{array}{cccccccccccccccc} 1 & 2 & 3 & 4 & 5 & 6 & 7 & 8 & 9 & 0 & a & b & c & d & e & f \\ 2 & 1 & 4 & 3 & 6 & 5 & 8 & 7 & 0 & 9 & b & a & d & c & f & e \end{array} = \theta,$$

by the method of Art. 39. There are in all $15\cdot13\cdot11\cdot9\cdot7\cdot5\cdot3\cdot1$ equivalent substitutions θ: if we employ them all, and with them 16 variations of each of the 30 equivalents of (g), we shall construct

$$\frac{1}{15}(16\cdot30\cdot15\cdot13\cdot11\cdot9\cdot7\cdot5\cdot3) = Q$$

different equivalent groups K of 16. Dividing Π (16) by this number of equivalents, we obtain for the order of the maximum modular, made by adding to K all its derived derangements (Theorem A, Cor.),

$$16\cdot15\cdot14\cdot12\cdot8,$$

and this maximum modular has Q equivalents.

This corrects the result of my article 96, and it may be of interest as affording when generalised, as it easily can be, a tactical verification of the very important theorem dedueed by M. Jordan from a theorem of Galois about a certain determinant, that if n^r be any power of a prime number n, there is a group made with n^r letters of

$$n(n-1)n^2(n^2-1)n^3(n^3-1)\ldots n^r(n^r-1)$$

linear substitutions. Vide M. Jordan's *Thèse sur le nombre des valeurs des fonctions* (Journal de l'Eeole Polytechnique, 1861), chap. v. It also shows that M. Jordan's group of linear substitutions is a maximum modular on the model (j) of the order n^r, having n^r-1 principal substitutions, *i.e.* consisting of (j) and all its derived derangements.

As this treatise of M. Jordan is the only one, so far as I ean learn, that has appeared in a revised form in the Freneh language since the days of Cauchy, on this difficult subject, it may be worth the while to give a little account of it. The memoir, as may be expected from the nature of the question, is neither short nor simple, but the aim is well defined, and the result is easy both to understand and to evaluate. The prineipal object of the work is to determine the order of a transitive group T made with M letters, whieh contains a set of similar substitutions $P_1P_2P_3\ldots$, such that the whole of them with their powers and products form an intransitive group Σ. M. Jordan shows that Σ will contain an intransitive group made up of $\frac{M}{n}$ transitive groups of n letters, and, therefore, n being prime, of the nth order; and if there be more than one such group Σ in T, he selects the *sous-groupe minimum* so constructible, and takes its prime order n as the modulus of his substitutions. His final result is, that $M = k \cdot n^{rp} = k \cdot m^p$, and that the order of T will always of necessity divide

$$1\cdot2\cdot3\ldots k\left\{n^{\frac{r(r+1)}{2}}\cdot(n-1)(n^2-1)\ldots(n^r-1)\right\}^{kp};$$

and thus, although he has not determined the order of T, nor assigned the form of his substitutions when $k>1$ (for he has not at all inquired into the degree of his unknown functions of i, the $(r+1)^{th}$ variable which has k values), he conceives that he has exactly given the superior multiple of any group T, which has a set of similar substitutions $P_1P_2\ldots$ the whole of which form, with their derivatives, an intransitive group Σ.

He has, indeed, assigned the superior limit of many such *woven grouped groups* T; but there is a vast number of them whose order is out of his limit. Take, for example, the woven grouped group of my theorem L (Art. 51), which is made by writing under 12345 the long-known group of 60 made with those elements, parallel to the same group of 60 made with 67890. Weaving the two sub-groups of 60, and then transposing them, or, which is the same thing, multiplying by 6789012345, we have a transitive group T of 60·60·2. If we collect the 48 substitutions similar to 1234578906, *i.e.* to 2345167890, of the fifth order, these determine an intransitive Σ, containing the five powers of 2345178906, and no regular substitutions but of the fifth order. No other intransitive Σ is thus constructible. Therefore $n=5$ is the order of the *sous-groupe minimum*, $M=2\cdot 5^{1\cdot 1}=kn^{rp}$, and the order of T ought, by M. Jordan's result, to divide $1\cdot 2\cdot\{5\cdot 4\cdot\}^2=800$. But 60·120 is no divisor of this number. We can also construct such a group T of the order 120·240, which is no divisor of 800.

For another example, write under 123456 the group of 60 positive substitutions, selected from the group of 120 of my article 65, parallel to the same group made with 7890*ab*. We have by weaving and grouping, as before, a transitive group T of the order 60·120, in which, if we collect all the substitutions similar to 2653417890*ab* of the third order, these determine a derivative intransitive Σ, the only one constructible, which has regular substitutions of the third order, and of no other. Then $n=3$ is the order of the *sous-*

groupe minimum, $M = 4 \cdot 3^{1 \cdot 1} = k \cdot n^{rp}$, and the order of T should divide

$$1 \cdot 2 \cdot 3 \cdot 4 \{3 \cdot 2\}^4,$$

but 60·120 is no divisor of the number. In like manner we can construct transitive groups T of the orders, 120·240, 360·720, and 720·1440.

We thus easily show that M. Jordan's limit is defective for such transitive groups T made with $2n^r = M$ letters, $(k = 2)$ whatever n^r may be > 4; and also for higher values of k.

All the groups T considered by M. Jordan are portions of the transitive maximum woven grouped group of my theorem L made with

$$kn^{rp} = n^r l = Aa$$

elements, of which the order is

$$\left(n^r \cdot (n^r - 1) \ldots 3 \cdot 2 \cdot 1\right)^l 1 \cdot 2 \cdot 3 \,.\,.\, l.$$

When $M = 2 \cdot 3^{1 \cdot 1} = k \cdot n^{rp}$, or $= 3 \cdot 2^{1 \cdot 1} = k \cdot n^{rp}$, or $= 2 \cdot 2^{2 \cdot 1} = k \cdot n^{rp}$, M. Jordan's limit coincides with mine. The tactical construction of this group is very simple, and requires no aid of analytic formulæ, or numerical computation. It is very desirable that this maximum group should be broken into its factors, but the key to this division is yet to be found. I shall show, that the required division can be in a great measure effected by selecting the positive groups and the mixed groups composing it.

If we treat M. Jordan's group of linear substitutions made with n^r elements, n being prime, by the method of my article (84) (*Theory of Groups*), we easily establish the theorem following, which is probably new :—

Theorem.—If n^r be any power >2 of a prime number $n > 2$, there is a transitive non-modular group formed with $n^r - 1$ elements of $(n^r - 1)n^{r-1}.(n^{r-1} - 1)n^{r-2}.(n^{r-2} - 1)\ldots n^2 \cdot (n^2 - 1)n(n-1)$ substitutions; also a transitive group made with $n^r - 2$ elements of $n^{r-1}.(n^{r-1} - 1)n^{r-2}.(n^{r-2}.\,(n^{r-2} - 1)\ldots n^2(n^2 - 1)n(n-1)$ substitutions, which is generally non-modular.

This theorem might be extended. When $n^r = 2^4 = 16$, there is a non-modular group made with 15 of the order 15·14·12·8, a non-modular made with 14 of 14·12·8, and a non-modular made with 12 of 12·8, which has a derived derangement, making it a modular group of 12·8·2. These are all transitive groups and easily constructed, either with or without the aid of the notation of linear substitutions made with 16 letters. We have here the true generalisation of the group of 7·4·3·2·1 made with 7 letters, about which, with the 30-valued functions given by it, so much labour, far more learned than lucrative, has been spent by MM. Betti, Kronecker, Hermite, and myself.

It is curious, that the true analytical expression of these transitive groups of $n^r - 1$ and $n^r - 2$ letters is obtained by considering them as intransitive groups of n^r letters.

7

MATHEMATICAL AND PHYSICAL SECTION.

February 12th, 1863.

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

Mr. W. L. Dickinson read a Paper "On the Eclipse of the Sun, May 17, 1863."

This eclipse, which is a partial one, will not be of great magnitude to any part of the United Kingdom. In this neighbourhood about three-tenths of the sun's diameter will be obscured by the moon. The following particulars, which have been computed from the numerical equations in the *Nautical Almanac*, are offered to the members, to enable them to compare the calculated results with their own observations.

Eclipse of the Sun, on Sunday afternoon, May 17, 1863.

Computation for Manchester (Royal Infirmary), lat. N. 53° 29′, long. W. 2° 14′: —

	h	m	s
Begins...............	5	37	12
Greatest Phase	6	25	32
Ends	7	11	4

Greenwich mean time.

Magnitude of the Eclipse (Sun's diameter = 1) 0·307.

Angle, from North Pole, of

first contact, 49° towards the West,
last contact, 45° towards the East,

Angle, from Vertex, of

first contact, 88° towards the West,
last contact, 9° towards the East,
for *direct* image.

The position of the points of 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, at the last *r* in the word *Britanniar:* and to leave it on the left side near the vertex, at the letter *i* in the word *Gratia*. A line drawn between these letters will indicate the direction of the moon's path.

There will not be another Eclipse of the Sun visible in England until October 19, 1865.

The following returns of the rainfall for 1862 were communicated to the Section : —

THE FLOSH, CLEATOR, near WHITEHAVEN.

BY THOS. AINSWORTH, ESQ.

1862.	Amount in Inches.	No. of Days on which Rain fell.
January	5·075	22
February	1·122	12
March	3·345	18
April	5·410	22
May	5·230	17
June	4·197	23
July	3·850	19
August	5·150	18
September	2·417	17
October	8·780	27
November	1·762	19
December	6·965	26
	53·303	240

OLD TRAFFORD, MANCHESTER.

BY G. V. VERNON, F.R.A.S.

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

1862.	Fall in Inches.	Days.	Mean of 5 years, 1858—1862.	Mean of 69 years, 1794—1862.
January	1·896	17	1·826	2·443
February	0·958	10	1·427	2·347
March	3·669	18	3·335	2·336
April	2·717	19	2·461	2·054
May	4·470	20	2·091	2·354
June	3·072	22	3·399	2·888
July	4·527	21	2·693	3·618
August	2·350	12	3·815	3·589
September	4·998	18	3·916	3·191
October	5·035	23	3·422	3·810
November	1·685	14	2·423	3·485
December	3·221	24	2·938	3·282
	38·598	218	33·746	35·427

The fall in 1862 was 4·852 inches above the average for the last five years, and 3·171 inches above the average for sixty-nine years.

The means for 69 years have been found by including the values for the years 1858—1862 in addition to the values determined by Mr. John Curtis for 64 years, in Vol. XV. of the Memoirs of the Society.

Thelwall, near Warrington.

By John Atkinson, F.G.S.

The height of the station above the mean sea-level is 96 feet. The gauge is about 12 or 14 inches above the ground.

1862.		Inches.	Days on which it fell.
6·821	January	2·142	18
	February	0·744	10
	March	3·935	18
9·139	April	2·335	18
	May	4·141	20
	June	2·663	22
10·328	July	3·970	19
	August	2·958	19
	September	3·400	16
9·552	October	4·915	24
	November	1·673	13
	December	2·961	22
		35·840	219

Ordinary Meeting, March 11th, 1863.

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

The Rev. Robert Harley, F.R.A.S., read the following communication "On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form."

Mr. Ekman, the Honorary Librarian of this Society, has called my attention to a remarkable investigation on the theory of algebraic equations, bearing date 14th December, 1786, by Erland Sam. Bring, who for some time was Professor of History in the University of Lund. That portion of Bring's investigation which relates to equations of the fourth and fifth degrees has recently been republished by the Royal Swedish Academy of Sciences, in their *Proceedings* for 1861, along with a Paper on the subject by Professor C. Hill. (See "Ofversigt af Kongl. Vetenskaps-Akademiens Förhandlingar. Adertonde Årgången." 1861, pp. 317—355.)

Bring's investigation was originally published as a "Disputation," but, sharing the fate of many other University disputations, it was suffered to drop into oblivion, and it remained there until about a year and a half ago, when Professor Hill discovered it and dragged it to light. The only complete copy of Bring's Paper out of Sweden which my friend, Mr. Ekman, has been able to trace, exists, he informs me, in the library of the Observatory of Pulkova, in Russia.

Bring succeeds in extending Tschirnhausen's method of transformation so as to reduce the general equation of the fifth degree to a trinomial form by means of equations of inferior degrees. It is commonly supposed, I believe, that this reduction was first effected by our own countryman, Mr. Jerrard, whose "Mathematical Researches," published about thirty years ago, gave a new impulse and direction to the efforts of those analysts who were labouring to conquer the difficulties connected with the higher equations. Mr. Jerrard has certainly taken a large and comprehensive view of the subject, and by his original researches has laid all cultivators of algebra under great obligations to him. But in his reduction of the general quintic equation to the trinomial form, by taking away its second, third, and fourth terms at once, he was anticipated, it seems, by Bring.

In a Paper entitled "A Contribution to the History of the Problem of the Reduction of the General Equation of the Fifth Degree to a Trinomial Form," about to appear in No. 21 of the *Quarterly Journal of Pure and Applied Mathematics*, I have briefly indicated what has been done in this problem by different investigators, and I have particularly noticed the labours of Mr. Jerrard, Sir W. R. Hamilton, Mr. Cockle, Professor Sylvester, M. Serret, and Mr. Cayley, as well as of E. S. Bring, whose reduction, written in Latin, I have given *in extenso*.

My object in this communication is simply to draw attention to Bring's investigation; and I shall now content myself with giving a brief sketch of the process which he employs.

Starting with the quadrinomial form,

$$z^5 + pz^2 + qz + r = 0 \ldots (A)$$

to which, as is well known, Tschirnhausen's quadratic transformation enables us to reduce the general equation of the fifth degree, and assuming

$$z^4 + dz^3 + cz + bz + a + y = 0 \ldots (B),$$

Bring, by the elimination of z between (A) and (B), arrives at an equation in y of the form

$$y^5 + \mathrm{a}y^4 + \mathrm{b}y^3 + \mathrm{c}y^2 + \&\mathrm{c}. = 0 \ldots (C)$$

where a, b, c, &c. are rational functions of the given coefficients p, q, r, and of the disposable coefficients a, b, c, d. He next proceeds to inquire how the three conditions

$$\mathrm{a}=0\ldots(D)$$
$$\mathrm{b}=0\ldots(E)$$
$$\mathrm{c}=0\ldots(F)$$

can be satisfied simultaneously; and he finds that the first (D) is satisfied by making

$$a=\frac{3pd+4q}{5}$$

This value being substituted in the second and third equations, viz. (E) and (F), and b, c, or d being eliminated, there results an equation of the sixth degree. But this elevation of degree is avoided by a very simple artifice. Writing

$$b=ad+\zeta, \text{ and } c=d+\gamma,$$

the equation (E) takes the form of a quadratic in d, which quadratic is made to vanish identically. The evanescence of the first coefficient determines a, by the solution of a linear equation, as a rational function of the known quantities p, q, r. The evanescence of the second coefficient, a being now treated as known, determines ζ as a linear function of γ. And the evanescence of the third coefficient, when the value of ζ in terms of γ is introduced, determines γ, by the solution of a quadratic equation, as a function of p, q, r. Hence a, ζ, γ are now severally known, and are so determined as to satisfy the second equation of condition (E). But it still remains to satisfy the third condition (F). Substituting for a, b, c their respective values

$$\frac{3pd+4q}{5},\ ad+\zeta, d+\gamma,$$

the resulting equation will contain only one unknown, viz. d, and a glance at the explicit form of c, given by Bring, shows that d cannot rise above the third degree; so that, by the resolution of a cubic equation, d may be determined and the last condition (F) satisfied.

A Paper was read, "On the Solution of the Differential Resolvent," by W. H. L. Russell, A.B., communicated by the Rev. ROBERT HARLEY, F.R.A.S.

"In the course of last year I was induced, at the request of Mr. Harley, to consider the very interesting differential equation which he has denominated the 'Differential Resolvent.' I obtained the solution of the quartic resolvent by series, which I summed by means of a triple integral. But Professor Boole intimated that he had discovered a process of transformation by which the quartic could be resolved by a single integral. This led me to examine my own series, and I find that the series representing the solution of the quartic could be summed by means of a single integral. I have since discovered that the general resolvent can be solved by means of a single integral. To effect this is the object of the present Paper."

The Author first deals with the resolvent of the quartic equation, viz.:—

$$y^4 - 4y + 3x,$$

$$y - \frac{\left(D-\frac{7}{4}\right)\left(D-\frac{10}{4}\right)\left(D-\frac{13}{4}\right)}{D(D-1)(D-2)}\,\epsilon^{3\theta}\,y = 0$$

where $\epsilon^\theta = x$; and he obtains the solution

$$\frac{C}{x}\int dx x \int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta\,\frac{\cos^{\frac{1}{3}}\theta\left\{\cos\frac{5\theta}{3} - \cos^4\theta\cos\frac{\theta}{3}\cdot\frac{(3x)^3}{2^4}\right\}}{1-\cos^4\theta\cos 2\theta\left(\frac{3x}{2}\right)^3 + \cos^8\theta\frac{(3x)^6}{2^8}},$$

C being a certain irrelevant function.

He next discusses the resolvent of the equation

$$y^n - ny + (n-1)x = 0,$$

viz.:—

$$y - \phi(D)\,\epsilon^{(n-1)\theta}\,y = 0,$$

where

$$\phi(D) = \frac{\left(D-\frac{2n-1}{n}\right)\left(D-\frac{3n-2}{n}\right)\cdots\left(D-\frac{n^2-n+1}{n}\right)}{D(D-1)(D-2)\ldots(D-n+2).}$$

The solution is expressed in a series, the sum of which is found to be as below:—

$$\frac{Cr}{x^r}\int dx\,x^{r-1}\int_{-\frac{\pi}{2}}^{\frac{\pi}{2}} d\theta \cos\frac{nr-3n+1}{n-1}\theta$$

$$\frac{\cos\frac{nr-2r+1}{n-1}\theta-\frac{2^n(n-1)^{n-1}}{n^n}\cos^n\theta\cos\left\{n-2-\frac{nr-2r+1}{n-1}\right\}\theta x^{n-1}}{1-\frac{2^{n+1}(n-1)^{n-1}}{n^n}\cos^n\theta\cos(n-2)\theta x^{n-1}+\frac{2^{2n}(n-1)^{2(n-1)}}{n^{2n}}\cos^{2n}\theta x^{2n-2}}.$$

In a brief note the Author indicates the method by which the series employed were derived from the differential equations.

Mr. Crookes F.C.S., exhibited a specimen, weighing 450 grains, of the new metal, thallium, which he discovered by spectrum analysis. He stated that he had been fortunate enough to find this element in comparatively large quantities in the deposit from the flues of Mr. Spence's pyrites burners.

Dr. Joule made the following communication respecting a new and extremely sensitive thermometer:—"Some years ago I remarked the disturbing influence of currents of air on finely suspended magnetic needles, and suggested that it might be made use of as a delicate test of temperature. I have lately carried out the idea into practice, and have obtained results beyond my expectation. A glass vessel in the shape of a tube, two feet long and four inches in diameter, was divided longitudinally by a blackened pasteboard diaphragm, leaving spaces at the top and bottom, each a little over one inch. In the top space a bit of magnetised sewing needle, furnished with a glass index, is suspended by a single filament of silk. It is evident that the arrangement is similar to that of a 'bratticed' coal pit shaft, and that the slightest excess of temperature on one side over that on the other must occasion a circulation of air, which will ascend on the heated side, and, after passing across the fine glass index, descend on the other side. It is also evident that the sensibility of the instrument may be increased to any required extent, by diminishing the directive force of the magnetic needle. I purpose to make several improvements in my present instrument, but in its present condition the heat radiated by a small pan, containing a pint of water heated 30°, is quite perceptible at a distance of three yards. A further proof of the extreme sensibility of the instrument is obtained from the fact that it is able to detect the heat radiated by the moon. A beam of moonlight was admitted through a slit in a shutter. As the moon (nearly full) travelled from left to right the beam passed gradually across the instrument, causing the index to be deflected several degrees, first to the left and then to the right. The effect showed, according to a very rough estimate, that the air in the instrument must have been heated by the moon's rays a few ten-thousandths of a degree, or by a quantity no doubt the equivalent of the light absorbed by the blackened surface on which the rays fell."

E. W. Binney, F.R.S., the President, said that of late years considerable attention had been devoted to the examination of the beds of sand and gravel found in the valleys formed since the deposition of the till or boulder clay. Sir Charles Lyell, in his valuable work on the Geological Evidences of the Antiquity of Man, has given us many facts connected with these valley gravels, especially relating to the terraces of the higher and lower level gravels found in the valley of the Somme, in which the flint instruments have been met with. As these two deposits are seen in the neighbourhood of Manchester, he wished to direct attention to all excavations that were being made in them, in order that any remains or implements which might be met with should be preserved. Doubtless many interesting specimens have perished, owing to the parties finding them being ignorant of their value. Many years since a former member of this Society, the late Mr. F. Looney, F.G.S., in speaking of the superficial gravel found in this neighbourhood, at page 23*, says:—"Imbedded in the gravel near the river courses are occasionally found the stone celts of the ancients, from which it is presumed that the rivers, since the country was inhabited, have either sawn their beds deeper or much exceeded their present volume of water; several large trees have been dug up from the sand and gravel; part of one is now lying near the residence of the Rev. J. Clowes, at Kersal Moor, which was dug from the Show Field on his estate, at upwards of 20 feet elevation above the present level of the river. A case more illustrative of this was beautifully shown in the winter of 1820, during the cutting away part of the high ground at Castlefield, near the tunnel mouth; for sixteen feet below the level of the grass a wooden box was found. It was square and formed of four upright posts, driven into a bed of clay; the sides and bottom were closed in with logs of wood; the logs were rudely hewn, had been riven, not sawn, from five to six inches square; some greenstone boulders lay at the bottom, and the whole was covered with sixteen feet of sand and gravel; twelve feet of the lower part had never been disturbed, the continuity of the layers being unbroken.

SECTION.

Alluvium, four feet.

Layers of gravel, twelve feet, unbroken.

Box.

Layer of clay.

Gravel."

* List of Organic Remains, &c., and where found, to accompany Mr. Elias Hall's Introduction and Map, by Mr. Francis Looney, member of the Literary and Philosphical Society of Manchester, published in 1836.

Mr. Looney was well known to be a most accurate and intelligent observer, and his mention of stone celts having been found at levels above the present river courses, appears to afford us fair hopes of their being found in the lower level gravel if carefully looked for. But the most interesting fact is the finding of the wooden fabric, by the author termed a box, which, although smaller in size, bears some resemblance to the *crannoges* found sometimes in Ireland under the peat bogs. He (the President) had lately been to examine the place where it was found, which is to the south of and just outside of the old Roman Station of Mancunium, in company with a party who saw the fabric when it was first exposed, and stood in it afterwards. The locality of course is now much changed, but the person was able to point out to him the position where it was found, which is now covered by No. 6 arch of the Altrincham Railway, and said that it was about six feet square and four feet high. The four upright posts driven into the clay were about five inches thick, and roughly riven, as were also the slabs forming the sides and bottom. His informant did not notice any top to the fabric, or how the slabs had been fastened to the posts, but he remembers a layer of cobble stones, each about six inches in diameter, forming a kind of pavement, being in the bottom of it. Upon these stones were some bones, which he examined, but did not recognise any of them as human. All the wood appeared to be oak, but it was very rotten, and fell to pieces soon after it was exposed to the air. None of it was preserved. Under the clay the red rock (Trias) was found at a short distance. The geological position where the fabric was found is in the lower level gravel, about 29 feet above the water of the present river Irwell. For whatever purpose it was made, there can be little doubt as to its having been the work of man, and as we are assured that the overlying sand and gravel was quite undisturbed, there can be as little question of its great antiquity, reaching as far back as the age of the lower level gravels of the valley of the Somme in France, where the flint implements have been found. Doubts have been raised as to these flints having been fashioned by human hands, but as to the origin of the wooden fabric, if, as stated, it was covered by 12 feet of undisturbed sand and gravel, no such doubts can rationally be raised.

A Paper was read, entitled, "Notes on the Action of Heat and Force upon Matter," by J. C. DYER, V.P.

Confining his observations to the "action of mechanical forces,"—apart from those depending on the electrical, magnetical, and vital forces—he held the former to depend on the inherent properties of matter, as gravity, inertia, and elasticity, each coextensive with the material universe.

That force cannot in any case be treated as an abstraction or as apart from the matter in which it resides, though several authors have had recourse to such an abstraction, to account for phenomena attending the operation of force. Mechanical action, depending on the *elastic force* generated by the agency of *carbon* and *water*, being of so great importance, it is desirable to obtain some settled knowledge as to how this arises. We all know that by combustion of the former the latter is converted into *steam*. That which passes from the burning coals through the boiler plates and into the water to form the elastic vapour, is called *heat*; but whether it be a union of heat with the water, or only a transmitted mechanical action on the water, causing its atoms to separate and assume the elastic state, remains a question for solution; and of late several able writers have offered explanations of this phenomenon founded on the supposed nonentity of heat and the agency of mechanical force to form steam, and thus rejecting the latent heat theory which before seemed to account for the heat absorbed to form steam. So far as the latent heat theory serves to explain phenomena, the mechanical creation of heat is uncalled for, since it is unphilosophical to assign new causes when those before accepted clearly meet the case. Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Professor Tyndall, each based on the *force heat* assumption, but without affording any solid proofs of its application to the phenomena adduced by them.

Mr. Rankine states that "Heat produces an increase of temperature and of expansive force in bodies; that the changes from the solid and liquid states to the gaseous state is accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as attaining the form of heat, and in short making the body hotter." Again, that "changes of volume are attended with changes of molecular arrangement, perceptible or imperceptible, and that the latent heat of expansion or evaporation consists of heat that disappears in overcoming pressure and the attraction of the particles of the body." In reference to the nonproduction of heat by the moving force exerted in waterfalls, the following solution is offered, viz.: "It is natural to suppose that the *motion* during this phenomenon (the falling of a mass of water) has not been destroyed, but has been converted into revolutions of the particles in vortices or eddies too small to be perceptible by any of our means of observation, and that the centrifugal force of such eddies is the *cause* of the tendency of hot bodies to expand, melt, and evaporate."

The first class of phenomena above cited are plainly accounted for by the doctrine of latent heat, but the latter explanation about waterfalls passes Mr. Dyer's comprehension. He submits that what is said to be *natural to suppose* concerning the revolutions of particles in vortices and eddies too small to be perceived is too obscure and questionable to be accepted in explanation of anything. If Mr. Rankine has no better mode of accounting for the nonappearance of heat from the force expended in waterfalls than the above, his defence of the generation of heat by force fails, and the balancing the force by the resistance at the foot of the falls is just as open to view as the falling force itself; so that the motions, too small to be perceived, are supererogatory inventions. The dynamical origin of heat is also set forth by Professor Tyndall, in a paper "On the Forms and Action of Water." Examples are therein adduced of vast *mechanical forces* exerted by the formation of water, and by its changes from the states of a solid, liquid, and vapour. He says that "on the combination of oxygen and hydrogen to form water weighing 10lb. an energy was expended, the atoms clashing together with a force equal to that of a ton weight let fall from a height of 23,757 feet. In falling from the state of vapour to that of water an energy was expended equal to that of a ton falling from a height of 3,700 feet, the moving force of the stone avalanches of the Alps was but as that of snowflakes, compared with the energy involved in the formation of a cloud. In passing finally from the liquid to the solid state, the atoms of 10lb. of water exercised an energy equal to that of a ton weight falling down a precipice 550 feet high."

Now, the union of the gases to form water, the evaporation of it to form steam, and freezing it into ice, are chemical changes, and the forces called into action by them are chemical forces, which cannot be measured by that of gravity in falling tons or even ounces; but the mutations of heat attending those changes are measured by degrees, when passing from the latent to the sensible state, and *vice versa*. Of vapour in the air, the Professor says, that "though forming only about 0·5 per cent of the atmosphere, for every ray of terrestrial heat struck down by the air, fifty, sixty, or seventy rays were destroyed by the aqueous vapour. This vapour permitted the solar rays to reach the earth, but intercepted the terrestrial rays in their escape towards space," and he adds that "in the presence of such experiments, it was easy to see how the snow of the Alpine mountains, and how the ice, should be squeezed through the moulds formed by the valleys, and then that these mountain glaciers were as much a proof of heat as of cold, and to produce the vapour to condense and form glaciers required as much heat as would raise five times the weight of the glaciers of cast iron to its melting point." The heat required to convert water into steam as it passes from the sensible into the latent state is a known quantity, so also is the amount required to convert ice into water; but that required to melt a pound of ice and evaporate a pound of water would fall far short of melting five pounds of cast iron.

That no mechanical energy is exerted in the conversion of water into steam is obvious to our senses, since evaporation is the most gentle, imperceptible, as well as the most universal action in Nature's laboratory; and although the elasticity of the steam itself exerts mechanical force, equal to the atmospheric pressure, chemical force alone is concerned in evaporation, and therefore cannot be compared with falling bodies, so that the alleged energy and *clashing force of atoms*, in the actions of water cited, are wholly imaginary; nor can philosophy assert any fact relating to mechanical action, which cannot be proved by experiment or established by plain induction; and in the cases cited, no one can know anything about the occult forces and energies of the motions of atoms attending the mutations of water. Great respect is due to both of the gentlemen whose essays have invited the above strictures; but the search after the truths of science "should not halt before high names."

Mr. Rankine, in offering demonstrations of the dynamical origin of heat, allows that heat disappears and becomes *latent in water*, in passing from the solid to the liquid state, and from this into vapour, as also that the same amount of heat is given out and reappears as sensible heat, upon the reversal of those changes; thus in substance admitting the latent heat doctrine, whilst denying the materiality of that which does so enter into a latent and reappear in a sensible state, and calling this heat the production of force, when no mechanical force whatever is called into action by those mutations of water. They are, however, plainly explained by the agency of heat, in its chemical union with water and its evolution from water. Although the evolution and absorption of heat from the compression and expansion of elastic fluids bears a constant relation to the amount of compression and to the compressing force, yet this is not the case, as Mr. Rankine supposes, in the resistance to compressing forces by liquids and solids; the amount of compression in these latter cases depends upon the relative mobility of the particles of the bodies, and we find forces of great energy, exerted against yielding solids, as in carriage and other springs and elastic bodies, are in continuous action without producing any sensible heat when no compression takes place. If it were true that acting forces always produced their equivalent in heat, then the powerful movements of railway springs, hydraulic presses, and the like, should raise intense heat in the steel springs and water exerting such force, and the absence of any heat in such cases, must be fatal to the dynamic creation of heat.*

The following Paper, "On the Chemical Constitution of American Rock Oil," by Mr. Schorlemmer, Assistant in the Laboratory of Owens College, was communicated by Professor Roscoe:—

In a Paper published in the October number of the Chemical Society's Journal, I showed that the products of the distillation of cannel coal at a low temperature contain a series of homologous hydrocarbons of the formula $C_n H_{n+2}$. I further showed that these are the hydrides of the alkohol radicles, as, upon treatment with chlorine, they yielded by substitution of one atom of hydrogen by one of chlorine the

* I have experimentally determined the thermal effects of compressing or dilating metals and fluids, and have found these effects to be strictly in accordance with the dynamical theory of heat. See *Phil. Trans.*, 1859.—Ed.

corresponding chlorides, from which other derivatives may be obtained.

In the portion of the oil boiling below 120° C, I found the following four hydrides:—

C_{10} H_{12} hydride of amyl, boiling point 39° C.
C_{12} H_{14} hydride of hexyl, ——— 68° C.
C_{14} H_{16} hydride of heptyl, ——— 98° C.
C_{16} H_{18} hydride of octyl, ——— 119° C.

Of these the hydride of heptyl or oenanthyl is the most interesting, as it was previously unknown, and I therefore undertook the investigation of its derivatives, concerning which likewise our knowledge is very limited and contradictory. Thus, for instance, many chemists state that the alkohol obtained by the distillation of castor oil with potash is heptylic alkohol, whilst others regard it as octylic alkohol, and it is only by the most recent experiments of Bouis * that we learn with certainty that this substance is octylic alkohol, inasmuch as he obtained the true heptylic alkohol by the action of nascent hydrogen on oenanthol. For the purpose of this investigation I endeavoured to obtain the hydride of heptyl from the American petroleum, as the yield of this substance from the cannel oils is but small, and the labour of purification tedious and disagreeable.

The existence of this hydride in the petroleum was rendered probable by the fact of the discovery of hydride of hexyl by Pelouze and Cahours.†

The oils which I examined are those known by the name of turpentine substitute, and obtained as the first products in the rectification of the crude oil. Different samples of the commercial articles possess very different properties, the spec. gr. lies between 0·70—0·75. One sample began to boil at 30° C, and the greatest portion distilled over below 100° C, whilst others between 80°—150° C, and others between 100°—200° C.

* Compt rend. 55. 140.

† Compt. rend. 54, 1241.

When these oils are subjected to fractional distillation no product of constant boiling point is obtained, the oils requiring for this purpose a preliminary purification with concentrated nitric acid.

The greatest portion of the oils remains unattacked, and the acid solution contains nitrobenzol (from which aniline was prepared), nitrotoluol and binitrotoluol, and small quantities of fatty acids produced from traces of olefines which are probably contained in the crude oils.

I tried to separate these olefines by adding bromine to the crude oil until the colour of the latter no longer disappeared; a few drops, however, are sufficient for a large quantity of the oil, and when the whole is subjected to distillation, a very few drops of bromine compounds of a high boiling point remained behind, the quantity of which was too small for further examination.

The oil after this treatment was well washed, dried over potash, and rectified repeatedly over sodium. By fractional distillation, the following four hydrides were obtained, and found to be identical with the hydrides from the cannel tar:—

$C_{10} H_{12}$ hydride of amyl,	boiling point	34° C.
$C_{12} H_{14}$ hydride of hexyl,	———	68° C.
$C_{14} H_{16}$ hydride of heptyl,	———	98° C.
$C_{16} H_{18}$ hydride of octyl,	———	119° C.

In addition to these I obtained a small quantity (about one gramme) of a liquid boiling between 20°—30° C, and hence we may infer that also hydride of butyl is present in small quantities.

I stated in the paper above alluded to that hydride of amyl boils at 39° C. The same compound from petroleum boils at 34° C. I find, however, that the presence of traces of foreign substances alter the boiling point of this body very considerably. Thus, for instance, I obtained from the crude oil about one ounce of a liquid which boiled from 15°—20° C;

after treatment with nitric acid, the volume of the liquid remained almost unchanged, showing that only a very small quantity of substance had been removed; but, after drying with potash and rectifying over sodium, it was found that a mere trace of the liquid boiled below 30° C; nearly the whole distilled at 34° C, and consisted of hydride of amyl. Of the four hydrides which I isolated, I have only prepared the hydride of heptyl in quantity; four gallons of turpentine substitute boiling between 80°—150° C, yielded three pounds of the pure compound.

In order to obtain from this other heptyl compounds, it was transformed into the chloride according to the excellent method described by Hugo Müller,* which consists in the addition of a small quantity of iodine to the substance which is to be treated with chlorine. The substitution occurs much more rapidly in this case than when chlorine alone is employed, and goes on in absence of the daylight, so that a rapid current of chlorine gas can be led into the liquid without any chlorine escaping with the hydrochloric acid vapours.

Hence it is seen that the constitution of American petroleum, at least that portion boiling below 120° C, is quite analogous to that of the oil from cannel tar.

Petroleum consists mainly of the hydrides of the alkohol radicles, it contains very small quantities of benzol and toluol, and probably traces of olefines, whilst in the cannel coal oil the hydrides are found in smaller quantities, and benzol and toluol in proportionally larger amounts. In the oil obtained by distillation of boghead coal, Greville Williams has discovered a series of hydrocarbons possessing the composition and physical characters of the hydrides $C_n H_{n+2}$, also benzol and its homologues and olefines.†

The rock oils obtained in other countries appear to possess a somewhat similar constitution.

* Journ. Chem. Soc., 15, 11.

† Phil. Trans., 1857.

Thus, for instance, Warren de la Rue and Hugo Müller* found in the Rangoon tar, benzol, toluol, xylol, and cumol and hydrocarbons of the formula $C_n H_{n+2}$. They were, however, unable to isolate from these a compound of definite composition and boiling point. The rock oil from Sehnde, in Hanover, consists, according to the investigation of Busenius, Eisenstuck,† and Uellsmann,‡ of hydrocarbons of the same general formula, but they likewise failed to obtain definite products.

Pebal and Freund§ found in the rock oil from Gallicia benzol and homologues, carbolic acid and homologues, and hydrocarbons which are not attacked by the strongest acids, and probably identical with those previously mentioned.

MICROSCOPICAL SECTION.

16th February, 1863.

Mr. JOSEPH SIDEBOTHAM, Vice-President of the Section, in the Chair.

Captain FLETCHER, of the ship "Tigris," presented a portion of harbour mud from Singapore, and five soundings from the coasts of Java and Sumatra.

Mr. R. D. DARBISHIRE presented specimens of mud and fossil shells (received through Dr. P. P. Carpenter) from the post-pliocene or latest tertiary deposits at Logan's Farm, Mile-end Quarries, near Montreal, Canada, described by Sir C. Lyell ("First Travels in North America," Vol. II., p. 135), and in Papers by Dr. J. W. Dawson in the "Canadian Naturalist," 1858 and in 1859. Mr. Darbishire, in a note to the Secretary, stated that one of the peculiarities of the deposit is that it seems to have been formed in a quiet hollow.

* Jahresbericht, 9, 606.

† Liebig's Annalen, 113—115.

‡ *Ibid*, 114—279.

§ Liebig's Annalen, 115, 19.

Spiculæ of sponges are found in position as if the sponge had grown and been quietly buried on the spot. Amongst other characteristic fossils are numerous Foraminiferæ, and a silicious and close textured sponge referred to Tethea, of the species Logani, which is now found in water from the tide line to 200 fathoms deep. Mr. Nevill undertook to examine and report upon the specimens.

Mr. H. A. Hurst presented a copy of Part IV., Vol. XII., of the "Journal of the Agricultural and Horticultural Society of India," published at Calcutta, containing the Prize Essay on Cotton Cultivation in India from foreign seed, by Dr. J. Shortt, F.L.S., Zillah Surgeon, Chingleputt, for which the prize of 1,000 rupees and the gold medal of the Manchester Cotton Supply Association were awarded. Mr. Hurst read a paragraph from page 499 relating to the early stage of the cotton pod, which, bearing upon points lately in dispute and not before published in this country, is given entire. "On examining a cotton pod soon after the ovary has been impregnated (which is known by the change in colour and the fading of the petals, or flower leaves, or corolla), it is found to contain a number of seeds according to its particular variety. If a single seed be separated and examined by the naked eye nothing is visible; but when seen through the microscope, it is found covered with a villous coat, formed apparently of elongated cells joined end to end. These are filled with sap. The young seed itself is somewhat pear-shaped, and resembles in miniature, some of the China candied fruits with the frosted crystals of sugar covering it. On letting out the contents of a single cell, it is found to consist of granular cells containing a centro-lateral nucleus. On examining a pod between three and four weeks old, the seed still retains somewhat of its pyriform shape, and appears quite shaggy. The fibres, tapering to a point at their free end, resemble hollow cylindrical tubes filled with fluid, and vary in length; and on submitting a single fibre, compressed between pieces of glass, to the microscope, the flattened surfaces become distinctly visible. Again, on substituting a mature fibre before it gets dried, the filament is found to consist of tubular hairs, which are now quite cylindrical. After the dehiscence of the mature capsule by the contraction and separation of its valves, the wool becomes dry from exposure. A filament now placed under the microscope is found to resemble a flattened piece of tape, twisted upon itself, and apparently formed of an extremely thin and transparent membrane, interspersed with dark granular matter, which, after a certain time, disappears in some of the varieties."

Mr. J. G. Lynde, F.G.S., M. Inst. C.E., read a Paper "On the Action of Magenta upon Vegetable Tissue," in which he described a series of experiments upon cuttings of Vallisneria immersed in a solution of that dye in cells under the microscope, and its effect upon the circulation in that plant. He found that so long as the vital action continued, the cell walls and the moving chlorophyll retained their green colour, but the injured cells were immediately deeply reddened, and their contents gradually acquired the same colour, the intensity of which was in proportion to the thickness or density of the tissue. Between the cell walls it would appear that there exists an intercellular membrane, devoid of vital action, which becomes rapidly coloured whilst the circulation continues active. On the inner surface of the cell wall, whilst rotation is going on, the author observed a luminous stratum suggesting the action of ciliæ, but in every observation as the dye permeated the tissue and the circulation ceased, the true cell wall became covered with irregular markings, either corrugated or having raised excrescences, scarcely alike in any two cells; in no case were the markings visible until the rotation had ceased, and they had the appearance which would be produced by ciliæ falling against the cell wall in all positions upon the suspension of vital action.*

The chlorophyll vesicles appear in three forms: in a gelatinous sac or mass rotating altogether in the cells; as independent vesicles apparently homogenous in their structure, rendered opaque by colouring matter; and lastly, as independent vesicles somewhat increased in size, of a pale green colour, almost transparent, containing nuclei, one, two, or three in number, which in reality appear to be immature vesicles within the parent, similar to Volvox globator, without rotatory motion. The chlorophyll vesicles appear to resist the action of the magenta for some time after their rotation has ceased, indicating a vitality, at least to a certain extent, independent of that of the cell. In some of the experiments a few of the cells assumed a purplish colour, whilst in the adjoining cells the circulation was active and the chlorophyll green; in those the chlorophyll appeared to be decomposed, and the cell to be nearly full of very minute dots, swarming like the granules in Closterium lunula. Upon this subject the author offered no opinion. The observations were made with $\frac{1}{5}$th and $\frac{1}{8}$th objectives, and the paper contained minutia of several experiments, with the hours of observation, temperature of the room, and other particulars.

Dr. Roberts observed that Mr. Lynde's remarks upon the separate vitality of the cell and cell contents were very

* Eminent microscopists do not entertain the idea of the circulation in Vallisneria being due to ciliary action; "this appearance is decidedly affirmed by Mr. Wenham to be an optical illusion." (See Dr. Branson, in "Quarterly Journal of Microscopical Science" for 1855, Vol. III., p. 274, and Mr. Wenham, p. 277, quoted by Dr. Carpenter, "Microscope and its Revelations," 3rd ed., p. 408.) This opinion was no doubt formed solely upon observations made during vital action, and may be modified upon examination of the supposed dead and dying ciliæ rendered visible by the action of the magenta dye. (Sec. Micro. Section.)

suggestive; he had noticed that fresh blood discs (for instance from a pricked finger) were not immediately affected by Magenta, but that some little time was required for the dye to permeate the envelopes.

Mr. Nevill exhibited a new form of cell cut out in card-board, containing seven divisions for seven different objects; it is very suitable for Foraminferæ, Diatomaceæ, &c., and may contain seven species, which will economise space in cabinets and facilitate exhibition. The perforations are about $\frac{1}{4}$ of an inch in diameter, and are made with a saddler's hand-punch; the disc is then covered with black varnish and secured to the glass slide.

Mr. J. B. Dancer exhibited new cells for opaque objects of various sizes; they are made of composition, and are cast in a mould.

Mr. J. G. Dale exhibited with the polariscope, crystallised films of Santonine, and of Picrate of Aniline; they are very rich in colour, and some of the forms are believed to be new.

MATHEMATICAL AND PHYSICAL SECTION.

Annual Meeting, March 5th, 1863.

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

On the motion of Mr. Atkinson, seconded by Mr. Dickinson, it was resolved that Rule 4 be altered by adding the following words: "No person shall hold the office of President for more than two years in succession."

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

President.

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

Vice-Presidents.

MR. ROBERT WORTHINGTON, F.R.A.S.

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

Treasurer.

MR. GEORGE MOSLEY.

Secretary.

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

In illustration of the abnormal character of the season, two barometric curves, neatly laid down on engraved forms by Mr. Sidebotham, from observations made by him at Ashton-upon-Mersey during the months of January and February last, and a summary of meteorological observations taken at Thelwall by Mr. Atkinson, F.G.S., during the month of February, were communicated to the Section. The mean height of the barometer at Thelwall for February, reduced to 32° Fahr., and to mean sea level, had the unusually high value of 30·218 inches; the mean temperature was 42·5°; and the total fall of rain was only 0·811 inches.

Mr. BAXENDELL communicated a table which he had calculated, showing the monthly sums of the oscillations of mean daily temperature at Greenwich during the thirteen years 1848—60; and also the mean daily values for the different months. The mean monthly results were as follows:—

January	102°8	July	82°8
February	84·9	August	78·4
March	83·4	September	72·3
April	90·6	October	84·4
May	83·9	November	94·5
June	87·3	December	107·1

The oscillations of mean daily temperature have hitherto been strangely neglected by meteorologists, although they undoubtedly form an important element in the question of climate. The curve laid down from the above numbers differs considerably from that of any other element of temperature, the principal maximum of disturbance occurring in December, and the principal minimum in September; and it is perhaps worthy of remark that the course of this curve appears to bear a well marked relation to the annual progress of the growth and decay of plants, the period of maximum disturbance of mean daily temperature corresponding with that when the vital principle in plants is least active; and the time of minimum with that when the ripening of the more important cereals and fruits takes place.

Mr. Baxendell also drew attention to the attempt which is now being made to organise an association for the systematic observation of variable stars, and presented to the Section a chart of the vicinity of the variable star R *Canis minoris*, neatly executed by Mr. George Knott, F.R.A.S., of Woodcroft Observatory, Cuckfield, Sussex. This chart extends over one square degree: the central portion of half a degree square includes all the stars which are visible with a 7-inch object glass, and the outer portion all Argelander's stars to the 9·5 magnitude. A list of twelve comparison stars is given, the magnitudes of which have been photometrically determined by the method of limiting apertures. In reference to the objects of the Association, Mr. Baxendell remarked that the importance of a careful study of the phenomena of variable stars will be apparent when it is considered that all the so-called fixed stars—our own sun included—are supposed to have a general similarity of constitution; and as several eminent astronomers have doubted whether the emission of light from the sun is absolutely constant, it is not improbable that an extended knowledge of the phenomena of variable stars may ultimately assist us in obtaining a more intimate acquaintance with the constitution and phenomena of the great luminary of our own system, and lead to the detection of changes at present not recognised, but which in their influence upon the earth may have an important bearing upon many questions of considerable interest in magnetical and meteorological science, and also upon the economy of the various forms of vegetable and animal life. The recent results of spectral analysis tend to show that the light of the sun has a general resemblance to that of the class of stars to which nearly all the known variables belong, and thus give additional weight to the probability that it may also be slightly variable.

Professor CLIFTON made a communication, in which he endeavoured to show that the principles of the mechanical theory of heat afford an explanation of phenomena attending the production of light by bodies, either when this production of light is due to incandescence or to fluorescence. His remarks and explanations were illustrated by a series of highly interesting experiments.

Ordinary Meeting, March 24th, 1863.

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

Among other donations for which the thanks of the Society were voted, was a framed photograph of the Executive Committee of the British Association in Manchester, by Mr. Alfred Brothers.

The following letter was read:—

Sir,

1. I have to call attention to some errors in Mr. Dyer's representation of the nature and contents of a short Essay of mine, which was read to the British Association, in 1854, and reprinted, with a few abridgments and emendations, in the "Engineer" for the 23rd of January, 1863, and which Mr. Dyer criticises in his "Notes on the Action of Heat and Force upon Matter," read to the Literary and Philosophical Society of Manchester on the 11th of March.

2. Those errors consist, in the first place, of a complete misconception of the object of my Essay; and in the second place, of misquotations, in which, by the alteration of some words and the omission of others, the meaning of my statements is perverted or reversed.

3. I have no doubt that those errors arose merely from inadvertency on the part of Mr. Dyer; and I trust that, when they are pointed out, his own sense of justice will make him see the propriety of having them corrected before his Paper is published at length in the "Transactions."

4. With regard to the object of my Essay, Mr. Dyer says (at page 78 of the abstract of his Paper), "Yet we have seen two elaborate essays, by Dr. W. J. M. Rankine and Mr. Tyndall, each based on the *force-heat* assumption, but without affording any solid proofs of its application to the phenomena adduced by them."

5. This passage is calculated to convey the idea that my Essay *professes* to give proofs of the mechanical theory of heat. But that Essay does not profess to give a single proof of that theory. It is entitled, " *Outline* of the Theory of the Mechanical Action of Heat:" it gives a sketch of the general principles and results of that theory only; and for the *proofs*, it refers to published detailed accounts, by various authors, of experimental researches, and calculations founded on the results of those researches, the whole of which published accounts are in the Library of this Society.

6. The proofs of this, as of every other physical theory, consist in a mass of experiments and calculations, which cannot be condensed within the limits of a brief popular Essay; and in order to examine and test those proofs, it is necessary to go to the fountain-head of information; that is, to the detailed accounts of calculation and experiment.

7. As to defective and erroneous quotations, I have in the first place to point out the following. The words in the genuine passage, whose omission in Mr. Dyer's quotation has the effect of materially altering the meaning of the passage, are enclosed in square brackets, thus: [ ].

THE GENUINE PASSAGE.	THE QUOTATION.
5. *Threefold Effect of Heat.*—The communication of heat to a substance produces [in general, three kinds of effects (setting aside chemical, electrical, and magnetic phenomena, as being foreign to the subject of the present paper) :—] 1st, An increase of temperature and expansive pressure; that is to say, an increased tendency to the communication of heat to	Mr. Rankine states " that heat produces an increase of temperature and of expansive force in bodies; that the change from the solid and liquid states to the gaseous state is accompanied by an increase in volume; that heat which produces those effects is known by the name of sensible heat, as retaining the form of heat, and in short making the body hotter."

other bodies, and to the development of mechanical power by expansion.

[2dly, A change of volume; which, under a constant pressure, is an *increase* for every substance, except some liquids near their freezing points.]

3dly, A change of molecular condition; as from the solid to the liquid state, or from the liquid or solid to the gaseous state, or any imperceptible change of molecular arrangement; the change to the gaseous state being always accompanied by an increase of volume.

The heat which produces [the first of] those effects is known by the name of *sensible heat*, as retaining the form of heat, and, in short, *making the body hotter*.

[The heat which produces the second and third of those effects is called *latent heat*, as having disappeared in developing a mechanical effect, and being capable of reproduction by reversing the change which caused it to disappear.]

8. It is evident that the omission of the words in brackets has the effect of precisely reversing the meaning of the definition which I have given of sensible as distinguished from latent heat.

9. I have next to point out that Mr. Dyer, in criticising the 4th Article of my Essay, represents me as making a certain statement, commencing as follows—"It is natural to suppose that the motion, during this phenomenon, has not been really destroyed," &c.—in order to account for what he calls the "*non-production* of heat by the moving force exerted in waterfalls."

10. Now there does not occur in my Essay one word about any such "*non-production* of heat," nor one phrase or allusion which by any conceivable amount of ingenuity can be construed to bear any such meaning. The "phenomenon" really referred to in the passage above cited, and distinctly described in the paragraph preceding it, is the well established fact of the *production* of heat by the fall of water; and that not as a thing to be accounted for, but as a fact to be reasoned from. In this instance my meaning has again been exactly reversed in Mr. Dyer's account of my Essay.

11. I shall not enter into any controversy respecting the theory of heat; for it appears to me that physical theories are questions to be settled, not by argument, but by experiment and calculation; and I have nothing at present to add to the experiments and calculations already published.

12. My only object in writing this letter is that my brief sketch of the mechanical theory of heat, which Mr. Dyer has criticised, may be judged according to its real contents, and may not be erroneously quoted in a publication so important as the *Transactions of the Literary and Philosophical Society of Manchester*.*

I am, Sir,

Your most obedient Servant,

W. J. MACQUORN RANKINE.

GLASGOW, 21ST MARCH, 1863.

* Mr. Dyer states that in making the quotations from the *Engineer* he had studied brevity, and had no thought of conveying any other than Mr. Rankine's meaning.—ED.

Mr. Sidebotham exhibited a large sheet of photo-lithographs, executed by Mr. Dean, of Douglas, Isle of Man. They consisted of copies of wood engravings, copper-plate, pen drawings, writing, music, and letter-press, all copied by the camera and then transferred to stone, and printed in the ordinary manner with printing ink. Mr. Sidebotham said he considered this by far the best specimen yet exhibited, from the extreme sharpness of the lines, and thought as this was a branch of art likely to be much used, it was of great interest to see its present state of perfection. He also called attention to the great facility this plan offered to the forger of Bank notes, &c.: hitherto, although there was no difficulty in getting a perfect *fac-simile* of a note or cheque, the fact of its being printed on paper by silver or iron salts made its detection easy; now that printing ink could be used a perfect *fac-simile* in every respect could be made, so far as the printing is concerned. Mr. Sidebotham thought some steps should be taken in the matter, and suggested as the most simple mode of defeating the photographer to use yellow paper and print in blue. So far as at present known, there would be no possibility of copying such a document by any process of photography.

Mr. Sidebotham read a Paper "On the Planet Mars," by James Nasmyth, Esq., and exhibited at the same time a large drawing of the planet, as seen by Mr. Nasmyth on the evenings of September 25th and October 11th, 1862, by the aid of his fine reflecting telescope of twenty inches diameter. Mr. Nasmyth called special attention to the patch of snow, as it was considered to be, at the south pole of the planet. This was unusually bright and round, like a white wafer on the pole of a globe; it had also a cliff-like edge. This supposed patch of snow does not coincide with the true pole of the planet, but, like the arctic poles of the earth, or poles of minimum temperature, is slightly eccentric.

Mr. Nasmyth also called attention to the different shades of the planet as exhibited in the drawing—which was a faithful representation of the appearance of the planet—and the probability of their being land and water. One ruddy spot in the blue band of the supposed sea he could not help thinking to be an island, and, as its first discoverer, he claims it to be called Nasmyth's Island. He also made some remarks on the superiority of the reflecting over the achromatic telescope, in giving the true colours of the heavenly bodies, and gave his explanation of the supposed cause.

Mr. W. H. L. Russell, A.B., wishes it to be stated that in consequence of errors in the printing of the abstract of his Paper "On the Solution of the Differential Resolvent," in the last number of the Proceedings, he fears that the results given as the solution of the differential resolvent will be unintelligible to the reader.

MICROSCOPICAL SECTION.

March 16th, 1863.

Mr. Joseph Sidebotham, Vice-President of the Section, in the Chair.

Mr. Watson presented specimens of Jungermannia tomentella and asplenoides, collected on Baguley Moor.

Mr. Sidebotham presented specimens of the following mosses, in fruit: Fissidens exilis, Fissidens adiantoides, Grimmia pulvinata, Weissia controversa, Bryum atropurpureum, &c., in a good state for microscopical examination.

Mr. J. G. Dale, F.C.S., presented a specimen of crystallised film of picrate of aniline; and in a note to the Secretary explained his method of preparation from picric acid and aniline. The equivalent of picric acid is 229; that of aniline is 93; and when dissolved in strong alcohol in those proportions by weight, mixed and set aside, the picrate of aniline will crystallise in yellow needles. The film for the microscope is formed from a solution of these needles in absolute alcohol, a drop of which being spread over a clean hot glass slide, the crystallised film is at once produced by the rapid evaporation of the alcohol, if the slide be at the proper degree of heat, which can only be found by repeated trials. If too hot, the salt will melt and become partially decomposed; if not hot enough, it will be crystallised in needles, or be deposited as an amorphous film. When properly crystallised, circular radiated discs will appear with more or less regularity, showing with the polariscope very brilliant colours, and a black cross in the centre. The crystallised films may be mounted in *new soft* balsam; but a mixture of chloroform and balsam dissolves them immediately.

The Natural History Society presented for distribution amongst the members a number of beetles not required for the Museum.

Mr. Nevill reported upon the fossil Foraminferous shells found in the Montreal deposit presented by Mr. R. D. Darbishire at the last meeting. They were mostly in a fine state of preservation, and many were as perfect as recent shells. He found—

Polystomella,	Entoselenia marginata,
Nonionina umbilicatula,	Ditto globosa, very fine,
Polymorphina lactea,	Patalina corrugata,
Miliolina seminulum,	Textularia,
Entoselenia squamosa, var: scalariformis,	Dentalina,
Ditto of a peculiar form and rare,	Lagena vulgaris.

The Polystomella and Nonionina were in great profusion, the other kinds were scarce; but Mr. Nevill was of opinion that remarkably fine specimens might be found of all the various kinds, if there were a larger quantity of material to operate upon. Mr. Nevill was indebted to the worthy President of the Section, Professor Williamson, for verifying the names, and he presented to the Section mounted and named slides for the cabinet. No diatomaceæ were found amongst the material.

Dr. Alcock exhibited a young living salmon, about 14 days old, attached to part of the ovum. Dr. Alcock particularly called attention to the form of the vertebral column, which, whilst young, is similar to that of the lower grade of cartilaginous fishes when fully grown; the skeleton of the salmon, however, becomes gradually changed, until at maturity it is that of the higher class of osseous fishes.

Dr. Alcock also exhibited a lingual riband of the Patella athletica, from Bray, in Ireland; he compared it with that of the common limpet, Patella vulgata, and pointed out the differences in the form of the teeth.

Dr. Roberts exhibited some mounted specimens of blood-corpuscles from an albuminous urine, which showed an appearance as if the contents of the cells had separated from the cell-wall, and become aggregated round the centre like a nucleus. When these corpuscles were treated with magenta, the central portion was either not coloured at all, or only faintly so, whereas the circumferential portions became deeply tinted. By treating fresh blood with an excess of a solution of carbolic acid this appearance could be produced at will. In the blood-corpuscles of the fowl a similar effect was produced by the carbolic acid solution; the cell contents appeared to detach themselves from the cell-wall and to collect round the nucleus. The appearances presented, strongly suggested the idea that the cell envelope of the blood-disc was a double membrane; that the inner separated under certain circumstances from the outer membrane and shrank in toward the centre. Dr. Hensen, of Kiel,* seems to have convinced himself that such is the case in the blood-disc of the frog, and he compares the inner membrane to the primordial utricle of the vegetable cell. Of the prolongations described by Dr. Hensen as stretching radially between the shrunk inner membrane and the outer one, Dr. Roberts saw nothing. If the said view of the structure of the blood-cells were substantiated, it would greatly facilitate the explanation produced on these cells by magenta and tannin.

Mr. Charles O'Neill, F.C.S., exhibited a mounted fibre of Orleans cotton, torn by a gradually increasing weight suspended to its extremity. It had sustained a weight (gradually

* Siebold and Kölliker's Zeitschrift, for 1861, p. 263.

increased) of 162 grains for many minutes. Mr. O'Neill stated that there were 143 such fibres in ·01 grains of cotton, each fibre therefore weighing less than the ten thousandth part of a grain. The strongest fibres were capable of supporting more than two million times their own weight. He is engaged in making experiments upon the tensile strengths of various fibres by a special apparatus, but they are not yet completed.

Mr. Brothers exhibited a number of fresh water insects, larva, &c.

Mr. Parry exhibited the transverse section of a Fossil Palm, from the Island of Antigua.

Ordinary Meeting, April 7th, 1863.

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

Mr. Edmund Salis Schwabe, B.A., was elected an Ordinary Member of the Society.

Messrs. Robert Worthington and G. V. Vernon were appointed Auditors of the Society's Accounts for the present Session.

Mr. Brothers exhibited and described an apparatus for determining the magnitudes of stars. (See Proceedings of Physical Section for April 2nd.)

Professor Roscoe communicated the following extract from a letter he had just received from Professor Bunsen, respecting the atomic weight of cæsium. Professor Bunsen formerly found the atomic weight of this metal to be 123; since then, Messrs. Johnson and Allen, Yale College, United States, found, by operating on large quantities, the number to be 133. He writes, "The atomic weight Cs.=133 is quite right. The difference is caused by an error of about one per cent on the chlorine contained in the chloride of cæsium, owing to an amount of impurity which cannot be separated according to the method I first used for purification. I have employed all the three grammes of Cl Cs, with which I was obliged to make the whole of the investigation of the cæsium compounds for another determination of the atomic weight of the metal, and I find the same number (133) as the Americans found, but by quite a different method. The alteration from 123 to 133 makes a difference in the analysis of most of the salts, of from a quarter to one per cent in the percentage composition."

Mr. G. C. Lowe exhibited part of a mercurial seconds pendulum, which, from its mode of construction, bears a strong resemblance to a horse-shoe magnet, and possesses considerable magnetic power. It is what is called the stirrup, and has attached to its lower extremities a circular brass disc, upon which the jar of mercury is placed. The stirrup is attached to the lower end of the pendulum rod, and has two steel rods, about ten inches in length, connected at their upper ends by a short steel bar, so that when magnetised the lower ends form one a north and the other a south pole. It had been suggested by Mr. Baxendell that some irregularities which are observed in the rates of clocks may be explained by referring them to variations in the intensity of the earth's magnetism influencing a pendulum which has thus become magnetised. He had noticed in two clocks which have mercurial pendulums sudden changes of rate amounting to nearly or in some instances quite one second per day. There were no considerable changes of temperature or of the atmospheric pressure at the time, and the compensation is so nearly perfect that so great a deviation from the rate could not be due to change of temperature. A very brilliant display of aurora borealis was observed by Mr. Baxendell on the evening when the last remarkable change of rate took place, which favours the view now suggested, viz., that the lower part of the pendulum having become magnetised, an increase or diminution of the intensity of the earth's magnetism may have the effect of accelerating or retarding the motion of the pendulum. Mr. Lowe suggested that if the rods forming the stirrup were connected with brass at the top instead of steel, they would have much less force as magnets. In a pendulum also under Mr. Baxendell's observation, the disturbance of rate was much smaller in amount, scarcely exceeding one-third of the change that was found in the two clocks already mentioned. This pendulum consists of a long steel rod and two shorter ones, and has a very correct compensation effected by a zinc tube. This pendulum has another advantage, viz., that the steel rods and the zinc tube are so nearly the same in size that sudden changes of temperature affect all the parts nearly simultaneously, whereas in the mercurial pendulum the steel rod is much more quickly affected than the large mass of mercury; the compensation therefore does not take place soon enough to prevent a small error in the clock's rate.

A letter by Mr. Dyer was read, relative to Dr. Rankine's reply, inserted in the Proceedings of March 24.

Mr. Dyer disavows all intention of unfairness towards Dr. Rankine, and states that although the brevity which it was necessary to observe in drawing up his abstract compelled him to give only detached passages, he conceived that the sense could not be considered to be perverted by reason of the omissions. He had always felt and expressed the highest regard for Dr. Rankine's genius and learned researches.

A Paper was read entitled "Note as to two Events in the History of Steam Navigation," by W. J. Macquorn Rankine, C.E., LL.D., F.R.S., Hon. Member of the Society.

(1.) An interesting Paper was lately read to this Society by Mr. Dyer, containing a history of a series of important events in the progress of Steam Navigation.

(2.) It is to be regretted, however, that the author has noted either very slightly or not at all, what appears to have been an event of paramount importance in the first adaptation of the double-acting cranked steam engine to drive a paddle wheel. Before that adaptation was made, the success of all attempts at steam navigation, such as those of Jouffray, Rumsey, Titch, Miller, and Taylor, &c., had been only temporary, because of the rudeness of the machinery for communicating motion from the piston to the shaft.

(3.) That first adaptation was unquestionably accomplished by William Symington in 1801, as is proved by authentic documents which have been published by Mr. Woodcroft in his "Origin and Progress of Steam Navigation." Symington, instructed by the failure of the ratchet-work engine which he had made for Miller's boat, fitted up the "Charlotte Dundas" in 1801, with a double-acting horizontal cranked engine, and this made her what Mr. Woodcroft has justly called "the first practical steamboat." Her speed, when running alone, and not towing other boats, was six miles an hour.

(4.) The use of this vessel was abandoned, not from any fault in her construction or working, but because the Directors of the Forth and Clyde Canal feared that she would damage its banks. Yet the man in all Britain who possessed, at that time, the greatest practical experience of the working of canals—the Duke of Bridgewater—was not deterred by any such apprehension from ordering, in 1802, *eight* similar vessels, from Symington, to be used on his canal.

(5.) The death of the Duke of Bridgewater, early in the following year, prevented the execution of that order. But Symington had evidently done all that lay in his power, and all that was necessary, to convert the steamboat from an awkward piece of experimental apparatus to a practically useful machine; and the honour paid to his memory ought not to be lessened because the career of his invention was cut short by a misfortune.

(6.) There is nothing in this to detract from the honour which is justly paid to Fulton, as having been the first to practise Steam Navigation on a great scale, as a commercially profitable art.

(7.) Another event passed over in the Paper to which I have referred, is the first introduction of commercial Steam Navigation into Europe, which was effected on the river Clyde, in 1812, by Henry Bell, as is proved by documents cited in Mr. Woodcroft's work, already referred to.

Dr. R. Angus Smith said that he had been using an amalgam of sodium for the production of hydrogen. When doing this it was found that the amalgam had become one mass of crystals. These crystals are fine needles or prisms of considerable length; some may be traced in the tangled mass for about half an inch; they present hollow ends, although it was not possible to trace this hollowness through the whole length of the crystals. They are hexagons, and contain 9·47 per cent of sodium; an excess of mercury remains; 10·3 per cent would be equal to 2 at. of mercury and 1 of sodium. He had not seen any notice of an amalgam of similar composition. It can be examined only through glass, as it rapidly absorbs oxygen from the moisture of the air, and cannot be retained in the air many seconds without change being perceived. If this change be examined under a microscope, bubbles of hydrogen are seen to rise from every corner, extremely minute certainly, so that many millions would be required to cover a square inch, but affording one of the most convincing and direct proofs of the existence of moisture. This property may in many cases be found to be of value. The inclination to combine with oxygen when dry seems comparatively slight.

Dr. Smith mentioned also a method of separating lime from phosphoric acid, which, as far as he was aware, was new. Many very elaborate methods had been given. After adding tartaric acid to a solution of phosphate of lime in muriatic acid, the lime may be precipitated by oxalate of ammonia and ammonia as in other solutions. Carbonate of lime has been obtained in this way free from every trace of phosphate, and the solution has been freed entirely from lime. Tartaric acid prevents the precipitation of phosphate of lime in weak solutions by ammonia, but has no influence over the oxalate of lime. The true action of tartaric and similar acids in preventing precipitation deserves a more careful inquiry than has yet been made.

April 2nd, 1863.

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

A Paper was read entitled "Notes of Observations of the Speed and Order of Succession in Magnitude of Waves observed in Gales of Wind off the Cape of Good Hope," by Mr. Thomas Heelis, F.R.A.S.

The highest waves measured were about 35 feet in height from the trough, no broken crests having been measured. Their speed varied (the force of the wind being 8 according to the Board of Trade scale, and equal to weather in which a ship on a wind can just carry treble-reefed topsails) from twenty to twenty-three miles per hour, the breadth of trough being 300 to 350 feet. The observations show that usually the succession of magnitudes (or heights) returns in series of twelve waves, the first and second of each series being very large, the sixth or seventh being also large but inferior in magnitude to the first and second, and the intermediate ones being small. The observations show that waves are limited in length, measured along their bases, the crest being apparently at the middle point of the base, and the length varying with the altitude of the crest, and that the order of succession of magnitudes depends upon their being arranged so that the crest of one wave follows on the same line as the lower flanks of a preceding wave. The speeds also of waves appear to vary, so that a following wave often coalesces with and is increased in size by absorbing one immediately preceding. When a wave is first formed it is small, and increases in size in its progress, until the crest topples over in foam, after which the height decreases rapidly, and there seems reason for thinking that if ordinates were drawn so as to represent the height of any wave at different periods of its existence, its height would be found to coincide with Mr.

Seott Russell's wave-line curve. The length of a wave in open water, measured along its base, seems also to depend upon and bear a definite relation to the width of the trough between two sueeessive waves. The speed of the waves is not so mueh affected as would naturally be imagined by the force of the wind. In a moderate gale they run as fast as in a heavy one. It is otherwise with their height.

Mr. ALFRED BROTHERS read the following Paper "On an Improved Diaphragm for determining the Magnitudes of Stars."

At a recent Meeting of this Soeiety, when Mr. Baxendell made a communieation respecting one of the variable stars of small magnitude, it oecurred to me that the system used by him for determining the variability of the smaller stars, must be not only tedious but often unsatisfaetory, owing to the neeessity for leaving the position of observation for the purpose of changing the diaphragms.

When apertures of various diameters are used as diaphragms, placed at the end of the dew-eap of large telescopes, and partieularly refraetors, it must at all times be diffieult to make these ehanges, even when the advantage of elock-work motion is available; but, with telescopes whose motions must be controlled by hand, the possibility of determining with aeeuraey sueh results as the one-tenth of a magnitude must be very uncertain; as, unless an assistant be at hand to ehange the diaphragms during the observation, the observer must be constantly liable to lose the star at the moment it becomes neeessary that it should be in the centre of the field of view, and the eye in position to note the effeet produced by the deerease of light on the ehange of diaphgram being effected. Several eontrivanees have been adopted to contract the aperture of the telescope, such as square and hexagonal forms, opened and closed meehanically; but hitherto, it is believed, no attempt has been made to retain the *circular* form of aperture.

In order to facilitate observations of this character, and to enable the observer to dispense with an assistant, I have recently adapted to my telescope (of 5in. aperture) an apparatus which may be termed the "Iris Diaphragm," as its action resembles to some extent the expansion and contraction of the pupil of the eye. The invention was, I believe, patented by Mr. Jennings, of London, as a capsule for closing jars, bottles, &c., but has been superseded by a more simple method for the same object. If found to be practically successful for the purpose I have now applied it to, it will, I think, be extremely useful in the Observatory, and form a desideratum long required by Astronomers. A great saving of time will be effected, and the results will be regarded with greater confidence.

The following is a description of the invention, and the method I propose for adapting it for astronomical purposes.

The *invention* consists of two rings of metal, one working within the other, of nearly the same diameter, but differing in width, to the outer edges of which, is attached a piece of sheet india-rubber joined at the ends, and so placed that when the rings are moved in opposite directions the india-rubber is stretched, and closes the aperture of the rings when they have made about one half of a revolution, the aperture remaining circular or nearly so, its perfect circularity depending on the proper attachment of the india-rubber.

The *adaptation* I have effected in the following manner. A flat ring of tin with a flange is made to fit accurately the end of the dew-cap of the telescope. To the flange the diaphragm is soldered; to the broader ring of the diaphragm is fixed a circle of brass, having teeth cut rather more than one-half round its circumference. Fixed to the ring, which is attached to the dew-cap, is a bearing for a small pinion, which gives motion to the half wheel by means of a slender rod (of brass tubing) the length of the telescope, having at the eye-piece end, a handle of convenient form, and worked by the left hand. The rod is supported at the eye-piece end of the telescope, on a bearing attached to a metal collar made to clip the telescope by a screw, and attached to this bearing is a ratchet wheel, which prevents the too rapid reverse motion of the india-rubber diaphragm. The ratchet is not absolutely necessary with the present apparatus, owing to the friction of the pinion against the wheel, preventing the india-rubber contracting too rapidly.

It is not considered necessary to give the dimensions of the parts of the apparatus, as in every case these must vary with the diameter of the object-glass of the telescope. The principal point to be attended to, is the width of the broader ring, which is covered inside by the india-rubber, and this must be sufficiently wide to close the aperture when in use. In order to prevent the over-winding of the diaphragm, stops are placed at each end of the teeth on the half wheel.

An index of some kind will be necessary to make the apparatus complete. A pointer might be fixed on the end of the dew-cap, and a scale might project from the wheel. The diameter of the various openings could also be determined by the ear, the number of teeth in the wheel and pinion being known, the ratchet wheel could be so adapted, that a certain number of *clicks* would indicate that a certain aperture is then in use. This method, however, might be tedious to the observer, owing to the necessity for counting. The index and scale will perhaps be more convenient, and if the light from the sky should not be sufficient to show the reading of the scale, the micrometer lamp could be placed so as to illuminate the scale and index only.

The additional weight at the end of the telescope renders a counterpoise necessary, and this is effected on my telescope by a slight addition to the counterpoise already attached.

It may perhaps be necessary to say, that I have as yet tried the apparatus merely for the mechanical working, but am very sanguine that it will answer the purpose intended more effectively, than any other method I am acquainted with.

Mr. G. V. Vernon, F.R.A.S., communicated the following returns of the Rainfall for 1862.

Royton, near Oldham.

By John Heap, Esq.

The rain-gauge is 10 inches diameter; and is 3 feet above the ground, and 500 feet above sea level.

1862.	Fall in Inches.	Days.	Mean of 27 Years, 1836—1862.
January	2·622	17	2·168
February	0·736	11	2·092
March	3·073	17	2·235
April	2·709	24	2·142
May	4·636	23	2·360
June	3·332	20	3·080
July	4·332	24	3·399
August	2·545	16	3·563
September	4·064	16	2·905
October	6·361	19	3·620
November	1·737	12	2·980
December	5·091	21	2·607
	41·238	220	33·151

Eccles, near Manchester.

By Thomas Mackereth, M.B.M.S.

Gauge 2 feet above the ground, and 105 feet above the sea.

	1862.	Fall in Inches.	Days it Fell.
8·005	January	2·820	22
	February	0·799	9
	March	4·386	17
9·533	April	2·460	18
	May	3·820	21
	June	3·253	25
10·986	July	4·217	21
	August	2·224	16
	September	4·545	17
9·140	October	4·589	24
	November	1·675	13
	December	2·876	22
	Total	37·664	225

Annual Meeting, April 21, 1862.

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

Mr. Leopold Hartley Grindon was elected an Ordinary Member of the Society.

The following reply to Dr. Rankine's Paper, read at the last meeting, was communicated by Mr. Dyer:—

In Dr. Rankine's Paper, entitled, "Note on two Events in the History of Steam Navigation," he calls attention to my Paper, "On the Introduction of Steam Navigation," and expresses regret that "the Author has noted either very slightly, or not at all, an event of paramount importance:—the first adaptation of the double acting cranked steam engine, to drive a paddle-wheel." Now, I own to have taken no notice at all of this double cranked motion, simply because I did *not* consider it of much importance in attaining success in *steam navigation.* I had before me Mr. Woodcroft's interesting treatise, and I fully appreciated his advocacy of the claims of Mr. Symington for having "fitted up the 'Charlotte Dundas,' as the first practical steam boat, in 1801." This *fact* is fairly stated in my Paper.

I aimed to place before the Society the several inventions and discoveries relating to the use of *steam power* to supersede that of wind to navigate vessels, and to prove that the final *success* was due to the invention of *Watt's steam* engine. No great stress need be laid on the double cranked action, or on any of the other methods used for *transmitting* the power to the paddle wheels. The principles involved in overcoming the resisting forces by steam power in navigation, are entirely apart from those relating to the mechanical means of converting rectilinear into circular motion. It seems strange that such able engineers as Dr. Rankine and Mr. Woodcroft should have given this prominence to the said double crank action. Whatever may have been the degree of success in the case of the "Charlotte Dundas" in 1801, and whatever may be said about the bar to further progress of Symington's inventions by the *Duke's death*, the fact remains clear that his schemes died out with the Duke, as no more was heard of them after the "Charlotte Dundas," and from some cause, she was discontinued; and it was in fact fifteen years after her advent before the "Margery" came out from the same waters to enter the Thames, in 1816. What shall be said of the "enlightened" of Glasgow, if "Symington's practical steam boat" was suffered to rot and be forgotten, leaving no successor, and the inventor himself to remain unrewarded and unnoticed for fifteen long years?

Through the limited space allowed for the abstract of my paper, Dr. Rankine was misled respecting the other *event*, also cited by him from Mr. Woodcroft's book, namely,—that I had "passed over the first introduction of steam navigation into Europe, by Henry Bell, in 1812;" for in my paper due notice is taken of this well known experiment of Mr. Bell. Although his trial boat proved a failure, on account of its being a very small one, and of his want of pecuniary means for continuing or extending the experiment, and the lack of any aid or encouragement afforded him by others, yet his trial boat served as the model for constructing the "Margery," three years after, which, as I have before said, was a success, and the first steam boat on English waters. I therefore think that due honour should be paid to the name of Henry Bell, for his spirit and enterprise, and a due stigma cast upon the capitalists who allowed him to sink under pecuniary pressure, from which he ought to have been relieved by them in so important an enterprise.

REPORT OF THE COUNCIL.

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

At the commencement of the present session, the number of ordinary members was 204; during the course of the session 18 members have resigned; 2 have been struck off the list as defaulters; one member, Mr. G. D. Fleming, has died; and 4 new members have been elected. The present number of ordinary members is therefore 187. No alteration has taken place in the list of honorary or corresponding members.

Your Council are glad again to be able to refer to the very satisfactory Financial Statement of the Treasurer, from which it appears that the balance in hand this year amounts to £320. 18s. 7d., against £248. 6s. 7d., in hand last year. Your Council are likewise glad to remark the increasing estimation in which the Proceedings of the Society are held by the scientific and reading public, as evidenced by their being regularly printed in full in several literary and scientific journals.

The valuable library of literary and scientific reference, which the Society now possesses, has during the past year been enriched by the additions of the memoirs and transactions of the numerous academies and scientific societies throughout the world, with which the Society is now connected. These additions are noted in the Librarian's Report annexed.

Amongst numerous gifts of literary and scientific value which have been made to the Society during the past year, your Council desire to mention the donation by Dr. W. C. Henry, of an extensive collection of letters, papers, and other documents belonging to the late Dr. Dalton; and also several manuscripts, diagrams, &c., belonging to the late Professor Hodgkinson, and presented by his widow. These papers, many of which have a high scientific value, are preserved in the archives of the Society.

The following is a list of the papers and communications which have been brought before the Society during the present session :—

October 7*th*, 1862.—" Memoir of the late Professor E. Hodgkinson, Part 2nd," by Robert Rawson, Esq., Hon. Mem.

October 21*st*, 1863.—" Notice of a Compressing Air Pump," by Dr. J. P. Joule, F.R.S.

November 4*th*, 1862.—" On a certain class of Linear Differential Equations," by the Rev. Robert Harley, F.R.A.S.

" Note on Differential Resolvents," by William Spottiswoode, Esq., M.A., F.R.S., communicated by the Rev. Robert Harley, F.R.A.S.

December 2*nd*, 1862.—" On certain Linear Differential Equations," by James Cockle, Esq., M.A., F.R.A.S., F.C.P.S., &c., communicated by the Rev. Robert Harley, F.R.A.S.

December 16*th*, 1862.—" On the Influence of the Earth's Rotation on Winds," by Thomas Hopkins, Esq., M.B.M.S.

" On the History of the Mechanical Theory of Heat, by M. Seguin," communicated by Dr. J. P. Joule, F.R.S.

December 30*th*, 1862.—" On the New Red Sandstone and Permian Formations as Sources of Water Supply for Towns," by Edward Hull, Esq., B.A., F.G.S., communicated by the President.

January 13*th*, 1863.—" Note on the Permian Deposit of the North-West of England," by E. W. Binney, Esq., F.R.S.

January 27*th*, 1863.—" On a New Variable Star U Tauri," by Joseph Baxendell, Esq., F.R.A.S.

" On Ocean Swell," by Thomas Heelis, Esq., F.R.A.S.

February 10*th*, 1863.—" Notes on the Introduction of Steam Navigation," by J. C. Dyer, Esq.

" On a Barometer for Measuring small Atmospheric Disturbances," by Dr. J. P. Joule, F.R.S.

February 24*th*, 1863.—" On the Spectrum of the Flame Evolved in the Bessemer Process," by Professor Roscoe, B.A.

" On the Existence of a Crystallizable Carbon Compound and Free Sulphur in the Alais Meteorite," by Professor Roscoe.

" On Maximum Groups," by the Rev. T. P. Kirkman, M.A., F.R.S.

March 11*th*, 1863.—"On Bring's Reduction of the Equation of the Fifth Degree to a Trinomial Form," by the Rev. Robert Harley, F.R.A.S.

"On the Solution of the Differential Resolvent," by W. H. L. Russell, A.B., communicated by the Rev. Robert Harley.

"On a New and Extremely Sensitive Thermometer," by Dr. J. P. Joule, F.R.S.

"On Evidences of the Antiquity of Man in the Valley Gravels in the neighbourhood of Manchester," by E. W. Binney, Esq., F.R.S.

"Notes on the Action of Heat and Force upon Matter," by J. C. Dyer, Esq.

"On the Chemical Constitution of American Rock Oil," by C. Schorlemmer, Esq., communicated by Professor Roscoe.

March 24*th*, 1863.—"On the Planet Mars," by James Nasmyth, Esq., C.E., communicated by Joseph Sidebotham, Esq.

April 7*th*, 1863.—"On a New Diaphragm for Determining the Magnitudes of Stars," by A. Brothers, Esq.

"Notes on Two Events in the History of Steam Navigation," by Dr. W. J. Macquorn Rankine, F.R.S.

Several of these have been ordered by your Council to be printed in the Memoirs of the Society, and others are under consideration.

LIBRARIAN'S REPORT.

Since the close of last Session 815 vols., 792 parts of vols., and 288 pamphlets have been added to the Library; of which 236 vols. and 43 parts of vols., by purchase, and the remainder by exchange or donations.

The periodical publications which the Librarian is authorised to procure for the Library by purchase, are:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Quarterly Journal of Pure and Applied Mathematics.
The Cavendish Society's Publications.
The Ray Society's Publications.
The Palæontographical Society's Publications.

The Natural History Review.
The Annals and Magazine of Natural History.
The Edinburgh New Philosophical Journal.
Les Annales de Chimie et de Physique.
Le Journal de l'Ecole Polytechnique.
Les Annales des Sciences Naturelles.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Poggendorff's Annalen der Physik und Chemie.
Annalen der Chemie und Pharmacie.
Journal für die reine und angewandten Mathematik.
Die Astronomische Nachrichten.

The Philosophical Transactions for 1845 and 1846 have been taken out from the Library without being entered in the Registers.

It is to be hoped that these volumes will be returned without delay.

On the motion of Mr. E. Hunt, seconded by Dr. Clay, the Report was unanimously adopted.

Cr.

	£. s. d.	£. s. d.
To Balance in the Bank of Heywood Brothers & Co....	220 7 9	
,, Balance in Treasurer's hands	27 18 10	
		248 6 7
,, Arrears of Subscriptions 1861-2, 2	4 4 0	
,, Subscriptions 1862-3, 173	363 6 0	
,, Half Subscriptions 1863, 2	2 2 0	
,, Subscriptions in Advance 1863-4, 2	4 4 0	
		373 16 0
,, Admission Fees 4		8 8 0
,, Memoirs sold—Vol. I. 3rd series	14 3 6	
,, ,, Former volumes	1 7 2	
,, Proceedings sold	1 7 5	
		16 18 1
,, Interest from Bankers	4 0 11	
,, Donation to Library	1 0 0	
		5 0 11
,, Sectional Contributions—Mathematical	2 2 0	
,, ,, Statistical	2 2 0	
,, ,, Microscopical	2 2 0	
		6 6 0
,, Photographic Society attendance, coals, &c., at Meetings	11 10 0	
,, Ditto, donation towards repairs	5 0 0	
		16 10 0
		£675 5 7

Library Fund	£39 16 0
Compositions	72 10 0
General Balance	208 12 7
Total Balance	£320 18 7

	£. s. d.	£. s. d.
By Chief Rent	12 8 10	
,, Fire Insurance	5 15 0	
,, Property Tax	6 7 6	
		24 11 4
,, Water Rent	1 1 0	
,, Gas	2 17 2	
,, Candles	0 4 9	
,, Coals	10 7 0	
,, Cleaning	3 10 5	
,, Furniture	4 14 7	
,, Repairs	33 11 1	
,, Receipt Stamps	0 19 2	
		57 5 2
,, Keeper of the Rooms—Salary	60 0 0	
,, Assistant to Keeper	15 12 0	
		75 12 0
,, Tea, Coffee, &c., for Members	18 6 4	
,, Postage and Parcels	22 14 0	
,, Printing and Stationery	20 15 6	
		61 15 10
,, Printing Proceedings	17 8 6	
,, Memoirs—Printing, Engraving, &c.	24 13 9	
,, Memoirs purchased	0 10 0	
		42 12 3
,, Library—Books, &c.	33 5 7	
,, ,, Binding—Special Fund	46 11 0	
		79 16 7
,, Keeper of the Rooms for attendance on Sections...	6 6 0	
,, Photographic Society for attendance	3 5 0	
		9 11 0
,, Solicitors for Registration of Rules		3 2 10
Total Disbursement		354 7 0
April 1st, 1863.—By Balance in the Bank of Heywood Brothers	314 8 8	
,, By ditto in Treasurer's hands	6 9 11	
		320 18 7
		£675 5 7

Examined and found correct, 10th April, 1863.

ROBERT WORTHINGTON, } Auditors.
G. V. VERNON

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

President.

EDWARD WILLIAM BINNEY, F.R.S., F.G.S.

Vice-Presidents.

JAMES PRESCOTT JOULE, LL.D., F.R.S., F.C.S., &c.
ROBERT ANGUS SMITH, PH.D., F.R.S., F.C.S.
JOSEPH CHESBOROUGH DYER.
EDWARD SCHUNCK, PH.D., F.R.S., F.C.S.

Secretaries.

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

Treasurer.

ROBERT WORTHINGTON, F.R.A.S.

Librarian.

CHARLES FREDRIK EKMAN.

Of the Council.

REV. WILLIAM GASKELL, M.A.
FREDERICK CRACE CALVERT, PH.D., F.R.S., &c.
PETER SPENCE, F.C.S.
GEORGE MOSLEY.
ALFRED FRYER.
GEORGE VENABLES VERNON, F.R.A.S.

MICROSCOPICAL SECTION.

April 20th, 1863.

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

Mr. Charles O'Neill, F.C.S., and Mr. John Shae Perring, M.Inst.C.E., were elected Members of the Section.

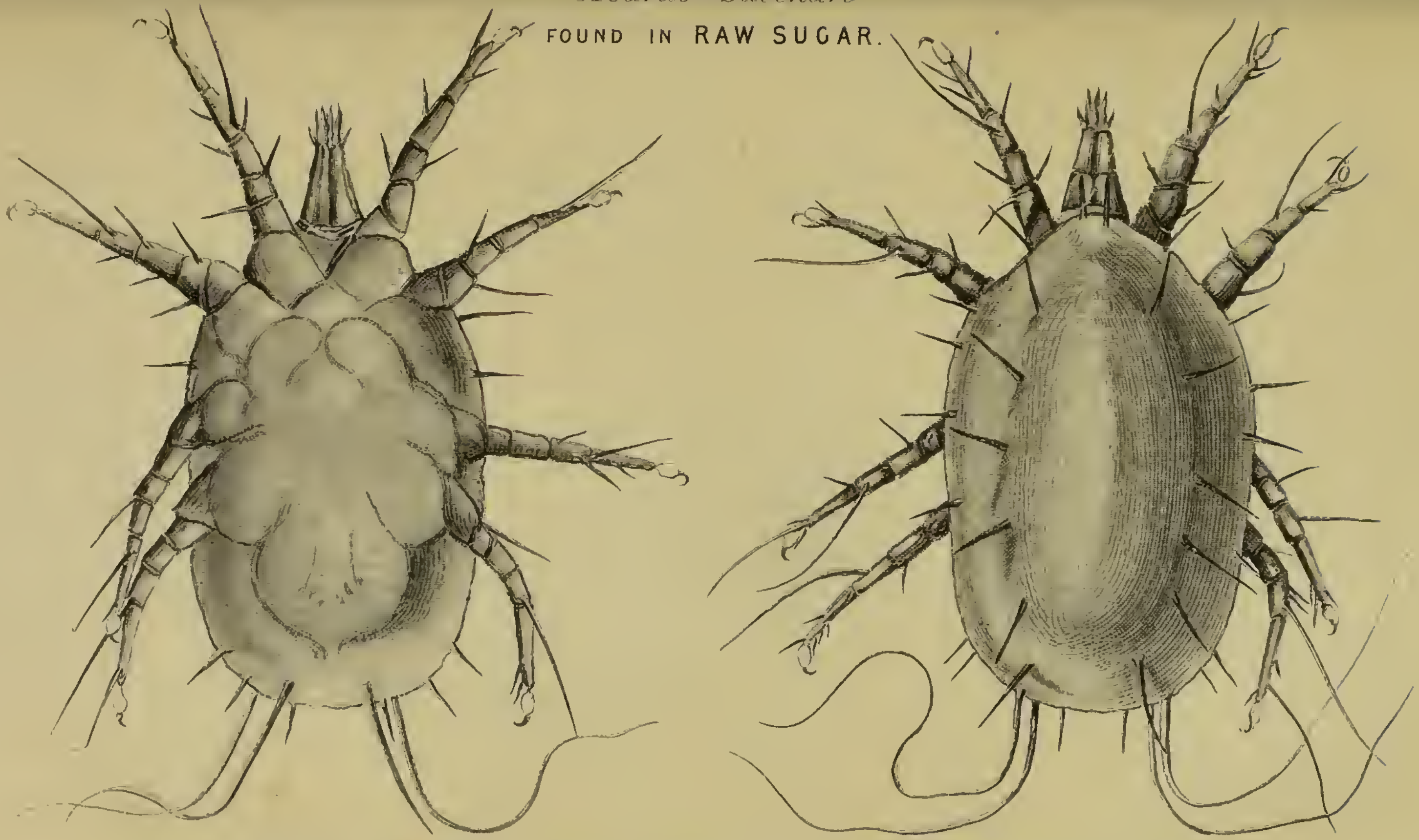
Mr. John Slagg, jun., and Mr. H. A. Hurst, were elected auditors of the Treasurer's accounts.

Mr. ALFRED FRYER presented for distribution amongst the members a number of impressions of an engraving of the Acarus sacchari found in raw grocery sugar, from Mauritius.

Mr. BROTHERS stated that he had made some observations upon the circulation in plants, and he found that a degree of heat which would cause free circulation in Vallisneria, entirely destroyed it in Chara vulgata. Mr. Brothers also described the appearances presented by the cilia of Melicerta ringens, which he had the unusual opportunity of observing whilst the animal was outside its case in a dying state. As the motion of the cilia gradually became fitful and then ceased, it was apparent that the cilia of the inner row are much longer than those of the outer row, over which the former appear to bend and to brush off whatever may be adhering to them into the channel between the two rows. Thus are produced the wavy lines and apparent onward progression of the cilia, which render this, under suitable illumination, so brilliant and interesting a microscopical object.

"Acarus Sacchari"

FOUND IN RAW SUGAR.



DRAWN FROM LIFE FROM INSECTS FOUND IN GROCERY MAURITIUS SUGAR.

By Smith, Beck & Beck, Microscopists London.

Photographed & Engraved by Maclure Macdonald & Macgregor

Mr. Charles O'Neill, F.C.S., made a communication "Upon the Appearances of Cotton Fibre during Solution and Disintegration." These experiments referred to the application of Schweizer's solvent. Two strengths were used; the weaker contained oxide of copper, equal to 4·3 grs. metal per 1,000, and 47 grs. dry ammonia; the stronger contained 15·4 grs. metal and 77 grs. dry ammonia per 1,000. The latter is about the most concentrated solution which can be made. Referring to the researches of Payen, Fresny, Peligot, Schlossberger, and others who have employed this solvent, the author said the only experimenter who seemed to have worked in the same direction with himself, and that apparently only to a small extent, was Dr. Cramer, whose paper he had only been able to see in a translation appended as a note to a memoir of M. Payen in *Comptes Rendus*, p. 319, vol. xlviii.

Mr. O'Neill considers that cotton exhibits, under the action of this solvent, (1) an external membrane distinct from the true cell wall or cellulose matter; (2), spiral vessels situated either in or outside the external membrane; (3), the true cell wall or cellulose; and (4), an inner medullary matter. The external membrane is insoluble in the solvent, and may be obtained in short hollow cylinders by first acting upon the cotton with the dilute solvent so as to gradually remove the cellulose, and then dissolve all soluble matters by the strong solvent. If the strong solution is first applied, the extraordinary dilation of the cellulose bursts the external membrane, and reduces it to such a state of tenuity that it is invisible. This membrane is very elastic, appears to be quite impermeable to the solvent, and when free from fissures protects the enclosed matter from its action. It is not seen in cotton which has been submitted to the action of alkaline acids and bleaching powder, being either chemically altered, or, what is most probable, entirely removed.

The spiral vessels are unmistakeably apparent, running round the fibre in more or less close spirals, sometimes single, sometimes double and parallel, and at other times double and in opposite directions, or again seemingly wound close and tight round the cylinder. They are well seen in the spherical swellings or beads, but are prominent at the points of strangulations of long ovals formed when the ends of the fibres are held tightly. They collect in a close mass, forming a ligature, and are frequently ruptured, the ends projecting from the side of the fibre.

The cellulose is enormously dilated by the weaker solvent, and expands the external membrane into beautiful beads, which are doubtless the result of the spiral vessels acting as ligatures at the points of strangulation; at the open end of a fibre it can be seen oozing out as a mucilaginous substance. The stronger solution bursts the beads, or dissolves all the cellulose into a homogeneous mass, amidst which the empty cuticular membrane and the spiral vessels remain nearly unacted upon.

The substance called medullary matter is seen occupying the axes of the fibres; it is nearly insoluble in the solvents. It may be well seen projecting from the open end of a fibre where the cellulose is exuding, and often remains *in situ* when the fibre has quite disappeared. It has many appearances of being a distinct body, but the author in some cases thought it might be only the thickened or modified inner cell wall; in others it looked like a shrunk membrane, probably the dried up primordial utricle. It is generally absent or indistinct in old cotton, or cotton which has been submitted to bleaching agents.

Mr. O'Neill intends to submit further details when his investigations are more advanced.

Mr. Hepworth stated that he had observed spiral markings in Sea Island Cotton, not subjected to Chemical Action, and that he had calculated there would be about 50,000 spirals to an inch of fibre.

PROCEEDINGS

OF

THE LITERARY AND PHILOSOPHICAL SOCIETY.

SESSION 1863—1864.

PHYSICAL AND MATHEMATICAL SECTION.

April 30th, 1863.

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

A Paper was read by Mr. THOMAS CARRICK, "On the Wave of High Water, with Hints towards a New Theory of the Tides."

The Author, starting from a new hypothesis on the relations of terrestrial matter to cosmical force, has arrived at the conclusion that the tidal motions of ocean surfaces are caused by a differential action of force centering on land areas. The nature of this hypothesis, and its relation to those motions, were briefly illustrated from the point of view of an assumed nebulous origin of the solar system. The Author nevertheless declined to endorse the received "nebular hypothesis" as a genetic theory, and adopted its ideas and phraseology in his Paper solely from considerations of brevity in this incidental portion of his subject.

Assuming the existence of a diffused nebula, composed of ultimate atoms of matter, each having a normal rotation on a fixed axis in a uniform direction, and with simple forces of attraction and repulsion arising thereout, then, from causes arising out of diverse molecular groupings of these atoms and their poles, the nebulous matter in condensing upon a centre might take up three successive states, constituting the normal types of the solid, liquid, and gaseous states of terrestrial bodies,—the solid matter forming a spherical nucleus, everywhere covered with a concentric layer of fluid, and this overlaid with a gaseous envelope; these varying layers of matter being in stable equilibrium at the respective surfaces of contact. The force exerted upon such a sphere by another of like origin would therefore act by and through the intermediation of these three states,—each successive stage of condensation, alike with the residual uncondensed nebulous matter of space, thus forming an essential link in the chain of gravitative action. But so soon as any portions of the solid nucleus emerge above the surface of the fluid covering, into abnormal contact with the gaseous envelope, a differential action of enormous magnitude, centering upon these upheaved land areas, would be at once originated, the first measure of which would be the cosmical value of the latent forces by which the fluid state of matter was constituted an essential intermediate link between the solid and gaseous states. In the view of the Author, terrestrial matter in all its phases is now related to space, and to bodies in space, in a manner analogous to that which might have resulted from such a hypothetical origin. Not only does this matter exist in the three leading states of earth, water, and air, but each of the simpler forms of inorganic matter can, under given conditions, successively assume the solid, liquid, or gaseous state without undergoing any chemical change. This universal threefold relation of terrestrial matter points strongly towards the simple hypothesis, that the causal laws which now regulate these interchanges of state are the reflex of fundamental laws underlying the entire constitution of matter in the solar system.

Passing over the possible relation of the first land-upheaval to the early changes recorded by geology, and to the formation of heterogeneous solids, liquids, and gases, the differential force arising therefrom would be the initiating cause in the formation of the envelope of comminuted water or vapour which now encircles the whole globe. This vapour-ocean constitutes an intermediate state of matter in unstable equilibrium with other states at all surfaces of contact. By interactions arising thereout, the simple static conditions of force existing prior to land-upheaval are now, in the view of the Author, partly replaced by more complex phases of force; and thus light, heat, electricity, and magnetism, which are expressions of these complex phases, have their root in local reactions between unstable states of terrestrial matter at surfaces of abnormal contact when under the tension of cosmical force—just as all these "imponderable elements" are evoked in the voltaic battery by surface reaction of dissimilar solids and liquids in presence of atmospheric tension. In short, the ceaseless molecular changes and local motions of terrestrial matter would, on this hypothesis, be mainly referred to the differential action arising out of land-upheaval.

Recurring to the "wave of high water" which formed the special subject of the Paper, another phase of the present residual of that differential action would give rise to the tidal motions of ocean surfaces, the perturbative action centering on land areas, and attaining a maximum value on the shores of those areas. By discussing the hours of high water at full and change for the principal places of the globe, given in the Admiralty Tide Tables for 1863 (the data being first reduced to Greenwich mean time), the author arrived at the following law of the progression of the wave of high water:—

In all land areas in the northern hemisphere the wave of high water tends to revolve round the coast in the direction of the hands of a watch, and in like areas in the southern hemisphere against the hands of a watch.

Theoretically, this law should hold good in proportion as land areas approximate to the circular form, with wide uninterrupted ocean spaces all round. In a perfectly circular area of this kind, the differential action would have points of maximum and minimum effect on opposite shores at every instant,—these together forming a nodal line, both ends of which would move simultaneously round the coast as the moon passed across the heavens, the wave of high water being everywhere the instantaneous expression of the differential force at its nodal point of maximum action.

By enclosing the continents and land areas which approach nearest to the prescribed conditions within one or more circles intersecting the salient parts of the coast, the Author showed that whenever any systematic progression of the hour of high water could be distinctly traced, that progression is almost invariably in the required direction. Owing, however, to the irregular shape of all existing land areas, to the impossibility of including some of these in a single circle approximating to the coast line, and to the way in which some large areas are massed upon others with little or no intervening ocean spaces, many instances of anomalous results are found; and yet, when rightly considered in relation to disturbing causes, even these tend indirectly to confirm the method of grouping the data of tidal hours in relation to land areas as causal centres.

A Paper was read "On the number of Days on which Rain falls annually in London, from observations made during the 56 years, 1807—1862." By G. V. VERNON, F.R.A.S., M.B.M.S.

This paper has been compiled to meet, to some extent, inquiries which have from time to time been made to me by medical men and others as to the number of days on which rain falls annually at any station. Of course, the remarks only apply to London and its immediate neighbourhood.

Howard's Climate of London, has been used for the years 1807 to 1831, *Philosophical Transactions* for the years 1832 to 1840, and the *Greenwich Observations* for the years 1841 to 1862. During the entire period of 56 years no month occurred in which rain did not fall.

The minimum number of days occurred in 1832, the cholera year, and 1834; the number of days being 86 and and 82 respectively. The maximum number occurred in 1848, the number being 223 days.

The mean monthly values are as follow:—

Month.	Mean number of days on which rain fell.	Month.	Mean number of days on which rain fell.
January	13·4	July	13·0
February	12·4	August	12·9
March........	11·9	September	12·7
April	12·7	October..........	14·4
May	12·7	November........	13·6
June	12·0	December	13·6
		Year........	155·3

Taking the quarterly values, we find that rain falls on the greatest number of days in autumn, and the least in spring.

Taking the means of five yearly periods, there appears to be a kind of periodicity in the number of days on which rain falls, having a maximum in 1813 to 1817, and a minimum in 1843 to 1847.

A Paper was read "On the Rainfall at Oldham during the years 1836-62," by JOHN HEAP, Esq., with Remarks by G. V. VERNON, F.R.A.S., M.B.M.S.

From 1836 to 1852 a twelve-inch circular gauge was used, 24 feet from the ground. From 1853 to 1857 the height was only 11 feet above the ground. In 1861 and 1862 the fall was determined by weight, whereas in previous years a float was used.

The mean fall for the first seventeen years, 24 feet above the ground, was 32·468 inches. The mean fall for 1853-57 was 30·802 inches, 11 feet above the ground. The mean fall for 1858 to 1862 was 38·069 inches. The period 1853 to 1862 combined gives 34·432 inches for the fall 11 feet above the ground. Owing to the elevation of the gauge during the first seventeen years, the rainfall at Oldham appears to have been greatly below that at Manchester, whereas it ought, from the locality, to be more.

From careful comparisons with the rainfall at Manchester I find 39·752 inches as the probable rainfall at Oldham for the twenty-seven years, 11 feet from the ground.

The mean monthly fall for each period was as follows:—

MONTH.	1836—1852.	1853—1857.	1858—1862.	1853—1862.
	Inches.	Inches.	Inches.	Inches.
January	2·195	1·955	2·537	2·246
February	1·825	2·130	2·029	2·079
March	2·090	1·509	3·481	2·495
April	2·147	1·740	2·230	1·985
May	2·328	2·305	2·577	2·441
June........	3·055	2·379	3·898	3·138
July	3·490	3·476	3·567	3·521
August......	3·493	3·358	3·471	3·414
September ..	2·880	1·912	4·622	3·267
October	3·230	4·637	4·020	4·328
November ..	3·399	2·075	2·222	2·148
December ..	2·336	3·326	3·415	3·370
Sums	32·468	30·802	38·069	34·432
Corresponding Periods at Manchester.	36·859	31·371	33·757	32·564

Mr. Joseph Casartelli exhibited a new Registering Barometer. The tube in this instrument is Gay Lussac's, or the syphon form, and is enclosed in a mahogany case, in which is an eight days clock. The arrangement is such that at the completion of every hour the hammer of the clock strikes upon a portion of the apparatus, which impels the index point on to the drum and pierces a hole in the diagram; thus forming an hourly record of the height of the barometer from week to week.

Professor Clifton exhibited an instrument devised by him for observing the phenomena of Conical Refraction, both internal and external.

The following letter was received on May 12th, 1863 : —

HISTORY OF STEAM NAVIGATION.

To the Secretary of the Literary and Philosophical Society of Manchester.

Sir,

(1.) I regret that my having been acquainted with the abstract only of Mr. Dyer's paper, and not with the entire paper, should have led me to suppose erroneously that Mr. Dyer had overlooked the claims of Henry Bell, and also that he had mentioned Symington's steamboat of 1801 more slightly than he really had done.

(2.) With regard to the bad pecuniary result of Bell's undertaking, I think it is to be ascribed to competition rather than to want of encouragement. In little more than a year after the "Comet" began to ply, a rival steamer of greater size and power was started on the Clyde, and was soon followed by others.

(3.) In supporting the opinion of Mr. Woodcroft, that the title of the "first practical steamboat" is due to that vessel in which the double-acting cranked steam engine — in short, Watt's rotative engine — was first applied to drive the propeller, I proceed on the principle, that to constitute a "practical" machine, that machine must be capable, not merely of working well during a series of experiments, but of continuing to work well for years, with ordinary care in its management and repairs.

(4.) Such certainly never was, and never could have been the case, with any steamboat in which the wheels were made to turn by means of chains and rachet-work:—a sort of mechanism which may answer its purpose during an experiment, but which must rapidly wear itself out by shocks and rattling. Such an engine is not a "practical steam engine;" and a vessel driven by it is not a "practical steamboat." Hence the importance which I am disposed to ascribe to the first actual use of a permanently efficient rotative steam engine to drive a vessel.

(5.) It may be true that as an original inventor, Symington ought to be ranked below his predecessors; because his steamboat of 1801 was only a new combination of parts which had previously been invented separately by others:—the paddle-wheel, by some unknown mechanic of remote antiquity; the application of steam to drive vessels, by a series of inventors, comprising Papin, Hulls, D. Bernouilli, Jouffroy, Miller, and Taylor; and the rotative steam engine by Watt: still the merit of having first used a "practical steam engine" to drive a vessel is due to Symington.

(6.) Considering the intimate personal knowledge possessed by Mr. Dyer of the early history of steam navigation, and the eminent services which he long ago did to the community by his efforts in promoting its introduction into Britain, it is very satisfactory to me to find that the main difference between us relates only to the *degree* of importance to be attached to a particular step in the history of that art.

I am, Sir,

Your most obedient Servant,

W. J. MACQUORN RANKINE.

GLASGOW, 11TH MAY, 1863.

A Paper was communicated on July 1st, entitled, "The Complete Theory of Groups, being the Solution of the Mathematical Prize Question of the French Academy for 1860. By the Rev. Thos. P. Kirkman, M.A., F.R.S., and Honorary Member of the Literary and Philosophical Societies of Manchester and Liverpool."

I have the honour to present to this Society a complete solution of the problem proposed early in 1858 by the French Academy, as the subject for their *Grand Prix des Mathématiques*, for June, 1860.

After the first contest of that date, at which something more was accomplished than what is common at a first competition on a question of the same difficulty, by the three candidates, of whom it was my fortune to be one, it was withdrawn from further competition, without any award of the prize, with an unusual suddenness, which created some remark in the world of science. The reason assigned for this abrupt dismissal of a question, on which the *savans* of the imperial Institute had solemnly invited their scientific contemporaries of all nations to undertake a two-years' toil, was this, that while the Academy briefly thanked and commended all the competitors for contributing what was new and really important, on a theory whose complete discussion it had not hoped to obtain, that learned body was unanimously agreed, that its intentions about the unknown subject were not fulfilled. This is placed on record, without the faintest definition of those intentions, or publication of the facts of the contest; the investigators receiving no definite credit, even in brief summary, for their numerous original results. *Vide* the curt report of the Referees, in the copious and weekly *Comptes Rendus de l'Académie*, Mars, 1861.

It is due to the question and to myself to state, that a letter was written by my friend Mr. Ekman, on my part, to the

Academy, on the 25th of April last, offering to them this solution of their prize-question, on condition that they would award to me some kind of *extra* medal, after satisfying themselves that my work is what it pretends to be, the whole of that theory of which, in their proposals of 1858, they desired to obtain only some notable extension.

He received a formal reply, dated May 11, informing him that the matter was referred to the Committee of the competition of 1860: he has since heard nothing more.

The prize-question for 1860 was the following:—

"Quels peuvent être les nombres de valeurs des fonctions bien définies qui contiennent un nombre donné de lettres, et comment peut-on former les fonctions pour lesquelles il existe un nombre donné de valeurs?"

A complete answer to this question, which is more than the Academy required in its proposals in 1858, must supply a clear and direct method of forming every possible transitive r-valued function of n letters not divisible by, nor containing, a symmetric function, with an enumeration of all its equivalent functions, that is, functions of like form and degree, of which no one is a value of another, and also with a demonstration that no other such r-valued functions of n letters are possible. Such a complete answer is given in the Memoir of which this Paper is the abstract.

To expect here definite results in general formulæ for every value of n, is about as reasonable a thing as to ask for a complete exposition of the theory of numbers in general formulæ. I have the honour to present the results demanded, both positive and negative, for values of n below $n=11$, and to shew that all further results can be obtained with certainty by my method up to any value of n. It is a great satisfaction to me to be able to say, that the positive results, *i.e.*, all the groups and functions thus far found, fall under the definite and general though defectively arranged theorems, of the treatise which I sent to Paris, and of the supplement, which have been together printed in the Memoirs of this Society; while there is a considerable portion of the positive, besides nearly all the negative results, *the more difficult to establish*, which are comprehended under no theorems elsewhere to be found, whether definitely expressed, or indefinitely by imperfect description of limiting cases. All that is necessary to be added to that treatise, including the supplement, for the complete mastery of the problem, is briefly given in the following nine articles. The tables which next follow give account of all transitive groups made with fewer than eleven elements.

I. Every transitive group T, made with n elements, is of the order nk, and contains a group K of the order k, made with $n-y-1$ elements, of which the nth, or final, is not one, which may be a transitive or intransitive group K, and which we call the *base* of the group T. Hence the problem is completely solved, if, when any such group K of the order k is given, we can construct every transitive group T made with n elements, which contains K, with an accurate description of T, and with the enumeration of its equivalents, or prove that no such group T exists. We thus establish all the required results, both positive and negative.

II. The group K made with $n-y-1$ elements, is given at first sufficiently by its *title*, and by the number Q_K of its equivalents, without its actual construction.

Definition. The title of any group K exhibits the number of its substitutions of every form, *i.e.*, having any circular factors. *e.g.* Here are the titles of three transitive groups K of the order $k=24$, with the numbers Q of their equivalents:—

$$24 = 1 + 6_4 + 6_{21^2} + 8_{31} + 3_{2^2}, \qquad Q = 1;$$

$$24 = 1 + 8_{62} + 6_{4^2} + 8_{3^21^2} + 1_{2^4}, \qquad Q = 840;$$

$$24 = 1 + 8_{62} + 8_{3^21^2} + 7_{2^4}, \qquad Q = 840.$$

The first is made with four elements, the other two with eight. The term 6_4 shews that there are six substitutions having each a circular factor of the fourth order. The term 6_{21^2} shews six substitutions, having each one circular factor of the second order, and two elements undisturbed; the term $8_{3^2 1^2}$ exhibits eight substitutions, each having two circular factors of the third order, and two elements undisturbed. Exponents in the *signature* (= sub-index) are merely coefficients; $3^2 1^2$ meaning the sum of 3311, and 2^4 meaning that of 2222.

The *clear terms* of a title (clear of units) as 6_4, 8_{6^2}, are those in which no units are read; these have no element undisturbed: the *unclear terms*, as 6_{21^2}, 8_{31}, shew units, that is, undisturbed elements, in the signature.

III. From the title of any group K, transitive or intransitive, made with $n—y—1$ elements, and of the order k, the *unclear* terms of the title of every transitive group T, made with n letters, and of the order nk, which contains K, are all written at once by the general theorem following.

Theorem. The number of substitutions of a given form and of any order p in the group K, is always the term $(a\text{R}_p)_i$ of the title, R_p being the number of integers, unity included, which are less than p and prime to it, and i being the signature, containing 1^m, or shewing m ($\gtreqless 0$) undisturbed letters: the whole unclear term J_j of the title of T is determined by $(a\text{R}_p)_i$, and is always $\left(\text{R}_p\dfrac{an}{m+y+1}\right)_j$ where the signature j differs from i only by having $y+1$ more units, or 1^{m+y+1} for 1^m.

If the number $\dfrac{na}{m+y+1}$ is not an integer thus obtained from every term of the title of K, no transitive group T made with n elements exists containing the base K.

IV. The unclear terms of the title of T being thus all found, the clear terms are next required. First, we know, by the elements of this theory, that no substitution can be in T, unless all its powers are T. The unclear powers of all substitutions in T are already written; hence we know by inspection what clear substitutions having unclear powers are possible. This always reduces the admissible clear signatures to a small number. Let L_1 be the sum of the unclear terms of T. Then $nk—L_1 = L_0$ is the sum of the clear terms, and we have

$$L_0 = A_a + B_b + C_c + \ldots$$

a, b, c . . being the admissible clear signatures, and ABC . . being sought numbers.

V. Def. *A positive substitution or signature has an even number* ($\overline{\gtrless}0$) *of even circular factors.* Thus, in the first of the three above written titles, the signatures of 1, 8_{31}, and $3_2{}^2$ are positive. In the other two titles all are positive. *A signature or substitution not positive, is negative,* as in 6_{21^2}, 6_4.

A *mixed group* has as many positive as negative substitutions, and the former of themselves are always a group. A *positive group* has no negative substitutions.

Hence if L_1 is all positive and greater than $\frac{1}{2}nk$, we know that L_0 has only positive signatures, and all its terms have to be found. If L_1 is mixed, we know that T is mixed, and we have its positive half in our tables of titles. It may be transitive or intransitive, and is easily recognised by comparison of L_1; and we know that the negative half is the positive half multiplied by any negative substitution in L_1; *i.e.* the group T is completely given by construction, although we have not yet its title. We extract from the positive half in our tables the positive clear terms in L_0, and the negative terms of L_0 alone have to be found. In any case we have, ($M\overline{\gtrless}0$),

$$L'_0 = L_0 — M = A_a + B_b + C_c + \ldots$$

where A B C . . . are sought numbers whose sum is known, and $a\ b\ c$ .. are either all positive or all negative clear signatures.

VI. An important theorem is the following :—

Theorem.—If θ_a and ϕ_a be two substitutions of any order p having the same signature a, in any group T, and if θ_a and ϕ_a are not one a power of the other, and have not a common power different from unity, T cannot contain (as A_a) fewer than $(2+R_p)R_p$ substitutions having the signature a, where R_p has the value above given (III).

If θ_a and ϕ_a have a common power different from unity, that is, if they be roots of the same substitution λ, and not one a power of the other, the number A may be less than $(2+R_p)R_p$. If λ is unclear, or clear and positive in a mixed group T, we have before us the number of substitutions in T of the form of λ. The number of rth roots that any substitution λ can have is easily deduced from the theorems of my published Memoir. Thus we have a conditioned limit of A. And, by the inferior groups already registered, which contain more than one rth root of λ, we know in general, by the presence or absence of certain other substitutions, whether our group T can or cannot contain more than one rth root of λ.

VII. The above considerations in most cases enable us to determine the values of ABC . . . . If obscurity remains, we must have recourse to the consideration of the numbers Q_K and Q_T, of the equivalents of K and T. Q_K is always given, and we easily prove that

$$Q_T = hQ_K,$$

and also that

$$hs = \frac{1 \cdot 2 \cdot 3 \ldots . n}{kn \cdot Q_K},$$

i.e., that h is some divisor of a given number. When the group T is mixed, we know the value of h exactly, by virtue of a remark in Art. V. We know also, by the theorems of my printed Memoir, the entire numbers $V_a\ V_b\ V_c\ V_g$ . . . .

of substitutions having any signatures $a\ b\ c\ g$, whether clear or unclear; and if $\rho_a\ \rho_b\ \rho_c\ \rho_g$ . . be the number of times that $V_a\ V_b\ V_c\ V_g$ . . . . are repeated in all the Q_T equivalents of T, we readily prove, as in the 86th article of that Memoir, that

$$Q_T = hQ_K = \frac{V_a\rho_a}{A_a} = \frac{V_b\rho_b}{B_b} = \frac{V_c\rho_c}{C_c} = \frac{V_g\rho_g}{G_g} \ldots;$$

whence we obtain

$$hA_a = F_a\rho_a,\ hB_a = F_b\rho_b,\ hC_c = F_c\rho_c,\ \&c.,$$

$F_aF_bF_c$ being known numbers. Hence

$$hL'_0 = F_a\rho_a + F_b\rho_b + F_c\rho_c + \ldots$$

where either h is known, or the possible factors of h are known. In every case yet considered in which T is impossible, its non-existence, if not exposed before, is proved by a simple absurdity, as that h does and does not contain a certain factor g. If, after clearing the two members of the last equation of common factors, no such absurdity appears, we select values of $\rho_a\rho_b\rho_c$ consistent with the preceding considerations, thus determining admissible systems of values of ABC $\cdots$ to fulfil the condition

$$L'_0 = A_a + B_b + C_c + \ \ldots\ ;$$

and we complete with each system a title of a group T.

VIII. It is sometimes a simpler consideration than (VII.) to take account of the products of the substitutions of the second order in the title. If it be a real one, we can account for every such product $\alpha\beta$. These are either permutables, giving the product $\alpha\beta=\gamma$ of the second order, or else didymous factors of higher substitutions of the title. The number of substitutions permutable with a of the second order, the number of different sets of didymous factors that ϕ of any order can have, the number of substitutions ϕ, ψ, χ, of like or different orders, under which a can be written as a didymous factor—all these are readily deducible from the theorems of my published Memoir. A title is thus often easily shewn to be impossible, by the fact, that no account can be given of the products of its square roots of unity.

IX. The groups T, whose titles are thus consistently described, are now considered possible: their reality is proved, if we can construct them. This construction is *always indicated* by the group K and by the completed title. We have in general to multiply K by a root or roots of one of its lowest substitutions. These roots are readily found by the *tactical method of evolution*, on which I believe that I have had the honour to write all that has been written, and what is equally important, in this theory, with *involution*. Sometimes we have to multiply K by a substitution θ, which has for one didymous factor one of the square roots of unity in K, and for the other a permutable with another in K. This θ is always most easily found by a tactical method. In every case in which a possible group is indicated by a completed title, I have thus far found that the construction follows by the most simple and rapid *tactical methods*. The construction of K is of course tactically known, as is that of every group transitive or intransitive in our register. And we can always give a clear account of the equivalent or non-equivalent groups T, which are built on the same base K.

The determination of the titles of groups T is assisted throughout by the general theorems of my Memoir, on grouped, woven, and woven-grouped groups, of which we know the majority for all values of n, by well-defined constructions, as well as the enumeration of their equivalents.

The greater number of intransitive groups are found to be inadmissible as bases K of transitive groups T, for all values of n, by simple rules deduced from (III). Of those not thus excluded, the bases K, which form part of no T made with n elements, are rapidly disposed of, chiefly by the theorem of (III). And the demonstrations, whether affirmative or negative, are always very briefly drawn from the preceding considerations and from what has before been written.

The most remarkable thing in this method is, *that we need no algebraical substitutions*: we are never conscious of their existence. It turns out, that the ingenious and learned efforts of the French and Italian mathematicians to conquer this theory by algebra, with its formidable army of congruences and imaginaries, have been from the beginning one brilliant error. The problem is tactical, and the solution is tactical. I have from the first suspected this, from the fact, that so very few substitutions have been shewn to be expressible by algebraic formulæ, without a crowd of imaginaries, and that, with these few, operation and computation are possible only at the cost of a most irksome complexity. My tactical methods handle at once the numerals (taken for the elements) which this vast algebra vainly attempts to symbolise, and all equivalent substitutions and groups, whatever the number of elements may be, are alike under control. And, *what is the most important thing*, the groups so constructed are exactly the many-valued functions desired. Having your group on the page, you have, in three more seconds, in its most simple and useful form for comparison or for all computations, one of the explicit functions required. You write a single line of exponents over unity, attributing the same exponent to every element in the same vertical row, and your function is before you on the paper. If you choose your exponents properly, you have the *simplest possible function which has the required number of values.* All this I have already shewn, in the tenth section of that Memoir which was consigned to oblivion at Paris in 1860; and it has been nowhere else shewn.

I believe that no transitive groups exist for values of $n \angle 11$, which are not found in the following tables. If any have escaped me, it is the fault, not of my method, but of my carelessness; and I hope that allowance will be made for the extent and difficulty of the negative which has to be proved. The tactical construction of all these groups is given in the Memoir here presented.

Titles of transitive groups made with fewer than eleven elements, with the numbers of their equivalents. (Vide Art. II.)

$3\cdot1=1+2_{3}$, $Q=1$; $3\cdot2=1+2_{3}+3_{21}$, $Q=1$.

$4\cdot1=1+2_{4}+1_{2^2}$, $Q=3$; $4\cdot2=1+2_{4}+3_{2^2}+2_{21^2}$, $Q=3$;

$4\cdot3=1+8_{31}+3_{2^2}$, $Q=1$; $4\cdot6=1+8_{31}+3_{2^2}+6_{4}+6_{21^2}$, $Q=1$.

$5\cdot1=1+4_{5}$, $Q=6$; $5\cdot2=1+4_{5}+5_{2^21}+10_{41}$, $Q=6$;

$5\cdot4=1+4_{5}+5_{2^21}+10_{41}$, $Q=6$;

$5\cdot4\cdot3=1+24_{5}+20_{31^2}+15_{2^21}$, $Q=1$;

$5\cdot4\cdot3\cdot2\cdot1=1+24_{5}+20_{31^2}+15_{2^21}+30_{41}+10_{21^3}+20_{32}$ $Q=1$.

$6\cdot1=1+2_{3^2}+1_{2^3}+2_{6}$, $Q=60$;

$6\cdot1=1+2_{3^2}+3_{2^3}$, $Q=20$;

$6.2=1+2_{3^2}+3_{2^21^2}+4_{2^3}+2_{6}$, $Q=60$;

$6\cdot3=1+4_{3^2}+4_{31^3}+3_{2^3}+6_{6}$, $Q=20$;

$6\cdot4=1+9_{2^21^2}+6_{42}+8_{3^2}$, $Q=15$;

$6\cdot4=1+8_{3^2}+3_{2^21^2}+8_{6}+3_{21^3}+1_{2^3}$, $Q=15$;

$6\cdot3\cdot2=1+18_{42}+4_{3^2}+4_{31^3}+9_{2^21^2}$, $Q=15$;

$6\cdot3\cdot2=1+4_{3^2}+9_{2^21^2}+4_{31^3}+6_{2^3}+12_{6}$, $Q=10$;

$6\cdot4\cdot2=1+8_{3^2}+6_{42}+9_{2^21^2}+13_{2^3}+3_{21^3}+8_{6}$, $Q=15$;

$6\cdot5\cdot2=1+20_{3^2}+24_{51}+15_{2^21^2}$, $Q=6$;

$6\cdot4\cdot3=1+4_{3^2}+18_{42}+4_{31^3}+9_{2^21^2}+12_{321}+6_{21^4}+12_{6}$
$+6_{2^3}$, $Q=10$;

$6{\cdot}5{\cdot}4=1+20_{3^2}+24_{51}+15_{2^21^2}+30_{41^2}+10_{2^3}+20_{6}$, $Q=6$;

$6{\cdot}5{\cdot}4{\cdot}3=1+144_{51}+45_{2^21^2}+90_{42}+40_{3^2}+40_{31^3}$, $Q=1$;

$6{\cdot}5{\cdot}4{\cdot}3{\cdot}2=1+144_{51}+45_{2^21^2}+90_{42}+40_{3^2}+40_{31^3}+90_{41^2}$
$+120_{321}+15_{2^3}+15_{21^4}+120_{6}$, $Q=1$.

$7{\cdot}1=1+6_{7}$, $Q=120$; $7{\cdot}2=1+6_{7}+7_{2^31}$, $Q=120$;

$7{\cdot}3=1+6_{7}+14_{3^21}$, $Q=120$;

$7{\cdot}6=1+6_{7}+14_{3^21}+7_{2^31}+14_{61}$, $Q=120$;

$7{\cdot}6{\cdot}4=1+48_{7}+56_{3^21}+42_{421}+21_{2^21^3}$, $Q=30$;

$7{\cdot}6{\cdot}5{\cdot}4{\cdot}3=1+720_{7}+504_{51^2}+630_{421}+70_{31^4}+280_{3^21}$
$+105_{2^21^3}+210_{32^2}$. $Q=1$;

$7{\cdot}6{\cdot}5{\cdot}4{\cdot}3{\cdot}2=1+720_{7}+504_{51^2}+630_{421}+70_{31^4}+280_{3^21}$
$+105_{2^21^3}+210_{32^2}+840_{61}+504_{52}+420_{43}+$
$420_{321^2}+105_{2^31}+21_{21^5}+210_{41^3}$, $Q=1$.

$8{\cdot}1=1+1+4_{8}+2_{4^2}+1_{2^4}$, $Q=1260$;

$8{\cdot}1=1+2_{44}+5_{2^4}$, $Q=630$;

$8{\cdot}1=1+6_{4^2}+1_{2^4}$, $Q=210$;

$8{\cdot}1=1+4_{4^2}+3_{2^4}$, $Q=630$;

$8{\cdot}1=1+7_{2^4}$ $Q=30$;

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

$8{\cdot}2=1+4_{4^2}+2_{2^21^4}+1_{2^4}+8_{8}$, $Q=630$;

$8{\cdot}2=1+4_{4^2}+9_{2^4}+2_{2^21^4}$, $Q=315$;

$8{\cdot}2=1+2_{4^2}+5_{2^4}+4_{8}+4_{2^31^2}$, $Q=1260$;

$8{\cdot}2=1+6_{4^2}+1_{2^4}+4_{8}+4_{2^31^2}$, $Q=1260$;

$8{\cdot}3=1+8_{3^21^2}+8_{62}+6_{4^2}+1_{2^4}$, $Q=840$;

$8{\cdot}3=1+8_{62}+8_{3^21^2}+7_{2^4}$, $Q=840$;

$8\cdot4 = 1 + 12_{4^2} + 9_{2^4} + 8_{421^2} + 2_{2^21^4}$, $Q = 315$;

$8\cdot4 = 1 + 8_{4^2} + 5_{2^4} + 2_{2^21^4} + 8_8 + 4_{2^31^2} + 4_{41^4}$ $Q = 630$;

$8\cdot4 = 1 + 20_{4^2} + 6_{2^31^4} + 5_{2^4}$, $Q = 630$;

$8\cdot4 = 1 + 12_{4^2} + 13_{2^4} + 6_{2^21^4}$, $Q = 105$;

$8\cdot6 = 1 + 8_{62} + 6_{4^2} + 8_{3^21^2} + 1_{2^4} + 12_8 + 12_{2^31^2}$, $Q = 840$;

$8\cdot6 = 1 + 8_{62} + 12_{4^2} + 8_{3^21^2} + 13_{2^4} + 6_{2^21^4}$, $Q = 840$;

$8\cdot8 = 1 + 12_{4^2} + 13_{2^4} + 6_{2^21^4} + 24_{42^2} + 4_{2^31^2} + 4_{21^6}$, $Q = 105$;

$8\cdot8 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 12_{42^2} + 8_8 + 8_{2^31^2} + 4_{41^4}$, $Q = 630$;

$8\cdot8 = 1 + 20_{4^2} + 6_{2^21^4} + 5_{2^4} + 16_8 + 8_{42^2} + 4_{2^4} + 4_{21^6}$, $Q = 630$;

$8\cdot8 = 1 + 28_{4^2} + 17_{2^4} + 8_{421} + 10_{2^21^4}$, $Q = 315$;

$8\cdot7 = 1 + 48_{71} + 7_{2^4}$, $Q = 240$;

$8\cdot4\cdot3 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 32_{3^21^2} + 32_{62}$ $Q = 105$;

$8\cdot4\cdot2\cdot2 = 1 + 28_{4^2} + 17_{2^4} + 8_{421^2} + 10_{2^21^4} + 16_8 + 28_{42^2}$
$\quad + 12_{2^31^2} + 4_{21^6} + 4_{41^4}$, $Q = 315$;

$8\cdot7\cdot3 = 1 + 48_{71} + 56_{3^21^2} + 7_{2^4} + 56_{62}$, $Q = 240$;

$8\cdot7\cdot3 = 1 + 48_{71} + 56_{3^21^2} + 42_{4^2} + 21_{2^4}$, $Q = 120$;

$8\cdot6\cdot4 = 1 + 12_{4^2} + 6_{2^21^4} + 13_{2^4} + 32_{3^21^2} + 32_{62} + 4_{21^6} +$
$\quad 4_{2^31^2} + 32_{3^22} + 32_{61^2} + 24_{42^2}$, $Q = 105$;

$8\cdot6\cdot4 = 1 + 60_{4^2} + 32_{62} + 32_{3^21^2} + 24_{421^2} + 25_{2^4} + 18_{2^21^4}$ $Q = 105$;

$8\cdot6\cdot3\cdot2 = 1 + 36_{4^2} + 21_{2^4} + 96_{62} + 64_{3^21^2} + 48_{32^21} + 16_{31^5}$
$\quad + 6_{2^21^4}$, $Q = 35$;

$8\cdot7\cdot6 = 1 + 48_{71} + 56_{3^21^2} + 21_{2^4} + 42_{4^2} + 84_8 + 56_{61^2}$
$\quad + 28_{2^31^2}$, $Q = 120$;

$8\cdot6\cdot4\cdot2 = 1 + 60_{4^2} + 32_{62} + 32_{3^21^2} + 24_{421^2} + 25_{2^4} + 18_{2^21^4}$
$\quad + 4_{21^6} + 32_{61^2} + 52_{2^31^2} + 32_{3^22} + 24_{42^2} + 48_8$, $Q = 105$;

$8\cdot6\cdot4\cdot3 = 1 + 96_{62} + 216_{4^2} + 21_{2^4} + 48_{32^21} + 72_{421^2} +$
$\quad 64_{3^21^2} + 16_{31^5} + 42_{2^21^4}$, $Q = 35$;

$8\cdot6\cdot4\cdot3\cdot2 = 1 + 96_{62} + 216_{4^2} + 21_{2^4} + 48_{32^21} + 72_{421^2} +$
$64_{3^21^2} + 16_{31^5} + 42_{2^21^4} + 144_{8} + 96_{61^2} + 108_{42^2} +$
$72_{431} + 36_{2^31^2} + 96_{321^3} + 12_{41^4} + 12_{21^6}$, $\quad Q=35;$

$8\cdot7\cdot6\cdot4 = 1 + 384_{71} + 224_{62} + 252_{4^2} + 49_{2^4} + 224_{3^21^2} +$
$168_{421^2} + 42_{2^21^4}$, $\quad Q=30;$

$8\cdot7\cdot6\cdot5\cdot4\cdot3 = 1 + 5760_{71} + 3360_{62} + 1260_{4^2} + 105_{2^4} +$
$1120_{3^21^2} + 2520_{421^2} + 210_{2^21^4} + 2688_{53} + 1344_{51^3}$
$+ 112_{31^5} + 1680_{32^21}$, $\quad Q=1;$

$8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2 = 8\cdot7\cdot6\cdot5\cdot4\cdot3 + 3360_{61^2} + 1260_{42^2} + 4032_{521}$
$+ 420_{41^4} + 420_{2^31^2} + 1120_{321^3} + 1120_{3^22} + 5040_{8} +$
$28_{21^6} + 3360_{341}$, $\quad Q=1.$

$9\cdot1 = 1 + 6_{9} + 2_{3^3}$, $\quad Q=6720;$

$9\cdot1 = 1 + 8_{3^3}$, $\quad Q=840;$

$9\cdot2 = 1 + 8_{3^3} + 9_{2^41}$, $\quad Q=840;$

$9\cdot2 = 1 + 8_{3^3} + 6_{63} + 3_{2^31^3}$, $\quad Q=2520;$

$9\cdot2 = 1 + 6_{9} + 2_{3^3} + 9_{2^41}$, $\quad Q=6720;$

$9\cdot3 = 1 + 18_{9} + 2_{3^3} + 6_{3^21^3}$, $\quad Q=2240;$

$9\cdot3 = 1 + 20_{3^3} + 6_{3^21^3}$, $\quad Q=840;$

$9\cdot4 = 1 + 8_{3^3} + 9_{2^41} + 12_{63} + 6_{2^31^3}$, $\quad Q=10080;$

$9\cdot4 = 1 + 8_{3^3} + 18_{4^21} + 9_{2^41}$, $\quad Q=2520;$

$9\cdot6 = 1 + 18_{9} + 2_{3^3} + 6_{3^21^3} + 9_{2^41} + 18_{621}$, $\quad Q=2240;$

$9\cdot6 = 1 + 18_{63} + 9_{2^31^3} + 6_{3^21^3} + 20_{3^3},$ Q=1680;

$9\cdot8 = 1 + 8_{3^3} + 18_{4^21} + 9_{2^41} + 36_{81}.$ Q=2520;

$9\cdot8 = 1 + 54_{4^21} + 9_{2^41} + 8_{3^3},$ Q=2520;

$9\cdot9 = 1 + 36_9 + 26_{3^3} + 12_{3^21^3} + 6_{31^5},$ Q=1120;

$9\cdot8\cdot2 = 1 + 36_{81} + 24_{63} + 54_{4^21} + 8_{3^3} + 9_{2^41} + 12_{2^31^3},$ Q=2520;

$9\cdot6\cdot3 = 1 + 36_9 + 26_{3^3} + 12_{3^21^3} + 6_{31^6} + 27_{2^41} + 54_{621}$ Q=1120;

$9\cdot6\cdot3 = 1 + 36_9 + 26_{3^3} + 12_{3^21^3} + 6_{31^6} + 18_{61^3} + 9_{2^31^3} +$

$54_{63}.$ Q=1120;

$9\cdot6\cdot3\cdot2 = 1 + 36_9 + 26_{3^3} + 12_{3^21^3} + 6_{31^6} + 27_{2^41} + 54_{621}$

$+ 108_{63} + 36_{2^31^3} + 18_{61^3},$ Q=1120;

$9\cdot6\cdot3\cdot2 = 1 + 144_9 + 26_{3^3} + 54_{621} + 27_{2^41} + 12_{3^21^3} + 6_{31^6}$

$+ 54_{32^21^2},$ Q=280;

$9\cdot6.4 = 1 + 56_{3^3} + 72_{621} + 54_{4^21} + 9_{2^41} + 24_{3^21^3},$ Q=840;

$9\cdot8\cdot6 = 1 + 56_{3^3} + 72_{621} + 54_{4^21} + 9_{2^41} + 24_{3^21^3} + 108_{81}$

$+ 36_{2^31^3} 72_{63},$ Q=840;

$9\cdot8\cdot7 = 1 + 168_9 + 216_{71^2} + 56_{3^3} + 63_{2^41},$ Q=240;

$9\cdot6\cdot4\cdot3 = 1 + 144_9 + 26_{3^3} + 54_{621} + 27_{2^41} + 12_{3^21^3} + 6_{31^6}$

$+ 54_{32^21^2} + 216_{63} + 36_{3^221} + 27_{2^31^3} + 36_{321^4} +$

$9_{21^7},$ Q=280;

$9\cdot6\cdot4\cdot3 = 1 + 144_9 + 80_{3^3} + 108_{432} + 108_{621} + 54_{2^41} +$

$54_{32^21^2} + 12_{3^21^3} + 54_{421^3} + 27_{2^41} + 6_{31^6}$ Q=280;

$9\cdot6\cdot4\cdot3\cdot2 = 1 + 144_9 + 80_{3^3} + 108_{432} + 54_{2^41} + 54_{32^21^2} +$

$108_{621} + 12_{3^21^3} + 54_{421^3} + 27_{2^41} + 6_{31^6} + 288_{63} +$

$36_{32^3}+162_{42^21}+36_{3^221}+36_{61^3}+45_{2^31^3}+36_{321^4}$

$+9_{21^7}$, Q=280;

$9\cdot8\cdot7\cdot3=1+504_9+56_{3^3}+216_{71^2}+504_{621}+63_{2^41}+$

$168_{3^21^3}$, Q=240;

$9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3=1+40320_9+25920_{71^2}+30240_{621}+2240_{3^3}$

$+7560_{421^3}+3360_{3^21^3}+168_{31^6}+945_{2^41}+15120_{432}$

$+3024_{51^4}+9072_{52^2}+7560_{32^21^2}+24192_{531}+$

$11340_{4^21}+378_{2^21^5}$, Q=1;

$9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2=9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3+10080_{61^3}+11340_{42^21}+$

$18144_{521^2}+756_{41^5}+1260_{2^31^3}+2520_{321^4}+10080_{3^221}$

$+45360_{81}+36_{21^7}+15120_{431^2}+20160_{63}+25920_{72}$

$+18144_{54}+2520_{2^33}$, Q = 1.

$10\cdot1=1+4_{10}+4_{5^2}+1_{2^5}$, Q=90720;

$10\cdot1=1+4_{5^2}+5_{2^5}$, Q=18144;

$10\cdot2=1+4_{10}+4_{5^2}+6_{2^5}+5_{2^41^2}$, Q=90720;

$10\cdot4=1+4_{5^2}+10_{4^22}+10_{4^21^2}+5_{2^41^2}+6_{2^5}+4_{10}$, Q=90720;

$10\cdot5=1+16_{5^2}+20_{10}+5_{2^5}+8_{51^5}$, Q=18144;

$10\cdot6=1+24_{5^2}+20_{3^31}+15_{2^41^2}$, Q=30240;

$10\cdot8=1+64_{5^2}+10_{2^21^6}+5_{2^41^2}$, Q=5670;

$10\cdot6\cdot2=1+24_{5^2}+15_{2^41^2}+20_{3^31}+24_{10}+20_{631}+10_{2^31^4}$

$+6_{2^5}$, Q=30240;

$10 \cdot 6 \cdot 2 = 1 + 24_{55} + 20_{3^2 1^4} + 15_{2^4 1^2} + 20_{62^2} + 24_{10} +$
$16_{2^5},$ $\qquad Q=15120;$

$10 \cdot 8 \cdot 2 = 1 + 64_{5^2} + 5_{2^4 1^2} + 10_{2^2 1^6} + 64_{10} + 5_{21^8} + 10_{2^3 1^4}$
$+ 1_{2^5},$ $\qquad Q=5670;$

$10 \cdot 8 \cdot 2 = 1 + 64_{55} + 40_{42^3} + 10_{2^2 1^6} + 25_{2^4 1^2} + 20_{4^2 1^2},$ $\qquad Q=5670;$

$10 \cdot 5 \cdot 4 = 1 + 16_{5^2} + 40_{52^2 1} + 25_{2^4 1^2} + 8_{51^5} + 10_{2^2 1^6} + 50_{4^2 2}$
$+ 10_{2^5} + 40_{10},$ $\qquad Q=18144;$

$10 \cdot 8 \cdot 3 = 1 + 24_{5^2} + 20_{3^2 2^2} + 20_{3^2 1^4} + 15_{2^4 1^2} + 10_{2^2 1^6} + 30_{4^2 1^2}$
$+ 24_{10} + 40_{62^2} + 26_{2^5} + 30_{4^2 2},$ $\qquad Q=15120;$

$10 \cdot 8 \cdot 4 = 1 + 20_{4^2 1^2} + 25_{2^4 1^2} + 10_{2^2 1^6} + 64_{5^2} + 40_{42^3} + 40_{42^2 1^2}$
$+ 10_{2^3 1^4} + 5_{21^8} + 64_{10} + 20_{4^2 2} + 21_{2^5},$ $\qquad Q=5670;$

$10 \cdot 8 \cdot 4 = 1 + 64_{5^2} + 80_{82} + 40_{42^3} + 100_{4^2 1^2} + 25_{2^4 1^2} + 10_{2^2 1^6},$
$Q=5670;$

$10 \cdot 8 \cdot 5 = 1 + 16_{5^2} + 100_{4^2 1^2} + 25_{2^4 1^2} + 8_{51^5} + 10_{2^2 1^6} + 40_{52^2 1}$
$+ 100_{4^2 2} + 80_{10} + 20_{2^5},$ $\qquad Q=4536;$

$10 \cdot 8 \cdot 5 = 1 + 16_{5^2} + 200_{82} + 40_{52^2 1} + 10_{2^2 1^6} + 100_{4^2 1^2} + 8_{51^5}$
$+ 25_{2^4 1^2},$ $\qquad Q=4536;$

$10 \cdot 8 \cdot 4 \cdot 2 = 1 + 80_{82} + 64_{5^2} + 40_{42^3} + 100_{4^2 1^2} + 25_{2^4 1^2} + 10_{2^2 1^6}$
$+ 21_{2^5} + 80_{81^2} + 64_{10} + 40_{42^2 1^2} + 100_{4^2 2} + 10_{2^3 1^4} + 5_{21^8}$
$Q=5670;$

$10 \cdot 9 \cdot 8 = 1 + 180_{82} + 144_{5^2} + 270_{4^2 1^2} + 45_{2^4 1^2} + 80_{3^3 1},$ $\qquad Q=2520;$

$10 \cdot 8 \cdot 5 \cdot 2 = 1 + 16_{5^2} + 200_{82} + 40_{52^2 1} + 10_{2^2 1^6} + 100_{4^2 1^2} + 8_{51^5}$
$+ 25_{2^4 1^2} + 80_{10} + 20_{2^5} + 20_{41^6} + 100_{42^2 1^2} + 80_{541} +$
$100_{4^2 2},$ $\qquad Q=4536;$

$10\cdot8\cdot6\cdot2 = 1 + 384_{5^2} + 80_{3^22^2} + 120_{42^3} + 80_{3^21^4} + 10_{2^21^6} +$

$64_{4^21^2} + 65_{2^41^2} + 160_{621^2}, \qquad Q=945;$

$10\cdot9\cdot8\cdot2 = 1 + 180_{82} + 144_{5^2} + 270_{4^21^2} + 45_{2^41^2} + 80_{3^31} +$

$30_{2^31^3} + 180_{81^2} + 144_{10} + 90_{4^22} + 240_{631} + 36_{2^5}, \qquad Q=2520;$

$10\cdot8\cdot6\cdot4 = 1 + 240_{82} + 320_{64} + 384_{5^2} + 80_{3^22^2} + 140_{42^3} +$

$300_{4^21^2} + 160_{621^2} + 80_{3^21^4} + 60_{421^4} + 125_{2^41^2} +$

$30_{2^21^6}, \qquad Q=945;$

$10\cdot8\cdot6\cdot4 = 1 + 384_{5^2} + 120_{42^3} + 80_{3^22^2} + 60_{4^21^2} + 65_{2^41^2} +$

$160_{621^2} + 80_{3^21^4} + 10_{2^21^6} + 384_{10} + 60_{34^22} + 61_{2^5}$

$+ 160_{3^221^2} + 120_{42^21^2} + 10_{2^31^4} + 160_{61^4} + 5_{21^8}, \qquad Q=945;$

$10\cdot8\cdot6\cdot4\cdot2 = 1 + 240_{82} + 320_{64} + 384_{5^2} + 80_{3^22^2} + 140_{42^3}$

$+ 300_{4^21^2} + 160_{621^2} + 30_{2^21^6} + 80_{3^21^4} + 60_{421^4} +$

$125_{2^41^2} + 384_{10} + 320_{3^24} + 240_{4^22} + 160_{62^2} +$

$81_{2^5} + 160_{3^221^2} + 120_{42^21^2} + 240_{81^2} + 80_{61^4} +$

$130_{2^31^4} + 5_{21^8}, \qquad Q=945;$

$10\cdot8\cdot6\cdot5\cdot3 = 1 + 576_{5^2} + 720_{52^21} + 960_{531^2} + 225_{2^41^2} +$

$600_{32^21^3} + 400_{3^21^4} + 30_{2^21^6} + 48_{51^5} + 40_{31^7} + 2880_{10}$

$+ 600_{62^2} + 120_{2^5}, \qquad Q=126;$

$10\cdot8\cdot6\cdot5\cdot3\cdot2 = 1 + 3600_{82} + 2400_{64} + 1200_{42^3} + 400_{3^22^2}$

$+ 576_{5^2} + 720_{52^21} + 1200_{4321} + 960_{531^2} +$

$1000_{32^21^3} + 225_{2^41^2} + 900_{4^21^2} + 400_{3^21^4} + 600_{421^4}$

$+ 48_{51^5} + 130_{2^21^6} + 40_{31^7}, \qquad Q=126;$

$$10\cdot8\cdot6\cdot5\cdot3\cdot2=1+576_{5^2}+400_{3^22^2}+720_{52^21}+1200_{4321}$$

$$+960_{531^2}+400_{3^21^4}+1000_{32^21^3}+225_{2^41^2}+$$

$$900_{4^21^2}+600_{421^4}+48_{51^5}+130_{2^21^6}+40_{31^7}+$$

$$2880_{10}+2700_{4^22}+960_{532}+600_{62^2}+60_{2^5}, \qquad Q=126;$$

$$10\cdot8\cdot6\cdot5\cdot4\cdot3=1+3600_{82}+2400_{64}+1200_{42^3}+400_{3^22^2}$$

$$+576_{5^2}+720_{52^21}+1200_{4321}+960_{531^2}+$$

$$1000_{32^21^3}+225_{2^41^2}+900_{4^21^2}+400_{3^21^4}+600_{421^4}$$

$$48_{51^5}+130_{2^21^6}+40_{31^7}+60_{41^6}+20_{21^8}+$$

$$1440_{541}+480_{521^3}+1200_{341^3}+900_{42^21^2}+$$

$$800_{3^221^2}+600_{32^31}+300_{2^31^4}+3600_{4^22}+$$

$$2880_{10}+960_{532}+600_{62^2}+120_{2^5}+440_{321^5} \qquad Q=126;$$

$$10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3=1+226800_{82}+151200_{64}+72576_{5^2}+$$

$$18900_{42^3}+25200_{3^22^2}+172800_{73}+403200_{91}+$$

$$22400_{3^31}+151200_{4321}+90720_{52^21}+86400_{71^3}$$

$$+151200_{621^2}+4725_{2^41^2}+120960_{531^2}+56700_{4^21^2}$$

$$+25200_{32^21^3}+18900_{421^4}+240_{31^7}+8400_{3^21^4}$$

$$+6048_{51^5}+630_{2^21^6}, \qquad Q=1;$$

$$10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3\cdot2=10\cdot9\cdot8\cdot7\cdot6\cdot5\cdot4\cdot3+25200_{61^4}+56700_{42^21^2}.$$

$$+60480_{521^3}+1260_{41^6}+3150_{2^31^4}+5040_{321^5}+$$

$$50400_{3^221^2}+226800_{81^2}+45_{21^8}+50400_{341^3}+$$

$$201600_{631}+259200_{721}+181440_{541}+25200_{2^331}$$

$$+362880_{10}+75600_{62^2}+120960_{532}+56700_{4^22}$$

$$+50400_{43^2}+945_{2^5}, \qquad Q=1.$$

This theory of many-valued functions, or what is exactly the same thing, this theory of groups, is, after all, though perplexing enough at first sight, merely a trifle compared with the theory of the polyedra, the subject of competition for the grand prize of the Academy for the years 1861, 1863, 1864, &c. This was also proposed early in 1858 thus: *Perfectionner en quelque point important la théorie géométrique des polyèdres.* The null results of the competitions of 1861 and 1863 may be read in the *Comptes Rendus.* Although I have had the honour to present to the Royal Society a complete discussion of this vast theory (which was first written in French, and intended for the contest of 1861), and which has already so far appeared in their Proceedings and in the Philosophical Transactions as to secure my humble claims, it is a fortunate thing for the competitors for the medal of 1861, that the most difficult part of the investigation has yet to be brought to light. As it is not a subject that many persons are anxious to read, I hope that the Royal Society will be in no hurry to anticipate the efforts of our French friends, and thus to spoil the interest of their contests.

The remarks of the distinguished Rapporteur (*Comptes Rendus*, Mars, 1861) on the competition of 1860, brief as they are, are to me the most tempting subject in the world for criticism, chiefly in their reference to the efforts of my two learned competitors. But, even if this were the place for such criticism, my sincere respect for the illustrious Academy, in whose name and authority those remarks are permitted to appear, would command me to forbear. Some will of course suspect that I have said too much already: many know full well how much I have refrained from saying. One security I have; that no man, who can form the dimmest conception of what I have had the honour to do, will cry out ὄμφακές εἰσι. The mere proposal of these two prize-questions has been to me a piece of rare good fortune. And I am more than content with the undivided glory, whatever that may be, of having thoroughly discussed, by methods which owe in one case little and in the other nothing to previous writers, the whole of those two extensive theories, on each of which the Academy, in its proposals of 1858, desired to obtain partial information.

Croft Rectory, near Warrington,
July 1, 1863.

P.S.—It is necessary to add, in reference to Mr. Ekman's letter named at page 133, that while these pages were passing through the press, that gentleman has been courteously informed of the decision of the Committee (*vide* p. 134), to the effect that, as the competition on the prize question of 1860 has been closed, the Academy cannot award any medal for any further discussion of this subject.

Ordinary Meeting, October 6th, 1863.

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

Mr. Bartholomew Stretton was elected an Ordinary Member of the Society.

R. B. Clifton, M.A., F.R.A.S., Professor of Natural Philosophy, Owens College, was elected a member of the Council.

Mr. Platt exhibited some fine specimens of rock salt, from a boring at Middlesborough. The following are records of the results of this and two other borings, executed by Messrs. Mather and Platt.

Middlesborough bore hole, executed for Messrs. Bolckaw and Vaughan; commenced January 4th, 1862.

	Feet.	In.
Well, 180 feet deep, consisting of made ground, river mud, sand, red marl, red sandstone, with occasional nodules of gypsum, to a depth of....	127	0
Fibrous gypsum	6	0
Brown shale	1	0
Red sandstone	12	0
Blue post stone............	3	0
Red sandstone	31	0
Total of well	180	0
After commencing to bore they went through a pretty homogeneous bed of red sandstone, occasionally varied with white or yellow veins and thin beds of red marl, to a depth of	588	4
Red marl	19	0
Red sandstone, with occasional beds of red marl ...	234	8
Red sandstone, with nodules of gypsum	53	0
Fine red sandstone and marl	10	0
Ditto ditto	97	0

	Feet.	In.
Light brown gypsum	5	8
White ditto mixed with red sandstone and limestone	6	10
Blue rock, blue clay, red sandstone, and white stone	11	6
Rock salt, with occasional small portions of rock like limestone	106	4
Total depth	1312	4

Bore hole at Vale Dye Works, Bradford, in the lower part of the Yorkshire Coal Field, made for Messrs. Oates, Ingham, and Sons; commenced July 28th, 1862.

Well, 55 feet deep.

	Feet.	In.
Soil	5	0
Clay	2	6
Gravel	12	6
Gravel and clay	3	6
River bed	11	6
Black shale	20	0
Bottom of sinking	55	0

Bore hole.

	Feet.	In.
Black shale	21	0
Hard rag stone	3	0
Stone shale	12	0
Close shale	13	0
Dark shale	27	0
Coal	0	3
Soft white stone	8	0
Hard grey stone	22	0
Hard white do.	5	0
Coal band	0	3
Hard white stone	8	0
Hard dark rag	24	0
Fine grain slaty rock....................	15	0
Coal	1	8
Fire clay	5	0

	Feet.	In.
Dark shale	32	0
Blue shale with fossil shells	11	0
Shale	3	0
Coal band	0	8
Shale	3	0
Dark rag	26	0
Dark shale	44	2
Millstone grit	8	0
Total	348	0

The third bore hole was at Wirrall, near Birkenhead, commenced March 8th, 1861. The well in which this bore hole was made is 23 feet deep, a portion being in the red stone (Trias). From the bottom of the well the boring was:—

	Feet.	In.
Red sandstone	32	0
Red and white sandstone	14	0
Close grained red sandstone with quartz pebbles	3	6
Red and white sandstone	10	6
White rock with bands of brown hard substance like iron cement	14	0
Red sandstone	51	0
Red sandstone, very fine	73	0
Red sandstone, finer still	8	0
Red and white sandstone, coarser and softer ...	41	0
Coarse soft red sandstone	2	0
Red and white marly clay...	0	6
Soft red sandstone	11	6
Red sandstone	13	0
Ditto	90	0
Coarse soft red sandstone	0	4
Total	364	4

Dr. Clay presented a fine specimen of granular magnetic oxide of iron, from the district "Taranaki," in New Zealand, where it exists in large quantities, lying in seams of considerable thickness near the surface, and exposed in the bluffs lining the water courses. It is so pure that out of 1,000 grains, five or six grains only were left unattracted by the magnet, and these fine silica. Its appearance at first sight is similar to the finest Dartford gunpowder. It has been used limitedly in this country by M. Bessemer, in his steel process, and others.

The Rev. W. N. MOLESWORTH drew attention to the earthquake which occurred that morning.

Mr. DYER, also, and other members, gave an account of the phenomenon as experienced by themselves.

Mr. R. D. DARBISHIRE, referring to observations made last spring, mentioned the remarkable multiplicity of coins current in Gibraltar, where silver money of many countries appears to circulate freely at ascertained values, and copper money in still greater varieties, to be used indiscriminately, subject only to a rude valuation according to size and weight.

In remarkable contrast with the British system, under which nearly the whole copper coinage has been recalled and replaced within the last five years, and a gold or silver coin fifty years old would scarcely pass, Mr. Darbishire noticed the fact that the current money of Spain, as observed in several of the cities of Andalusia, Granada, and Valencia at this day, includes gold from the middle and silver from the earliest years of the eighteenth century, and copper from (at least) the year 1601.

Referring to the copper currency, he mentioned the introduction, about 1848, of new pieces to represent decimal parts of the Real, and the ineffectual character of the measure owing to the small quantity of these new coins put into circulation. Practically they are suppressed amidst copper money of two centuries and a half.

As illustrating other peculiarities of the copper currency, Mr. Darbishire stated that he had collected in small change from petty tradesmen, peasants, and market people in Seville and Granada, many rude pieces of money, apparently of provincial mints or the earliest dates of (if not anterior to) the monarchy. In Malaga, especially, he had taken not only the like old Spanish money, but a number of coins of the Arabs in Spain (including silver, tin, and copper pieces), and also a large number of still more ancient types,—Roman and ante-Roman. These coins, no doubt, had from time to time been found in the fields or ruins, and thus again put into circulation. Being mostly small, he had taken them as ochavos (half-farthings). He had brought home above sixty selected coins, of dates from Julius Cæsar to Honorius, several of which were of peculiar rarity and interest, and ten autonomous coins of even greater antiquity. The latter were the subject of a Report:

"On ten ancient Spanish coins taken in change at Malaga," by John Harland, Esq., F.S.A.

I. *a*. Two coins of *Malaca* (Malaga) of the same type, from different dies.

Obv. Head with a square topped cap to the right: behind forceps, and in the left margin portions of Phœnician characters M.L.C.A.

Rev. No legend; a star of eight points within a garland of laurel.

I. *b*. A coin referred with doubt to the same mint.

Obv. A bearded head to the right, with conical cap; behind forceps.

Rev. No legend; a large sun with eleven long rays and a human face.

II. One coin of *Obulco* (Porcuna), apparently of the local mint, in alliance with Edeta and Sætabis.

Obv. Large female head to the right. "Obulco."

Rev. Legend in two lines "L. Aimil. M. Juni. Aid." Above a plough, below an ear of corn.

III. Two coins of *Acinipo* (Ronda la vieja).

Obv. A bunch of grapes, between two stars.

Rev. "Acinipo," between two ears of corn. One coin has only one star and the legend "Acinio."

IV. One coin of *Carteia* (Tartessus).

Obv. Very large head to the right with fillet and a well marked profile outline.

Rev. "C. Vib. Aid (Cart . . . ?)" and a Dolphin.

V. One Coin of *Castulo* (Cazlona) probably minted before the Roman conquest.

Obv. Laureated or filleted head to the right, with upraised hand in front of the mouth.

Rev. Bonneted Sphinx with head thrown back and a spine on the crown, walking to the right with one foreleg raised high.

VI. Two small coins much corroded.

Professor Christie explained the occurrence of coins so ancient, by the absence in Spain of the large class of small collectors, which in England and countries usually frequented by English has put a premium on such specimens, and thus causes their rapid absorption when a find takes place. He had not noticed a similar occurrence in the North of Spain.

Mr. Hurst mentioned other particulars as to the variegated coinage in use at Gibraltar, and the almost total removal from the Spanish currency of the old silver dollar with twisted pillars, which he explained in connection with the remarkable demand for that coin by the Chinese, who pay for it a premium of 25 to 40 per cent on its money value in Europe.

Mr. Darbishire exhibited a tablet of modern standard Spanish coins (a decimal series), and of the present money of Morocco; the latter singularly rude, and much inferior to that of the Spanish Arabs.

Since the meeting the following letter, by Professor W. Thomson, LL.D., F.R.S., Honorary Member of the Society, has been communicated by Professor TAIT: —

Kilmichael Brodick,
Isle of Arran, Oct. 10, 1863.

My Dear Tait,

Yesterday evening, when engaged in measuring the electrostatic capacities of some specimens of insulated wire designed for submarine telegraph cables, I had occasion frequently to discharge, through a galvanometer coil, a condenser consisting of two parallel plates of metal, separated by a space of air about ·007 inch across, and charged to a difference of potentials equal to that of about 800 Daniell's elements. I remarked at an instant of discharge a sharp sound, with a very slight prolonged resonance, which seemed to come from the interior of the case containing the condenser, and which struck me as resembling a sound I had repeatedly heard before when the condenser had been overcharged and a spark passed across its air-space. But I ascertained that this sound was distinctly audible when there was no spark within the condenser, and the whole discharge took place fairly through the 2,000 yards of fine wire, constituting the galvanometer coil. I arranged the circuit so that the place where the contact was made to produce the discharge was so far from my ear that the initiating spark was inaudible; but still I heard distinctly the same sound as before from within the condenser.

Using instead of the galvanometer coil either a short wire or my own body (as in taking a shock from a Leyden phial), I still heard the sound within the condenser. The shock was imperceptible except by a very faint prick on the finger in the place of the spark, and (the direct sound of the spark being barely if at all sensible) there was still a very audible sound, always of the same character, within the condenser, which I heard at the same instant as I felt the spark on my finger. Mr. Macfarlane could hear it distinctly standing at a distance of several yards. We watched for light within the condenser, but could see none. I have since ascertained that suddenly charging the condenser out of one of the specimens of cable charged for the purpose produces the same sound within the condenser; also that it is produced by suddenly reversing the charge of the condenser.

Thus it is distinctly proved that a plate of air emits a sound on being suddenly subjected to electric force, or on experiencing a sudden change of electric force through it. This seems a most natural result when viewed in connection with the new theory put forward by Faraday in his series regarding the part played by air or other dielectric in manifestations of electric force. It also tends to confirm the hypothesis I suggested to account for the remarkable observation made regarding lightning, when you told me of it about a year ago, and other similar observations which I believe have been reported, proving a sound to be heard at the instant of a flash of lightning in localities at considerable distances from any part of the line of discharge, and which by some have been supposed to demonstrate an error in the common theory of sound. I may add that Mr. Macfarlane tells me he believes he has heard, at the instant of a flash of lightning, a sound as of a heavy body striking the earth, and imagined at first that something close to him had been struck, but heard the ordinary thunder at a sensible time later.

Yours truly,

WILLIAM THOMSON.

Ordinary Meeting, October 20th, 1863.

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

A Paper by the Rev. T. P. Kirkman, M.A., F.R.S., "On the Complete Theory of Groups," was read.

The following communication was also made by Mr. Kirkman:—

I observe that I have not accurately expressed my meaning at page 138, art. VI., of the Proceedings of this Session

The theorem, which can hardly be new, is correctly enunciated thus:—

Theorem. If θ_a and ϕ_a be two substitutions of any order p, and of the signature a, in any group T, and if

$$H = 1 + \theta_a + \theta_a^2 + \ldots$$

contains no group of powers, h, permutable with

$$F = 1 + \phi_a + \phi_a^2 + \ldots$$

F being different from H, T cannot contain, as A_a, fewer than $R_p\ (2+R_p)$ substitutions having the signature a; R_p being the number of integers, unity included, which are less than p and prime to it.

The proof is simple. Let $\theta^b\theta$ . . be the R_p principal substitutions of H. Then the $2+R_p$ groups

$$F,\ H,\ \theta^b F\theta^{-b},\ \theta^c F\theta^{-c}\ \ldots$$

are all different, unless either

$$F = \theta^e\ F\theta^{-e},$$

or

$$\theta^e\ F\theta^{-e} = \theta^f\ F\theta^{-f},$$

θ^e and θ^f being principal.

The first gives

$$\theta^{e}\mathrm{F} = \mathrm{F}\theta^{e},$$

whence

$$\theta^{re}\mathrm{F} = \mathrm{F}\theta^{re},$$

whatever be r, *i.e.* $\mathrm{HF}=\mathrm{FH}$.

The second gives

$$\theta^{e-f}\mathrm{F} = \mathrm{F}\theta^{e-f},$$

whence, if h be the group of powers of θ^{e-f}, $h\mathrm{F}=\mathrm{F}h$.

When a group T contains more than R_p and fewer than $\mathrm{R}_p\,(2+\mathrm{R}_p)$ substitutions having the signature a, we have an easy tactical method of constructing T upon H or upon F.

The correction above made does not in the least degree affect either my method or my results, in the solution of the Prize-question of the Academy.

In the list of titles at page 142 of the same abstract there is an accidental omission of the following, which is before me in my first manuscript —

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

A Paper was read by the President, entitled "Further Observations on the Carboniferous Permian and Triassic Strata of Cumberland and Dumfries," by E. W. BINNEY, F.R.S., F.G.S.

When in 1848 the red sandstones of the neighbourhood of Dumfries first came under the Author's observation, in company with his friend Professor Harkness, doubts came into his mind as to the propriety of their being classed with the trias, their characters and organic remains clearly indicating more of a permian age.* Accordingly in his first paper published on this subject in the Society's Memoirs† in 1855, allusion was made to these beds, and they were classed as

* In the *Quarterly Journal of the Geological Society* for 1851, p. 162, Sir R. I. Murchison doubted the sandstone of Dumfries being of triassic age, and preferred to class it with the permian.

† On the Permian Beds of the North-West of England, vol. xii., p. 209, of the Society's Memoirs.

permian after tracking the permian beds of Lancashire through the north-western counties of York, Westmoreland, and Cumberland. His attention was chiefly directed to the red marls, magnesian limestones, conglomerate, and soft red sandstone strata, those being the common Lancashire types; and where the red sandstones of the neighbourhood of Carlisle and St. Bees were incidentally mentioned, they were treated as upper new red sandstone or trias, as Professor Sedgwick has described them in his valuable memoir. But in his second communication,* published in 1857, where the Howrigg, Shawk, and Westward sections are described, he came to the conclusion that "the brick red sandstones of those places, with their underlying red clays, as well as the breccia at Shawk, I have little doubt will be proved to be permian. It is true that no fossil organic remains have yet been found in them, with the exception of the track alluded to in this paper; but if mineralogical characters and geological superposition are to be taken as evidence of their age, they are as good permian beds as those of West House, Kirby Stephen, and Brough, in England, and Dumfries and other places in the south-west of Scotland, with the latter of which they are most probably connected."

In a paper published by Professor Harkness in 1862,† that geologist adopts in substance this view, and agrees with the Author's opinion of the Howrigg, Shawk, and Westward red clays and sandstones being of permian age, and describes a very beautiful section at Hilton, in Westmoreland, which strongly confirms it. Of course it was not intended to question the triassic age of the soft red sandstones of Dalston and Holmhead, near Carlisle, which are covered by waterstones, red marls and lias, as stated in the Author's paper on the latter deposit.‡

* Additional Observations on the Permian Beds of the North-West of England, vol. xiv., p. 101, of the Society's Memoirs.

† *Quarterly Journal of the Geological Society* for August, 1862, p. 205.

‡ *Ibid* for May, 1859, p. 549.

The Shawk sandstones are well seen at Westward Chapel near Wigton, West Newton, near Aspatria, near Allonby, and to the north of Maryport, and after the Maryport, Workington, and Whitehaven coalfield is passed, they appear again to the south on the coast, in the magnificent promontory of St. Bees Head, and continue southward certainly to Netherton, Seascales, Gosforth, and Drigg Cross, and probably, as Professor Sedgwick suggests, into Furness.

On the north of the Solway the permian strata on the opposite side of the Vale of Eden are well exposed near Riddings Junction, on the Waverley line of railway, about Carwinlay, Moat, and Canobie, and the range of the Moat sandstone (the same age as that of Shawk) by Glenzier Quarry, Cove, near Kirkpatrick Fleming, above Annan, on to Dumfries, is well marked.

In addition to a description of several permian sections at Penton, Riddings, Carwinlay Burn, Barrow Mouth, and Ben How, two sections were given, which showed the occurrence of the upper coal measures, similar to those described by the author some years since in the valley of the Ayr, near Catrine, and thus rendering it extremely probable that such coal measures extend under the valleys of the Eden and the Esk, their southern outcrop being exposed in the Raw Beck, south of Dalston, and their northern outcrop at Canobie. These carboniferous strata may not be rich in coal, but they contain the limestone of Ardwick, Leebotwood, and Ballochmoyle Braes (formerly termed a freshwater one), and show a great development of coal measures, which are useful to be known if it be only to show the depth that has to be sunk through before the middle and profitable coalfields of Whitehaven and Canobie can be reached. This portion of the coal measures both in Scotland and the north-west of England has generally been termed permian, and summarily dismissed as unprofitable "red measures." In the Author's paper on the Ballochmoyle limestone,* it was shown that a great thickness of unprofitable coal measures had to be traversed before the profitable coalfield at Common could be reached, in that district some 550 yards.

The Canobie section exposes far more coal measures above the limestone than the one at Ballochmoyle, at least 200 yards, and it shows a passage of carboniferous into permian beds, so far as the eye can detect, better than any that has hitherto come under his observation. The strata of these two formations in the bank of the river above the bridge at Canobie from the fine breccia into the underlying clays and shales are most difficult if not impossible to separate from the red shales and sandstones seen between that point and the bridge there.

The district about Canobie, Penton, and Longtown, has been described at length by Mr. Edmund Gibsone, in an elaborate and well illustrated memoir printed in the Transactions of the North of England Institute of Mining Engineers.† In the Penton Linns section that Author describes the mountain limestone seams of coal, in the Penton Railway section the millstone grit series, and in the Canobie coalfield the middle series; and he shows a fault on the south of the latter coalfield which throws the coal measures down and brings in the permian strata. All the red measures south of this fault Mr. Gibsone appears to consider permian, and the fault which brings them in he calls the Great Permian fault. After examining these red measures, the Author said he had come to the conclusion that although a portion of them are permian strata, as Mr. Gibsone describes them to be, a great part of them are unquestionably upper

* On some Upper Coal Measures containing a Bed of Limestone, at Catrine in Ayrshire.—*Quarterly Journal of the Geological Society* for August, 1862, p. 437.

† A geological paper on the Border Districts of Dumfriesshire, Cumberland, and part of Roxburghshire, including the coal formation of Canobie, &c., by Edmund Gibsone, vol. xi., p. 65.

coal measures. The profitable Canobie coalfield, like that at Common in Ayrshire, belongs to the middle or valuable coalfield; but there is also at Canobie a great thickness of upper coal measures containing a seam of limestone, in all respects like the Ballochmoyle Braes, near Catrine, the Ardwick and Leebotwood limestones. Consequently, the permian fault should be called by some other name, say the Great South fault.

Practical mining engineers have frequently classed all the red and variegated beds which they find in the upper part of the coal measures as "red measures" or permian strata. Now, there is no doubt often great difficulty in drawing the line of demarcation between the upper coal measures and the permian strata, and it is possible that, in some sections, one may pass into the other, as appears to be the case in the river section above the bridge at Canobie, previously alluded to, but in the north-west of England this transition is not generally to be seen. The further we investigate the organic remains of these two formations probably more genera and species will be found to be common to both than is at present supposed; but in all cases where the remains of *stigmaria* and *spirorbis carbonarius* (*microconchus*) have been found in the strata the Author has termed them carboniferous. In the absence of organic remains, which is generally the rule and not the exception, the permian character of the strata has been decided by the mechanical character of the deposits and the order of superposition, the beds of breccia and the soft red sandstone generally affording pretty good evidence of the permian age of the strata over a great extent of country, and varying with the character of the older rocks found *in situ* in the district. If the permian beds are taken as the Moat sandstone, the red shales with gypsum and four breccias lying in soft red sandstone at Canobie and Riddings, their identification is pretty easy; but in continuing them downwards into the upper coal measures, or in tracing their boundary upwards into the trias, there is greater difficulty, as natural sections showing the passage of one into the other are not often met with; but he considers the soft red sandstone of Longtown, West Linton, Rockliffe, and Dalston to be of triassic age and covered by the waterstones and red marls of Carlisle, and these, in their turn to the west, overlaid by the lias of Quarry Gill and Oughterby.

The Knotty Holm sandstone and a similar rock at Penton, especially in their lower portions, reminded him of the Whitehaven sandstone, and it is possible that they may be of the same geological age as that rock, but for the present he has included them in upper coal measures.

In the valleys of the Esk and Liddel he described some interesting permian sections, and detailed at length the particulars of the strata found over a distance of above twenty miles from the upper coal measures at Canobie to the same beds, as seen in the Raw Beck, near Dalston, wher ethe following strata are met with, viz.:—

	ft.	in.
1. Red and variegated clays	13	1
2. Bed of limestone with *spirorbis* &c.......	1	0
3. Red clays	10	0
4. Purple shales containing *stigmaria*	80	0
5. Soft red sandstone........................	40	0
6. Purple shales.............................	16	2

After tracing the Shawk sandstone by Westward Chapel, Wigton, West Newton, near Allonby, to Maryport, he passed over the West Cumberland coal field, and followed it by St. Bees to the south of Cumberland, as far as Drigg Cross. He described at length the permian strata of Barrow Mouth and Ben How, south of Whitehaven. At the former place the beds occurred in the following descending order, viz.—

		ft.	in.
1.	Fine-grained red sandstone, laminated and ripple-marked, same as that seen at Moat, Glenzier, Cove, Shawk, Westward, Maryport, and other places, which may be conveniently termed the St. Bees sandstone, fully	1000	0
2.	Shaly marls	30	0
3.	Red marls containing granular gypsum	29	6
	Magnesian limestone of a cream colour, containing shells of *Bakevellia* and *Schizodus*	10	6
	Breccia composed of pebbles of coal measures, sandstones, and slate rocks	3	0
	Red and purple sandstones	110	0
	Conglomerate sandstone full of white quartz pebbles, with per oxide of iron and volcanic ash, containing common coal plants	30	0

The two last beds have been long known as the Whitehaven sandstone, and Professor Sedgwick many years since classed them as lower red sandstone. After further investigation the Author is inclined to endorse this opinion, as he cannot find any difference between these sandstones and his lower permian beds of Astley, Bedford, and Moira, near Ashby-de-la-Zouch. These singular sandstones lying unconformably to the breccia above and the coal measures underneath, he thinks will be found to be the English representatives of the lower rothliegendes of the Germans.

The Author showed that although the upper coalfield of Lancashire and the Midland Counties of England contained several workable seams, the same beds in Cumberland and Scotland contained none. On the other hand, the mountain limestone series in the latter districts contained numerous seams of coal, whilst none were to be found in the former.

PHYSICAL AND MATHEMATICAL SECTION.

October 15th, 1863.

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

Mr. Baxendell, referring to Mr. Brothers' "Iris Diaphragm" for determining star magnitudes, stated that, theoretically, the aperture of the diaphragm should vary as the cosine of half the angle of rotation of the moveable ring, and exhibited a diagram showing the close agreement between the theoretical and actual apertures in an instrument he had had constructed under Mr. Brothers' direction, and adapted to Mr. Worthington's equatorially mounted achromatic of five inches aperture. He also stated that the only difficulty he had experienced in using the diaphragm was that of not being able to read off readily the divisions of the scale without artificial illumination; but this difficulty he had found could be obviated by the use of a small telescope magnifying about six times, which showed the divisions of the scale on the darkest night with sufficient distinctness for all practical purposes, and thus enabled the observer to make a series of observations without exposing the eye to extraneous light or leaving the eye end of the telescope.

Mr. Baxendell also read the following extract of a letter from Mr. Heelis, F.R.A.S., dated at sea, off Genoa, Sep. 9th, 1863:—

I promised to write to you if I had anything to communicate. I have very little, but that little relates to a rare phenomenon in the Mediterranean, no less than a genuine fog. In the last voyage of the Palestine, on the 7th of July last, being at noon in 2° 51′ W., a little to the westward of Cape de Gatta, she experienced so dense a fog from day-break to nearly four p.m., that the engines had to be run slow, and on this voyage on Sunday morning last a few hours after leaving Gibraltar, and between that place and Malaga, we ran, about 6 a.m., into a dense fog, which lasted, with short intervals of comparatively clear weather, until a little after 11 a.m. The fog was lying in dense lanes, with clearer weather between them, and by a rough approximation of its height, made by noting how far it extended up the mountain sides during the intervals when the weather with us cleared, extended upwards about 4,000 feet. Barometer about 30·100in. The ship had to be run slow and service dispensed with. I see that Smyth in his "Mediterranean" only mentions fogs in the Syrtis. Our captain has made more than seventy voyages up the Mediterranean, and considers the occurrence of fogs where we met with them very rare indeed. Hence, I hope that this may be of interest to you. Of course the weather was calm.

Ordinary Meeting, November 3rd, 1863.

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

Edward Hull, B.A., F.G.S., was elected an ordinary member of the Society.

A Paper, entitled "On the Theory of Equations," by His Honour James Cockle, M.A., F.R.A.S., F.C.P.S., &c., the Chief Justice of Queensland, was communicated by the Rev. Robert Harley, F.R.S., F.R.A.S.

Despite the accession to the subject of Mr. Harley and Mr. Cayley, there remains something to be done in the theory of quintics. I think that a root of the given equation should be expressed in terms of a single value of the resolvent product, and (in the *Philosophical Magazine* for March, 1860, and the *Quarterly Journal* for June, 1860) I have taken some steps in that direction. The latter calculations are pushed somewhat further in a manuscript which I forwarded to Mr. Harley some little time since, but as yet I am not aware whether that intrepid calculator has arrived at any conclusion as to which course it would be advisable to adopt. To express a root in terms of a single value of Mr. Harley's τ or of Mr. Cayley's ϕ would, I believe, be desirable.

Shortly before leaving England for Australia it had occurred to me that the theory of what I propose to call "coresolvents" might throw light on such a question as "What is the most general form of linear differential equation of the second order soluble by means of an algebraical (not necessarily a cubic) equation?" and also on the determination of the points of inflection of curves. I have not pursued these inquiries, though I hope soon to be in a condition to call attention to a class of functions for which I shall propose the name of "differential covariants," the existence of which is suggested by that of "differential critical functions." My memoranda however have not yet arrived here.

With respect to the theory of transcendental solution, I would venture to suggest the publication of Professor Boole's solution of a certain quartic resolvent, in a form involving a single definite integral. Mr. Rawson's researches, as yet unpublished, on the algebraical equations on which given differential equations depend are, too, of high interest.

In reference to my communication printed at pages 16-17 of the "Proceedings of the Literary and Philosophical Society of Manchester," (meeting of December 2, 1862), and to Mr. Harley's remarks thereon, let me observe that we may write the Boolian of (a) in the form

$$4^3[2\mathrm{D}]^3 y - 2^2[4\mathrm{D}-3]^3 e^{\theta} y = 0,$$

and that the portion

$$[4\mathrm{D}-3]^3 e^{\theta}$$

of the sinister is deducible from the portion

$$\left[nx\frac{d}{dx}-r-1\right]^{n-r-1}\left[\frac{n}{r}\,x\frac{d}{dx}-\frac{n+r}{r}\right]^{r} x^{r}$$

of the sinister of (F) by changing r into $r-1$ in the *exponent* of the first factorial and throughout the *whole* of the second expression and substituting. For this change and substitution gives

$$\left[nx\frac{d}{dx}-r-1\right]^{n-r}\left[\frac{n}{r-1}\,x\frac{d}{dx}-\frac{n+r-1}{r-1}\right]^{r-1} x^{r-1}$$

or, making $n=4$, $r=2$,

$$\left[4x\frac{d}{dx}-3\right]^{2}\left[4x\frac{d}{dx}-5\right]^{1} x$$

or

$$\left[4x\frac{d}{dx}-3\right]^{3} x$$

which is identical in substance with the selected portion. The calculation of the differential resolvents of

$$y^5 - 5y^3 + 2x = 0$$

and

$$y^5 - 5y^2 + 3x = 0$$

which seem requisite for completing the discussion of trinomial forms, would in all probability throw further light upon the question.

Brisbane, Queensland, Australia,
Wednesday, Aug. 12, 1863.

Mr. HARLEY added the following remarks "On Recent Researches on the Theory of Equations."

In seeking to express a root of the general quintic equation in terms of a single value of the resolvent product, we are met with considerable practical difficulties. Starting from the Eulerian equations, which connect the coefficients of the quintic with the constituents of its roots, the question becomes one of elimination, and, looked at in a theoretical point of view, it is simple enough. But the eliminations to be effected are so formidable that I have not hitherto had the courage to attempt to carry further the calculations of my distinguished friend Considering, however, the enormous calculations which have of late years been effected in this department of algebra, and considering also the facilities of verification which recent investigations afford, I should be unwilling to affirm that the work is altogether impracticable. If any mathematician who has the necessary leisure is disposed to undertake the work, I shall be happy to supply him with a copy of the manuscript referred to in the first paragraph of the foregoing Paper, and to offer him such suggestions and assistance as I may be able.

In my last communication to the Society, published at pp. 69-71 of the current volume of the "Proceedings," I have given a brief sketch of Bring's reduction of the general quintic equation to a trinomial form; and at the close of a Paper on the history of that celebrated problem, published at pp. 38-47, of the 6th vol. of the *Quarterly Journal of Pure and Applied Mathematics*, I have given Bring's solution *in extenso*. Since the publication of these Papers Mr. Samuel Bills, of Hawton, has communicated to me a very ingenious simplification of Bring's method. In place of transforming the quadratic in d by means of the assumptions

$$b = ad + \zeta \text{ and } c = d + \eta,$$

and making the resulting quadratic vanish identically, Mr. Bills, observing that the equation (E) may be written

$$5(3pc + 4qd + 5r)b + 10qc^2 + 25rcd - 15p^2c - 3p^2d^2 - 23pqd$$
$$- 2q^2 - 20rp = 0,$$

assumes

$$3pc + 4qd + 5r = 0$$

or, what is the same thing,

$$c = -\frac{4qd + 5r}{3p},$$

and by substitution in (E) obtains a quadratic in d, the solution of which gives d in terms of p, q, r; c and a then also become "known"; and substituting for a, c, d their several values in the coefficient of y^2, and equating the result to zero, there arises a cubic in b. Mr. Bills also observes, that if the values of a, c, d thus found were substituted for these letters in the coefficient of y, in place of that of y^2, and the result were equated to zero, we should be conducted to a biquadratic in b. Hence it follows that the general quintic equation may be deprived of its second, third, and fourth terms by the solution of auxiliary equations none of which rise above the third degree; and of its second, third, and fifth terms by the solution of equations none of which rise above the fourth degree. This method of solution may be extended, as I propose to show shortly elsewhere, to the corresponding problems for any general algebraic equation of a degree higher than the fourth.

I am indebted to Sig. F. Brioschi for politely forwarding to me, through this society, a copy of his interesting work, entitled, *Sulla Risolvente di Malfatti per le Equationi del Quinto Grado.* From this work, which is printed in the 9th vol. of the *Memoirs of the Royal Institute of Lombardy,* I learn that Malfatti, in a Memoir published in 1771, in the Transactions of the Academy of Siena, entitled, *De æquationibus quadrato-cubicis disquisitio analytica,* gives a discussion of quintic equations which anticipates to some extent certain recent investigations on the subject. After having proposed and applied an original method for the solution of the lower equations, after having, that is, deduced from each equation another of one degree inferior, which he calls its *resolvent* ("risolvente"), because the solution thereof leads to that of the given equation, Malfatti proceeds on the same method to deal with the equation of the fifth degree, and by a process of elimination arrives at the actual calculation of its resolvent. This resolvent he finds to be an equation of the sixth degree, that is, one degree higher than the proposed equation. Sig. Brioschi is unable to say certainly whether or not Malfatti expected this result, but he inclines to the opinion that Malfatti would scarcely have persisted in so laborious a calculation if he had not hoped that the final equation would be such as to help to a solution of the given one. While Malfatti was engaged upon this problem, Lagrange was publishing in the *Berlin Memoirs* (1770–71), his celebrated method for the general solution of eqnations, in which he bases the theory on the number of values which functions can take by permutation of the variables. Eight years later Ruffini gave to the world his *Theory of Equations* (published at Bologna), and accompanied it with a demonstration of the impossibility of resolving by radicals any general algebraic equation of a degree higher than the fourth. Malfatti at first entertained some doubts as to the validity of Ruffini's demonstration, and in a Memoir published in the

Transactions of the Italian Society for 1804, he set forth these doubts, supporting his views by a reference to the results obtained by himself in his earlier researches. This work of Malfatti gave rise to two excellent Memoirs by Ruffini, also published in the Transactions of the Italian Society, in the first of which he disposes of Malfatti's objections, and in the second he shows *a priori* that the quintic resolvent is of the sixth degree, and that each of its roots is of four dimensions with respect to the roots of the quintic. Sig. Brioschi in the Memoir above referred to, shows that the sextic equation calculated by Mr. Cockle (now His Honour The Chief Justice of Queensland) and myself, and which we communicated to this Society in 1858-59 (see *Memoirs*, vol. XV., pp. 131-142, and pp. 172-219), is substantially the same as that calculated by Malfatti. Sig. Brioschi further shows, in an Appendix to his Memoir, that Malfatti's resolvent may be readily transformed into the sextic equation recently calculated by Mr. Cayley. (*Phil. Trans.* for 1861, pp. 263-276.)

In a work entitled *Die Auflösung der Gleichungen fünften Grades*, by Adolf von der Schulenburg, Hauptmann, a D., published by H. W. Schmidt, Halle, 1861, I find calculated a certain sextic equation whose roots are of two dimensions with respect to the roots of the quintic. On comparing this sextic with Mr. Cayley's, and making allowance for difference of notation and for the circumstance that Herr Schulenburg's sextic is calculated for the quintic wanting in its second term, whereas Mr. Cayley's is for the complete quintic, I find that the two exactly coincide. By means of the seminvariant process I have completed the coefficients of Herr Schulenburg's sextic, that is to say, I have determined the form of those coefficients for the complete quintic, and I have thus verified Mr. Cayley's result.

Touching the theory of transcendental solution, I may state that my own efforts in this department are at present being directed to the determination of the most general forms of differential resolvents, and to the detection of such properties as are common to these forms. The field is fruitful of results.

Mr. R. D. DARBISHIRE read a Paper "On Marine Shells in Stratified Drift at High Levels on Moel Tryfaen, Caernarvonshire."

After recapitulating shortly the development of the present theories of alteration of level and glaciation in the Caernarvonshire district, and especially referring to the discovery, in 1831, of marine shells in drift-beds near the top of Moel Tryfaen by the late Mr. Joshua Trimmer, F.G.S., Mr. Darbishire described his rediscovery of the fossiliferous deposit in extensive sand cuttings made during the present year in opening out the Alexandra Slate Quarry on the north side of the hill.

Professor Ramsay had traced marine drift to the height of 2,300 feet above the level of the sea in the recesses of Carnedd Dafydd and Carnedd Llewellyn, and had found fragments of shells near Maenbras, two miles west of Snowdon, at about the same height as Mr. Trimmer had given; but otherwise the discovery of 1831 had scarcely been followed up, nor even had any complete list of Mr. Trimmer's shells been published.

By repeated barometric observations, Mr. Darbishire had ascertained the fossiliferous beds to extend upwards from 1,330 to 1,360 feet above the sea level, confirming Mr. Trimmer's own measurement.

Last week, the section being about 35 feet in vertical height, exposed on one line the following series:—

FT.	IN.	UNDER THE SWARD.	
0	9	Black peaty soil.	
1	9	Yellowish brown sandy clay, finely and tortuously stratified with scattered, rounded, and angular pebbles	No shells or fragments observed.
1	9	Fine sandy gravel, stratified...............	Ditto.

FT.	IN.		
5	0	Fine clean gravel or shingle, stratified with rounded or partially rounded pebbles of white quartz, greenstone, porphyry, green sandstone, slate, and pinkish granite	Shells and fragments.
5	0	Fine clean brownish-yellow sand, stratified	Fragments not rare.
6	3	Fine clean gravel or shingle, stratified with pebbles as in No. 4	Shells and fragments.
7	6	Coarse greenish clayey gravel	None observed.
0	8	Curious very fine slate gravel or shingle, with greenish and clayey sand.........	A few fragments.
0	8	Edges of slate rock much broken and distorted....................................	None.
6	0	Unbroken slate rock, vertical.	

The sandy clay roof had doubtless tended to the preservation in this spot of the fossil shells and fragments found below. They are nowhere common, though occasionally aggregated in horizontal patches.

A series of the shells and fragments found during repeated visits was exhibited.

The whole assortment of fossils might be denoted as Arctic, but with a characteristic infusion of "Celtic" or British-Atlantic species, or otherwise as glacial within the British province; the series already indicating the character of the present provincial association of species, though under glacial conditions, and with many northern forms.

In the table of species, as follows, is added a list of species identified from Mr. Trimmer's original specimens in the Museum of the Geological Society, London, by Mr. J. Gwyn Jeffreys, F.R.S., F.G.S.

Scale of frequency: *a*, abundant; *c*, common; *f*, frequent; *r*, rare; *v r*, very rare.

		Mr. Trimmer's Specimens.	Mr. Darbishire's List.	REMARKS.
1	Saxicava rugosa......	..	*r.*	Scarce fragments, not large.
2	,, var. arctica..	..	*v. r.*	
3	Mya truncata........	*	*f.*	Small fragments.
4	,, var. Uddevallensis	..	*v. r.*	Fragments.
5	Corbula nucleus......	..	*v. r.*	Broken valves.
6	Donax anatinus......	..	1	Broken valve.
7	Tellina solidula......	..	*a.*	
8	,, var. balthica..	*	*v. r.*	
9	,, proxima, Brown	..	*r.*	
10	Mactra solida........	..	*r.*	Var. approaching elliptica.
11	,, elliptica......	*	1	
12	Venus casina........	..	1	Fragment.
13	,, striatula......	*	0	E. Forbes.
14	,, ? sp.	..	1	Valve.
15	Artemis exoleta	..	1	Fragment.
16	Astarte elliptica	*	*f.*	Whole valves and fragments.
17	,, compressa	..	*c.*	Whole valves, small.
18	,, borealis......	*	*c.*	Fragments.
19	,, crebricostata..	..	*v. r.*	Two fragments.
20	Cyprina Islandica....	*	*c.*	Small fragments.
21	Cardium edule	*	*c.*	Fragments of young and old valves.
22	,, echinatum..	*	*r.*	
23	,, Norvegicum	..	1	Fragment.
4	,, ? sp.	..	1	do.
25	,, ? sp.	..	1	do.
26	Mytilus edulis	*	*f.*	Fragments much worn.
27	Modiola modiolus	*	*r.*	do.
28	Leda pernula........	..	*r.*	do.
29	Pectunculus glycimeris.	..	*v. r.*	do.

5~

		Mr. Trimmer's Specimen's.	Mr. Darbishire's List.	REMARKS.
30	Pecten opercularis....	..	*r.*	Fragments much worn.
31	Ostrea edulis........	..	*v. r.*	do.
32	Dentalium entale . ..	*	*v. r.*	do.
33	,, abyssorum, Sars.	..	*v. r.*	do.
34	Fissurella reticulata ..	..	1	Fragment.
35	Littorina littorea	*	*f.*	Rolled shells and fragments.
36	,, rudis var. ..	..	1	
37	Lacuna vincta	..	1	Perfect.
38	Turritella communis..	*	*a.*	do. large and small.
39	Aporrhais pespelecani..	..	*v. r.*	Two fragments.
40	Natica clausa........	*	*r.*	
41	Trichotropis borealis..	..	1	Perfect.
42	Murex erinaceus	..	*r.*	Broken shells and fragments.
43	Purpura lapillus	..	*f.*	do.
44	Nassa reticulata	..	*r.*	Shells and fragments.
45	Nassa incrassata	..	*v. r.*	
46	Buccinum undatum ..	*	*f.*	One broken, 2 inches long, and fragments of larger individuals.
47	Fusus gracilis	..	*v. r.*	Two broken, one a large and coarse form.
48	,, antiquus......	*	*c.*	One $2\frac{1}{2}$ inches long, many rolled fragments of much larger shells.
49	,, ? craticulatus ..	..	2	Fragments.
50	,, ? Kroyeri......	..	1	One young shell.
51	Trophon clathratus ..	*	*f.*	Large and small shells, perfect.
52	,, scalariformis .	..	*f.*	Not large.

	Mr. Trimmer's Specimens.	Mr. Darbishire's List.	REMARKS.
53 Trophon Gunneri....	..	*f.*	
54 Mangelia turricula ..	*	*f.*	Some in very good condition.
55 „ nebula	*	0	
56 „ rufa	*	0	
57 „ ? pyramidalis Ström	..	*v. r.*	Also at Fort William, Jeffreys.
58 Balanus Hameri	..	2	Fragments.
59 Cliona (? two species).	..	*v. r.*	In pieces of Cyprina, &c.

Total, 56 species in 59 forms.

Charles Clay, M.D., read a paper "On the Existence of the Moa of Naturalists in New Zealand," and related two instances in which it had been seen by different individuals, and its very recent footprints by many others, on the shores of the Middle Island; once on the coast near the gold digging locality, and once on the banks of the Mockilinui River, about two miles from the coast, and twenty-five miles north of the Buller River. It is described as at least eight feet in height; the head as large as that of a full grown calf, with a semicircular scarlet patch of about three fingers breadth under each eye, a thick short straight bill; its legs very thick and clumsy; the neck very short. In fact, the head, neck, and legs appeared out of proportion even with its huge body. The bird was feeding from the tops of the underbush, never attempting to feed from the ground, the shortness of its neck rendering such a process impossible. The neck for a considerable distance from the head downward was denuded of feathers, like the vulture, and the general colour of its plumage was of a dusky brown. Both observers differed slightly from the description as given by Professor Owen, but agreed as to the wonderfully correct idea the zoologist had formed of the animal which he had derived from the inspection of a few bones only.

Owen's sketch, altered according to the suggestions of the above observers, will be shortly laid before this society. The average length of the toes, as measured by different individuals from the footprints, is about ten inches. The bones are very frequently met with, and it is hoped from these facts, more will be known of this gigantic animal so very interesting to naturalists.

Dr. CLAY also read a Paper entitled "Remarks on the Habits and Character of the Maoris of New Zealand." The author's remarks were illustrated by an excellently formed skull of one of the Maori chiefs, showing (with the exception of somewhat narrow frontal regions) a skull of fair development, with considerable amount of brain space, and considerable capability of mental cultivation if begun early in life. The present condition of this tribe is most degraded and lamentable.

Ordinary Meeting, November 17th, 1863.

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

Mr. Samuel Barton Worthington, C.E., was elected an Ordinary Member of the Society.

Mr. F. R. Fairbank exhibited several specimens of Flint Implements which had been ploughed up near Lake Erie in Canada.

A Paper was read "On a New Method of Producing Carbonic Oxide," by Dr. F. Crace Calvert, F.R.S.

In 1820, my learned master, M. Chevreul, published a most interesting paper on various colouring matters, and drew the attention of the scientific world at that time to the property which most colouring matters possessed, of absorbing oxygen under the influence of alkalies; and although he demonstrated the great advantage possessed by gallic and pyrogallic acids, as agents for analysing the atmosphere, by the extraordinary rapidity with which they absorbed oxygen under the influence of alkalies, still this valuable observation of M. Chevreul remained dormant for nearly twenty years, when Liebig again proved with what facility air could be analysed by this method. I wish now to call your special attention to this mode of absorbing oxygen in a gas mixture, as it is considered by many chemists to be quite as accurate as those methods which are based upon the employment of phosphorus, or metallic copper as suggested by Gay-Lussac; but this is not correct, for I have observed that it contains a serious source of error, because during the absorption of oxygen by the gallic or pyrogallic acid under the influence of alkalies, a certain quantity of a permanent and combustible gas is produced, viz.—oxide of carbon.

It was during a series of experiments which I had undertaken to verify a most interesting fact, published by M. Boussingault, viz., the production of oxide of carbon during vegetation under water, or the decomposition of carbonic acid under the influence of solar rays (see *Comptes Rendus*, vol. 63, p. 862, 1862), that I discovered that the oxide of carbon which M. Boussingault had obtained in his analysis of the gas mixture produced during vegetation, was not due, as he thinks, to the reduction of carbonic acid into oxide of carbon; but that this gas was a product of oxidation, or the result of the action of oxygen on the pyrogallic acid that he used for analysing his gas mixtures.

The following details of my experiments will, I trust, clearly prove this curious production of oxide of carbon:—

1. If over a mercury trough 100 cubic centimetres of oxygen are introduced under a bell jar together with pyrogallic acid and potash, although the greatest portion of the oxygen is rapidly absorbed, still whatever number of hours the substances are left in contact, there will always remain a certain residue incapable of further absorption, and on examining this residue, it will be found to burn with a blue flame, and to be converted thereby into carbonic acid, which compound is easily characterised.

2. If two litres of oxygen, free from any trace of carbonic acid, are made to pass through a solution of pyrogallate of potash placed in Liebig's bulb apparatus, and from thence over carefully prepared oxide of copper, heated to a dull red heat, carbonic acid will be found to issue from the apparatus, and can be collected and weighed.

3. If an unabsorbed portion of gas remaining in the receiver (Experiment, No. 1), be brought into contact with a solution of protochloride of copper, dissolved in hydrochloric acid or

ammonia, the residue will be absorbed, which is an additional characteristic property of oxide of carbon, as indicated by M. Le Blanc.

4. If one volume of this residual gas be introduced into a eudiometer, and after being mixed with one volume of oxygen the mixture be exploded by an electrical spark, it is found that half the volume of oxygen employed has disappeared, and that the volume of carbonic acid formed is equal to that of the oxide of carbon introduced into the eudiometer.

As to the quantity of oxide of carbon produced during these experiments, it depends firstly upon the concentration of the pyrogallate employed, and secondly upon its neutral or alkaline state, the minimum being with the acid liquor, and the maximum with an alkaline.

In taking the mean of ten distinct experiments, I found that 100 volumes of oxygen gave two volumes of oxide of carbon, but in some instances the proportion was as high as four per cent.

The production of oxide of carbon also occurs when atmospheric air is substituted for oxygen, only the oxide of carbon is so much diluted by the nitrogen that its presence cannot be detected by several of the above methods. But if, on the other hand, several quarts of air, carefully deprived of carbonic acid, are passed through an alkaline solution of pyrogallate, or gallate of soda, and from thence over some heated oxide of copper, carbonic acid will be found issuing from the apparatus, and can be easily characterised, and even weighed, as I have done in some experiments.

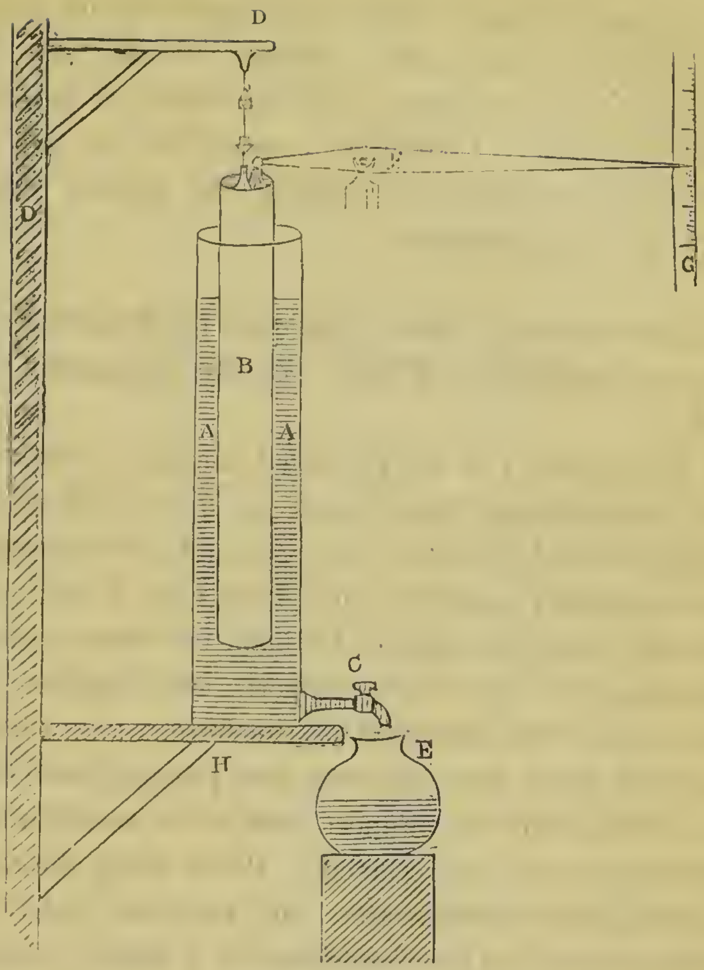
It is no doubt owing to the difficulty of detecting a small quantity of oxide of carbon when diluted with nitrogen, that the presence of that gas has not been observed before by chemists, who have employed Chevreul's and Liebig's method of analysing the atmosphere.

M. Boussingault having observed the presence of a light carburetted hydrogen in the residuum which he obtained in analysing the gases which were produced by vegetation under water, and under the influence of solar rays, I made careful and repeated efforts with the object of detecting it, but failed to do so. I therefore suspect that this gas was the result of decomposition of some of the organic substances present in his experiments.

A paper was read "On an Apparatus for Measuring Tensile Strengths, especially of Fibres," by Mr. Charles O'Neill, F.C.S.

In the sketch, A is a cylindrical metallic vessel to hold water, and provided with a cock C. B is a hollow cylinder of glass or metal, closed at the lower end, and so weighted as to float vertically in stable equilibrium with a portion out of the water; upon its upper end a hook or clamp to hold the fibre is fixed. D is a fixed support, with another hook or clamp to hold the other end of the fibre. F is a lever with a long and short arm, the long arm passing over the scale G. H is the table or support, and E is a vessel into which water drawn from A is received. When using the apparatus it is nearly filled with water, and the fibre to be tested is properly secured to the fastenings on B and D, then drawn taut. Water is now allowed to flow slowly from C until the fibre breaks. The quantity of water drawn off is ascertained, and from it the strain put upon the fibre calculated. The indications of the long arm of the lever are also noted in order to show the stretch, and also to give the elements for a correction to be made upon the quantity of water drawn off.

Stops and guides not shown in the sketch serve to keep the floating cylinder off the sides of the vessel, and prevent it falling too far upon the rupture of the fibre.



The principle upon which the apparatus works is so simple that it hardly requires explanation. At the beginning of the operation the weight of the tube is wholly supported by the water; by drawing off the water the support is very gently removed and the weight thrown upon the fibre. The relation between the actual weight put upon the fibre and the weight of water drawn off will vary for every different dimension of the containing vessel and floating cylinder, but in regularly shaped vessels it will always be in the direct ratio of the sectional areas of the floating vessel and the difference between this and the sectional area of the containing vessel, *i.e.*, in cylindrical vessels the sectional area of the ring of water surrounding the floating cylinder.

In the apparatus brought down for illustration and actually in use by the writer, there were three floating cylinders whose sectional areas bore the following ratios to the ring of water by measurement:—

Larger cylinder	1 : 0·925
Medium cylinder........................	1 : 20·610
Smaller cylinder	1 : 492·6

In actually testing, by means of a chemical balance, the relation between the weights of water drawn off and the weights put upon a fibre, the following numbers were found:—

Larger cylinder..	0·926	gr. water = 1 grain strain.
Medium cylinder	21·09	gr. water = 1 grain strain.
Smaller cylinder	476·10	gr. water = 1 grain strain.

The large discrepancy in the case of the smaller cylinder is owing to the difficulty of measuring it correctly; its sectional area was computed to be 0·001989 inch.

This apparatus has several advantages: the strain is put on in the most gradual manner, without jerks or shocks; it can be put on at any rate per minute or hour, and there is hardly any assignable limits to either its power or delicacy. By the smaller floating cylinder a strain of 0·0002 grain can be measured, and by increasing the size of the apparatus a strain of a hundred tons could be put on with the most perfect gradation.

Mr. O'Neill also read a Paper, entitled "Experiments and Observations upon Cotton."

(1.) The author began to make experiments upon the chemistry of cotton dyeing, but found himself compelled to abandon experiments upon manufactured cotton and to come down to the primary fibre or hairs of cotton.

(2.) He has made very numerous experiments upon seventeen samples of cotton supplied to him from reliable sources, and compared their physical and chemical properties.

(3.) He has given about 400 experiments upon the length of cotton hairs measured separately by a simple process, which he fully described, and exhibited a diagram upon an enlarged scale, showing the mean, maximum, and minimum lengths of the seventeen qualities of cotton experimented upon. The table below is a résumé of the experiments, but the author furnishes it in this abstract with the caution that, taken apart from the detailed measurements as given in the full paper, it may give rise to incorrect conclusions.

NAME.	Price.	Date.	Longest Fibre. in.	Mean Length. in.	Shortest Fibre. in.
Sea Island Edisto	26d.	Dec., 1860	2·00	1·680	1·35
Sea Island	54d.	Mar. 1863	1·95	1·501	1·10
Queensland Cotton			1·80	1·475	1·20
Sea Island	16d.	Dec., 1860	2·05	1·441	1·10
Egyptian	$9\frac{1}{4}$d. to $9\frac{1}{2}$d.	„	1·55	1·252	0·95
Egyptian fair	22d.	Mar., 1863	1·50	1·185	0·85
Maranham			1·40	1·220	0·95
Benquella			1·50	1·177	0·85
Pernambuco	23d.	Mar., 1863	1·50	1·1675	0·75
Maranham	$8\frac{1}{2}$d.	Dec., 1860	1·35	1·127	0·85
Mobile	$6\frac{7}{8}$d.	Dec., 1860	1·20	1·035	0·75
Orleans	$7\frac{1}{8}$d.	Dec., 1860	1·25	1·002	0·70
Upland	$6\frac{7}{8}$d.	Dec., 1860	1·20	0·9925	0·80
Orleans (good middling)	$22\frac{1}{2}$d.	Mar., 1863	1·15	0·970	0·85
Surat (fair Dhollerah)	$17\frac{3}{4}$d.	Mar., 1863	1·15	0·9425	0·75
Surat (Dhollerah)	$5\frac{1}{4}$d.	Dec., 1860	1·10	0·925	0·55
Surat (middling Comptah)	15d.	Mar., 1863	1·05	0·905	0·70

(5.) The author has determined the tensile strengths of the hairs of the various qualities of cotton by means of the apparatus described in the previous abstract, and has given, in a series of tables, the breaking weights in grains of every hair tested, with remarks upon them. The following table gives the mean and maximum strengths of the hairs; but, like the preceding table, it ought not to be taken apart from the detailed tables, where the particulars of the breaking of about twenty hairs of each kind of cotton are given:—

	Mean. Grains.	Maximum. Grains.
Edisto Sea Island	83·9	142·5
Sea Island (good quality)	90·0	132·0
Benquella	100·6	218·8
Sea Island Cotton	102·6	203·0
Uplands	104·5	212·6
Surat (fair Dhollerah)	105·8	215·5
Maranham	107·1	187·2
Egyptian (fair)	108·0	157·9
Mobile	118·8	172·3
Egyptian	127·2	191·0
Orleans	139·7	289·4
Pernambuco	140·2	251·1
Surat (Dhollerah)	141·9	236·6
Maranham (good middling)	142·9	242·4
Queensland	147·6	246·2
Orleans (good middling)	147·7	264·0
Surat (middling Comptah)	163·7	280·2

PHYSICAL AND MATHEMATICAL SECTION.

November 12th, 1863.

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

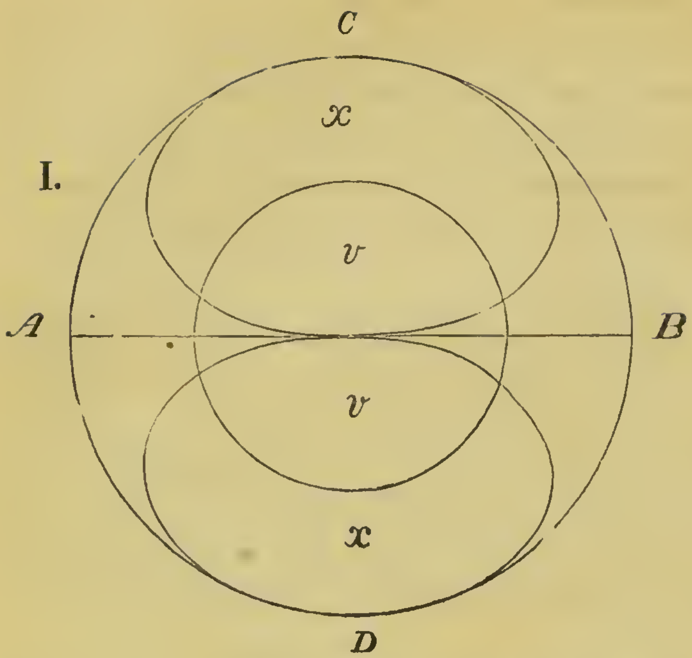
A Paper was read by Mr. J. ATKINSON, F.G.S., "On the Construction of Bi-focal Lenses for Spectacles."

The author stated that he had for many years been obliged to wear concave glasses to remedy what is called "short-sight." About seven years ago he found that from some change in his eyes he could no longer read with the concave glasses which he used for looking at distant objects. Not being able to see things distinctly at the distance of even a few feet without spectacles, and at the same time being obliged to take off his glasses to read or to look at any thing close at hand, was such an inconvenience that he sought to obviate it by applying to an eminent optician to make him a pair of glasses with bi-focal lenses. After some delay Mr. Atkinson was informed by this gentleman that the thing could not be done; that the only way for making a spectacle glass to meet the defect complained of, was to cut a convex and a concave lens, each of the proper focus, and to put them edge to edge in the frame, so as to have the convex part below and the concave part above. His arguments to the contrary, on the plea of "ne sutor supra crepidam," not being listened to, Mr. Atkinson was obliged to adopt spectacles of the kind recommended. These glasses answered their intended purpose very well, so far as to seeing both near and distant objects, but besides having an unsightly bar directly across the centre of each glass, they were very liable to accident from falling out of the frame.

A few months ago, having seen a gentleman with a pair of spectacles with bi-focal glasses, Mr. Atkinson ascertained the name of the optician who made them, and applied to him to have a pair made of a similar kind. He was told that the glasses alluded to were composed of two convex ones of different focal lengths—a kind that could easily be made; but that a convex and a concave lens could not be made as required on the same piece, and that the only method of making spectacles of the peculiar kind he wanted was that which he had already had recourse to. Being still satisfied that this representation was incorrect, Mr. Atkinson succeeded in inducing the optician in question to listen to his suggestions for effecting the object he had in view.

The plan suggested will be best understood by reference to the accompanying diagram, in which *A C B D* represents a circular piece of glass sufficiently large to admit of two spectacle glasses being cut out of it. This, in the first place, is to be ground into a plano-convex lens of the required focus; then in the centre of this is to be ground a concave lens of the right focus, as represented by the inner circle *v v*.

I.



II.

After this the entire piece is to be divided into two equal parts, by any diameter *A B*, and out of each of these a spectacle glass is to be formed as in fig. I. In these *x x* will be the convex portions, forming the lower part of each glass when completed; and *v v* the concave portions, or upper halves of each glass. In fig. II. *C D* represents a section of fig. I. along a line from *C* to *D*. It will be seen that each glass when finished consists of a portion of a plano-convex and a plano-concave lens; and that these can of course be made of any focal length required for correcting the abnormal action of the eye upon the visual rays.*

After the above plan of construction had been described by means of a diagram, the optician last referred to was satisfied of its practicability. He sent the diagram to the firm who manufactured lenses for him (and who had before pronounced the making of such glasses impossible), and in a few days Mr. Atkinson obtained a pair of glasses of the kind he wished

* Glasses each being part of a double convex and a double concave lens might, if required, be constructed by similar means.

for, and which answered his purpose so perfectly as to leave nothing to be desired in the way of improvement.

As he has met with many persons inconvenienced by a defect of vision similar to his own, he considered the foregoing plan of constructing glasses for remedying it worthy to be put on record in the Proceedings of this society.

Mr. Baxendell read the following extracts of a letter from Mr. Heelis, F.R.A.S., dated St. George's Channel, October 23rd, 1863:—

On the 17th Obtober, while running between Cape de Gatte and Malaga in long. 2° 49′ W., lat. 36° 19·1′ N., the captain and I were observing the planet Venus, which was visible to the naked eye and showed her crescent beautifully in one of the ship's signal telescopes, when the quarter-master at the con suddenly, at 10 45 a.m., called my attention to an object in the sky, I looked up and saw a meteor nearly, I should think, twice as bright as Venus, and apparently consisting of two nuclei, with a bright streak joining them, the whole about a degree in length, passing rapidly across the sky in a due easterly direction. It disappeared at an angle of 40° from the horizon, measured by the captain's sextant, which happened to be on deck. The sky was cloudless and very clear. From inquiries from the quarter-master, it appears that he, while looking aloft for Venus, first saw it about 10° or 15° S. of the zenith, and the duration of the apparition was as nearly as I could judge about fifteen seconds. On the two preceding nights we had had many magnificent meteors; and on our passage across the bay, on the 21st and 22nd instant, have since seen several more, notwithstanding the moonlight. At the time of the appearance of the meteor we were nearly abreast of the highest peak of the Sierra Nevada, which was covered with snow. The wind and sea were both calm. Venus was observed every day until the 19th, on which day she was so bright that the captain took her altitude with the sextant soon after her meridian passage. A little before noon on this day we rounded Cape St. Vincent, but shortly before doing so, and when off Cape Sagres, we ran through several large patches of the gulf weed or fucus natans. I could not be mistaken in this weed, and my judgment was corroborated by the captain, who is an old East Indian commander, and who told me also, but without mentioning dates, that he has on several occasions seen this weed in the same place before, and even has at times seen it inside of the Gut of Gibraltar. If this be so, the statement which is, if I remember rightly, made by Maury that it is hardly ever found except on the right of the axis of the Gulf stream, must be incorrect,—and drains of the stream must find their way more commonly than is generally imagined on to the coast of Portugal.

Connected as I think with this, we have had dense fogs along the coast, from Cape Espichel to this side of Cape Finisterre, clearing during the day, but settling down in the afternoon and night, except off Finisterre, where the sun seemed unable to pierce them.

On the 9th September, on the passage up from Gibraltar to Genoa, we were in the morning off the Hyeres Islands, near Toulon, our distance from them when abeam being about twelve miles. We steered E.N.E. by compass all day until 7 p.m., the long. at noon being by chron. 6° 54′ E., lat. observed 43° 00′, variation by amplitude of the sun at sunrise, including deviation 16° 26′ W. At sundown, Cape Della Mele being on the port bow, and the land composed of spurs of the Maritime Alps, being about fifteen miles distant, we found by an observation of Polaris that the variation, including deviation as before, had increased to 22°, and the captain told me that he had on three or four other occasions found the same thing, or rather had found that the ship steered by compass had not at this particular point made a correct course, but that he had always before set the error down to a strong current, which he had assumed to sweep along the coast. This seems a well authenticated case of error induced in highly magnetised compasses by these mountains. From the courses steered by small wooden coasters in company with us, we gathered that their compasses were not affected. Working the ship's dead reckoning from noon, I place her at the time of the morning amplitude in lat. 42° 25·1′, long. 6° 07·7′ E., and at night when the variation was ascertained by an observation of Polaris in lat. 43° 34′, long. 7° 34′ E.

Mr. BAXENDELL read a Paper "On a New Variable Star (T Aquilæ)."

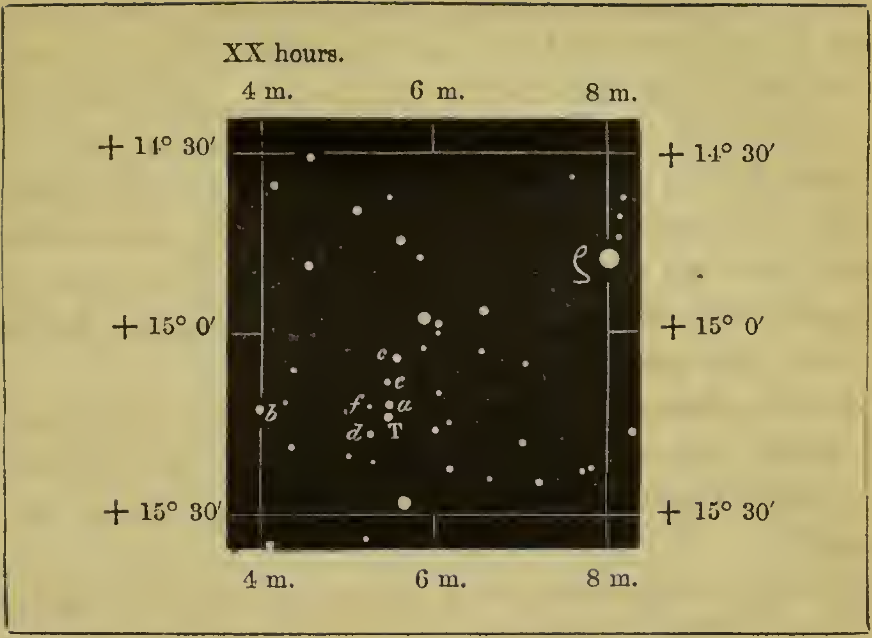
On the night of the 28th of July last, I observed that the star No. 4078 of Zone + 15° of the "*Bonner Sternverzeichniss*," was about *one magnitude and a quarter* less than its neighbour No. 4079, although on the night of the 25th of June I had noted down these two stars as being nearly equal in brightness. Afterwards, on referring to my notes of reviews of this part of the heavens, made at Mr. Worthington's observatory on August 19, 1859, and June 18, 1860, I found that at the former date both these stars were seen, and were then nearly equal in magnitude; but that a third star, No. 4077, was invisible; while under the latter date it is stated that No. 4077 was then visible and estimated to be of the ninth magnitude, but no mention is made of the two stars Nos. 4078 and 4079. I have since made a careful examination of the positions of these and some of the neighbouring stars, and have satisfied myself that the star seen on June 18th, 1860, was No. 4079, and not No. 4077 as was then supposed, and that therefore both 4077 and 4078 were at that time invisible, or, at all events, below the eleventh magnitude. After the 28th of July last No. 4078 gradually diminished in brightness until about the 24th of August, when it passed a minimum. It has since increased, and attained a maximum about the 25th of October. Its magnitude at minimum was 11·3, and at maximum 8·9, the range of variation being therefore 2·4 magnitudes. As the ratio of light employed in determining the magnitudes is 2·512, it follows that the brightness of the new variable is nine times greater at maximum than at minimum. The observations made since the 28th of July last, compared with those of August 19th, 1859, and June 18th, 1860, indicate a period of about *four* months.

As two telescopic variables had been previously discovered in *Aquila,* the new variable will, according to Professor Argelander's system of nomenclature, be denoted by the letter T.

Taking the naked-eye star ρ *Aquilæ,* of the fifth magnitude, as a point of departure, the small chart annexed to this communication will enable observers whose telescopes are not mounted equatorially to find the new variable without difficulty. The magnitudes of the comparison stars are:—

a=9·2.	d=10·4.
b=9·4.	e=11·0.
c=10·1.	f=12·0.

These magnitudes have been determined photometrically by means of Mr. Brothers's "Iris Diaphragm." I may however remark that the results for the star f have been rather discordant, and it may therefore prove to be slightly variable. It is very near the place of Argelander's No. 4077, and may probably be the star which he observed and which he estimated to be of the 9·4 magnitude. The place of T Aquilæ brought up to 1865 is 20h. 5m. 25·4s. + 15°·13·4′.



MICROSCOPICAL SECTION.

Annual Meeting, May 18th, 1863.

A. G. Latham, Esq., in the Chair.

The Annual Report of the Section for Session 1862-63 was read, and officers appointed for the ensuing Session.

A communication on "The Structure of the Cotton Fibre," by Mr. Charles O'Neill, F.C.S., was then read, in which the author states that chloride of zinc, as neutral as it could be made by digesting with metallic zinc, and also diluted sulphuric acid, would under favourable circumstances exhibit all the phenomena described by the author in his first communication. Chloride of zinc, however, required to be heated to its boiling point, and sulphuric acid appeared very capricious in its action. The appearances produced by these re-agents lead him to the same conclusions with regard to the structure of cotton; but he is more decidedly of opinion than he was before, that the so-called medullary matter is in reality a shrunk membrane similar in appearance to the membrane in dried quills. Finding that all known solvents of cotton gave the same appearances, Mr. O'Neill tried the action of solvents on gun-cotton, and found a further confirmation in the action of ether upon it.

It is well known that there are two modifications of gun-cotton, one soluble, the other insoluble in ether; but the author finds three varieties—(1.) Soluble in ether, but insoluble in ammoniuret of copper; (2.) Insoluble in ether, but soluble or dilutable in ammoniuret of copper; and, (3.) Perfectly unacted upon either by ether or ammoniuret of of copper. Operating on the first variety on the stage of the microscope with ordinary ether, it is almost instantly dissolved with no evidence of structure, until, after a while, careful observation shows some remains of spiral vessels. By gradually diluting the ether with alcohol, the action is slackened until a point is arrived at when exactly the same phenomena are produced as by the copper solution. About two-thirds ether and one-third alcohol was found to be a suitable mixture; but this will evidently vary with different preparations.

Mr. O'Neill considers the number of turns of one spiral to be certainly not greater than from 1100 to 1300 in the inch, and generally much less than this, the mean of many countings running between 600 and 700 for the contracted fibre.

Mr. A. G. Latham made the following communication:—

It may be remembered that some few months ago I proposed to this Section as a subject for discussion, "The Causes of the Metallic Lustre of the Scales on the Wings of certain Moths."

I then suggested that the metallic markings, and lustre of the scales themselves forming these markings, are consequent on the fact of the scales containing a particular pigment or colouring matter, while other members thought it might proceed solely from light reflected from the irregular surfaces of the scales.

On examining lately, by transmitted light, the wings on one of the clear-winged moths — Sesia Tipuliformis — I found on the transparent portion of the wing, and in addition to the markings on the wing, certain other scales of battledore form and perfectly transparent.

An examination with a higher power showed these scales to be highly striate, and therefore in the most proper condition for producing, according to the advocates of the theory I oppose, metallic lustre and metallic markings; and that they are in a condition to produce these effects, were the theory correct, is further shown on examination by reflected light—when, as might be expected from the markings, the scales are most beautifully opalescent, but wanting internal pigment give out no metallic markings on the wing, and a strong proof is, therefore, given in favour of the theory broached by me.

Ordinary Meeting, October 19th, 1863.

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

The following paper "On Transparent Injections," by Messrs. J. G. Dale, F.C.S., and Thos. Davies, was read by the Secretary.

After enumerating the various desiderata of a transparent injecting fluid, it was observed that soluble colouring matters failed to fulfil them, owing to the action of endosmos, causing them merely to dye the tissue sought to be injected. This defect is shown to be remedied by the use of insoluble colouring matters in an exceedingly fine state of subdivision, which can only be prepared by precipitation under constant agitation, and the following recipe is stated to succeed admirably, showing vessels of $\frac{1}{2000}$ of an inch, with a clear outline even under a $\frac{1}{5}$ objective, without any grain or extravasation of the colouring matter :—

Take 180 grains best carmine, $\frac{1}{2}$ fluid oz. ammonia, com. strength, SG 0·92, or 15 degrees ammonia meter, 3 to 4 oz. distilled water. Put into a small flask, and allow to digest without heat 24 to 36 hours, or until the carmine is dissolved. Then take a Winchester quart bottle, and with a diamond mark upon it the spot to which 16oz. of water extend. The coloured solution must then be filtered into the bottle, and to this pure water must be added until the whole is equal to 16oz. Next dissolve 600 grains in potash alum in about 10 fluid oz. of water, and add to this under constant boiling a solution of carbonate of sodium, until a slight permanent precipitate is produced. Filter and add water up to 16 fluid oz. Boil, and add this solution while boiling to the cold ammoniacal solution of carmine in the Winchester quart, and shake vigorously for a few minutes. A drop now placed upon white filtering paper should show no colouring ring; should it do so, the whole must be rejected. Supposing the precipitation to be complete or very nearly so, shake vigorously for half an hour, and allow to stand till quite cold; the shaking must then be renewed and the bottle filled up with cold water.

After allowing the precipitate to settle for a day, draw off the clear supernatant fluid with a syphon. Repeat the washing till the clear fluid gives little or no precipitate with chloride of barium. So much water must be left with the fluid that at last it must measure 40 fluid oz. For the injection fluid take 24oz. of the above coloured fluid and 3oz. of good gelatine, allow these to remain together all night, then dissolve by the heat of a water bath, after which it should be strained through fine muslins. On injecting, the ordinary precautions for a gelatine injection are alone necessary.

Professor Williamson stated that, owing to the unexpected absence of his esteemed friend Mr. Sidebotham, he had been suddenly called upon to give the members of the Society an address at the opening of the session. With so short a warning it was not an easy task; still, as a few stimulating words might lead to extra exertion, he would make a few remarks on the present position of the microscopic observers. Their numbers in Manchester were necessarily small compared with London. Perhaps there were not twenty microscopists in this city really at work; few were able to devote the time to the energetic and laborious efforts which original investigation required, and of these fewer had the talent or even the ambition to undertake what requires weeks, months, nay, often years of arduous toil. The hindrances are increased by the fact, that there is rarely a definite end sufficiently certain of attainment in the way of a new discovery, calculated to repay the expenditure of labour.

Hence, in a small society like ours, we cannot expect great or brilliant results. But further, the present is not an epoch like that when Ehrenberg revolutionised an entire branch of science, or when Greu laid the foundations of vegetable physiology, and Malpighi that of the animal kingdom. These men revealed entirely new fields of enquiry. But though no such new worlds of histology are opened out to us, there are such a multitude of secondary details requiring elucidation, that we cannot take up a plant or insect without stumbling upon a multiplicity of problems awaiting investigation. One shrewd observer, when eating his orange, discovers upon them some brown scales. He follows up the enquiry they suggest, and the result is an elaborate paper on the coccus of the orange.

Even where members are not prepared for original researches, they still may do excellent service by examining the ground gone over by other men, whose views require corroboration before their somewhat startling conclusions can be unhesitatingly received. He would refer to such inquiries as Dr. Hincks's on the conversion of the protoplasm of the volvox into free moving Amœbæ and to those of Dr. Balbiani on the sexuality of the Polygastrica, as illustrations. These researches require re-examination and further confirmation; and whilst the latter would give the results attained a fixed place in scientific annals, their rejection, should they prove erroneous, would remove stumbling blocks out of the way. In fact, all discoveries required careful re-investigation. Observers were often too sanguine, and drew large inductions from small and defective data, and this work of supervision was one in which our members might successfully engage. He also thought it desirable to warn the members against the contracting tendencies of minute microscopic research as opposed to philosophic breadth. If men limit their ambition to resolving the small markings of diatoms apart from the great physiological questions to which they bear relation, they will inevitably succumb to this paralysing influence. They must be careful not to lose themselves in the mere examination of details, but to keep in view that the discovery of general laws should be their object, to the attainment of which the former was only a means. Mere details were useful, but to limit our attention to them crippled the intellect, and rendered it unable to combine them and trace out their connection with general laws. It was by keeping the attention fixed on this higher object that placed our most distinguished histologists on the pedestals they now occupy—and, as it is the duty of every man to do what he does in the best manner he can, it behoves all members to keep this lofty aim carefully in view.

The results would then not only advance science and benefit their fellow-men, but, if worldly fame were their object, they would reap it in the fullest measure to which they were entitled.

Ordinary Meeting, December 1st, 1863.

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

A Paper was read by J. P. Joule, LL.D., entitled "Note on the Meteor of February 6, 1818."

This meteor is noticed in Mr. Greg's very complete list of these phenomena in the British Association Report for 1860. The account of it is, however, given at greater length in the *Gentleman's Magazine*, a periodical which I think is not included in the large number of works consulted by Mr. Greg. This meteor is one of the few which have been seen in the day time, and is also interesting as having been one of the first whose observation afforded materials for the estimation of its altitude. In the work just referred to it is described as follows:—

"At two o'clock p.m. a large and luminous meteor was seen descending vertically from the zenith towards the horizon, in the northern part of the hemisphere, by persons in the neighbourhood of Cambridge. The most remarkable circumstance attending this phenomenon is, that it was thus visible in broad daylight, the sun shining at the same time in great splendour, in a cloudless sky. The same meteor was seen at Swaffham, in Norfolk, at the same hour. It was seen also at Middleton Cheney, near Banbury, in the county of Northampton, not in the zenith, but perhaps 45° from it, in the north eastern quarter of the heavens, shooting along towards the north. It seemed to be divided into two before it became extinct."

The distance between Middleton Cheney and Cambridge in a straight line is 61 miles; which, therefore, will be the elevation of this meteor above the surface of the earth, according to the above observation.

The phenomena presented by meteors have for a long time occupied a good deal of my attention. Many years ago I published my opinion that they arise from the resistance of the atmosphere to the motion of bodies which, wandering through space, become entangled by the earth's attraction. I endeavoured to show that at a very considerable elevation there would be sufficient air to cause the liquefaction and even vaporisation by heat of bodies moving at a velocity so enormous as must be assigned to them if arrested in their motion through space. Latterly new facts have been collected by the Committee of the British Association, and also new experiments in physics have been made bearing on the subject. My object is now to investigate these to see how far they confirm, oppose, or modify my original opinion.

In the Report of the Association for 1862 we have the following results for five remarkable meteors:—

Date.	Height at Beginning.	Height at End.	Velocity.
1861—July 16.	195	65	55
Aug. 6.	126	21	35
Nov. 12.	95	20	48
Nov. 19.	55	30	24
Dec. 8.	110	45	23
1862—Jan. 28.	44	47	46
Feb. 2.	190	15	39
Average	116	35	39

Simultaneous observations at Hawkhurst and Cambridge of the meteors of August 10th of the present year give for the average of 10 — 81 ...... 66 34

The above results give us a very good idea of the height and velocity of meteors. The observations are attended by some difficulty, so that errors are liable to occur in individual instances, but we may rely on the general statement that the larger meteors have their origin at a higher elevation than the smaller ones, and that they attain a nearer distance from the earth at the end of their course.

In addition to the most valuable facts collected by Mr. Herschel, we have a telescopic observation by Schmidt, of the Observatory of Athens, of a splendid meteor on the 19th of October, at 2 55 a.m. Mr. Schmidt was occupied in observing shooting stars, and saw the one in question at its commencement. At first it appeared like a star of the fourth magnitude; after two seconds, it was of the second magnitude; at the third and fourth second, it surpassed the splendour of Sirius. It slowly passed towards the west, appearing a dazzling meteor of 10′ to 15′ diameter. At this moment M. Schmidt followed the meteor with his telescope for fourteen seconds. Its appearance was most remarkable. There were two brilliant bodies of a yellowish green in the form of elongated drops, each followed by a well defined tail of reddish colour. These were followed by smaller luminous bodies of the same shape, each followed by its red trace. The meteor disappeared at an elevation of 1° above the horizon. Four minutes afterwards M. Schmidt still observed the remains of the meteoric train of a yellowish white, and covering an area of nearly 5°.

The above observation is extremely important. It shows, firstly, that meteors are probably for the most part of a compound nature, consisting of a greater or less number of bodies associated together by the force of gravitation—small systems in fact; and, secondly, that they present evident marks of fusion, either in the whole or in part, after passing some time through the rarer portions of our atmosphere.

The researches by Professor Thomson and myself enable us to arrive at reliable conclusions with respect to the temperature acquired by bodies moving rapidly through the air. The law is very simple, viz.:—The temperature ultimately acquired by the moving body is the equivalent of the force with which the particles of air come in contact with it. It is, in fact, the temperature acquired by each particle of air on being caught and suddenly dragged on. This temperature, cleared from the effects of radiation, is 1° C. for a velocity of 145 feet per second, and goes on increasing with the square of the velocity.

Hence we find that the ultimate temperature acquired by a body moving through air of whatever density is for the velocity of 39 miles per second 2000000° C. The question to be solved is whether at the known height of meteors, as above stated, the density of the air is sufficient to give rise to effects in quantity sufficient to account for the actual phenomena.

Now if we reckon the decrease of density to be one quarter for every seven miles, we shall find the quantity of air in a column of, say a mile long and one square foot section, to be about ·0003 of a grain at the height of 116 miles, the elevation at which meteors in general are first observed. The temperature acquired at the surface of a meteorite of a foot section and of the specific heat ·23, moving at the average velocity at the average highest elevation through one mile, will be ·0003 gr. raised 2000000° C., or otherwise ·2 gr. raised 3000° C. which would be doubtless able to fuse any known substance, and bring it to a condition of dazzling brilliancy. The meteorite of a foot section might, I believe, have $\frac{1}{5}$th of a grain of its surface brought to this condition in its passage of one mile in the $\frac{1}{39}$th of a second. I do not think that the luminous effect would be neutralised by conduction of heat to the interior of the meteorite, as it is very likely that the spheroidal condition would be produced. I can therefore easily believe that the $\frac{1}{5}$th grain at 3000° C. would give sufficient light to attract an observer at the distance of 100 or 200 miles.

From data given in Herschel's "Outlines of Astronomy" I find that, at the elevation and velocity above stated, viz., 116 and 39, and supposing as before the entire effect to be given out by radiation, a meteorite of five feet diameter would have the brilliancy of *a* Centauri.

As the meteorite descends towards the earth its brilliancy will increase to a certain point, when, from the quantity of fused matter, a longer tail will be left behind. This process will of course be sustained for a longer time by the larger meteorites, which are thus enabled to penetrate to a nearer distance from the earth's surface.

Ordinary Meeting, December 15, 1863.

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

Mr. Robert Leake was elected an Ordinary Member of the Society.

J. C. Dyer, V.P., read a Paper entitled "Notes on some recent Discoveries in Elemental Physics."

He stated that the discoveries in question related to the nature of heat and force. From the earliest times the nature of heat had been treated upon opposite theories; the one maintaining heat to be a material element *sui generis*, pervading space and bodies, but in itself imperceptible except by the effects it produces in connection with bodies: the other theory considering heat to be the result of mechanical force exciting the particles or molecules of bodies, and producing the effects of sensible heat in them. Boerhaave was among the most able expounders of the former theory, Bacon and Boyle of the latter.

In later times the materiality of heat has been forcibly sustained by the discoveries of the existence of latent or specific heat in bodies, by the experiments of Drs. Black, Irvine, Crawford, and others; whilst the mechanical origin of heat has been sustained by the experiments of Count Rumford and of Dr. Joule.

Upon these conflicting views many able treatises have been written on both sides; yet the question as to the nature of heat remains involved in such obscurity as to require far more decisive evidence than has yet appeared for settling either of them upon a sound and immovable basis.

The author gave several extracts from the able address of the President (Sir W. Armstrong) delivered at the last meeting of the British Association — Newcastle, August, 1863 — wherein Sir William, in support of the mechanical theory of heat, adduced several facts and considerations which appear inconsistent with that doctrine, or the non-materiality of heat; and as he assumed the dynamical theory of heat to be probably the most important discovery of the present century, Mr. Dyer was led by the same facts and considerations, as well as by many others which he has himself adduced, to conclude that no such definitive discovery has been made in our times, and that the question as to the entity or nonentity of heat remains in the like obscurity in the present as it was in the beginning of the last century.

The author then quoted some passages from the lectures of Professor Tyndall, "on the forms and action of water," and on radiation through the atmosphere, wherein the Professor had assumed that great mechanical forces were exerted in the combination of the gases to form water, as also in the changes of water from its several conditions of solid, liquid, and vapour, and *vice versa*, giving the sum of such forces in equivalents of a ton weight falling from different heights; which phenomena Mr. Dyer considers as solely due to the chemical forces called into action by those changes in the forms and action of water; and as the latter class of forces consists of the affinities and repulsions of the particles of the water excited by the different degrees of heat combined therewith, the assumption of such mechanical forces being exerted in those mutations is untenable.

In giving the results of his interesting experiments of the absorption of heat by aqueous vapour in the air, and of its free radiation through dry air, the Professor says that "this newly discovered property of transparent aqueous vapour must exercise immense influence on the phenomena of meteorology;" but Mr. Dyer gave quotations from Dr. Dalton (in the Memoirs of this Society) and from Dr. Thomson's System of Chemistry, published some sixty years ago, wherein the absorption and radiation of heat by aqueous vapour in the formation and dissolution of clouds, dew, fogs, &c., are clearly explained upon the established doctrine of the mutation of heat reciprocally from and into latent and sensible states; hence the doubt as to any *new* discovery on these points.

To give the course of reasoning submitted by the author against the mechanical theory of heat, and to sustain that of a material calorific element, would require a more copious abstract of his paper than the limits of the printed Proceedings would allow; so that while calling in question both the novelty and the soundness of the discoveries on which he has commented, he refers to his paper *in extenso* for the explanation of his own views on the nature of a material calorific element, and of the elastic forces exerted by the mutations of such elements.

MICROSCOPICAL SECTION.

October 19, 1863.

Professor WILLIAMSON in the Chair.

A letter from Captain J. Mitchell, dated Madras, the 13th May, 1863, was read, of which the following is an extract:

The view that the universal form of cotton hairs is a flattened fibre consisting of scarcely anything but membrane, is by no means novel; for much the same description of the cotton fibre has been given by Quekett, Henfry, and many others. Indeed that opinion is general. Almost every one you ask will tell you that the cotton fibre is flat and twisted, or perhaps a flat spiral. That this is not the general form of the hairs of good cotton any microscopist may satisfy himself in five minutes. An examination of many varieties of cotton has led me to believe that in those descriptions which find most favour in the English market there is a very large proportion of hairs that are entirely or nearly filled with secondary deposits; and on the contrary, in the low priced cotton the flat fibre consists of hardly anything but membrane; in fact, an apparently undernourished cell predominates. The knotty portions, which I believe are considered refuse, consist almost entirely of these flat fibres. I believe that the absence of secondary deposit is an indication of careless culture, or, what is much the same thing, of a poor soil.

November 16th, 1863.

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

The President exhibited some spurious gold slides, on which he remarked that the first offered for sale were made by rolling gummed sand in gold leaf, some even more roughly by dabbing gold leaf on sand gummed to the slide; whereas the practice now appears to be to electro-plate plumbago, the result of which however differs from the nugget-like form of real gold by being nicely broken off at what would be the angles. Some very good imitations were made by boiling plumbago in oxalic acid and chloride of gold.

Mr. C. O'Neil stated that very beautiful spires of gold were observable on the surface of auriferous quartz boiled in carbonate of soda and then submitted to the action of heat for a short time under the muffle.

The President exhibited Phascum rectum and Hookeria lucens in fruit.

Ordinary Meeting, December 29th, 1863.

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

Mr. SIDEBOTHAM exhibited two photographs which he had taken from a book in the possession of Mr. Buxton, of Daresbury, entitled "Histoire de la Navigation aux Indes Orientales, par les Hollandois," Amsterdam, 1609. One of these represents the title page, which is interesting as showing, in the map of Africa which it contains, the course of the Nile and the two lakes from which it springs, one of them having two outlets. The other photograph shows, among figures of other productions of the Mauritius, that of the Dodo. It would appear that this is the earliest figure of this now extinct bird.

T. T. WILKINSON, F.R.A.S., &c., communicated the following note on the late meteor.

On December 5th, 1863, at about 7h. 55m. p.m., I observed a very brilliant meteor at Haslingden. It passed from the north towards the west, and was first visible to me when near to γ *Ursæ Majoris.* The nucleus was of an egg-like form, the head being intensely bright and considerably broader than the rest. The atmosphere was quite clear at the time, and hence I had a good view of its whole passage. Its color was pale blue, and the light it emitted was so intense that the public gas lamps were immediately put into shade and suddenly threw their shadows across the road. The tail of the meteor gradually tapered to a point, and appeared to give out sparks of a purplish tint. When about 4° beneath α *Lyræ,* the meteor exploded, and then the sparks were so variegated in color—the red and purple tints prevailing—that they gave me the idea of a rocket, and I for a moment thought that the Haslingden people were doing this in honor of the Marquis of Hartington's visit to their town. This illusion was, however, immediately dispelled. The whole time of passage did not occupy more than a few seconds. Although several hundreds of persons witnessed the phenomenon, I cannot find that any one heard any sound made by the meteor either during its passage or on its explosion.

EDWARD HULL, B.A., F.G.S., read a paper, entitled "Additional Observations on the Drift-Deposits and more recent Gravels around Manchester."

The object of this paper was to show that the drift or post-pliocene deposits of the borders of Lancashire, Cheshire, and Derbyshire, are divisible into three stages, viz.:—

1. Upper boulder clay.
2. Middle sand and gravel.
3. Lower boulder clay.

The 2 and 3 members had already been described by the president, Mr. E. W. Binney ("Mem. Lit. and Phil. Soc." vol. viii., 2nd series), as also a lower bed of sand and gravel, of whose existence as a distinct subdivision the author had considerable doubts, and considered it as merely accidental. The upper boulder clay had also been alluded to by Mr. Binney, but the author considered it to be quite as important as the lower, both in thickness and area.

The author had found that these subdivisions rose from the plain, or the Valley of the Mersey towards the hills both northward in the direction of the Bolton moorlands, and eastward towards the Derbyshire and Cheshire hills; and amongst these uplands only the upper boulder clay and the gravel made their appearance. These divisions of the drift had already been traced in an area of several hundred square miles.

The author also described a very wide-spread river terrace stretching along the Valleys of the Mersey and Irwell from Didsbury and Manchester westward to near Warrington, and stretching from Altrincham to Eccles. He considered this old terrace of gravel to have been formed at a time when the sea extended further up the valley than at present, and the rivers covered during floods a much wider area than at present.

Ordinary Meeting, January 12, 1864.

J. C. Dyer, Esq., Vice-President, in the Chair.

The President, in the name of a number of members, presented to the Society a Portrait of one of its Vice-Presidents, J. P. Joule, LL.D., F.R.S., by G. Patten, A.R.A., of London, and intended to be hung up on the walls of the meeting room.

On the motion of Professor Christie, seconded by Mr. Atkinson, it was resolved, "That the best thanks of the Society be given to Mr. Binney and the other donors of Dr. Joule's valuable portrait."

Mr. John Rogerson was elected an ordinary member of the Society.

On the motion of Mr. Dyer, seconded by Mr. Sidebotham, it was resolved, "That the Society do hereby authorise the Council to procure a die for a medal, and to settle the design."

The following extract from a letter from T. T. Wilkinson, F.R.A.S, was read:—"On referring to the article 'Dodo,' in the *English Cyclopædia*, I find that Mr. Sidebotham is mistaken in supposing that his photographs exhibit 'the *earliest* figure of this now extinct bird.' The frontispiece of De Bry's '*Quinta Pars Indiæ Orientalis*,' A.D. 1601, contains 'a pair of these birds on the cornice on each side' of the ornamental border. Clusius, in his '*Exotica*,' A.D. 1605, also gives a figure, which he says is copied from the Journal of a Dutch Voyager, who had seen the bird in a voyage to the Moluccas in A.D. 1598. Mr. Buxton's '*Histoire*,' appears to follow next in order; since the Journal of Admiral Peter Wilhelm Verhuffen, as quoted by the late Mr. Strickland, was printed at Frankfort, in A.D. 1613."

The PRESIDENT said he wished to make a few remarks on the Lancashire and Cheshire Drift. In the year 1841 he first attempted to class the drift deposits found in the neighbourhood of Manchester, in a small Paper with a map, which he prepared for the Statistical Society of Manchester. In that Memoir he divided the foreign drift in the ascending order: (1) lower sand and gravel; (2) till; (3) upper sand and gravel; and he described the more modern deposits found in valleys; (No. 4) as valley gravel. This order he adopted in a Paper read before the Manchester Geological Society on the 22nd December, 1842—"Notes on the Lancashire and Cheshire Drift,"—and printed by that Society in their proceedings of 1843. In that Paper, in treating of the upper beds of sand and gravel, he says—"At Manchester, it (the Higher Drift) is composed of lower gravel, till, and sand and gravel, while at Heywood and Poynton, near the base of the Pennine chain, the beds of sand and gravel are parted by several beds of loam and clay." Again, in speaking of No. 3 deposit, he says—"The gently rising lands of the two counties are generally composed of this deposit. It varies much both in its composition and thickness. Near the sea at Ormskirk, the till is sometimes found without it; but as you proceed to the East it makes its appearance, and gradually thickens until it attains its greatest thickness near the base of the Pennine chain: not only does it increase in thickness, but it becomes more complex, and contains beds of clay, marl, and loam of several yards in thickness. The country lying between Manchester, Bolton, Bury, Rochdale, Ashton, and Stockport, for the most part is upon it, and forms one great sand bank, which continues South into Cheshire." The same classification he adopted in two Papers, one on the Drift of Manchester, and the other on the same deposits at Blackpool, printed in Vols. VIII. and X. of the Society's Memoirs, as well as in a Paper printed in the Manchester Geological Society's Transactions for June, 1862.

Mr. Hull, in his communication, read at the last meeting of the Society, divided the higher drift deposits into (in descending order):—"(1) Upper Boulder Clay; (2) Middle Sand and Gravel; and (3) Lower Boulder Clay. The Nos. 2 and 3 had been described by the President as also a lower bed of sand and gravel, of whose existence he (Mr. Hull) had considerable doubts, and considered it as merely accidental." Now, in his (the President's) Paper on the Drift of Manchester, 11 sections of wells and bores are given, and in ten of those the lower sand and gravel had been met with; thus shewing that it can scarcely be considered as merely accidental, as Mr. Hull states. In many other sections since examined in Lancashire, this deposit has also been found under the till. With regard to the upper bed of boulder clay, Mr. Hull stated that he (the President) had alluded to it, but Mr. Hull considered it to be quite as important as the lower, both in thickness and area.

The old term "till" is as good as that of boulder clay, and as it has been long used there is not much use in changing it. During the last twenty years he had collected many facts, which he intended to publish when he had completed his collection, but these did not show one bed of clay or marl which could be called upper boulder clay, but several; in fact there were numerous intercalations of it in the sand and gravel, one of which he had seen occurring at Kersall Moor entirely surrounded by sand. To show the complexity of these deposits, and the difficulty of reducing them to two, he gave two sections, one near Hyde and the other at Outwood, where the following strata were met with:—

HYDE.	feet	in.
Clay	11	0
Quick sand	2	6
Strong marl	22	6
Quick sand	2	6
Loam with pebbles	12	6
Buck leaf marl	19	0
Dry sand	9	0
Quick sand and loam	6	0
Gravel	3	0
Loam	7	6
Gravel and sand	3	0
Clay and loam	15	6
Gravel and soft metal containing pebbles	10	0
Coal measures		
	124	0

OUTWOOD.	feet	in
Bog	11	0
Quick sand	53	3
Buck leaf marl	31	2
Red sand and gravel with a yard of clay in it	15	0
Toad-back marl	32	3
Gravel	3	0
Coal measures		
	145	8

From the position of the Outwood section in a slight depression, and the higher grounds adjoining being capped with a bed of clay containing pebbles eight or ten feet in thickness, another deposit of clay should be placed on its top. Thus in one case there are six beds of boulder clay, and in the other only three. These are two of the many instances which could be adduced, and suggest caution in attempting to classify these deposits without collecting and consulting numerous sections.

A Paper was read entitled " Enquiry into the question, Whether excess or deficiency of Temperature during part of the Year, is usually Compensated during the remainder of of the same year," by G. V. Vernon, F.R.A.S.. M.B.M.S.

In order to see what truth there might be as to the temperature of part of the year, if above or below the average, being compensated during the remainder of the year, the mean monthly temperatures for 92 years for Greenwich, have been made use of. From 1771 to 1849, the observations are those supplied by Mr. Glaisher, in the Philosophical Transactions for 1850. The mean monthly temperatures from 1850 to 1862, are those given in the Greenwich Observations.

The difference between the mean temperature of each month and the 92 years' average for the same month was found for every month during the year, and the figures were collected into two columns according as they were positive or negative. The sums of these two columns will be found in the table annexed to this Paper, and serve to show the number of degrees the mean temperature was above and below the average during the year. These figures have been laid down in a diagram annexed, and which show the irregular character, and at the same time proves most distinctly that if the mean temperature is above or below the average during part of the year, it by no means follows that the converse holds good during the remainder of the year.

During the 92 years, there were 23 in which the total excess or deficiency of the mean temperature amounted to over 20°. There were 28 in which it amounted to from 10° to 20°; 19 years in which it amounted to at least 5°, and 22 in which it was less than 5°.

During a very small number of years only is the mean temperature, viewed in this manner, at all in a proximate state of equilibrium during the year.

The variations below the average reached their maximum value during the period 1781 to 1791, every year but 1781 being below the average. From 1841 to 1851 there was only one year below the average; and from 1851 to 1861, only two years below the average.

Careful inspection of the annexed diagram at once shows, that these variations are exceedingly irregular, and show no approach to periodicity of any kind whatever.

Another Paper was also read by Mr. VERNON, entitled, "Examination as to the Truth of the Assertion that when November has a Mean Temperature above the average, it is usually followed by Excessive Cold between the December and March following."

In the table annexed to this Paper all the years since 1771, in which November had a mean temperature above the average, are tabulated, as well as the differences from the mean of the succeeding months of December, January, February, and March.

Following a warm November we find the following figures:

Months.	Number of Months above the Average.	Number of Months below the Average.
December	25	15
January	22	19
February	21	20
March	23	16
Sums	91	70

Or 91 months above the average against 70 months below the average.

In place of a warm November preceding excessive cold, we find that in most of the years in which severe frosts have occurred early in the year, the November previous has had a mean temperature below the average.

November, 1784, had a temperature 1·7° below the average, succeeded by December 7·8° below, January 0·4° above, February 7·8° below, and March 7·0° below the average.

The great frost which set in severely on January 6, 1814, was preceded by a November 2·2° below the average, and December 2·2° below the average temperature; January was 8·8° below, February 4·2°, and March 5·8° below the average temperature.

The cold period in January and February, 1838, was also preceded by a November 1·3° below the average, and

December 2·4° above; January, 1838, was 6·8° below, and February 5·3° below the average temperature.

Cold winters succeeding a warm November appear to be very few in number, and generally these winters are preceded by a November not much above the average, as in 1783, 1794, and 1799, in which years the mean temperature of November was only 0·5°, 0·9°, and 0.5° above the average respectively.

November, 1822, and 1846, were the only two Novembers much above the average which were followed by a cold period immediately afterwards.

A Paper was read entitled "Note on the amount of Carbonic Acid contained in the air of Manchester," by HENRY E. ROSCOE, B.A., Ph.D., F.R.S.

Determinations of the quantity of carbonic acid contained in the air of towns have been made by Dr. Angus Smith in Manchester, and by Lewry in Paris; but as the experiments hitherto made upon the subject are few in number, and have yielded somewhat remarkable results, it appeared of interest to carry out a series of determinations of atmospheric carbonic acid, made by unimpeachable methods, and extending for a considerable time, under wide variation of weather.

The analytical method employed was the excellent volumetric one proposed by Pettenkofer, and this was checked in several instances by simultaneous weight determinations made by absorption in caustic potash. The close agreement of the results in experiments Nos. 8 and 9; Nos. 15 and 16; Nos. 42 and 43; and Nos. 45 and 46, in the accompanying table, gives proof of the reliability of the methods. The experiments were made under my supervision by Mr. Arthur Mc. Dougall.

In the case of the weight analyses, a given volume (not less than 35 litres) of air was drawn in the first place over three weighed tubes containing sulphuric acid and pumice-stone, the weight of the third tube being shown to remain constant; the air then passed through a Liebig's Bulbs containing potash, over two tubes containing solid potash; and, lastly, through two tubes containing sulphuric acid and pumice-stone, the weight of the second of these remaining constant. The volumetric analyses were made in globes of 7-10 litres capacity, with standard solutions of Baryta-water and Oxalic acid, exactly according to the method described by Pettenkofer.

The accompanying Table gives the results of 54 separate determinations of the carbonic acid in the open air of Manchester and the neighbourhood, made during the autumn and winter months of 1862-63-64; days being especially chosen upon which the amount of carbonic acid might be considered likely to be the greatest.

The first and most important conclusion to which these experiments lead is that the amount of carbonic acid contained in Manchester town air differs but very slightly (if at all) from that contained in the air of the neighbouring country. Thus, from experiments Nos. 23 and 24, made at Stretford (four miles west of Manchester) with the wind blowing towards Manchester, the quantity of carbonic acid found on Feb. 3, 1863, was 3·85 volumes in 10,000 volumes of air, as mean of two experiments, whereas on the same day the quantity found in the centre of Manchester (Owens College) was found to be 3·90 vols. in 10,000 vols. of air, as a mean of two experiments. Nos. 31, 32, and 33, made at Stretford on Feb. 19th, 1863, a damp day, with wind blowing from Manchester, showed a mean of 2·77 vols. of carbonic acid, whilst at Manchester, on the same day, the volume of carbonic acid was found to be 2·8 in 10,000 vols. of air.

The maximum quantity of carbonic acid was found in Manchester air on January 7th, 1864, (on which day there was a dense fog) when the amount reached 5·6 vols. per 10,000 of air; the minimum quantity on February 19th, 1863, being 2.8 vols, per 10,000 of air. The mean of 46 determinations made in the centre of the town of Manchester, gives the volume of carbonic acid as 3·92 in 10,000, and that of eight experiments made outside the town gives the number 4·02 as the composition of the country air regarding carbonic acid. These numbers closely agree with a determination by weight which I made in London, on February 27th, 1857, from which the carbonic acid in London air was found to be 3·7 vols per. 10,000.

Experiments 47, 48, and 49 prove that continuous rain may lower the amount of atmospheric carbonic acid from 4·8 to 3·3 volumes per 10,000.

The above results show that the maximum quantity of carbonic acid contained in Manchester air, even in a dense fog, and when there is no wind, does not exceed 6 volumes per 10,000 of air; whilst the mean quantity, 3·9 volumes, closely agrees with that (4·0) generally assumed, from Saussure's early experiments, to represent the average composition of the atmosphere as regards carbonic acid.

CARBONIC ACID IN THE AIR OF MANCHESTER AND THE NEIGHBOURHO

No.	Place.	Date.	Wind, Weather, &c.	Barometer, in Millimetres.	Temp. C.°	Vol. of Carbonic acid in 10,000 vols. of air.	Descrip of Metho Employ
		1862.		MEAN.			
1	Manchester	Oct. 19	... ...	753·0	10·5	4·0	Pettenk
2	"	" 22	... ...	740·0	16·0	4·0	"
3	"	" 22	... .	741·0	15·8	4·0	"
4	"	Nov. 7	Heavy, very damp mist—calm	767·0	5·0	3·9	"
5	"	" 10	Snow, with rain—wind strong	740·7	6·0	3·6	"
6	"	" 11	Snow and rain—east wind	750·5	3·5	3·6	"
7	"	" 12	Exceedingly thick fog—no wind	760·0	2·0	4·5	"
8	"	" 13	Very clear—frosty and calm	758·5	4·0	3·7	"
9	"	" 13	" " "	"	"	3·5	By wei
10	"	" 18	Very thick fog—calm and cold	769·0	2·0	5·2	Pettenk
11	"	" 19	Slight rain—no wind	766·8	5·6	3·5	"
12	"	" 20	" "	766·0	5·0	3·7	"
13	"	" 25	Slight fog—calm	755·0	1·0	4·1	"
14	"	Dec. 3	Fine and clear	751·8	10·0	3·6	"
15	"	" 10	Clear—S.W. wind	754·5	8·0	3·6	"
16	"	" 10	" "	"	"	3·63	By wei
17	"	" 11	... .	754·0	8·0	3·60	Pettenk
		1863.					
18	"	Jan. 16	Damp—slight wind........................	754·0	8·0	3·6	"
19	"	" 26	Fine and dry—fresh wind	758·0	10·0	3·5	"
20	"	" 27	" "	765·0	7·0	3·3	"
21	Lytham ...	" 31	Clear, bright—wind N.W. from the sea.	745·5	5·5	4·3	"
22	"	" 31	" " " "	"	"	4·6	"
23	Stretford ...	Feb. 3	Clear—wind N.W. towards Manchester.	753·0	9·5	3·9	"
24	"	" 3	" " " "	"	"	3·8	"
25	Manchester	" 3	" " " "	"	"	3·9	"
26	"	" 3	" " " "	"	"	3·9	"
27	"	" 3	" " " "	"	"	3·7	"
28	Stretford ...	" 10	" " " "	762·4	11·0	5·0	"
29	"	" 10	" " " "	"	"	5·0	"
30	Manchester	" 10	" " " "	"	"	4·6	"
31	Stretford ...	" 19	Wind S.E. from Manchester—damp...	772·0	10·0	2·7	"
32	"	" 19	" " " "	"	"	2·85	"
33	Manchester	" 19	Damp—wind S.E...........................	"	"	2·8	"
34	"	" 20	" "	768·0	10·0	3·0	"
35	"	" 23	" "	767·0	9·0	2·9	"
36	"	" 25	" "	...	...	3·7	"
37	"	" 25	... ...	...	...	3·7	"
38	"	" 27	... ...	763·0	11·0	3·6	"
39	"	" 27	... ...	"	"	3·6	"
40	"	Mar. 2	... ...	753·0	11·0	3·6	"
41	"	" 3	Fine—wind due north	752·0	18·0	5·2	"
42	"	" 3	" " "	"	15·0	5·1	"
43	"	" 3	" " "	"	15·0	4·7	By wei
44	"	" 6	" " "	752·0	15·0	3·5	Pettenk
45	"	" 6	" " "	"	"	3·5	"
46	"	" 6	" " "	"	"	3·7	By weig
47	"	Dec. 28	Damp, foggy—a little snow	765·6	2·5	4·9	Pettenk
48	"	" 28	" " "	"	"	4·7	"
49	"	" 29	Raining all night previously, and during the experiment—wind W.	752·5	10·0	3·3	"
50	"	" 31	Fine—wind fresh, S.E.	755·0	4·0	2·56	
		1864					

Hence we may conclude that the combustion of coal and the respiration of animals exert no appreciable influence on the quantity of carbonic acid contained in the town air of Manchester collected in an open situation; gaseous diffusion and the great motions of the atmosphere serving completely to disperse the millions of tons of this gas which every year are evolved by the above-mentioned causes in this neighbourhood.

I may add the following determinations, made in the same way, of the carbonic acid contained in the air of closed inhabited spaces:—

1. Chemical Theatre, Owens College, during lecture.
   Temp. 9° C. Bar. 755 mm. CO_2 in 10,000 of air 9·5 vols.

2. Chemical Laboratory, Owens College.
   9 o'clock a.m. Temp. 17·5° Bar. 733·5. CO_2 = 8·3 vols. in 10,000.
   12 o'clock Temp. 19·5° Bar. 733·5. CO_2 = 9·0 vols. in 10,000.

3. Large bedroom, with invalid and attendant.
   Temp. 12°. CO_2 in 10,000 vols. of air = 7·4.

4. Parlour (capacity 3,000 cubic feet), four persons, three gaslights, good fire.
   Temp. 19°. CO_2 in 10,000 vols. of air = 13·2.

5. Ditto ditto.
   Temp, 18°. CO_2 in 10,000 vols. of air = 13·6.

6. Ditto ditto.
   Temp. 15°. CO_2 in 10,000 vols. of air = 14·5.

7. Crowded meeting-room of artisans in Penny's Mill, Gaythorn—Guardian Schools—1,000 persons.
   Temp. 18°. CO_2 in 10,000 vols. of air = 36·5.
   CO_2 in 10,000 vols. of air = 35·5.

MICROSCOPICAL SECTION.

December 21, 1863.

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

Various valuable donations were announced, among others a paper by Walter Crum, Esq., F.R.S, on Cotton Fibre, accompanied with mounted specimens, in illustration of it.

Mr. HEYS repeated his observations on the the cotton fibre, and said that there was little difference observable in cotton freshly gathered, from that as usually received in this country. His observations lead him to the conclusion that the structure of cotton fibre is as follows:—First, there is an external envelope, or tube, generally moniliform; inside, a spiral vessel which seems to prevent the collapse of the tube: inside the spiral, there is generally present another substance like a pith or core. Mr. Heys then described at length his observations on the cotton when under the influence of the solvent recommended by Mr. O'Neil, and the conclusions he drew from them. Mr. Heys's paper was illustrated by diagrams and mounted specimens.

Mr. HEYS then read a Paper on "Mounting Objects in Canada Balsam," and minutely explained the various details of the process: he strongly advocates the use of the Canada Balsam, dissolved in Chloroform, by which the trouble is lessened, and the beauty of the preparations increased.

The SECRETARY then exhibited a Drawing of the Apparatus used by Captain Baker, of the Niphon, for obtaining Soundings, free from Grease.—Description: A tube 18 inches long, 1¼ diameter, with a wooden cap, and a leather, fitted as a pump-box; this, lashed to the lead, sinks into the earth, and brings up a cylinder of mud or sand, free from grease; the surplus water being forced out through the valve at the top.

PHYSICAL AND MATHEMATICAL SECTION.

January 7, 1864.

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

Two Papers were read by Mr. Vernon. (These were afterwards read at the ordinary meeting of the Society, January 12. See page 216.)

The following returns of the Rain Fall for 1863, were communicated to the Section.

Old Trafford, Manchester.

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

Rain Gauge three feet above the ground, receiving surface 106 feet above the mean level of the sea.

Quarterly periods. 1862. Days.	Quarterly periods. 1863. Days.	1863.	Fall in inches.	Average of 70 Yrs.	Differ.	Number of Days on which Rain fell in 1863.	Quarterly Periods. 1863.	Quarterly Periods. 1862.
				Inches.	Inches.		Inches.	Inches.
45	45	January...	4·425	2·471	+ 1·954	17	6·170	6·523
		February .	0·941	2·356	— 1·415	16		
		March ...	0·804	2·314	— 1·510	12		
61	53	April......	1·391	2·044	— 0·653	17	7·746	10·259
		May	1·724	2·345	— 0·621	14		
		June	4·631	2·913	+ 1·718	22		
51	56	July	1·630	3·619	— 1·989	7	12·216	11·875
		August ...	5·027	3·610	+ 1·417	25		
		September	5·559	3·225	+ 2·334	24		
61	61	October...	6·242	3·845	+ 2·397	22	12·208	9·941
		November	2·902	3·477	— 0·575	18		
		December.	3·064	3·279	— 0·215	21		
218	215	Total......	38·340	35·498	+ 2·842	215	38·340	38.598

The rain fall during the past year, as will be seen above, has been 2·842 inches above the average of the last 70 years, and almost identical with the rain fall of 1862, the difference being only 0·258 inch. In 1862 rain fell on three more days, or on 218.

During the four months of February, March, April, and May, 1863, the fall of rain was only 4·860 inehes, or 4·231 inehes below the average of the same months for 69 previous years; in 1806, the fall for these four months was 4·278 inches, and this is the only ease from 1794, in which the fall was as small for the period in question as in 1863. As eompared with 1862, 1863 had a remarkably dry spring, but an exeess of rain fell during the last six months of the year. Upon eomparing the last three months of the year, in the table above, it will be seen that rain fell upon the same number of days in 1863 as in 1862 for these months, but 2·267 inehes more fell in 1863 than in 1862. The faet just noticed shows that the number of days upon whieh rain falls may be quite independent of the amount of the fall, and how desirable it is to have the number of days upon which the rain actually falls, as well as the amount.

THELWALL, near WARRINGTON.

BY JOHN ATKINSON, F.G.S.

The height of the station above the mean sea-level is 96 feet. The gauge is about one foot above the ground.

	1863.	Inches.	Days on which rain fell.
5·529	January	3·715	14
	February	0·811	13
	March	1·003	13
9·307	April	1·389	13
	May	2·276	11
	June	5·642	22
11·000	July	1·977	8
	August	3·897	25
	September	5·126	21
12·624	October	6·801	21
	November	3·488	12
	December	2·335	17
		38·460	190

In 1862 the rain fall at Thelwall amounted to 35·840 inches, distributed over 219 days. Of this quantity, 6·821 inches fell in the first three months; 9·139 inches in the second; 10·328 inches in the third; and 9·552 inches in the last three months of the year. The fall in 1863 was in excess of that of 1862 by 2·62 inches. In each of the months—January, February, April, June, August, September, October, and November, in 1863, the rain fall was in excess of that registered in the corresponding months of 1862. It will be seen that more than half the qnantity of rain in 1863 (viz. 19·312 inches) fell in the four months August, September, October, and November, whereas these four months gave about one-third only of the total amount in 1862. Excessive amounts of rain fell on the following days in 1863, causing floods of greater or less extent:—January 1 and 2, 1·184 inches; June 7, 8, 9, 1·25 inches, and on the 10th and 11th, 2·642 inches (a very great flood); July 21 and 22, 1·5 inches; and October 29 and 30, 1·35 inches.

The Flosh, Cleator, near Whitehaven.

By Thomas Ainsworth, Esq.

1863.	Inches.	Number of Days of Rain.
January	6·34	28
February	2·59	20
March	1·90	18
April	2·91	21
May	4·05	16
June	3·98	20
July	1·25	7
August	4·08	22
September	6·33	26
October	6·51	28
November	6.97	26
December	3·64	23
	50·55	255

Eccles, during 1863, and a Comparison with 1862.

By Thomas Mackereth, M.B.M.S.

1862.	1863.		Amount in Inches.	Days it Fell.
Inches.	Inches.			
		January	3·959	19
8·005	5·794	February	0·872	16
		March	0·963	14
		April	1·254	16
9·533	7·408	May	1·792	13
		June	4·362	22
		July	1·690	7
10·986	11·702	August	4·736	25
		September	5·276	25
		October	5·393	23
9·140	11·312	November	3·164	17
		December	2·755	24
37·664	36·216	Total	36·216	221

Royton, near Oldham.

By John Heap, Esq.

1863.	Inches.	Number of Days of Rain.
January	6·412	26
February	1·803	20
March	1·223	17
April	1·599	19
May	2·325	12
June	3·785	19
July	1·700	8
August	5·230	24
September	6·625	26
October	6·205	22
November	3·415	19
December	3·775	21
	44·097	233

Mean fall of 27 years, 33·151 inches.

Ordinary Meeting, January 26th, 1864.

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

The Chairman showed a copy of the "Mundus Subterraneus" of Athanasius Kircher, edition of 1678, Amsterdam, written in 1662. In the first volume are five maps, on which the Nile with some of the adjacent country is delineated. The earliest is taken from an Arabian work on geography. In this the Mountains of the Moon are depicted about twenty degrees south of the equator, and running east to west: from the eastern half run five rivers which meet in a small lake; out of this small lake run three rivers which run into a large lake, the north of which is south of the equator. The same number, *i.e.* five rivers, run from the west, and passing through a small lake, run in three divisions to the same great lake, which is called Zambie. Out of this are made to flow three rivers, one the Nile, another the Zaire.

A map by Odoard Lopez makes the centre of Africa a hilly country well supplied with lakes and rivers; all these lakes are united, and send their waters in every direction. This makes the Zaire, the Zambesi, and the Nile unite, by uniting the lakes from which they spring. One of these lakes is called the Zaire. It would seem as if there were some knowledge of the existence of lakes at the time, whilst a bold guess completed the map.

A third map is ushered in with great ceremony. Kircher obtained it from the procurator of India at Rome. It is drawn by Peter Pais, a jesuit, who visited the fountains of the Nile on the 21st of April, 1618, along with the Emperor of Ethiopia. In this the Mountains of the Moon are also prominent. On a hill north of them are two fountains, "oculi Nili" or "fontes Nili." These form the Nile, which falls into the lake Bed, on the shores of Goyam, Bed, and Dambia. This seems as clear an account of the Blue Nile as one might expect from a traveller of the period. The Jesuits were, at the time mentioned, in Abyssinia, and Pais, whose name is written also Paez and Payz by other writers, is said to have gone there in 1603.

Kircher desires to inform posterity of this great discovery, which to his mind puts aside the preceding maps. We see in these three maps the Blue Nile actually gained, and the mind wandering after the other sources with much uncertainty, but apparently not without some such foundations for truth as could be obtained from the wandering reports of men ignorant of latitudes and equators, of any mode of measuring distance, and of any necessity to be very particular in describing that which so little concerned them. In order to make the fountains of the Blue Nile serve for the whole Nile, the lake Bed, on a fourth map, is put very far to the south of Congo, from which place it flows into the Zambic. This seems to be one of Kircher's attempts at ingenuity, which are certainly very often extremely bold. On this map is drawn a lake near the situation of the Nyassi, with an outlet to the east.

On another map, which however is rougher, and intended to show the general form of the world, the Nile is brought from about 15 degrees south of the equator to a large lake-like widening beginning about 10 degrees north. A branch takes somewhat the direction of the Blue Nile.

In all these the existence of lakes may be considered by some as the result of the guidance of tradition or report.

The exultation of Pais when he discovers the fountains is not without interest. Although he gives but few of the proper class of details, he does his best, and such as a man with his class of knowledge might be expected to give. He says—"I saw with the greatest pleasure that which was denied to Cyrus king of Persia, and Cambyses, Alexander the Great, and the famous Julius Cæsar himself."

MICROSCOPICAL SECTION.

Ordinary Meeting, January 18th, 1864.

Professor WILLIAMSON, F.R.S., Vice-President of the Section, in the Chair.

Mr. GRINDON was elected an ordinary member of the Section.

The adjourned debate on Mr. Heys's Paper on the structure of the cotton fibre then commenced.

Professor WILLIAMSON did not agree with Mr. Heys as to the moniliform character of the fibre. He conceived it to be, like other cell structures, merely an elongated cell with its usual wall and contents. The moniliform appearance observed by Mr. Heys was owing to the hard starch or gum granules in the spiral contents of the ripe cell, preventing the collapse of the tube in those places where they existed on the drying of the cell, thus giving the appearance alluded to. He thought the only way to arrive at a correct conclusion was to examine the fibre in a growing state in all stages from its earliest appearance in the flower bud until its maturity. The *twist* of the fibre so often alluded to he considered to be owing entirely to the drying and consequent curling up of the cells, as in other vegetable structures. Professor Williamson strongly urged those members who had convenience, to grow a number of cotton plants and carefully examine the fibre in all its stages. Surely Manchester ought not to allow the question as to the structure of the cotton fibre to be discussed and settled elsewhere.

The President of the Section, Mr. Sidebotham, then read a paper on mounting objects for the microscope in fluid.

The paper referred chiefly to the mounting of Desmidiaceæ and other freshwater algæ. Mr. Sidebotham recommended a cell of japan black, the covering glass fastened on with gold size, then a coating of lac varnish, and afterwards several coatings of japan black, applied each when the previous coating was dry.

Mr. Sidebotham exhibited Desmidiaceæ, &c., mounted by himself in the years 1842 to 1847, in various fluids, and concluded that for this class of objects water was the best medium, the specimens mounted in 1842 being as perfect in structure and colour as when first prepared. Many specimens had become spoiled owing to the air penetrating into the cells, from the cracking of the varnish, but with care and a coating of lac varnish before the japan black he thought this would in future be of rare occurrence.

Mr. Sidebotham then explained a plan of mounting diatomaceæ in balsam, so as to preserve their forms and colours, and also keep the frustules attached to each other.

Professor Williamson corroborated Mr. Sidebotham's experience as to water being the best medium for the Desmidiaceæ; those specimens mounted by himself more than twenty years ago, where the fluid had been preserved, were as perfect as when first prepared.

Mr. Dancer mentioned that he had specimens of *Volvox globator* mounted in water in the year 1843 by himself, quite perfect in form and colour; he preferred gold size as a material for cells to japan black.

Mr. Nevill exhibited the larva of a gnat mounted in water four years ago, showing that this mode of preparation is applicable to animal as well as vegetable structures.

Ordinary Meeting, February 9th, 1864.

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

George Harris, Esq., Barrister-at-Law, was elected an Ordinary Member.

Professor Roscoe exhibited the light emitted by burning a portion of a fine specimen of pure magnesium wire 1 mm. in diameter and 10 feet long, which had been manufactured by Mr. Sondstadt. Professor Roscoe remarked that it afforded him great pleasure to be able to state that a suggestion made by Professor Bunsen and himself in their photochemical researches, and printed in the Philosophical Transactions for 1859, page 922, was about to be practically adopted. Mr. Sondstadt is now commencing to manufacture the metal magnesium on the large scale, and the first important application of the metal is the employment of burning magnesium wire as an illuminating agent, especially for photographic purposes. In the researches above mentioned Professor Bunsen and the speaker had examined the photochemical action of the sun compared with that of a terrestrial source of light, and for the purpose of this comparison they chose the light evolved by the combustion of magnesium wire. They showed that a burning surface of magnesium wire which, seen from a point at the sea's level, has an apparent magnitude equal to that of the sun, effects on that point the same chemical action as the sun would do when shining from a cloudless sky at a height of 9° 53′ above the horizon. On comparing the *chemical* with the *visible* brightness of these two sources of light it was found that the brightness of the sun's disc as measured by the eye when the sun's zenith distance was 67° 22′, is 524·7 times as great as that of the burning magnesium wire, whilst, at the same zenith-distance, the *chemical* brightness of the sun is only 36·6 times as great. Hence the value of this light as a source of the chemically active rays for photographic purposes becomes at once apparent. The extract from the memoir referred to is as follows:—

"The steady and equable light evolved by magnesium wire burning in the air, and the immense chemical action thus produced, render this source of light valuable as a simple means of obtaining a given amount of illumination expressed in terms of our measurement of light. . . . . The combustion of magnesium constitutes so definite and simple a source of light for the purpose of photochemical measurement, that the wide distribution of this metal becomes desirable. The application of this metal as a source of light may even become of technical importance. A burning magnesium wire of the thickness 0·297 millimetre evolves, according to a measurement we have made, as much light as 74 stearine candles of which 5 go to the pound. If this light lasted one minute, 0·987 metre of wire, weighing 0·1204 grm., would be burnt. In order to produce a light equal to 74 candles burning for 10 hours, whereby about 20lbs. of stearine is consumed, 72·2 grms. of magnesium would be required. The magnesium wire can be easily prepared by forcing out the metal from a heated steel press having a fine opening at bottom; this wire might be rolled up in coils on a spindle, which could be made to revolve by clockwork, and thus the end of the wire, guided by passing through a groove or between rollers, could be continually pushed forward into a gas or spirit-lamp flame in which it would burn."

Professor Roscoe stated that great credit was due to Mr. Sondstadt for the able manner in which he had brought the difficult subject of the metallurgy of magnesium into the present satisfactory position, and expressed his opinion that, even for photographic purposes, the application of the metal will prove most important. Mr. Brothers, Mr. Parry, and other photographers present, corroborated Dr. Roscoe's opinion respecting the value of such a source of light for photography. Since the meeting Mr. Brothers made an experiment upon the magnesium light, which he reports as follows:—

"The result of an experiment I have just tried is that in 50 seconds with the magnesium light I have obtained a good negative copy of an engraving—the copy being made in a darkened room. Another copy was made in the usual way in daylight, and in 50 seconds the result was about equal to the negative taken by the artificial light. The sun was shining, but there was a good deal of fog in the atmosphere."

A paper was read, entitled, "On the Tensile Strength of Cotton, as effected by various Chemical Treatments." By Mr. Charles O'Neill, F.C.S.

The author has given a great number of experiments upon the subject, made in order to elucidate some important practical and scientific points not hitherto much worked upon. By means of his apparatus for testing tensile strengths (Proceedings of this Society, No. 6, 1863-64, p. 186) he was enabled to obtain data which by any previously known method could only have been obtained, if at all, by an incredible amount of labour.

Effect of bleaching upon strength of cotton. These experiments were made upon printing cloth of 18 threads to the quarter inch, American cotton, and bleached for printing by the low pressure process. The process included, among other treatments,—

(1) Passing three times over a red hot copper plate.

(2) Boiling 16 hours with milk of lime.

(3) Boiling 16 hours with soda and rosin.

(4) Steeping in solution of bleaching powder for several hours.

(5) Steeping in dilute hydrochloric acid for several hours.

	WARP.	WEFT.
Strength of threads in the grey cloth S.V. (mean of 30 experiments) ...	3140 grs.	1714 grs.
Ditto in the bleached cloth S.V. (mean of 30 experiments)...........	2920 grs.	2785 grs.

The warp threads from two other pieces of cloth give as follow, being the means of 40 experiments :—

	A.	B.
In the grey state	3407 grs.	3512 grs.
In the bleached state	3708 grs.	4025 grs.

The cloth S.V. became elongated in the bleaching process, and contracted in width. The contraction in width, owing to the fulling up of the weft, will explain the increase of its strength, while the elongation may explain the diminution of strength in the warp. The cloth A., and also B., were chemically treated exactly as S.V., but were washed and dried loose, and not being fulled so much by the mechanical processes did not sensibly alter in length. The increased strength may be explained by the complete bedding of the cotton hairs, so forming a more compact thread. At any rate, it seems proved that the strength of cotton is not injured by the ordinary process of bleaching for printing.

Effect of printing, dyeing, soaping, &c. A portion of S.V. was printed, dyed, and finished as a first-class madder purple, and 20 experiments made upon the warp and weft threads, with the following results:—

Strength of printed and finished warp 3569

Ditto of printed and finished weft 2669

Here it is seen the warp threads have gained more than they lost in bleaching, while the weft threads have lost something. The increase of strength in the warp threads is partly, if not wholly explained by the diminution of length, the two yards gained in bleaching, and rather more, being absorbed in the processes of dyeing, soaping, &c., and the thread consequently becomes thicker and stronger.

Mordanted cloth treated with acid. These experiments are interesting, as touching upon the chemical *versus* the physical theory of dyeing. A piece of calico was chosen printed by blocks in wide longitudinal stripes, with the usual mordants for madder or garancine dyeing; it was aged, cleared, &c., as usual, to remove all loose mordant. Portions containing mordanted and unmordanted parts were treated by hydrochloric acid to remove the mordants, then carefully washed, and the strength of the threads tested. The results are as follow:—

Iron mordant for black, warp threads, 10 experiments.. 3450 grs.

Warp threads contiguous to the mordanted threads, 10 experiments........................ 3715 grs.

Same mordant, weft threads, 6 experiments... 2201 grs.

Same weft threads in the unmordanted parts, 6 experiments.................................. 2906 grs.

The above experiments show a decided diminution of strength in those threads which had received the mordant. The unmordanted threads had, of course, been submitted to the same acid and other treatments.

A repetition of these experiments upon another piece of cloth of different origin but similarly printed, and which had been dyed in madder and then treated with hydrochloric acid, gave the following results:—

Alumina mordant for red, warp threads, 10 experiments.................................. 1906 grs.

Unmordanted threads, contiguous, 10 experiments .... 2031 grs.

showing a diminution of strength.

Iron mordant for black, warp threads, 10 experiments.................................. 1631 grs.

Unmordanted threads, contiguous, 10 experiments.. 2260 grs.

showing a considerable diminution of strength.

Experiments on cotton hairs, taken from mordanted and unmordanted threads, which had been treated by acid.

Alumina mordant for red, 8 experiments ...... 60·3 grs.

Not mordanted adjacent hairs, 5 experiments. 71·6 grs.

The uncertainty as to whether the mordant had actually touched the hairs in the centre of the threads caused these experiments to be discontinued and the following instituted.

Cotton mordanted in the wool. Small parcels of New Orleans cotton were steeped separately in ordinary iron liquor (crude pyrolignate of iron) and red liquor (crude acetate of alumina), saturated with these liquors, the excess expressed out, and then dried gently, and aged for 24 hours; afterwards well washed in lukewarm water and dried. A portion of each parcel was then treated with dilute hydrochloric acid, some of the original stock of New Orleans cotton being placed in the same acid, and going through the same treatments. The mordants being dissolved out, the cotton was well washed to free it from the acid, dried, and its strength ascertained with the following results:—

New Orleans Cotton not mordanted, 10 experiments	143·9 grs.
Ditto mordanted with iron, 10 experiments	96·8 grs.
Ditto mordanted with alumina, 10 experiments	94·1 grs.

A very notable diminution of strength has occurred in the mordanted parcels.

Gun cotton. The only undeniable chemical compound which cotton forms with other elements, without undergoing conspicuous physical change, is in gun cotton. A sample of New Orleans was treated with equal volumes of concentrated sulphuric and nitric acids; it increased in weight 66 per cent, burned well, and was soluble in alcoholised ether. Twenty hairs being measured gave a mean length of 0·997 inch; twenty hairs before treating gave a mean length of 0·996 inch. The difference 0·001 being within the range of error, it may be said not to have changed in length. The strength of 10 hairs was taken, and gave a mean of 85·4 grs. The mean strength of 19 hairs before treatment being 138·1, there has been a considerable diminution of strength. This may not be true of all kinds of gun cotton.

Mercerised Cotton. It is very well known that cotton cloth treated with concentrated solution of caustic soda shrinks in length and becomes stronger. The following experiments corroborate this. New Orleans cotton was treated with soda solution sp. gr. 1·250. Twenty hairs being measured gave a mean length of 0·857 inch, being a contraction of 0·139 inch on the mean of twenty hairs measured before treatment. The mean strength of 10 hairs is 154·1 grains against a mean strength of 138·1 grains before treatment.

The reliability of these results is discussed by the author. The experiments in detail show wide discrepancies, and the maximum and minimum of a series are often at a considerable distance from the mean. This is a difficulty inherent to the subject, and can only be overcome or lessened by multiplying the experiments. Ten experiments seem to give a reliable mean, for in making twenty or thirty, the first, second, and third tens give nearly the same mean. Most of the results are means of twenty experiments; and several of them having been repeated at long intervals and by different hands, without any important difference in the means, the author is of opinion that they express the truth, at the same time that they may be open to some numerical rectification.

Ordinary Meeting, February 23rd, 1864.

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

Walter Crum, F.R.S., was elected an Honorary Member of the Society.

The formation of a Photographic Section was announced. On the motion of Mr. Sidebotham, seconded by Mr. Parry, it was unanimously resolved that the Sections of the Society, with the consent of the Council, shall have power to elect sectional associates, subject to rules which have been drawn up by the Council. The arrangement to take place immediately, and to continue in force until the end of the ensuing Session.

Professor Roscoe stated that the question of the possibility of taking photographic portraits by means of the magnesium light was now satisfactorily settled; he exhibited some prints of a portrait which Mr. Brothers and he had taken at five o'clock p.m. on Monday the 22nd, by burning fifteen grains of magnesium in the form of fine wire, at a distance of about eight feet from the sitter. The negative thus produced was stated by Mr. Brothers to be fully equal to any obtained by sunlight in the most favourable state of the atmosphere, the distribution of light and shade was most agreeable, any harshness of the shadows being completely removed simply by slightly moving the wire whilst it is burning. During the meeting, Mr. Brothers took an excellent negative copy of Chantrey's fine bust of the late Dr. W. Henry, in the possession of the society, by burning ten grains of magnesium wire, the light lasting for 59 seconds. It is expected that the quantity of wire necessary for taking a photographic portrait can be sold at the cost of a very few pence.

Mr. G. C. Lowe described a meteor seen by him on Sunday, Feb. 7th, at 6h. 11m. p.m. Greenwich mean time. It was first seen just below the constellation Cassiopeia, and was in full view throughout its entire passage, which occupied about four seconds. It descended towards the horizon at an angle of about 40°, passing between the two stars η Pegasi and Scheat, and disappeared without breaking up when at the altitude of α Pegasi. It was of an elongated pear shape, and was followed by a short train of sparks of a dull red colour, the meteor itself being a very pale blue, somewhat less than a diameter of the moon in apparent length, and about six or eight times brighter than the planet Jupiter.

This meteor was also seen by Mr. Wylde, Mr. Parry, and Mr. Pochin, who all described it as one of great brilliancy.

Mr. Sidebotham exhibited a copy by photo-lithography of one of the earliest editions of Shakespere, which he believed to be the first published work in which that process was employed.

Mr. Brockbank exhibited a bead-like fossil body which had been found in a sandstone near Stainmoor, and had been traced to full four feet in length.

The President said that a similar fossil had been described by Mr. George Tate, F.G.S., of Alnwick, in the Transactions of the Berwickshire Naturalists' Club for 1858, under the name *Eione Moniliformis*, which was found in the sandstones of the mountain limestone of Howick, Seremerston, and Haltwhistle, in Northumberland and in Yorkshire. Mr. Hindson of Kirby Lonsdale, has also found the same fossil in sandstones of a similar age in that district, and probably they would be met with in the beds of the millstone grit further south, at Dyke Nook, near Keighley, and Saltersbrook, near Woodhead, where he (the president) had observed specimens of several species of M. Loy's genus of *Crassopodia* with which they have been found associated.

Dr. Roscoe read a Paper by Mr. Edward Sondstadt, entitled, "Note on the Preparation of Calcium."

Although Davy demonstrated the existence of Calcium, and obtained it in an impure condition, by his well known method more than half a century ago, there are but two methods at present known whereby this most abundant of all the metals, excepting perhaps aluminium, can be obtained in a state of comparative purity. Matthiessen, following a method which was, I think, first indicated by Bunsen, who obtained magnesium by the electrolysis of the fused chloride of magnesium, obtained calcium by the electrolysis of a mixture of the fused chlorides of calcium and of strontium. This method, however, of obtaining the metals of lime and of magnesia is exceedingly troublesome, principally because of the floating up and burning of the metal on the surface of the salt electrolysed. The second of the two methods referred to is that adopted by Liès Bodart, and Gobin, who obtained calcium by heating iodide of calcium with sodium in an iron crucible, the cover of which was securely fastened down. The only objection to be made to this process is the troublesomeness and expense of preparing anhydrous iodide of calcium, which, like chloride of magnesium, is apt to undergo partial decomposition during ignition—and it must be remembered that partial decomposition of iodide of calcium involves the formation of lime, a substance practically infusible, which, during the reaction with sodium, must prevent, if present, the aggregation of the minute particles of reduced calcium into globules. In order to overcome this, and the before-named objection to the process, the author fuses together equivalent quantities of iodide of potassium and of chloride of calcium. The fused mass is poured into an iron crucible and covered till cool enough to handle; the mass is then dropped out, and a rather less than an equivalent quantity of sodium is put into the crucible, and the mixture of calcium and potassium salts is placed above it. The crucible is then closely covered, and heated to redness. The heat need neither be strong nor long continued. The best results are obtained when the crucible cover is fastened down, but calcium in lump may be obtained without using more pressure than that afforded by a well fitting lid. The reaction does not appear to be violent, and hence the advantage of considerable pressure. Is the slight violence of the reaction between sodium and the calcium salt owing to the near approach of the two metals as to atomic weight, and thence specific heat? It is easier to obtain calcium in lump by the modification of Liès Bodart's process, that I have described, in a small way, than it is to obtain magnesium in lump on a like scale.

A paper was read by J. C. Dyer, V.P., "On the Nature of Friction in Mechanics," in which he stated that:—

Resistance to motion by surface contact arises from distinct kinds of obstruction, according to the nature of the moving bodies and to the condition of their surfaces. These present problems that have been investigated by many writers and experimenters; but no fixed law or rule has been discovered for measuring such resistances as applied to different kinds of friction. The inquiries have mostly related to the action of solid surfaces, and some approach to the measure of their resistances has been attained, by finding the relation of falling weights to those sliding over horizontal surfaces of different kinds of woods, metals, glass, marbles, &c.; but little or nothing has been accomplished towards showing the nature of the surface resistance to solids moving through water or other liquids. The experiments of Captain Beaufort and Robert Fulton afford data for the sum of such resistance, but not for its mode of action.

In the case of solids, if the surfaces in contact were perfectly level and hard, they would slide over each other without friction; but as none such are known, their resistance to motion arises from the projecting points and indentations, which grapple with more or less force, causing the moving body to rise and fall and to abrade and wear down the surfaces; thus gravity and cohesion constitute the counteracting or compound force called *friction.* In railway wheels, as small portions of the peripheries are in contact with the rails, when both are in good order but slight resistance is offered; but even in this case the common measure of the weight into the distance does not strictly apply, and the amount of tractive power is obtained from experience only. This retarding force too is dividedbetween the friction on the rails and that of the air passed through. The action of rolling surfaces such as wheels and pinions of watches, clocks, and other machinery, is amply explained, and rules given for minimum friction, in the works of Berthoud, Camming, and Halton, on clocks and watches, by Camus on the teeth of wheels, and in D Fairbair n's work on mill-gearing, wherein we see that if the rolling surfaces were all smooth, hard, and of curvatures adapted to such motion, they would offer no friction.

The motions of shafts on fixed bearings, if the metal surfaces come in contact, will draw in and compress the air, and by heating the shaft prevent safe working; hence lubricating matter is used to limit the contact of the metals, so that only some of their prominent parts touch, to produce attrition, or friction.

The motion of bodies through water involves several kinds of resistance of a more complex nature, the measure of which has not been given by any of the able writers on that subject, nor has any formula been applied thereto for solving the questions of the compound resistances to be overcome by ships and other bodies moving through water. Some authors have taken the measure of this resistance to be as the squares of the velocities; but this fails because the different kinds of resistance are not alike called into action in the different cases adduced. Those of cannon balls ricocheting from water, the sinking of plummets in deep soundings, and floating bodies at different depths from the surface, present each of them reacting forces too complex to admit of any simple rule of measurement, the separate nature of which need not be here repeated; but it may be said that the aggregate of the resisting forces called friction must of necessity consist of the reaction of gravity, inertia, cohesion, elasticity, and adhesion of bodies, in their varied degrees of action, so that no simple measure of them as *friction* is likely to be discovered, and we thus see that friction is a compound term, pointing to the action of several retarding forces, and to comprehend the nature of these, we must analyse these natural forces as they are respectively called into action under the common but vague term of friction.

MICROSCOPICAL SECTION.

Ordinary Meeting, Feb. 16, 1864.

JOSEPH SIDEBOTHAM, ESQ., President of the Section, in the chair.

Mr. BROTHERS presented some very beautiful photographs from microscopic objects to the Section.

Mr. LYNDE mentioned that one of the members, Mr. Grindon, had promised to procure a supply of fresh cotton plants, in all stages of growth, beginning in June, and suggested that, as the time for their examination would be during the recess, a Committee should be appointed to examine the subject of the cotton fibre very carefully, and report thereon at the first meeting in the next Winter Session.

Mr. Sidebotham suggested that in using very high powers, such as the 1-25th, if slips of mica were used for mounting the objects, together with mica covers, there would be no risk of damage to these lenses, which could thus be used in many examinations where the fear of damage now prevents their employment.

Mr. Dancer exhibited a new and improved Oxycalcium Microscope; the lenses were adapted to the ordinary form of lantern, and consisted of new combinations of lenses, giving a large flat field, and plenty of light, along with very fine definition.—The room was darkened, and Mr. Dancer exhibited an extensive range of objects with several powers, on a large stretched paper screen, 8ft. by 5ft., showing the application of this instrument to the uses of lectures on various subjects. Besides the usual objects, such as sections of wood, objects mounted in fluid, living animalculæ, the process of crystalisation, electrical action, &c., were exhibited.

A new combination forming a one-inch object glass for the achromatic microscope was also exhibited by Mr. Dancer, and was much admired.

Ordinary Meeting, March 8th, 1864.

J. C. Dyer, Esq., Vice President, in the chair.

Horatio Micholls, Esq. and Thos. Windsor, M.R.C.S., were elected ordinary members of the society.

Mr. Brothers presented to the members several prints from a negative photograph of Slater's fine bust of the late Professor Hodgkinson, taken by the magnesium light.

The following communication from Dr. Fairbairn was read:—"I beg to forward, for the information of the society, some singularly corroded plates, taken from two boilers that exploded and killed thirteen persons a few days since, at Aberdare, South Wales. The extraordinary effects are supposed to arise from the water, into which some mineral springs from the mines have been flowing for some time past. Mr. Fletcher our engineer, has submitted this water for analysis to Dr. Angus Smith; to whom I beg to refer. I doubt not the specimens will be interesting to the society."

The plates appeared honeycombed on the interior surface to the depth of half the thickness of the plate, and the corrosion evidently proceeded from chemical action.

Mr. Fletcher stated that the corrosion was very irregular; in some portions, especially close to a seam of rivets, the plate being completely eaten away; the part of the boiler situated immediately over the furnace was that upon which the corroding action was most noticed.

Mr. SPENCE drew the attention of the society to the great importance of the question of the action of acid water, especially in this district, upon boilers; he employs the water from the Rochdale Canal in his boilers, and unless he were regularly to use soda to neutralise the acid, his boilers would undergo constant corrosion. By the addition of one pound and a half of soda per diem to each boiler, the plates were found to remain perfectly uninjured.

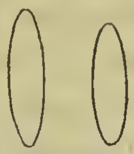
Mr. NASMYTH communicated the following letter he had received from E. J. STONE, M.A., First Assistant at the Royal Observatory, Greenwich :—

"Royal Observatory, Greenwich,
"London, Feb. 25, 1864.

"Dear Sir,

"The Astronomer Royal has placed your letter of the 20th February in my hands.

"Your discovery of the 'willow leaves' on the solar photosphere having been brought forward at one of the late meetings of the Royal Astronomical Society, my attention was attracted to the subject. At my request the Astronomer Royal ordered of Mr. J. Simms a reflecting eyepiece for our great equatorial. The eyepiece was completed about the end of January last, and at the first good opportunity I turned the telescope on the sun. I may state that my impression was, and it appears to have been the impression of several of the assistants here, that the willow leaves stood out dark against the luminous photosphere. On looking at the sun I was at once struck with the apparent resolvability of its mottled appearance. The whole disc, so far as I examined, appeared to be covered over with relatively bright rice-like particles, and the mottled appearance seemed to be produced by the interlacing of these particles. I could not observe any particular arrangement of the particles, but they appeared to be more numerous in some parts than in others. I have used the words rice-like particles merely to convey a rough impression of their form; I consider them like the fig.



"I have seen these rice-like particles on two occasions since, but not so well as on the first day, when the definition was exceedingly good.

"Yesterday (Feb. 24) I saw them for a few minutes, but with great difficulty. I use the full aperture, $12\frac{3}{4}$ inches, and a low power.

"I am, dear Sir,

"Yours very truly,

"E. J. Stone.

"J. Nasmyth, Esq.

"P.S.—On the first day I saw them I called Mr. Dunkin's attention to them. He appears to have seen them, and considers the fig. above to represent them fairly. He says, however, that he should not have noticed them if his attention had not been called to them."

Mr. Baxendell, F.R.A.S., read a paper "On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface."

Considerations arising out of an investigation of the irregularities which take place in the changes of some of the variable stars led the author, some time ago, to regard it as highly probable that the light of the sun, and also its magnetic and heating powers, might be subject to changes of a more complicated nature than has been hitherto supposed, and that, besides the changes which are indicated by the greater or less frequency of solar spots, other changes of a minor character, and occurring in shorter periods, might also take place. In the hope of detecting these supposed changes the author resolved to undertake the discussion of a series of magnetical observations, and for this purpose he selected the observations made at the Imperial Observatory of St. Petersburg, the most northern station at which hourly magnetical observations have been made for any lengthened period. Commencing, therefore, with the year 1848, the greatest and least values of the magnetic declination for every day were extracted from the observations, and taking the differences and arranging them in order, it was found, on a careful examination, that they indicated changes of activity taking place in a period of 31 days. The daily oscillations were then arranged in a table of 31 columns, and taking the means of the numbers in these columns, their differences from the general mean for the year were found to be as follow, the unit of value being one division of the scale of the magnetometer, or 26·3″ of arc:—

Day.	Diff. from General Mean. pts.	Day.	Diff. from General Mean. pts.	Day.	Diff. from General Mean pts.	Day.	Diff. from General Mean. pts.
1	− 8·18	9	− 7·86	17	+ 0·49	25	− 1·11
2	− 8·25	10	− 7·99	18	+ 6·35	26	− 2·38
3	− 3·32	11	− 0·70	19	− 3·05	27	− 1·43
4	+ 1·18	12	+ 7·45	20	+ 2.65	28	+ 8·41
5	− 4·80	13	+ 14·83	21	+ 4·21	29	− 6·51
6	− 0·21	14	+ 9·22	22	+ 5.80	30	− 6·51
7	− 1·52	15	+ 1·19	23	+ 3·39	31	− 5·69
8	− 6·66	16	+ 5·99	24	+ 5·08		

A projection of these numbers shows that of the *seventeen* consecutive days, 12 to 28, the amount of oscillation, or range of the magnetic needle, was *above* the mean value on *thirteen* days, and *below* the mean on only *four* days; while of the remaining *fourteen* days the range was *below* the mean on *thirteen* days, and above on *one* day only. The total amount of the differences for the seventeen days of maximum was 67·09, or 3·95 per day, and for the fourteen days of minimum it was 67·02, or 4·78 per day, the mean for the year being 39·86. The ratios of the average excess of a day of maximum, and of the average deficiency of a day of minimum to the mean value, are therefore as 1 to 10·09, and as 1 to 8.34.

On proceeding to examine the observations for the succeeding years it was found that they could not be represented by a period of 31 days. It appeared, therefore, at first sight that the period which had been obtained for 1848 was merely accidental; but, guided partly by conclusions drawn from his variable star investigations, and partly by the high degree of improbability that the results for 1848 could be due to mere accident, the Author was led to think that the period he had found for 1848 might be variable, gradually diminishing for a series of years, and afterwards gradually increasing, to diminish again when it had completed its cycle of change. Assuming, therefore, that in every year periodic changes took place in the magnetic activity of the sun, the Author proceeded to determine for each year the most probable approximate value of the period, and he obtained a series of values gradually diminishing till 1856, when the period was only about 23 days, and afterwards rapidly increasing, until in 1859, it amounted to about 32 days. A glance at these results at once suggested the idea that the variable period thus found was in some way connected with, and dependent upon, the great solar spot period, the minimum value occurring in the year of minimum frequency of the solar spots, and the maximum values in the years when the spots were most numerous.

Several series of thermometrical observations were now examined for indications of periodical changes in the element of mean daily temperature, and it was found that they exhibited, with unexpected distinctness, changes in this element occurring also in a variable period, the range of variation being, however, somewhat less than in the case of the magnetic element, although the times of maximum and minimum were almost exactly the same. The maximum and minimum values were respectively 31 and 23½ days.

A table is given showing the number of days included in maximum and the minimum portions of each mean period for the years 1848 to 1859, and the number of exceptional days, or those on which during the maximum part of the period the temperature was *below*, and during the minimum part *above*, the mean value. From this table it appears that out of a total number of 165 days of maximum only 14 were exceptional; and out of a total of $164\frac{1}{2}$ days of minimum the number of exceptional days was only 16. The mean gives a ratio almost exactly as 1 to 11. Considering that the values of the period in the different years are only approximate, this result may be regarded as affording satisfactory proof of the existence of a variable period of temperature; but a comparison of the total amount of the differences of temperature from the mean, and the amount of exceptional differences which is given in another table, is much more striking and conclusive. From this comparison it appears that against a total amount of 255·61° of *plus* differences on maximum days, there were only 8·17° of *minus* differences; and against 258·61° of *minus* differences on minimum days there were only 11·79° of *plus* differences, the mean ratio of the amount of exceptional differences to the total amount being therefore as 1 to 25·7.

At St. Petersburg the average temperature of the warmer half of the period is not less than 3° greater than that of the cooler half, and as this difference of temperature is repeated at least twelve times in every year, it must necessarily exercise a powerful modifying influence over many meteorological phenomena.

With reference to the differences between the maximum and minimum values of the magnetic and the corresponding values of the temperature period, it is remarked that owing to occasional interruptions of the magnetical observations and to the enormous extent of the oscillations of the needle on particular days, the values of the magnetic period given in the paper may require some correction; still it is believed that the ranges of the two periods are not identical.

Another period of change having a mean duration of rather over eighteen months is then referred to. The Author was first led to it from a discussion of the Greenwich magnetical observations for the years 1848 to 1859; and it has been confirmed by the results of a discussion of temperature observations made at Brussels, in Europe, and at Yakoutsk, in Asia. It is obvious that this period will at times interfere sensibly with the shorter one, and it is probable that some of the cases which have been called exceptional may be due to this interference.

As it may perhaps excite surprise that in this investigation no use has been made of the fine series of observations taken at the British Colonial Observatories, it is remarked that a little consideration will serve to show that in the early stages, at least, of an inquiry like the present, little or no reliance could be placed on results derived from series of observations in which every seventh day, or fifty-two days in the year, were complete blanks. It has, in fact, been found that the omission of a single day in a year will in some cases produce a very sensible effect upon the final results for the magnetic period. Hence it is that the Author regards some of the values he has obtained for this period as being open to correction when he may have an opportunity of discussing more complete sets of observations. General Sabine, in his very elaborate discussions of the magnetical observations made at the colonial observatories, separates the larger movements of the needle from the general mass, and treats them as extraordinary disturbances, assuming apparently that the two classes of ordinary and extraordinary disturbances are due to different causes. The Author has, however, not ventured to adopt this mode of proceeding, but has preferred to regard all the movements of the magnet, whether large or small, as having a common origin, and his discovery of the temperature period, by acting upon this view, may be regarded as affording strong evidence in its favour.

With regard to the probable cause of the variability of the short period, the author remarks that the subject is one of great difficulty, for, while the facts seem clearly to belong to the domain of astronomical science, he has found it impossible to frame any hypothesis to account for them without calling in the aid of some principle which has not hitherto been applied to the explanation of astronomical phenomena. It is therefore not without considerable hesitation that he ventures to observe that the facts would perhaps be best explained by supposing—

1st. That a ring of nebulous matter exists differing in density or constitution in different parts, or several masses of such matter forming a discontinuous ring, circulating round the sun in a plane nearly coincident with the plane of the ecliptic, and at a mean distance from the sun of about one-sixth of the radius of the earth's orbit.

2nd. That the attractive force of the sun on the matter of this ring is alternately increased and diminished by the operation of the forces which produce the solar spots, being greatest at the times of minimum solar spot frequency, and least when the spots are most numerous.

3rd. The attractive force being variable, the dimensions of the ring and its period of revolution round the sun will also vary, their maximum and minimum values occurring respectively at the times of maximum and minimum solar spot frequency.

In reference to the nature of the varying attractive force, it is not improbable that the matter of the supposed ring may be highly diamagnetic, and being much nearer to the sun than any of the known planets, of much greater bulk and lightness, and being subjected to a much higher temperature, it will be very sensibly affected by the changes which take place in the magnetic condition of the sun, and when interposed between the earth and the sun, it may act not only by reflecting and absorbing a portion of the light and heat which would otherwise reach the earth, but also by altering the direction of the lines of magnetic force. The changes of temperature at the surface of the earth will thus be due partly to differences in the amount of heat received from the sun, and partly to changes in the movements of the great currents of the air produced by alterations in the earth's magnetic condition. If the larger part of the difference of temperature is due to the latter mode of action, we might expect that during the warmer half of the period, the mean direction of the wind at any given station would be sensibly different from that during the cooler half; and also, that the epochs of maximum and minimum temperature would not be the same at all parts of the earth's surface. Both of these conclusions are borne out by the results given in the paper. Thus at St. Petersburg in 1859 the mean direction of the wind on maximum days was S. 54° W., and on minimum days S. 73° W. at 19° more to the west of South; and at Sitka, on the north west coast of North America, in 1851, the mean direction on maximum days was S. 32° W., and on minimum days S. 56° W., the difference being 24°. As striking instances of the differences in the epochs at distant stations, it may be stated that in 1859 the epoch of maximum at St. Petersburg corresponded precisely with the epoch of minimum at Madras; and that at Pekin, in 1851, the epoch of minimum was exactly coincident with the epoch of maximum at Sitka.

Changes in the amount of heat received from the sun sufficient to produce the variations of temperature observed at any given station would no doubt affect the movements of the great currents of the atmosphere, though not to the extent indicated by the observations; but it is difficult to conceive that they could produce the differences in the epochs which are found to take place. We may therefore fairly conclude that the action of the supposed ring of nebulous

14 a

matter is principally of a magnetic, and but slightly of a thermal character.

It is suggested that the greater range of variation of the magnetic as compared with the temperature period may be due to the inertia and elasticity of the great currents of air, the inertia tending to lengthen the temperature period and the elasticity to shorten it; but as the inertia will act with greatest effect when the magnetic period is at its minimum, while on the other hand the elasticity will be most effective when this period is at its maximum, the result will be a range of variation of the temperature period, somewhat less than that of the magnetic.

Adoping for the present the maximum and minimum values of the temperature period as being determined with greater accuracy than those of the magnetic period, the greatest and least values of the sidereal period of revolution of the ring will be 29·12 and 22·03 days respectively. From these numbers we find that the greatest distance of the ring from the sun is 0.185, the radius of the earth's orbit being taken as unity; the least distance 0.154; and the mean 0·169. Taking Mr. Hind's value of the mean distance of the earth from the sun, namely, 91,328,600 miles, we have

Greatest distance	of the ring	=	16,921,000	miles.
Least	,, ,,	=	14,068,000	,,
Mean	,, ,,	=	15,494,500	,,

and the range of movement to and fro in a radial direction = 2,853,000 miles. The greatest attractive force of the sun on the ring being taken as unity, the least will be 0·691. The difference is therefore nearly one-third of the maximum amount. It will be evident that this difference may be regarded as a measure of the forces which are concerned in the production of the solar spots.

The results of the elaborate investigations of the motions of the planet Mercury, made by M. Leverrier, led that accomplished mathematician to attribute a certain unexplained excess in the motion of its perihelion to the action of a disturbing body circulating round the sun within the orbit of Mercury; and from a discussion of the probable mass of the disturbing body he concluded that it could not be concentrated in a single planet, and that it consisted of a ring of small bodies similar to that which is known to exist between the orbits of Mars and Jupiter; and it is remarkable that the mean distance, which he seemed to regard as the most probable, is precisely that which the Author has found for the ring of nebulous matter whose existence he has assumed to account for the phenomena described in his paper. This unexpected and unlooked for agreement between results arrived at from considerations and by methods so totally different, seems to establish the existence of this ring with quite as much certainty as the results of the profound researches of Adams and Leverrier established the existence of Neptune before that planet had been actually seen. This ring, however, owing to its proximity to the sun, may never be seen, and, like the dark companions of Procyon and Sirius, it may only be known to us through its action on the other bodies of the system of which it forms a part Should future researches place its existence beyond doubt, this will, it is believed, be the first instance in which the conclusions of physical astronomy have been confirmed by the results of an investigation of magnetical and meteorological phenomena. Whether, however, the hypothesis which the Author has ventured to put forward be accepted or not, it is now very evident that observations of solar phenomena merit a much larger share of attention than has ever yet been devoted to them. It has long been suspected that the same causes which produce the spots on the sun's disc must in some way have an important influence on the phenomena of our own atmosphere. The facts now given convert this suspicion into a certainty; and it is perhaps not too much to say that meteorology can never take rank as a true science while our knowledge of the sun remains in its present imperfect state. Moreover, there is little doubt that many questions of high physical interest depend for their solution upon our obtaining a more intimate acquaintance than we yet possess with the operations which are going on in the great centre of our system. It is therefore much to be desired that some of the many observatories which are now established in various parts of the world should be specially devoted to observations of the sun, and solar phenomena generally; and that the principal magnetical and meteorological elements should be observed daily, without interruption, at all the regular magnetical and meteorological observatories.

It may be stated that the values of the variable temperature period, given in this paper, were derived from observations made at St. Petersburg, Wardoe, Gorki, Barnaoul, Irkoutzk, Nertchinsk, Yakoutsk, Pekin, Madras, Novo-Petrovsk, Lougan, Zurich, Geneva, Milan, Brussels, Greenwich, Jakobshavn in Greenland, and Sitka on the north-west coast of North America. The comparison of the variable temperature period with the solar spot period, extends over the twenty-seven years 1833–59, and therefore includes three maxima and three minima of solar spot frequency.

Ordinary Meeting, March 22nd, 1864.

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

Messrs. Oliver Heywood, James Mudd, Leslie J. Montefiore, and C. A. Duval, were elected Ordinary Members of the Society.

Mr. Hurst communicated the following letter from Capt. John Mitchell, Superintendent of the Madras Museum: —

Madras, 13th January, 1864.

To H. A. Hurst, Esq., 61, George Street, Manchester.

Dear Sir,

I have the pleasure to acknowledge the receipt of your letter of the 10th November, 1863.

There are very few microscopists here, and as I do not know any person who is likely to undertake the examination of cotton fibre in the various stages of its growth, I have resolved to do so myself, in the belief that the subject is one of sufficient importance to justify me in devoting one day in the week to the inquiry so long as may be found necessary.

I have accordingly made arrangements with Dr. Hunter, Honorary Secretary of the Madras Agrihorticultural Society (who at once promised me every assistance), to receive weekly on Saturday a few pods from the Society's grounds, where cotton of all kinds is growing.

I have already given four days to this inquiry, and although it is still in its infancy, I have obtained some interesting results, which I will at once briefly communicate.

I began the examination with pods that were supposed to have been just formed. In this, the earliest stage, I found the cotton hairs just becoming visible upon the surface of the seed as minute transparent hemispheres containing a few motionless granules—I should perhaps say translucent, for the cotton hairs do not seem ever to be transparent.

In a more advanced stage the seeds were covered with hairs which contained numerous minute granules floating in a very fluid and colourless mucus. An active rotation of the cell contents, exactly like that in Nitella, was seen in all the hairs that had not been injured by pressure, and continued for a considerable time, at least half an hour. I found it could be seen with Ross's half-inch and the higher eyepieces, but I used chiefly a $\frac{1}{12}$th.

In a pod apparently somewhat older the appearances only differed by the cell contents, which I have called colourless mucus above, becoming thickened, and the granules somewhat smaller, so that we had a fine granular mucus of a pale buff colour.

As the pod becomes older the cell contents appear to increase in density and rotation to cease; at least I have not seen rotation unless in the hairs of young seeds, *i.e.* seeds from young pods of perhaps from two or three to ten or twelve days' growth. I have not, unfortunately, been able to learn the exact age of the pods.

On Saturday last I plucked a fine pod of Queensland cotton (growing in the garden of a friend) that was supposed to be nearly full grown, and was upwards of two inches in vertical diameter. In the hairs of this I found generally, but not always, a considerable amount of secondary deposit, made evident by the thickening of the walls and by its action on polarised light. But in the hairs of younger pods there was nothing of the kind, and the walls were so thin as scarcely to afford evidence of their presence, it requiring considerable power to bring out the usual double contour line, and they had no action, singly, on polarised light, although they became a little luminous in a mass of many.

The growing cotton fibre is an elongated cone with a hemispherical apex, and, of course, a circular transverse section. Each hair is a single cell. I have sought in vain, with all powers and every kind of illumination that I thought likely to render it visible, for any section or transverse division in the hairs, and I have been equally unsuccessful in my search for spiral fibres, which Mr. O'Neill says he found in cotton by means of re-agents, and I believe I am justified in saying that spiral fibre did not exist in any cotton hairs hitherto examined by me. But I have yet to examine pods of a later growth, and spiral fibre may yet appear, but I must confess I do not expect it.

I have not seen any twist in growing fibre, and, notwithstanding the pressure to which the hairs are probably exposed, I have seen no flattening from this cause, but the hairs of course collapse and become flat when from any cause the cell contents are absent. From sections I have made and examined I believe that in the younger pods the hairs wind round the seeds; in the more advanced stages the hairs of neighbouring seeds intermingle, and this may account for the bent and twisted appearance of dry cotton, that is, in some degree; but the principal cause will doubtless be found in the desiccation of the cotton after it is exposed to the sun by the bursting of the capsule.

I must not omit to mention that when by pressure a portion of the contents is expelled from the cotton hairs, it frequently appears in the form of small spheres, in which an active molecular movement of granules is seen, just as in the mucous corpuscles from the mouth, from which, in appearance, they only differ in their larger size.

I have seen in some dry Sea Island cotton, with a $\frac{1}{12}$th and polarised light, what Mr. Sidebotham (was it he?)

took for spiral fibres, I presume; but they are only visible in places and not in all hairs. I confess that at present I am a sceptic on this point.

With apologies for this hasty letter,

I am, Dear Sir,

Your most obedient servant,

J. Mitchell, Captain,

Superintendent Madras Museum.

Professor Roscoe read the following extract from a letter which he had received from Professor Boettger, of Frankfort, respecting the occurrence of the salts of Cæsium, Rubidium and Thallium together, in the salt obtained by evaporating the mineral water of Nauheim, near Frankfort.

"The Nauheim salt is a highly valuable substance, as being perhaps the only material from which Cæsium can be obtained in quantity. According to my experiments, 1cwt. of this salt yields 1lb. of double chloride of Cæsium and Platinum, containing small quantities of the Rubidium and Thallium double salt. At my special request the "Kurfürstliche Salzamt" in Nauheim has arranged to sell this evaporated salt, packing included, at the exceedingly low rate of one thaler (3s.) per cwt. In my spectrum investigations I almost invariably employ a flame of hydrogen in place of the common Bunsen's lamp, as by this means I obtain a higher temperature, and the lines appear therefore more distinct. The spectrum of the triple platinum-chloride of Cæsium, Rubidium, and Thallium thus obtained exhibits in the first place the well-defined emerald green line of Thallium, soon afterwards the two brilliant blue Cæsium lines appear close together, then the very broad and ill-defined blue bands of Rubidium are seen, as well as the two narrow red lines characteristic of this metal.

"I have lately discovered the presence of Thallium not only in many other mineral waters, but also in the vegetable kingdom, in very minute, although perceptible quantities. Thus Thallium occurs in the ash or the charcoal from wine yeast, in molasses, tobacco, chicory root, &c. If four pounds of any of these substances are taken, a quantity of the double chloride of Platinum and Thallium is obtained sufficient for many experiments. According to my most recent experiments, Thallium occurs in the dust from the pyrites burners as Thallium-iron-alum. I have lately prepared this salt directly from the sulphates of Thallium and Iron; it is the most easily soluble of all the Thallium salts; it possesses the pale reddish amethystine colour of ammonia-iron-alum, it crystallizes like this salt in large regular octohedra, and contains 24 atoms of water. Sulphate of Thallium is isomorphous with sulphate of Potassium, and the fact of the formation of the above double salt is another proof of the close relationship between Thallium and the alkaline metals, independently of the fact that it almost invariably accompanies Potassium, and not unfrequently both Caesium and Rubidium."

A Paper was read by J. C. Dyer, Vice-President, entitled "Notes on Spinning Machines: Part 1; The Mule Jenny:" who stated 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 5 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." This process gives to the mule twist, the peculiar soft property that distinguishes it from the throstle or water twist. When the Crompton Mule came into general use, several improvements were made in its construction, by different eminent mechanicians, of whom Mr. John Kennedy and Mr. Peter Ewart were the most eminent; but it would form a long list to point out the authors of those improvements before any notable efforts were made to render it a self-acting machine.

It was the task of the "hand-spinner" to guide and regulate the "putting up" the carriage and winding the threads on the spindles, after they had been spun, and to perform these operations, the self-acting movements were devised, to render the mule independent of his skill. The real difficulty to render the mule a self-acting machine was chiefly confined to winding the threads on the continually changing forms and size of the cops.

The first notable attempt to construct a self-acting Mule was made by Mr. Niel Snodgrass, about the year 1815, but his plan appears not to have attracted much notice. The next was the patent obtained by Mr. William Eaton, of Wiln Mill (on the Trent in Derbyshire), dated in December 1818. Mr. Eaton has laid down with great exactness, the mechanical means of giving all the movements required for a self-acting mule, which are separately and minutely described in his specification. Mr. Eaton and his sons established a spinning mill, with his patent mules, to the extent of about 3000 spindles, in Chepstowe-street, Manchester, which were in operation in the years 1821 and 1822, where they were open to general inspection.

The next self-acting mule was patented by Mr. Richard Roberts, in 1825. His winding-on process, however, being imperfect, prevented his mule from coming much into use until his second patent was brought out, in 1830, which was far superior to the former and was more extensively adopted. In the meantime, Mr. Maurice de Jongh, of Warrington, obtained a patent for his self-acting mule, dated 1827. This mule was worked with moderate success for some years. We next come to the patent of Mr. Peter Ewart, dated in 1833, in which he very accurately describes all the movements required to render "the mule independent of the attention and skill of the spinner," viz.—(1) The backing off; (2) guiding the faller wires; (3) putting up the carriage; and (4) the winding-on: which last "I consider the most important movement of the machine." Mr. Ewart's mules were established in his mill, in East-street, Lower Mosley-street, and continued in regular operation until after he left Manchester. The next self-actor was patented by Mr. James Smith, of Deanston, in 1834. There was much novelty and some good properties in Mr. Smith's mule, so that it and Mr. Robert's second mule were the chief competing machines for many years. Besides the foregoing a patent was obtained by Messrs. J. and J. Potter for a self-acting mule, dated in 1836. The specification of this patent "being out of print" at the Patent Office, no account can be here give of it.

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, as all the former plans becoming public, any of them might be combined to produce a better machine than those before patented, and which 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.

The advances made in the wealth-creating powers of the people are more conducive to the public welfare than even the accumulation of wealth produced by their exercise. These powers of adding to the fixed capital of the nation come mainly from the use of labour-saving machines in our manufactories, and especially in those of the cotton and other textile fabrics. Hence it seemed desirable to record some of the changes and improvements made in the principal machines employed in our cotton mills. Of these the mule jenny is perhaps the most important, and the successive changes it has undergone give a fair type of the gradual progress of mechanical inventions generally.

Great inventions may come perfect from the *head*, but seldom, or never so, from the *hand* of their authors. Thus the Crompton mule (as before stated) underwent many useful changes by the skill of others, which prepared it for the inventions to render it self-acting. In the foregoing account of the labours bestowed on the mule I have given the names and claims of the inventors as they appear in their several patents, and I may assume that the public will desire to have the simple facts relating to the parties concerned, thus plainly set forth.

Ordinary Meeting, April 5th, 1864.

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

Mr. John Eastham was elected an Ordinary Member of the Society.

Messrs. H. M. Ormerod and G. C. Lowe were appointed Auditors of the Society's Accounts for the present Session.

Mr. Bottomley gave the results of some experiments upon the estimation of sulphur made according to a method devised some time ago by M. Pelouze. This method had been brought under the notice of the Society by Dr. Calvert, as being a quick and approximate one; the present experimenter had, however, obtained these approximations within such wide limits that he had not found the process available for the purpose intended. In these experiments, 20 grains of the ore, very finely powdered, were mixed with 100 grains of carbonate of soda, 100 grains of chlorate of potash, and 140 grains of common salt. The carbonate of soda and common salt were always heated previous to an experiment, to ensure their being quite dry. These materials were well mixed for about ten minutes in a porcelain mortar.

In all cases the mixture quietly deflagrated, without throwing any particles out of the platinum crucible.

The following were the results obtained :—

	True Percentage.	Percentage by Alkalimetry.
1	36·98	33·20
2	40·75	36·95
3	38·44	38·80
4	39·64	42·11
5	36·87	41·70

In the two first experiments the materials were kept fused at a low red heat for some time after deflagration had ceased. In the three last experiments the lamp was removed soon after deflagration had taken place.

In the filtrate from the first experiment arseniate of soda was found.

In the fourth and fifth experiments arsenic was also found, but not in quantities adequate to account for the difference between the two percentages.

These wide differences between the results in the two columns no doubt depend upon the fact that the chlorate of potash does not act merely as an oxidizing agent, but gives up some chlorine. In all cases some oxygen compound of chlorine has been evolved with the carbonic acid.

It is worthy of remark that many years ago it was stated by Vanquelin that the residue obtained after decomposing chlorate of potash was sensibly alkaline. ("Ann. de Chimie et de Physique," xcv., 101.) A few grammes of chlorate of potash were heated in a platinum dish for a few minutes, until the fluid mass began to solidify. This residue, dissolved in water gave a solution that seemed faintly alkaline, so faint, however, as to be scarcely perceptible. Prolonged heat and perfect decomposition of perchlorate of potash might perhaps have made it more marked.

The above experiments were made in Mr. Spence's laboratory. The same method had been tried in Mr. Rumney's laboratory, by Mr. Bocharoff; his results, however, did not agree with the true percentage. He also ascribed the disagreement to the same cause as that suggested in this notice.

Mr. BAXENDELL gave the details of some observations he had lately made at Southport to determine the velocities of rifle balls. Three kinds of rifle were used by the firing party, the Whitworth, the Henry, and the Enfield; and the distance of the target being 600 yards, it was found that the average times required by the balls to traverse this distance were—

	Seconds.
With the Whitworth rifle	1·59
,, Henry	1·75
,, Enfield	1·87

The velocities were therefore—

	Feet per Second.
With the Whitworth rifle	1,132
,, Henry	1,028
,, Enfield	962

The differences between individual results were greatest with the Enfield rifle, and least with the Whitworth; and the number of points made was greatly in favour of the Whitworth instrument, the accuracy of fire apparently increasing in a greater ratio than the increase in the velocity of the balls.

It was understood that the charges of powder used were those which experience had shown to be the best adapted for each form of rifle.

Annual Meeting, April 19th, 1864.

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

Capt. John Mitchell, Superintendent of the Madras Museum, was elected a Corresponding Member of the Society.

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

REPORT OF THE COUNCIL.

In presenting their Annual Report the Council are glad to be able to congratulate the Members upon the generally satisfactory condition of the Society. During the past year fourteen new ordinary Members have been elected, whilst on the other hand the losses have been by deaths four, defaulters two, and resignations four, the present number of ordinary members being one hundred and ninety against one hundred and eighty-six of last year. The Council are also glad to call attention to the satisfactory condition of the Society's finances as seen in the Treasurer's Report annexed, the balance in the hands of the Treasurer now being £371. 8s. 1d. against £320. 18s. 7d. of last year. The two parts of the second volume of the third series of the Memoirs are now ready, the third part completing the volume, and containing papers read during the present Session, is already in hand, and will be finished before the commencement of the ensuing Session. The increasing value of the library of scientific reference is a subject to which the Council may justly refer with pride, the number of academies and scientific bodies with which the Society is in communication has steadily augmented, and now exceeds that of almost any other similar institution. The ordinary Members deceased during the last financial year are Mr. George Mosley, Mr. John Gould, Mr. John Jessie, and Dr. Edward Stephens.

Mr. Mosley was associated with the Society for some years, having been admitted an ordinary member in January, 1859, and elected a Member of the Council in April, 1861. He took a prominent part in the formation and working of the microscopical section of which he was the honorary secretary. He organised a scheme for procuring soundings from different parts of the world, for the purpose of investigating the various formations of the sea and ocean beds; his labours in this direction met with great success, and the Society now possesses a large number of specimens which are in process of arrangement and classification. As an accurate observer and indefatigable student of the microscope, his place will not be easily filled.

The list of honorary members has suffered a great loss in the death of Mr. Richard Roberts, C.E. This gentleman, now generally acknowledged to have been one of the first mechanicians of our time, was admitted as an ordinary Member of the Society in the year 1823, April 16, and on his removal from Manchester he was elected in April, 1861, an Honorary Member. The mechanical inventions of Mr. Roberts were of the most varied and important description; the best known of his inventions was the improvement of Crompton's mule, so as to render it self-acting, and his improvements in engineering tools. His first patent for the former purpose was taken out in March, 1825, and a second patent for an improved process was granted to him in July, 1830. Mr. Roberts obtained nearly thirty patents in England for the following useful subjects:—hand and power looms, machines for preparing and spinning fibrous substances, steam engines, locomotive carriages, apparatus for punching, shearing, and perforating metals, beetling and mangling machines, clocks, watches, and other timekeepers, machine for hoisting weights, telegraphic communication, weaving and cutting plushes, velvets, carpets, &c., fluid meters, pumping apparatus, screw propellers, churns for agitating and evaporating fluids, turbines, hydraulic presses, steam ships and other vessels, anchors, steam boilers, life boats, war ships and their armaments, casks, machinery for cutting paper, leather, cloth, &c., machinery for punching, drilling, and riveting, omnibus and other carriages, folding fabrics, mechanism for engraving and otherwise copying paintings and other designs on flat and carved surfaces, &c., &c.

The following list of communications made during the past year proves that the scientific activity of the Society has in no wise diminished:—

April 30*th*, 1863.—"On the Wave of High Water, with Hints towards a New Theory of the Tides," by Mr. Thomas Carrick.

April 30*th*, 1863.—"On the Number of Days on which Rain falls annually in London, from Observations made during the 56 years, 1807-1862," by G. V. Vernon, F.R.A.S., M.B.M.S.

April 30*th*, 1863.—"On the Rain Fall at Oldham during the years 1836-62," by John Heap, Esq. with remarks by G. V. Vernon, F.R.A.S., M.B. M.S.

May 12*th*, 1863.—"Note on the History of Steam Navigation," by W. J. Macquorn Rankine, F.R.S., &c.

July 1*st*, 1863.—"The Complete Theory of Groups, being the Solution of the Mathematical Prize Question of the French Academy for 1860," by Rev. Thos. P. Kirkman, M.A., F.R.S., &c.

October 6*th*, 1863.—"On Ten Ancient Spanish Coins taken in change at Malaga," by John Harland, Esq., F.S.A., communicated by Mr. R. D. Darbishire.

October 6*th*, 1863.—"On the Sound produced in the Electric Discharge," by Professor Wm. Thomson, F.R.S., communicated by Professor Tait.

October 20*th*, 1863.—"Further Observations on the Carboniferous, Permian, and Triassic Strata of Cumberland and Dumfries," by E. W. Binney, F.R.S., F.G.S.

November 3rd, 1863.—"On the Theory of Equations," by His Honour James Cockle, M.A., F.R.A.S., &c., communicated by Rev. Robert Harley, F.R.S., &c.

November 3rd, 1863.—"On Recent Researches on the Theory of Equations," by the Rev. Robert Harley, F.R.S.

November 3rd 1863.—"On Marine Shells in Stratified Drift, at High Levels, on Moel Tryfaen, Caernarvonshire," by R. D. Darbishire, F.G.S., &c.

November 3rd, 1863.—"On the Existence of the Moa of Naturalists in New Zealand," by Charles Clay, M.D.

November 3rd, 1863.—"Remarks on the Habits and Character of the Maoris of New Zealand," by Charles Clay, M.D.

November 12th, 1863.—"On the Construction of Bi-focal Lenses for Spectacles," by J. Atkinson, F.G.S.

November 12th, 1863.—"On a new Variable Star (T. Aquilæ)," by J. Baxendell, F.R.A.S.

November 17th, 1863.—"On a new method of producing Carbonic Oxide," by Dr. F. Crace Calvert, F.R.S.

November 17th, 1863.—"On an Apparatus for Measuring Tensile Strengths, especially of Fibres," by Charles O'Neill, F.C.S.

November 17th, 1863.—"Experiments and Observations upon Cotton," by Charles O'Neill, F.C.S.

December 1st, 1863.—"Note on the Meteor of February 6th, 1818," by J. P. Joule, LL.D., &c.

December 15th, 1863.—"Notes on some recent Discoveries in Elemental Physics," by J. C. Dyer, V.P.

December 29th, 1863.—"Additional Observations on the Drift Deposits and more recent Gravels around Manchester," by Ed. Hull, B.A., F.G.S.

January 12th, 1864.—"Enquiry into the Question, whether Excess or Deficiency of Temperature during part of the year is usually compensated during the remainder of the same year," by G. V. Vernon, F.R.A.S., M.B.M.S.

January 12th, 1864.—"Examination as to the Truth of the Assertion that when November has a mean temperature above the average, it is usually followed by excessive cold between the December and March following," by G. V. Vernon, F.R.A.S., M.B.M.S.

January 12th, 1864.—"Note on the Amount of Carbonic Acid contained in the Air of Manchester," by Henry E. Roscoe, B.A., Ph.D., F.R.S.

February, 9th 1864.—"On the Photochemical Effect of the Light emitted by burning Magnesium Wire," by Henry E. Roscoe, B.A., F.R.S.

February 9th, 1864.—"On the Tensile Strength of Cotton as affected by various Chemical Treatments," by Chas. O'Neill, F.C.S.

February 23rd, 1864.—"Note on the Preparation of Calcium," by Mr. Edward Sondstadt, communicated by Professor Roscoe.

February 23rd, 1864.—"On the Nature of Friction in Mechanics," by J, C. Dyer, V.P.

March 8th, 1864.—"On the Existence of the Willow Leaf-shaped Bodies on the Sun's Surface," by E. J. Stone, M.A., communicated by Mr. James Nasmyth.

March 8th, 1864.—"On Periodic Changes in the Magnetic Condition of the Earth, and in the Distribution of Temperature on its Surface," by Jos. Baxendell, F.R.A.S.

March 22nd, 1864.—"On the Microscopic Structure of the Cotton Fibre," by Captain John Mitchell, of Madras, communicated by Mr. Hurst.

March 22nd, 1864.—"Notes on Spinning Machines: Part I., The Mule Jenny," by J. C. Dyer, V.P.

LIBRARIAN'S REPORT.

Since the close of last Session 396 vols., 216 parts of vols of non periodical works, 214 pamphlets, and 27 quarterly, 161 monthly and 303 weekly numbers of periodicals, have been added to the Library, of which 169 vols., 13 quarterly and 35 monthly numbers by purchase; the remainder by exchange or donation.

The periodical publications which the Librarian is authorised to procure for the Library by purchase, are:—

The London, Edinburgh, and Dublin Philosophical Magazine.
The Quarterly Journal of Pure and Applied Mathematics.
The Cavendish Society's Publications.
The Ray Society's Publications.
The Palæontographical Society's Publications.
The Natural History Review.
The Annals and Magazine of Natural History.
The Quarterly Journal of Science.
The Registrar General's Annual Reports.
Les Annales de Chimie et de Physique.
Le Journal de l'Ecole Polytechnique.
Les Annales des Sciences Naturelles.
Le Journal de Pharmacie et de Chimie.
Les Archives des Sciences Physiques et Naturelles.
Die Astronomische Nachrichten.
Die Annalen der Physik und Chemie.
Die Annalen der Chemie und Pharmacie.
Der Journal fur die reine und angewandte Mathematik.
Neues Jahrbuch fur Mineralogie, Geologie, und Palæontologie.
Die Zeitschrift fur wissenschafsliche Zoologie.

The Philosophical Transactions for 1845 and 1846, mentioned in the last Annual Report as having been taken out from the Library without being entered in the Register, have not been returned, notwithstanding repeated announcements in the Circulars convening the Meetings. It is to be feared that these volumes must be considered as lost.

ROBERT WORTHINGTON, TREASURER, IN ACCOUNT WITH THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

From 31st March, 1863, to 31st March, 1864.

Dr.

			£. s. d.	£. s. d.
1863. April 1.	—To Balance in the Bank of Heywood Brothers and Co.		314 8 8	
	,, Balance in hands of Treasurer		6 9 11	
				320 18 7
1864. Mar. 31.	—To *Members' Contributions:*			
	Arrears, 1861-2 … 1		2 2 0	
	,, 1862-3 … 12		25 4 0	
	Members on the Roll, April 1st, 1863, 186, at 42s.	£390 12 0		
	Deduct Compounders … 3			
	,, Paid in advance … 2			
	,, Dead … 2	37 16 0		
	,, In arrear, March 31, 1864 … 11			
			352 16 0	
	Six members elected in 1863, April—Dec.		12 12 0	
	Eight ditto, in 1864, half subscription	£8 8 0		
	Deduct in arrear	6 6 0		
			2 2 0	
	Admissions paid … 8		16 16 0	
				411 12 0
,, ,,	To *Sale of Publications:*			
	Memoirs—Ser. 3, Vol. I.		1 1 0	
	Proceedings		0 15 0	
	Dalton's Meteorology		0 3 0	
				1 19 0
,, ,,	To *Sundry Income:*			
	Sect. Contrib.: Mathematical	£2 2 0		
	,, Statistical	1 1 0		
	,, Microscopical	2 2 0		
			5 5 0	
	Photographic Society—Attendance, Coal, &c., at Meetings		4 10 0	
	Interest allowed by Bankers		10 3 4	
				19 18 4
				£754 7 11
	Library Fund		£86 7 0	
	Compositions		72 10 0	
	General Balance		212 11 1	
	Total Balance		£371 8 1	

Cr.

			£. s. d.	£. s. d.
1863. March 31.	By *Charges on Property:*			
	Chief Rent		12 10 3	
	Fire Insurance		7 17 6	
	Property Tax		4 19 2	
				25 6 11
	,, *House Expenditure:*			
	Water, Gas, Candles, and Coals		16 5 9	
	Cleaning and Petty Expenses		2 13 10	
	Furniture		0 4 3	
	Repairs		0 14 6	
	Tea and Coffee at Meetings		16 13 11	
				36 12 3
	,, *Administrative Charges:*			
	Salary—Keeper of Rooms	60 0 0		
	Wages—Assistant ditto	12 8 0		
			72 8 0	
	Attendance on the Photographic Society and Sections		7 10 0	
	Postage and Parcels		18 6 7	
	Stationery, Printing Circulars, &c., and Receipt Stamps		22 18 7	
				121 3
	,, *Publishing:*			
	Memoirs—Printing, Engraving, &c.		96 18 0	
	Printing Proceedings		44 17 0	
				141 15 0
	,, *Library:*			
	Periodicals, Binding, &c.		53 15 10	
	Memoirs Purchased		0 6 0	
	Subscription to Cavendish Society		1 1 0	
	,, Ray Society		1 1 0	
				56 3 10
	,, *Sundry Expenditure:*			
	Solicitors, for Registration of Alteration of Rules			1 18 8
	Total Disbursements			382 19 10
	Balance in Bank of Heywood Brothers and Co.			371 8 1
				£754 7 11

ROBERT WORTHINGTON, TREASURER.
1st April, 1864.

Examined and found correct, 8th April, 1864.

HENRY M. ORMEROD, }
GEORGE CLIFF LOWE, } AUDITORS.

On the motion of Mr. Ormerod, seconded by Mr. Parry, the Annual Report was unanimously adopted.

The following Gentlemen were elected Officers of the Society for the ensuing year:—

President.

ROBERT 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.

CHARLES FREDERIK EKMAN.

Other Members of the Council.

REV. WILLIAM GASKELL, M.A.
FREDERICK CRACE CALVERT, Ph.D., F.R.S., F.C.S.
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.