MEMOIRS OF THE LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

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OF THE

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NOTE.

The Authors of the several Papers contained in this Volume are themselves accountable for all the statements and reasonings which they have offered. In these particulars the Society must not be considered as in any way responsible.

MEMOIRS

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LITERARY AND PHILOSOPHICAL SOCIETY OF MANCHESTER.

I. *Notes on some Superficial Deposits at Great Orme's Head, and as to the Period of its Elevation.* By R. D. Darbishire, B.A., F.G.S.

Read October 15th, 1867.

Amongst the most interesting inquiries of the geology of to-day are those that aim at tracing the changes of the earth's surface which have last preceded the general conditions under which our present life on it obtains. These changes, if any, are due to causes still in operation, and therefore are such as in an especial degree allow the exactest analogies, to be tested by the, so to speak, experimental results of daily and local observation.

The situation of superficial formations and most of their phenomena place them within the reach of rapid survey. Sections are numerous and accessible, or easily made so. The matrix is usually readily worked, and such remains of organic or mineral character as occur are generally not very difficult of identification.

If to these inherent facilities be added the increasing interest in such questions, the growing habit of frequent travel, and the excessive opportunity of publication of the present day, it is perhaps not wonderful if it is particularly in this field that, by the side of the more eminent scientific students, many less-qualified observers should offer their contributions to the better knowledge of our own country or of special districts of it.

If I venture to occupy the attention of the Society with the following notes, it is because circumstances and local convenience have enabled me for some years past to make frequent visits to the Great Orme's Head, and because I think that a certain familiarity with, and a definite study of, modern sea-beaches and sea-beds, their forms and inhabitants, and the preservation and disintegration of their peculiar remains enables me to offer some special illustrations of its peculiar recent history.

It will be convenient to preface what I have to say by a short summary of observations already recorded. Professor Ramsay (Quart. Journ. Geol. Soc. viii., 1852, and again in 'Old Glaciers of Switzerland and N. Wales,' 1860), after discussing the superficial appearances of Caernarvonshire and Anglesey, states, as a well-ascertained fact, that previously to the Tertiary Glacial epoch the grander contours of hill and valley were nearly the same as now. Much of the land was then slowly depressed beneath the sea; and icebergs drifting from the north, and pack-ice on the shores, ground along the coasts and sea-bottoms, smoothing and striating the surfaces over which they passed in contact, and, in the course of ages, depositing clay, gravel, and boulders over wide areas that had once been land. The grooves and striations on ice-smoothed rocks still bear witness to the general southward course of the ocean-currents of those icy seas. The true glacial drift Mr. Ramsay traces to a height of about 2300 feet on some of the Caernarvonshire mountains. The land then slowly rose at least this height above the sea-level; and during this long period, while the sea was rearranging and rewearing the deposits of successive shores, a second system of land-glaciation, at the same time that it bore down clay and sand and boulders of its own making, carried down off the new land much of the older superficial drift, in its turn to be also rearranged in the, so to speak, retreating ocean.

The more ancient of these epochs has been elaborately discussed by an acute observer in 'Frost and Fire' (1865), an instructive and interesting attempt to group and generalize glacial phenomena as a part of cosmical history.

The story of Great Orme's Head, as one of the lower elevations of the district, and of its adjacent low-level lands, falls within the latest of these, and passes beyond it into the most modern of geological periods. Lately an attempt has been made by the Rev. T. G. Bonney (Geol. Mag. iv., 1867) to trace ice-action over this hill and the neighbouring range of the Little Orme's Head. This gentleman gives his reasons for supposing that during the Glacial epoch the Head was a low island with its undulating cap of ice of no great thickness,—and, as to the Little Orme's chain of hills, observes that its undulating outlines are strongly suggestive of glacial action, and says that the peculiar curves of these ridges and their inner slopes can only be explained by this cause.

Mr. Binney, F.R.S., in a paper read to the Manchester Geological Society (Trans. iii. 97, 1861), notes his observation, at a point below the Bath-house at Llandudno, of a piece of brown limestone *in situ*, scored and polished probably by ice. In the same paper Mr. Binney identified, below a superficial series of slight depth and varying constituents, a bed of brownish-coloured till, "containing angular, rounded, and partly rounded pebbles," appearing on both coast-lines of the Llandudno isthmus. In this bed he found fragments of *Turritella terebra*, and pointed out its similarity with the till of the Blackpool cliffs, described by him some years ago in a paper read to this Society (vol. x. N. S.).

At a higher level on the east side of the Head he noted a blackish-brown clay, with angular fragments of limestone, traceable up to 162 feet above mid-tide, containing abundance of *Mytilus edulis, Patella vulgaris,* and *Littorina littorea* shells. He traced the clay 100 feet higher up, and at a height of 400 feet he distinguished a singular deposit of fine sandy shingle of small slate and Silurian gravel, very similar to that on the beach below, mixed with large pebbles of white quartz and chert, and resting on large angular pieces of limestone. In this bed he found *Mytilus, Ostrea, Patella,* and *Littorina.* The shingle appeared at first sight to have been brought up for road-repair; "but the rounded pebbles of quartz and chert, and the fact of having traced the shells all the way up the hill, convinced him that it was a natural deposit. The fossils were similar to the shells of the adjoining sea, and clearly proved the elevation of the Head at least 400 feet in a very modern epoch, gradually raising with it the banks of shells now found on its sides, the living shells holding their place between high and low water, though the land was continually going up."

In the paper already mentioned, Mr. Bonney refers to well-marked wave-marks on the face of the exposed cliff, 200 feet above the present sea-level, and mentions the occurrence of *Patella* and *Littorina* in the talus near Pen Trwyn, the north-east point of the Head. He notes a bed of clay in Gwydfyd hollow, between the Head and Pen y Dinas, its south-eastern hill, with *Tapes* (*pullastra*?), *Mytilus, Ostrea, Patella,* and *Littorina,* and conceives the clay both here and elsewhere to have been deposited "after the ground had pretty nearly assumed its present configuration, and not to have undergone much denudation during the progress of upheaval." After noticing certain deposits which he identifies as kitchen-middens at low levels on the west side of the Head, he indicates the clay (Mr. Binney's till) on the western cliff of the isthmus, disappearing under sand in which are seams of *Mytilus edulis,* beds of which occur at intervals along the coast-section.

In conclusion Mr. Bonney considers that, "after the limestone hills of the district had acquired their leading forms by upheaval and marine denudation, the whole district was depressed; the summits of the low rocky islets became covered with ice-fields, which in places descended in glaciers into the sea. At this period there were oscillations of level, during which the two lower beds were subject to slight denudation. After the deposition of the upper bed of clay, there must have been considerable denudation from the action of the retreating sea. To this must have succeeded a period of depression, during which the mussel-beds were formed; and then the whole was gradually upheaved above the level of the sea."

In his 'Excursions of a Naturalist,' 1867, Mr. Garner refers to the existence some years ago of a great accumulation of midden or refuse-heap on the west of the Head, but implies that what he describes is not now to be seen.

Special references to other details of these or some other observations will be made in their places in the following notes.

I introduce my observations by a short description of the more ancient rocky basis of the Head.

Rudely outlined, this hill may be described as a great oblong mass of mountain-limestone, lying, generally speaking, east and west, the strata somewhat depressed along the centre line in the same direction. On the north side the tilted strata are cut down in alternate cliffs and talus-hidden scars to the sea-level in magnificent and often almost vertical precipices. On the south side a similar but less impressive profile is footed in the isthmus of Llandudno. The greater part of the western boundary consists chiefly of a fine inland cliff half-clothed by an enormous talus, from which in turn has been cut by the present sea-wash a littoral cliff. The principal cliff shows in fine sections the elevation of the strata northwards and southwards. The middle of the superficial valley is occupied by a long and high ridge to the east of the old semaphore-station, consisting of a coarse brown grit. On this may be seen one or two small patches, scarcely a foot square, of mountain-limestone. This and the underlying grit are the last remnants of unmeasured though doubtless enormous denudation. The huge bays that flank the Head, eastwards towards Rhos Head, and westwards towards Puffin's Island and Anglesea, are themselves more obvious memorials of a similar removal of vast masses of the Lower Limestone. This, however, is the story of very ancient times. On either side of the Telegraph-hill the valley falls rapidly eastward, and uniting again forms the mine-plateau, and then runs down to the south of east by the line of the "Old road" into the east side of the isthmus, and the centre of what is now Llandudno, separating Pen y Dinas, the eastern point of the hill, from the rest of the southern ridge.

North of Pen y Dinas, and likewise opening towards the east, a shorter and steeper valley debouches below Gwydfyd farm towards the Llandudno bay, ending in a cliff of talus, overhanging a limestone precipice and the modern beach.

The surface of those parts of the Head which are not occupied by the gritstone hill are either separate hillocks or ridges (that is to say, islands or reefs) of mountain-limestone, each with its greater or lesser sea-side cliffs or plateaux of denuded strata. Between them and on the sides of the Head, and especially in the falling valleys, are great grass-covered slopes of superficial clays, more or less loaded with limestone detritus of all sizes, from blocks containing many cubic yards of stone downwards. These detached rocks crowd the sward and steeper inclines and add much to the picturesqueness of the north face and its romantic walk. They are often sharply angular, but occasionally are weathered or waterworn into rounded boulders. In the flatter hollows or the lower plains of the central depression there is under the sward a certain amount of humus of no great depth. The Head is connected with the mainland of Denbighshire by the Llandudno isthmus, widening eastward to the foot of the range of limestone hills which ends northwards in Little Orme's Head. Southwards the isthmus, embracing two or three piles of igneous and altered Silurian formation, passes towards Conway and its eastern estuary. The river Conway separates it from unaltered and altered Silurian beds and the great Trap ridge of Conway Mountain. The Conway estuary on the south-west, and the tides of the Menai Straits, the Irish Sea, and Llandudno Bay on the west, north, and east, and rain and frost above high water-mark, are continuing in this our own day the processes which in the course of ages have shaped the outline and the profiles of the head to what we now see.

Its general superficial outline may be assumed to be due to prolonged subaerial and later marine erosion, possibly aided at some period of the process by the friction of land or floating ice—but in the last instance, and ever since its first emergence, to a long-continued and extremely tranquil elevation and concurrent secular subaerial degradation and deposit, under the never-ceasing alternations of wind and rain, heat and cold, and vegetation.

The isthmus appears to consist of ancient sea-bottom, gradually raised, and denuded in the process by currents or tide-wash, and then augmented by beach-accumulation and blown sand, each apparently derived from the eastern bay.

The western limit of this strip of land has probably undergone considerable waste. Its beach, especially where it lies nearer the open sea, is covered with boulders of all sizes, the remains of Glacial clay, to be described in the sequel.

The details of the superficial marine and subaerial beds of the Head and the isthmus are the subject of this paper. As the following notes contain the results of many examinations of each point, they will be arranged at once, in the order of the presumed antiquity of the successive formations, under the following heads:—

I. Marine formations and old land.
  A. Sea-bottom.
  B. Sea-beach and beach-marks.
    b. "Pholas"-burrows.
  C. Clay and "sunk-forest" bed.
II. Subaerial deposits.
  Detritus in clay and earth slopes (including traces of occupation by animals or men).
III. Refuse-heaps and other relics of recent human occupation.

I. *Marine Formations.*—A. *Sea-bottom.*

i. Apparently the earliest of the superficial beds hitherto discovered is a remarkable deposit of yellow or white clay, which is to be seen, as to its chief exposure on the Head, at a point about 350 feet above the sea, near Gwydfyd farmhouse, at the upper part of the open glen between the north-eastern point of the Head and Pen y Dinas on the south-east, where it is worked in a quarry for exportation. This bed consists of siliceous sandy clay of very fine texture with a large proportion of chert fragments, and lies here (as similar beds do on the Little Orme's range) in a hole or pocket of the Limestone rock. There the boulder-clay has been traced overlying the deposit. No fossils have occurred to mark its age; but it has the appearance of being *in situ*, and of having probably been deposited after the limestone had taken its present form.

It has been fully described by a good geologist, Mr. Maw (Geol. Mag. ii., 1865), from whose account the foregoing description is abridged.

Another, but less important, patch of the same deposit may be traced in a very similar position, namely, higher up the larger valley of the "Old road." Mr. Maw did not find chert bands on Little Orme's Head; nor have I been able to detect any in the limestone of the larger hill; but there are traces of a cherty stratum amongst the grit-stone rocks which form the east end of the Telegraph-hill ridge.

ii. The next of the deposits are glacial and boulder-clays of a late age. These appear in four distinguishable forms.

a. Probably the oldest of these is that described by Mr. Bonney as a bed of tenacious dark-blue clay, full of small pebbles of a dark slaty rock, rising 1 or 2 feet at the foot of the western cliff of the isthmus, and traceable for some distance below high-water mark. It may be that this and the following bed (*b*) are the same, seen under somewhat different conditions.

A not dissimilar bed is well seen in the sections of the railway-cuttings near Bangor Station. That bed occurs in two strata, one full of small stones and coarse sand, and the other, an upper layer, with more clay and finer sand, and containing a few fragments of shell too small and too much worn to be determinable. It is, however, at a considerable elevation above the sea-level, and is not overlain by any boulder-clay.

b. A close, greyish, olive-coloured, rather sandy bed, with rounded stones of various rocks, comes up on the beach, and occasionally at the bottom of the sea-cliff, on the west side of the isthmus. It is apparently without traces of shells.

c. A chocolate-brown clay of close texture, with boulders (some scratched) and seaworn pebbles distributed throughout, and occasionally, but very rarely, slightly bedded, forms the general base of both eastern and western cliffs. This bed was best seen on the beach and at the foot of the low shore cliff in the eastern bay, where are now built the new stone wall and boat-stairs opposite Ty-gwyn in Llandudno.

At this spot I have found characteristic fragments of

Tellina solidula.
Mactra solida.
Astarte arctica.
Cardium edule.

The bed appears, but not so well marked, and not exhibiting fragments of shells, at the foot of the western cliff over the last-named and passing upwards into the next-named bed.

The local exposure of strata in the western cliff is very various, and alters considerably, as each year exhibits a fresh face or, under the influence of rain and wind, a fresh disguise. Inch-measurements are of very trifling value.

This third bed appears at the bottom of the ballast-cutting behind Colwyn Station, four or five miles east of Llandudno, under sandy clay and old beach-shingle. At that place I found good fragments of *Saxicava, Astarte arctica,* and *Cardium edule.* The *Astarte* is distinctly a shell of Glacial age, and of common occurrence in the Moel Tryfaen beds, near Caernarvon.

d. Upon the brown clay is a very similar bed of lighter-coloured, reddish, looser clay, with rounded and angular stones. It is exposed on the western cliff to the thickness of 20–30 feet, and in most of the superficial cuttings on the isthmus, as at the brickfields and in the new road from

Diganwy Hotel, past Bryngosol, to the main road. In the west cliff, fragments occurred of

Mya truncata.
Tellina solidula.
Mactra.
Cardium edule.
Mytilus edulis.
Buccinum undatum.

Collectors must be careful not to confound with genuine fossils from this bed the weathered but not worn fragments of *Mytilus, Cardium,* and Willow-pattern, which not unfrequently occur on the face of this section, and are due to tillage of the surface and some rain-wash.

This seems to be a true boulder-clay. It is just distinguishable at Llandudno from the bed below, whose *Astarte* it does not appear to share. The bed with similar fragments occurs at a 70-feet elevation above the bath-house. I may mention a very fine section of it near some white cottages on the road-side, at the eastern foot of Penmaenbach, to the west of Conway, showing, besides the usual promiscuous assortment, one fine horizontal bed of large boulders high up in the clay cliff. From the same bed, at a brickfield at the foot on the west side of Penmaenbach, I have obtained worn shells of *Littorina littorea* and *Fusus antiquus.* Both of the Llandudno beds are represented, as Mr. Binney has pointed out, in the Blackpool cliffs.

The greater variety of species which figures in the Lancashire list* may be attributed to the occurrence there of larger beds of sand and shingle, apparently deposited under littoral (or at all events shallow-water) conditions very different from, and possibly somewhat more recent than, those which alone can have prevailed amongst the rocky islets of the two Heads. The fragments from Llandudno are much more worn than at Blackpool. Mr. Maw identifies the boulder-clay as recurring to the south-east of the Head at a height of 170 feet in the Gwydfyd valley, and says it forms a terrace about the same height on the south side.

* See Geol. Mag. ii. 298, 1865.

I have not been able to satisfy myself of the existence of this bed elsewhere than at the side of the Gwydfyd valley, not far from the Bath-house, undisturbed; but the occurrence of travelled boulders and pebbles in subaerial beds, to be described presently, records its former existence at even higher levels than that just named. Thus, a single boulder of greenstone was found amongst angular fragments of limestone, talus, and superficial clay above Gwydfyd farm, at 380 feet above the sea.

The foregoing deposits are those of sea-bottom,—the boulder-clay telling unmistakeably, in its great rounded or angular stones, of the beach-gravel carried along by the travelling ice of the drift ocean, or the *débris* of the glaciers of the rising land, lodged in the depths of a more or less profound sea. The beds have probably successively suffered great vertical denudation during the process of elevation, and they are still subject to considerable horizontal waste from the west. The beach is covered with great boulders of greenstone, granite, slate, and other rocks, polished and sometimes scratched. The same boulder-clay appears in the shore cliff at the west end of Little Orme's Head. If I may offer a conjecture as to the period of the deposit of the upper or boulder-clay beds, I would say that it took place towards the close of the Glacial epoch, and was due to the redistribution of the more ancient northern drift, the remains of which it now covers, under the influence of the elevation and of the descent of glaciers from Snowdonia (in whose recesses the *Astarte* has been found on beaches), very long after the vicissitudes of the earlier period had removed much of the superincumbent rock (already shaped by rain and frost before its first submergence) and had left a rocky sea-bottom filled up with Glacial clays and at the time rising and, though still under water, shaped much as we now see it.

I will presently give reasons for thinking that the actual elevation of each Head above the water-level has taken place since those seas bore icebergs, and in fact at a time more recent than the prevalence on this particular spot of land-ice.

There are not wanting certain indications which may possibly imply a former continuity between the boulder-clays of Llandudno and Penmaenbach; but this question and the consequent hypothesis of a subsequent diversion of the course of the Conway river from an ancient debouchment to the eastward into its present estuary do not fall within the scope of the present essay.

B. *Sea-beach and Beach-marks.*

In order of antiquity I take the highest first. Mr. Binney (Manchester Geol. Soc. Trans. 1861–1862, p. 99) has described what he calls a singular bed of shingle at an elevation of 400 feet from the sea-level, at a point on the "old road," a little to the east of where the road to St. Tudno's Church branches off. This is a very peculiar deposit, and will be discussed in detail in the sequel.

There are to be seen on most parts of the Head, where the limestone rock is bare, large isolated masses of the like stone, more or less rounded and weathered. One of these has been figured by the Rev. T. G. Bonney (Geol. Mag. iv. pl. xii.) apparently, though the statement is not definitely so framed, as an indication that the surface of the higher ground has in many cases been affected by ice. These blocks occur in considerable numbers on the north-western region of the Head. They lie on the bare rock, sometimes conformably, but oftener not, and are of course much weathered. Some have already been subjected to the more obvious disintegration of frost-splits, and lie in fragments. There are two characteristic blocks of this kind near the eastern ends of two reefs of rock which cap the more precipitous portion of the southern declivity.

It seems to me out of the question to regard these blocks as perched blocks. They are uniformly of a stone apparently identical with the beds in their immediate neighbourhood, and may often be connected with a neighbouring reef of rock, both by the character of the stone and by the style of wearing. A few may be found actually *in situ,* as isolated pillars or tables, not unlike the Brimham rocks, near Ripley, or the rocks at Edale Head, in the Peak, described by Mr. Hull.

I have not found anywhere at the higher levels one block of any other rock than mountain-limestone.

Moreover it is not easy to say whence these blocks could have been transported. The mountain-limestone in Puffin's Island and Anglesey, to the west, is from 300 to 400 feet lower down, and generally, I think, of somewhat different character. That of the eastern portion of the range of this formation (namely, at Penmaen Rhos, to the east of Colwyn) is also somewhat different in mineral character. I know of no other facts to support such a variation of the scheme of the glacier-system of North Wales as would carry blocks from east to west across a gap like that of Rhos Bay. There is no limestone between the Head and the Silurian centre of that system in Snowdonia.

iii. It appears to me, therefore, that these loose and fixed rocks have no glacial origin, but are simply the remains of the play or the rage of the waves about the head of the new-born island, and, as such, emphatically raised beach of the grandest kind. Similar masses may be seen on the present beach, and on many a bare scar between tides at the northerly foot of the Head.

As has been already noticed, these blocks occur in great abundance amongst the angular fragments that form the great talus-heaps all round the Head, mixed with angular masses of more recent fall.

But these plateaux furnish yet other indications of marine beach-wear. The strata of the Head are more or less horizontal. Some beds are extremely hard and compact, and apparently suffer superficial degradation only in the very slightest or, at any rate, most uniform measure. Others weather rapidly, and disintegrate to a considerable depth, so as to shatter easily under the hammer. There are several places near the highest levels of the hill where a hard layer has been stripped clean of all the superincumbent stone except the beach-boulders just referred to. The wear of these flats is various. Sometimes (as, for instance, on the hill at the south of the lighthouse) the surface is cut and furrowed, and worn in fissures, potholes, and other forms, very similar to those of the like beds in like position in the intertidal spaces below. Whether it be that there has been no perceptible wear since the waves covered this rock, and the lighter touch of wind and rain has only corroded surfaces that the denser and more constant fluid carved, or that we have in this sculpture nothing more than the effect of the rain of ages, it seems probable that in the bared stratum we see only an ancient scar or tidal level. I submit that, so far from proving ice-action, the blocks, and still more these flats (if indeed tidal, or perhaps whether tidal or not), in truth discredit that supposition.

A similar, or even more marine-looking, horizontal scar of apparently tide-worn rock forms the highest plateau of the Little Orme's Head, and carries the like proof there.

In my next section I shall adduce still other evidence that these hill-tops have not been ice-clad since they were last below the sea-level.

Other indications of long-continued tidal action, if not exactly in the shape of beaches, are abundantly to be seen in the actual vertical outlines of the cliffs which bound the

Head on every side. In many parts of the northern exposure, the profile of denudation is less altered by falls and less hidden by talus than elsewhere, and in its alteration of cliff and scar is marine from the present beach to the very top. So also are the grand curves of successive contour-lines of the Head.

iv. On most of the vertical precipices a somewhat more minute examination and a favourable light will detect more than one of the peculiar wave-worn grooves, or longitudinal caves, which record a long continuance of marine wear at high-tide level along a stationary cliff. These occur at various heights, and are often still horizontal for considerable distances.

On the fine inland cliff that overhangs the footpath on the west side and north of the Gogarth ruins, there is a remarkable concurrence of two lines of wave-hollow. One, still horizontal, and cutting right across the inclined lines of stratification, tells of a long pause in the upward movement of the Head. The other, starting at one point from the last-mentioned groove, follows with equal distinctness the line of a particular layer of stone. If the observation be correct, and if the latter excavation be not merely atmospheric in a softer stratum, we must suppose the quiescence of the upper wear to have been succeeded by a period of alteration of level of great duration and uniformity. The latter feature is further implied in the undisturbed horizontality of many of these old tide-marks. A magnificent specimen of this cave of high-tide erosion may be seen at the foot of the Little Orme's Head.

These wave-marks are mostly inaccessible, and I have not taken the height of any. It would be worth while to study them in detail, especially for some one who could aid his observations with exact lithological knowledge. In some cases the weathering of a particular bed forms a groove which close inspection alone can distinguish from the wave-worn hollow. On the south-eastern inland cliff, the wave-marks seem to have suffered considerable dislocation, as if this part of the hill had undergone a more violent elevation.

v. Distinct remains of raised beach are to be seen at a spot on the east side of the Head, near the bath-house, where masses of shingle hang cemented to the face of the rock, at a height of about 50 feet above the sea-level.

vi. Lastly, there lies upon the boulder-clay of the isthmus, and especially developed and visible in the cliff-section of the western shore, a deposit, varying in thickness at different points along this line, from a few inches to 3 or 4 yards, of coarser or finer beach-shingle, sometimes mixed with a little clayey material. The bed is seen at a lower level in foundations in Mostyn Street (where it yields abundance of mussel-shells, double, as if dead *in loco*), and may be continuous with the present beach-shingles of Llandudno Bay. Its constituent pebbles are not dissimilar to those of the recent beach, though there often seems to be a somewhat larger proportion of stones from granite, Silurian, or trap beds. The beach itself is now chiefly supplied from the limestone heads at each end of it, or one of them.

vii. Over this beach-shingle lies, in many places, more or less of drift sand. I have heard it stated by Mr. Thomas Glover (who has been familiar with the locality and with such observations for fifty years past) that within his own recollection a large expanse of low ground was, forty or fifty years ago, an impassable swamp, and has since been converted into solid ground by this drift sand, and is now overlain by it.

b. *Pholas-burrows.*

viii. I have reserved for a separate section, in order to present this series of facts together, another class of unmistakeable traces of beach-wear—the holes made by burrowing mollusks, necessarily made while the rock in which they appear was near, if not below, low-water mark. It is convenient to designate these holes as those of *Pholades,* which they most nearly resemble.

There is so endless a variety in the forms in which atmospheric erosion marks limestone rocks in positions of ancient exposure, that under some circumstances it might not be difficult for a geologist unfamiliar with the habit of the living animal either to overlook, as the effect of inorganic destructive processes, what are true *Pholas*-holes, or, if seeking these, to mistake those for them. A working naturalist is not so liable to this error; and, insisting on a perfect symmetry of the hole, and the grouping of several holes in close proximity, he will more readily detect the true molluscan excavation, and distinguish it from structural decay.

I think I may venture, on the ground of a considerable familiarity with such holes, to assert that there is the greatest probability in the identification of the series of holes I am about to mention.

Owing, I presume, to the situation of the Head, and the want of an assistant observer, I have found it very difficult to ascertain measurements of elevation. The heights I give are in every case the mean results of at least duplicate observations; but I cannot speak quite confidently of their exactness. *Pholas*-holes occur frequently at various heights all over the Great Orme's Head where the limestone is of a sufficiently compact texture, and certain conditions of shelter from atmospheric wear obtain.

They are found either in the rock (where they are to be sought near the edges and on the undersides of projecting slabs, *in situ* or fallen), or in larger or smaller detached, rounded, sea-worn blocks of stone, such as lie on or in the sward on many of the grass-grown slopes of the hill.

These stones doubtless once lay at the feet of the now overhanging cliffs amongst the breakers, and in most cases may be assumed to have fallen from a level somewhat above that of the places where they now rest.

All the holes, that I have seen, occur in surfaces which have obviously suffered some superficial waste. Hence the smaller holes, which usually crowd the surface of a burrowed stone off a recent beach, have generally disappeared, and the fossil holes which remain are the ends of the larger perforations.

The list following is purposely arranged along a rude line of profile of the Head, from the north-east over the Telegraph-hill to the precipices which overhang the isthmus on the south.

1. At the stile above the archery-ground, in the footway up to Gwydfyd farm, are two large slabs, each showing several fine *Pholas*-holes. One is on the ground below; the other forms the coping of the westerly pillar of the stile. Height 216 feet.

2. In the cutting made to remove overlying clay above the yellow clay-pit at Gwydfyd farm, a large cubical boulder of limestone, measuring from 3 to 4 feet each way, and nine-tenths buried in the superficial clay and humus, afforded a very perfect group of three large burrows, with part of a fourth. The largest hole is 4″ deep, and 1¼ in diameter at its widest part. These holes were just visible above the sward. It will interest naturalists to note that each was tenanted by a large *Helix aspersa*. It is not quite safe to pronounce upon the species of *Pholas* without the shells; but, from the shape of these three holes, which are singularly well preserved, I am inclined in this case to suppose that they were made by *Pholas crispata*, a species of well-ascertained quaternary occurrence*. Height 350–360 feet.

2 *a*. The same spot afforded another, smaller stone (2 feet

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* In the cases thus marked, specimens were exhibited.

× 1½ × 1), in one flatter surface of which, in and about a teacup-like depression, were 10 well-marked holes, of a smaller and more cylindrical form than the last mentioned. Much of the surface of this specimen has weathered away; but one hole, ¾″ in diameter, still measures 1½″ in depth*.

3 and 4. This Gwydfyd excavation lies at the foot of a vertical cliff, which, however, towards the west runs out into a rugged but still accessible hillside, with numerous exposed ledges of rock *in situ*. Well-preserved groups of holes of the narrower type occurred at 370 feet and at 435 feet above the sea-level.

5. After passing over the eminence, and having gained the roadway that leads from the mine-plateau towards the Telegraph-hill, one reaches a mass of limestone rock, the first that breaks the sod on the left of the path as it skirts the northern slope of that hill. In this were to be seen several holes, well formed, and, though exposed, preserved by their downward opening*. Height 440–450 feet.

It is important and instructive, in this rock, to distinguish the genuine *Pholas*-burrows both from the irregular burrows of aerial erosion in the same mass and from the wind- and rain-worn (or perhaps even sea-worn) honeycombing that marks the outcrop a few yards higher up on the path-side, of a mass of superincumbent gritstone.

The Telegraph-hill consists of this gritstone, and yielded no burrows to my search.

Walking southward from the Telegraph-hill, one passes first a wide plateau, and then the edges of successive strata cropping out like reefs or low sea-cliffs, the third or fourth of which all but overhangs the isthmus. In the plateau a reef is just traceable through the grass, running southwesterly.

6. Here occurred, amongst weather-worn beach-pebbles of limestone, a fragment with two holes*. Height 570 feet.

On the more exposed reefs from the south the holes are not infrequent. They occur in the edges of tables of rock still *in situ* or broken off and lying in the sward.

7. On the second or third reef (marked by a loose spherical boulder at its eastern and higher extremity), a fallen slab of very hard stone showed in its underside 27 holes, all of the narrower form, and one 2″ deep*. Height 570 feet.

8 and 9. The next reef yielded good specimens at 540 feet; and the one below, others at 520 feet.

Leaving the eastern precipices of these reefs by a path which skirts a long, enclosed field on the north side, and leads to the mining-works, one passes a great egg-shaped boulder 6 or 7 feet long, lying on the sward. This, with the true *Pholas*-holes, exhibits also, from its exposure, many of the tubular and semitubular corrosions which might easily be mistaken for the remains of burrows. It shows also a very perfect polishing of parts of its surface, which I have heard described as glacial, but which, in truth, is only the effect of the secular attrition of *Ovis aries*.

10. As a further illustration of the distribution of these *Pholas*-burrows, I may mention that they occur in considerable abundance and remarkable preservation in various parts, and especially in the topmost plateau, of the Little Orme's Head. This flat looks sea-worn, as if, except for lichenous growths, but yesterday tide-washed; and I do not exaggerate when I say that on that ancient scar I seldom failed to find the holes, on examining any piece of rock which seemed to me such as, on a modern beach, would have afforded suitable habitat for the mollusk. I exhibit two very characteristic groups of six and nine holes respectively*. One splendid specimen with seven holes, each more than 2 inches deep, was unfortunately destroyed in the process of quarrying it out.

In the preservation of these burrows in loose beach-stones, and in the edges of the tables of outcropping strata, and in the level sea-stripped scars, I submit we have final and irrefragable proof that the surface of these anciently submarine hills had not been touched by the iceberg or glacier for some time before, nor at any time since, they respectively emerged from the waves.

I may add, that I have not been able satisfactorily to identify any rock-surface as moutonnée, and was equally unsuccessful in finding the rock which appeared to Mr. Binney to have been scored and polished, probably by ice, below the bath-house. Limestone is peculiarly ill-adapted for the preservation of these superficial markings, and especially so when within the reach of the waves and the fret of a shingle beach.

C. *Old Land.*

ix. Between tide-marks on the western shore, near to the foot of the Head, I saw, some years ago, when a heavy tide had removed the sand and shingle, a light-blue clay without stones. This is, I believe, the same bed as appears at the like level between Little Orme's Head and Colwyn, and again between Abergele and Rhyl to the east, and to the west between the hills Penmaenbach and Penmaenmaur.

x. At Colwyn and Penmaenbach it is covered by a vegetable deposit of flags, leaves, and nuts, with prostrate stems and the roots of large forest trees. Traces of this vegetable deposit were, on the occasion I refer to, to be seen on the Orme's-Head exposure. This bed, and the vegetable bed above it, apparently testify to a slight and very recent depression of the land.

The Abergele beds prove a slight subsequent elevation, by containing many fossil colonies of *Tellina solidula, Scrobicularia piperata,* and *Cardium edule.* From the vegetable bed at the Penmaenbach-brickfield cuttings, where part of it appears as veritable peat of very modern condition, I have received bones and a tooth of an ox, and a very large horn of a red deer. A similar horn was obtained between Abergele and Rhyl while the railway was being made.

II. *Subaerial Deposits.*

xi. On each side of the Head, wherever a section exposes the structure of the grassy slopes that conceal the bases of every inland cliff, there appears a mass of unstratified, reddish, calcareous clay, with more or less frequent, larger or smaller, angular fragments of the limestone of the overhanging rocks. These fragments are not seldom, especially at the lower part of the layer, so numerous as to form a positive breccia. There occur throughout this bed, and in some places not unfrequently, pieces of such stone, bearing in soft superficial curves the marks of sea-wear. Here and there also, even at considerable elevations, on both north and south sides of the Head, it yields well-rounded larger or smaller pebbles of greenstone and other non-calcareous rocks.

xii. On most of the less elevated or less steep declivities, the deposit last indicated generally passes upwards into, and is covered by a very similar layer, which, however, is distinguishable by a darker colour and more earthy consistency. It contains the like angular, and occasionally the sea-worn fragments of limestone, and passes upwards gradually into humus and the sward. In this upper layer it is noticeably the case that the flatter fragments of stone are usually disposed on planes about parallel to the slope of the sward. It is this which Mr. Binney traced as blackish-brown clay with angular fragments, up to 262 feet above the sea.

The position of these two beds at the foot of sea-worn cliffs, the almost total absence of rolled stones, and the entire want of tidal or current assortment of material, and the great inclination of the slopes preclude the possibility of ascribing these beds to the action of the sea on the sides of the rising Head.

On the other hand the predominance of angular fragments, their promiscuous aggregation, and the slight bedding along the line of surface mark the whole as subaerial talus. The formation is modified in local character by the greater or less gradient of the slope, and latterly by the decrease of that inclination and the increasing luxuriance of vegetable growth of grass and bushes.

The clayey material is probably principally derived from the decomposition of the limestone rock*, modified and perhaps augmented by vegetable growth, secretion, and decay; but the occasional occurrence of rounded pebbles of other minerals probably indicates a redistribution, under the like influences, of patches, at least, of boulder-clay.

It is in one or other, and especially in the latter, of these two beds that observers have frequently noticed the occurrence of shells of the species *Mytilus edulis*, *Cardium edule*, *Ostrea edulis*, *Patella vulgata*, *Littorina littorea*, *Purpura lapillus*, and *Buccinum undatum*.

Mr. Binney mentions having tracked these shells up the hill towards the bed of shingle at a height of 400 feet, and deduced proof of the elevation of the Head at least that height during a very modern epoch.

In the same year Mr. Sidebotham (from whom I received my first specimens) exhibited extensive collections of these remains; and he and others have since extended his observations. I believe Mr. Dancer was the first of our members to note the occurrence of these fossils.

In 1867 Mr. Bonney described (Geol. Mag. iv. 289) a deposit of shells, near Gwydfyd farm, from the uppermost bed.

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* I am told that this limestone is noticeably argillaceous in character.

With regard to these shells, it is to be observed that they occur either massed in heaps or layers (generally of one or two kinds together), or more sparsely distributed throughout the bed. They are all of species eaten by birds or man. All the specimens are full-grown, or at least of edible size. They are nearly in every instance whole, the exceptions being the *Mytilus*, a shell peculiarly liable to disintegration, and the *Littorina*, which often occurs with a broken outer lip, as if the shell had needed this fracture to allow of the extraction of the animal. There are neither any other species, nor any young specimens of those that do occur.

Lastly, the clayey detritus cannot have been a habitat for any one of these species, even if it were of marine deposit, which it is not; nor would it be possible for the shells to mass in the patches in which they sometimes occur, at the foot of sea-washed cliffs, or in fact, I venture to say, under any conditions of original natural deposit; or if they did, to remain so, on an exposed beach or a sea-bottom, during a protracted period of elevation through the tide.

The fact of elevation is undoubted; but these shells are no evidence of it.

It is not necessary to give separate lists of the particular lots of these species which occur at different heights. They or some of them have been found in the eastern, northern, western, and southern slopes, and especially in the cuttings of the "old road" up the hill, and the Hen Dafarn road towards the east. On these lines they have been noted at 120 feet, at 140 feet, at 180 feet, at 200 feet, at 300 feet, and at 315 feet above the sea.

On the old-road sections, especially in the upper levels, the shells are not unfrequently accompanied by teeth and fragments of bones of Mammalia and bits of charcoal.

Thus, at 126 feet, there occurred to me part of the jaw of a pig; at 186 feet, sheep's teeth and frequent fragments of bone; at 366 feet, the molar of a horse and many fragments of bone. At 315 feet, on the north side of the road, along with *Patella* and *Littorina,* were found a cow's tooth, part of the jaw and other bones and several teeth of a pig, fragments of other bones, bits of charcoal, and two human molars.

At 365 feet there occurred fragments of bone. The bones have all lost much animal matter.

Upon the whole it would seem that all these remains are most probably relics of the inhabitation of the Head by birds or men. The shells are all of such kinds as sea-birds often carry ashore for food. I have myself found limpets and periwinkles on the sea-birds' crags, and on the highest sward, all of which had been so transported.

In Connemara I have often traced shells of the very same kinds on the hillsides, to or from a heap outside a cabin or the site of one, on or under the sward, according to the antiquity of the deposit.

The bones probably belong to the human period. The shells, lying on the already elevated ledges of the cliffs, or the shells and bones cast abroad outside some cabin, would in the course of years be distributed down the declivity of the hill along with, and in process of time be buried with and under, the superficial detritus. Wind* and rain, and chance footsteps of animals or men would assist in scattering such objects.

If this be a correct explanation of the occurrence of these shells and bones in the clay with angular fragments,

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* The power of wind in exposed places to move even heavy shells ought not to be overlooked. On the sand-flats at Tunara, north of the Spanish lines at Gibraltar, I have watched a Levanter roll along the heavy shells of *Pectunculus* and *Cardium tuberculatum,* and even a massive valve of *Panopæa glycimeris.*

it is probable that their original deposit on the Head is of very ancient date. I may mention that once when I was collecting specimens, an old Welchman remarked that "Irishmen eat them."

There are on the central plateau certain traces of foundations, which the same man pointed out to me as Irishmen's houses. I have no means of fixing a meaning or a date to these references. They may represent either a comparatively modern, or, under vague disguise, an ancient tradition. The fact of the occupation of the Head for mining-purposes in very early times is well established.

It is by no means impossible that a close search or a fortunate accident might discover some cave of ancient habitation in the valley of the old road. If the opportunity should occur, it should if possible be fully and carefully made use of by competent observers. Some years ago a skeleton of a man was found in a stalactitic deposit in a cave on the western face of the Head; but I think it was not shown to be of special antiquity.

xiii. Before I pass on to the undisturbed deposits of human origin, I may, in order to complete this sketch of the superficial formations of the Head, mention a small patch of shells which I found water-worn into a crevice on the Western Cliff. After washing off the clay matrix, there remained, along with some bones of a frog, shells of the following species:—*Zonites cellarius* and *crystallinus, Helix aspersa, nemoralis, caperata, rotundata,* and *pulchella, Pupa umbilicata, Clausilia rugosa, Carychium minimum,* and *Cyclostoma elegans,* comprising, I believe, all the terrestrial mollusca of the Head except *Helix virgata, ericetorum* and *rupestris, Bulimus acutus,* and two or three slugs.

III. *Refuse-heaps and other Relics of Human Habitation.*

xiv. Mr. Bonney's bed of shells at Gwydfyd pit has already been mentioned. He found there the *Mytilus, Ostrea, Patella,* and *Littorina,* with three valves of a *Tapes,* which he doubtfully identifies as *pullastra,* in clay 2 to 3 feet thick, which appeared "not to have undergone much denudation during the process of upheaval."

Mr. Maw has already pointed out that this deposit is probably of human origin. There is no doubt of it. The deposit was some 2½ feet thick, and was entirely within the uppermost layer of detritus, the blacker earthy clay. My own collections on this spot have yielded :—*Tapes decussata* (not *pullastra*) in pairs and valves, whole and broken, all full-grown, and some large; *Cardium edule,* whole and broken, some large; *Mytilus edulis, Ostrea edulis, Patella vulgata*; *Littorina littorea,* very abundant, very large, and very fresh-looking; *L. littoralis* and *Purpura lapillus.*

The *Tapes* is still gathered in Llandudno sands by the natives of the Head for food.

No bones nor any implements occurred. The whole deposit has unfortunately been removed in the enlargement of the clay-pit.

xv. A very extensive midden or collection of such deposits has long been exposed at the foot of the slope which connects the Inland Cliff at the south-western angle of the Head with the Shore Cliff. For several years past it has been open both on the shore section, and also superficially on the slope above, by means of a large cutting. The whole group of deposits apparently extended on the slope more than 30 yards from the shore, and along it more than 150.

Mr. Bonney has published (Geol. Mag. iv. 343) full observations on this mass, especially as exposed in the sea-cliff section.

The remains consist of layers or heaps of *Mytilus edulis, Patella vulgata, Littorina littorea,* with some oyster-shells, a few *Purpura lapillus,* and at least one heap of *Buccinum undatum.* At the lower edge the heaps yet remain massed. Above they are more distributed on the slope.

Bones and teeth of ox, sheep, and roebuck, and bones of some birds occur. I have found at various times burned bones, and once a piece of red earthenware, but hitherto no utensils of any kind. Most of the bones and shells have a very recent appearance.

xvi. Lastly, Mr. Bonney (Geol. Mag. iv. 292) mentions as occurring at intervals in the sand cliffs on the western or Conway-Bay shore seams of *Mytilus edulis,* which he has assumed to indicate "a period of depression during which the mussel-beds were formed" and of subsequent upheaval.

I am afraid those mussel-beds must be set down as appertaining to the most recent human period. A correspondent of 'Loudon's Magazine of Natural History' for 1830 describes the taking of large quantities of mussels and their being boiled to get the fish out, and the searching of the latter for pearls. He then says, "the huts which have been erected for convenience of boiling the fish are on the extremity of the marsh about a mile from Conway," on the west of the estuary. There are still to be seen at that spot great beds of mussel-shells, which are so abundantly distributed in the sections and on the surfaces of the sand dunes as to give them a blue colour distinctly visible in suitable light to a great distance. Mr. Glover tells me that he has often watched the process in the eastern bank of the river, *i. e.* making Mr. Bonney's beds.

The shells are not aggregated as if dead *in loco,* but consist of separate valves confusedly massed in patches too limited in extent, and often too thick vertically, to bear any comparison with the mussel-beds of the Bay.

There is a spot near Tywyn, between the railway and the beach, where the business of pearl-seeking is yet pursued. The pile of mussel-shells is in itself a remarkable object.

As zoologists describe apart, as "incertæ sedis," a specimen which they are unable to classify, I have kept for special description Mr. Binney's bed of shingle before referred to.

This bed is described (Manch. Geol. Trans. 1860, i. p. 97) as a deposit of fine sandy shingle of small slate or Silurian gravel, about 2 feet thick, mixed with larger pebbles of well-rounded white quartz and chert, and resting on still larger pieces of limestone, which are angular. The shingle at first appeared to its discoverer to have been brought up for the purpose of repairing the road; but the rounded pebbles of quartz and chert, and the fact of his having traced the shells all the way up the hill from the sea-beach, convinced him that it was a natural deposit. In it occurred *Mytilus edulis* and *Ostrea edulis*, *Patella vulgaris*, and *Littorina littorea*.

In connexion with some other raised-beach observations, I have studied this bed, year after year since 1861, with unusual care, and I confess I am not able to give hearty assent to its marine character.

It appears on the south side of the old road, in a low section, at a point a few yards below the line where the mine-plateau breaks into the declivity of the old-road valley. As worn by rain, it has always appeared deeper in the section than it really is. I have never with a spade found it so much as a foot thick. It is composed of very beach-like small shingle, and contains a certain proportion of larger rounded pebbles varying in size from a hen's egg downwards, and also some of the angular fragments of limestone *débris*. About a third part of the smaller material consists of very coarse sand, with undistinguishable worn bits of shell. It contains much-worn fragments of the following species :—

Pholas crispata.
Mya truncata.
Tapes decussata.
—— virginea.
Cardium echinatum.
Mytilus edulis.
Modiola modiolus.
Ostrea edulis.
Pecten varius.
—— pusio.
Patella vulgata.

Trochus cinerarius.
Littorina littorea.
—— rudis.
—— littoralis.
Purpura lapillus.
Buccinum undatum.
Fusus antiquus.

Balanus (3).
Serpula.
Clione-holes in bits of Ostrea.

Every species occurs only in very small fragments, that pass the half-inch sieve and are very much worn—except only *Mytilus* and *Patella* (which occur of adult size in more or less sharp-edged fragments), *Ostrea* (which occurs as very old worn valves), and *Littorina littorea* (which occurs either as very worn fragments, or whole, full-sized and very fresh, like the specimens from the superficial clays lower down the hill). A certain proportion of the mussel-shells are filled with a hard substance most like mortar.

The bed lies under loam about a foot thick, and upon the lower superficial clay with angular fragments described above; and, while it follows the general inclination of that bed, it thins off below and above, as seen along the roadside within a space of 10 yards. It has scarcely any appreciable extension from the roadside backwards, and does not appear at all 3 yards from the section, nor on the other side of the road. There appeared to me a sort of horizontal arrangement of the larger pebbles. Of these a very large proportion (twelve to sixteen out of twenty) were of white quartz, chert, or limestone, of a nearly uniform size, about as large as a hen's egg. No other sign of bedding was discernible.

The shingle is certainly very beach-like, but it is peculiarly assorted and considerably less uniform in size than is usual within so small a space. It is mixed with angular fragments of limestone.

A lump of wood-charcoal occurred, which broke into small pieces on removal. Either from this substance or from some other cause, the deposit was, without being mixed with the earth above, particularly dirty to handle.

The fragments of shells, especially of *Mytilus*, form a considerable proportion of the deposit. Except those of *Mytilus* and *Patella*, they are all very much worn. Of those two species they are very little worn, many edges being quite sharp, with little sign of beach fracture or beach wear. There are no fragments of young shells, nor any small species.

The bed is situated on the breast of the incline of the old-road valley, and at about the middle of a transverse section of that hollow—that is to say, in a position of maximum exposure to degradation, whether by the waves of a retiring sea or by atmospheric action and the superficial drainage from the plateau.

It is upon the lower clay with angular fragments, which I suppose to be of subaerial origin. It has been subjected to very considerable redistribution of comparatively recent data; for the small shingle is traceable, not as a bed, but loosely dispersed amongst the superficial clay with mussel-shells, periwinkles, and bits of bone, for a long way downhill in the sections on the side of the old road; but this drift does not appear to extend into the lowest layer of the superficial clay.

Now there is visible in the sward behind the sectional face of this bed a trench which looks most like the remains of the foundations of a building about 4 or 5 yards square, as if the stones of such an erection had been removed.

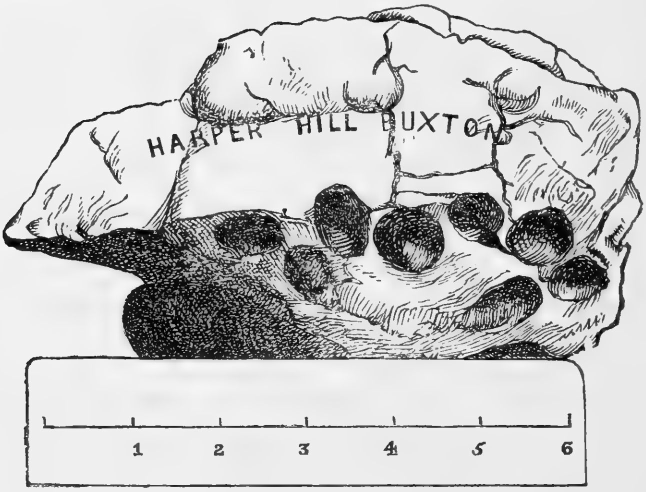
Upon the whole I am inclined to think the bed in question the result of some domestic arrangement connected with that hut, the shingle having been brought up for some purpose from the beach, and perhaps augmented by the refuse of mussel-baskets. The *Mytilus* and *Patella* seem to have been crushed by being trodden on. The larger pebbles may have been brought for pavement, the white ones selected for ornament, as is still the custom in Wales and elsewhere.

I am extremely unwilling to pronounce against the genuineness of a bed which Mr. Binney has described as raised beach, and do so with the greatest hesitation; but, to say nothing of negative probabilities, the shell-remains do not look like fossils, nor does the bed look like a beach-bank. If raised beach could be preserved in this spot, it might be expected that something of the kind would occur at lower levels of still less antiquity; but no sign of it has occurred to me.

I may mention that in order to test the mussel-refuse conjecture I made the following experiment. I got a sackful of mussels from a bed in Conway Bay, just as they are now picked for food, and carefully washed off the fragments of shells and small stones which were amongst them, attached to the byssus and otherwise entangled in the mass. These were sorted and put side by side with the result of a similar classification of the constituents of the bed.

The mineral portions, small shingle and coarse sand, were scarcely distinguishable, except that the stones from the sack were more or less coated with a film of vegetable matter. Of the twenty-three species of the bed, eleven occurred amongst this batch of mussels, presenting very similar conditions of fracture and wear. I presume that the decay of the vegetable film would account for the dirty feel of the shingle which I have referred to above.

Scale of inches.



Scale of inches.

NOTE *as to "Pholas"-holes at high levels in Derbyshire, from the Proceedings of the Society,* 1*st Oct.* 1867.

Mr. Darbishire referred to a paper "On the Existence of a Seabeach on the Limestone Moors near Buxton" (Trans. Manchester Geol. Soc. v. p. 273), in which Mr. John Plant, F.G.S., had described as seabeach the surface of the limestone rock as the same is seen when bared of sward and surface clay above the quarries on Grin Edge and Harper Hill, south-west of Buxton, and to Mr. Plant's conjecture that this worn surface probably extended nearly to the crown of the hills.

His own observation had marked on each hill, above the stratum whose upper surface exibited those indications of supposed sea-wear, a stratum of somewhat different texture still subsisting in the shape of a slight vertical cliff or reef. This bed had not been worn in the same manner as the so-called "beach" (that is to say, with many interlacing fissures leaving a close *chevaux-de-frise* of limestone points), but rather in great blocks with round, curved edges or holes.

In connexion with this bed Mr. Darbishire had obtained specimens from each hill, exhibiting what he believed to be the remains of the burrows of *Pholas* or some similar shells.

On the top level of Grin Edge, close to the ruins of the tower, one stone had a group of seven holes. They were placed like Pholas-holes as he had collected them on Great Orme's Head; and, though the surface of the stone about them was much worn, taken along with the specimen next described, it seemed more fitting to ascribe to them a similar origin than to attribute them to the natural wear of the stone, notwithstanding the variety and singularity of many of the forms in which atmospheric or aqueous corrosion affect the limestone rock.

This stone lay amongst a heap of others near the ruins of the tower, and had doubtless been brought up a few feet. The height above the sea of the tower is stated on the Ordnance Map as 1435 feet.

On Harper Hill, in two large blocks of the overlying stratum, he had detected more-characteristic holes. Both blocks were lying in the sward above the "beach"-surface, and were a few feet below the rock *in situ*, from which they had evidently been detached. Of the first of these specimens he exhibited a photograph. It showed in the underside of the edge of a projecting ledge or table of stone six well-marked holes from $\frac{3}{4}$ in. to 1 in. in diameter, and one an inch deep, and traces of two others. The holes were grouped just as Pholas-holes usually are, and apparently were quite independent of the structural fissures of the stone.

According to measurement with an aneroid barometer, the stone in which those holes occurred was about 1380, and the reef from which it had fallen about 1400 feet in elevation.

If the holes were really burrows of marine shells, they would indicate the elevation of these hills since the period of glacial action by sea or land*.

The woodcuts on p. 34 represent a specimen of rock with burrows from Little Orme's Head, and the specimen from Harper Hill, Buxton.

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* The specimens referred to were produced to the Society on the 15th of October.

II. *On the Examination of Water for Organic Matter.*

By R. Angus Smith, Ph.D., F.R.S., &c.

Read December 24th, 1867.

In 1865 I received a request to write on the organic matter of water. I wrote a memoir, which was, without being published, extensively circulated among officers of health, especially in India.

I there made a division which was entirely new to chemists. The substance styled "organic matter," instead of being given as one to which little or no meaning was attached, was considered as existing chemically under at least seven different headings, not forgetting the numerous organized forms which it might assume. These divisions were—

1st. The organic matter decomposed, or putrid.

2nd. Organic matter readily decomposed, and probably ready to become putrid.

3rd. Organic matter slow to decompose.

4th. Recent organic matter.

5th. Old organic matter.

6th. Vegetable organic matter.

7th. Animal organic matter.

The first was known by the gases which instantly decomposed permanganate.

The use of the permanganate of potash, recommended by Forchhammer for ascertaining the organic matter of water, has been tried and admired by some, and found entirely wanting by others.

I endeavoured to show, in the memoir alluded to, and in a subsequent paper, how far that salt could be trusted, quite certain of its value for ascertaining bodies of the first and second classes, but having no experience of my own which would guide me to consider it a sufficient test for the total quantity of organic matter.

Bodies of the third class I was therefore inclined to estimate quantitatively by drying and weighing, afterwards ascertaining the condition of the matter as of most importance.

Organic matter of the fourth class was estimated by the amount of nitrites in the water, because their existence showed that the matter was oxidizing but had not had time to complete the action.

Bodies of the fifth class were estimated from the nitrates, as in that case the action was finished.

Bodies of the sixth class were reckoned from the absence of bodies of the seventh class, which latter class, again, were considered to exist when they were accompanied by an unusual amount of chlorides, supposing care to be taken to discover that other sources of chlorides did not exist.

It is not to be expected that any of these divisions should be sharply defined; but the one most difficult to decide upon, to all appearance, is really that which I have found, with proper precautions, scarcely ever to fail me, though it has been used in hundreds of instances, and for many years—namely, the test for animal matter.

Mr. Wood had written previously to the publication alluded to, and about the same time Prof. W. A. Miller; but I am not aware that any one endeavoured to distinguish the various conditions of the organic matter. I am, and was, desirous of showing that organic matter is not necessarily hurtful; we speak of it in water as an evil, to be measured by the balance, whilst there is as much difference in the qualities of that found in water as there is between good wholesome meat and putrid flesh.

I was desirous also of showing the true value of the permanganate, which is capable of acting on one, but scarcely on the other, and of being used for analysis for sanitary purposes, but not for all the demands of the chemist.

Dr. Frankland next took up the subject, and proceeded to show the amount of organic matter, or of sewage matter, which might flow into a stream; and this he did by estimating the nitrogen gas obtainable from the organic matter of the water. By ascertaining the amounts existing in the water supplied to a town and flowing in the sewage, he found the total thrown out by the inhabitants.

For this he made an organic analysis of the residue of the water, and by a very beautiful arrangement, using Sprengel's pump, he drew out the nitrogen for measurement. I do not doubt that this mode adopted by Dr. Frankland will always be useful, and will remain permanent as a method, but not for constant use, on account of the time required, excepting by those chemists who perform many of such analyses, and have the apparatus in constant readiness. It is, however, always pleasant to find an addition made to our capacity for accuracy, and especially when such beautiful methods are employed.

By this means Dr. Frankland obtains the organic nitrogen, nitrates and ammonia being removable. I have not generally estimated the latter, as being still more unstable than the former, as I have frequently found it to disappear with great rapidity, affording by no remnant any clue to the past. Urea and uric acid must also be excluded from the organic nitrogen before we obtain a clue to the albuminous matter. I think, however, that we require to know whether it is in a state to putrefy, or to produce living forms. If there are organic germs, that condition ought not to be neglected.

Immediately, I believe, after his plan was adopted by Dr. Frankland, Mr. Wanklyn discovered a mode of decomposing albumen by permanganate of potash, obtaining ammonia.

This process was used by Messrs. Chapman and Miles Smith. I shall not attempt here to give the exact amount of honour due to those gentlemen, or to tell precisely the boundaries of their labour.

The result seems to be, that the nitrogen may be obtained without the trouble of the organic analysis, although it may be uncertain which process takes the longest time.

Mr. Wanklyn considers that he can, by his method, first remove the ammonia which exists as such, by boiling with carbonate of soda; next, by decomposing the albumen with permanganate of potash and boiling in the caustic potash, obtain the ammonia resulting from the decomposition. This is a decided gain to us, and may turn out to be the mode of separating the ammonia of the putrescible and unputrescible organic bodies.

We are rather apt, when we find a method of analysis, to look at it as final. I shall endeavour to show how much we obtain by Dr. Frankland's method, and how much by Prof. Wanklyn's. I shall perhaps also, as in such cases, be inclined to show that my own method reveals a greater amount of the peculiarities of water. It makes more divisions, is more complicated therefore in one respect; but every point tells its own tale, and it is the number of tales we have to tell regarding the history of water that measures the fulness of our reports. I shall probably adopt all the methods for a time.

In the paper mentioned I pointed out the error of supposing that organic matter was all equally unwholesome, or that it was, in all cases, even in the slightest degree so. It was the custom too much to confound the simplest peaty and the most noxious putrescible matter. On this subject I may here quote the paper.

"*Quality of the Organic Matter.*

"Water manifestly containing organized matter is to be avoided; but it may frequently be purified by some mechanical mode of separation, such as simple straining.

"If the water contains organic matter in solution, or a condition approaching in all appearance to solution, it may be wholesome or unwholesome. The mere existence of organic matter is no proof of impurity. We must know if it brings animalcules or vegetable life or products of putrefaction. We must know the *quality* as well as the quantity. If the matter is peaty, consisting of the ordinary humous class of acids and salts, the colour may be very dark and the water very unpleasant to look at, without being in any way, so far as I have ever heard, injurious to health, although such water cannot be quite so wholesome as pure water, since the oxygen of solution is diminished. The taste and other sensible qualities will be the chief guides.

"If the matter is wholly or nearly colourless, it may still be wholesome or unwholesome. It may, for example, contain the juices of plants of a wholesome character. If these juices are fresh, they may do no injury; but they will not long remain fresh, they will putrefy. Water containing organic matter ready to putrefy ought to be avoided, as we cannot tell when the moment of danger begins; whilst the quality at best is never known to us exactly.

"To ascertain the nature of the organic matter, the water is allowed to stand for a day or two, in which case it may be found that organized bodies show themselves. Sometimes plants seem completely to fill the vessel, having come out of a moderately clear solution. When standing, in this case, the water must be prevented from evaporating, and it must be in glass, so as to be exposed to light; a temperature suiting vegetation is also to be given. Animalcules may appear in great numbers; they are an indication of nitrogenous matter, and one proof of the presence of substances capable of putrefaction. It may be that some form of putrefaction will be the only result; but whether this occurs alone, or along with organized forms, an excess of organic matter is proved.

"Water that will not bear the test of standing will in most cases be rejected at once. If no other can be obtained, it ought to be used before the putrefactive process has begun; but this is very dangerous. The next best method is to wait till after putrefaction has terminated, and the products are separated as much as possible. This is popularly known to be the case when the water has for some time become clear and colourless, and free from smell and taste.

"Water with green organic matter in suspension or semisolution is generally full of germs of living things and nauseous to the taste.

"A microscope is very useful in detecting the smaller forms of life. Good water is clear and colourless, or only slightly browned by peat. Clear bright water shows no microscopic objects. It is quite a mistake to suppose that all water contains animalcules. Those who have sent abroad this saying could not have known what pure water is.

"If the signs of organic matter are clear, it may not be needful to go further, but to reject the water at once. After standing for two or three days, varying with temperature, and showing nothing to the eye except a little film of green on the bottom of the vessel, we may conclude that not much organizable matter is there. We may then proceed to the chemical examination."

Some years ago it was customary, more than now, to examine with the microscope water intended for the use of towns. Attempting at one time to join the chemical and microscopic together, as they were too much in separate hands, I came to certain general conclusions :—That no ordinarily pure water produced much vegetable or animal life, and that microscopists were mistaken in allowing people to suppose that every drop of water was inhabited; that nevertheless water, generally considered very pure for drinking, might contain some forms not possessed with distinct locomotion; that whenever distinct locomotion began, especially among soft-bodied forms, the water was more than suspicious; that forms increased with the quantity of organic matter containing nitrogen, until the amount was so great as to cause gases of decomposition which eventually stifled life.

If the water was impregnated with sewage-water (as the Thames at London for example), bad cases excepted, the amount of vegetation which spread over the sides of a bottle filled with it and set in the sun, was so great as to give the appearance of one solid mass. If the water were taken from the river above Reading, the amount of vegetable matter was seen as a delicate green colouring of the bottom of the vessel; and if it were taken up at the source, no greenness whatever appeared, but, after long standing, minute crystals of limestone. If the water contained sewage, the phosphates and magnesia were generally found collected in some form of the *Navicula,* frequently in large masses of the *Navicula fulva,* and with it also was found phosphoric acid and magnesia. It was sufficient to note the condition of the water with relation to these deposits to ascertain at what part of the Thames the specimen was taken; and without the examination of these deposits, I do not consider that it would have been possible to obtain a distinct idea of the nature of the organic matter present. It is, however, needful that we know the general character of the water; had we a specimen from another situation and found no deposit whatever, it would have been very unfair or very unwise to conclude that it was similar in quality to that from the source of the Thames. It might have been saturated with nitrates, and even a large amount of organic matter not capable of taking the forms alluded to. Perhaps, however, nitrates prevent decay in water as in meat. As the germs of the lower forms of life may pass by us unobserved, it is necessary that we should give them time for development, in order that they may become sufficiently prominent to attract our attention. For this reason I consider it an imperfect examination of water when this is not attended to, in cases at least where we have not the slightest idea of the nature of the water. We find water with less than two grains of organic matter per gallon giving rise to forms by no means agreeable to look at in our food, and water with several grains of organic matter giving rise to no such forms.

It is true that we cannot tell which of these living forms are most dangerous; we cannot tell which of them, if any of them, produce either cholera or any other disease; but we can say that water which contains them may contain the elements of disease, whilst water which does not contain them is innocent. So far as this we are obliged to go at present. It was for these reasons that I have not recommended the method of estimating the total nitrogen in water. I was inclined to believe that the danger arising from water could not be measured by the amount of organic matter, or even by the amount of nitrogen in the organic matter.

Still it is of advantage to estimate the nitrogen of the organic matter, in order to compare the state of one source of water from day to day, as, for example, Dr. Frankland does so frequently in the case of the Thames. It is not, however, the organic matter only that gives out nitrogen; that is given out by ammonia and nitric acid; and these must also be estimated when seeking albumenoids. And this course is adopted by Profs. Frankland and Wanklyn.

We may now inquire how far all these bodies together will give us the amount of the organic matter falling into the water.

When azotized compounds decompose and form ammonia, how long is this ammonia retained in the water? On examining a very putrid stream, I estimated the amount of ammonia at the most putrid portion, where carburetted hydrogen was passing off in great volumes, and where a cubic foot could be obtained in a very few minutes by stirring.

In the sewage-stream of which I have spoken, the amount of ammonia was from 0·5 to 0·7 grain per gallon. After going 14 miles, the amount was only 0·07, and after 20 miles none at all was found.

The mud of the same stream was in a state of putrefaction, and contained, per cent:—

Ammonia	0·797
,, a mile lower	0·420
,, at second mile	0·171

The ammonia rapidly disappeared, and the mud itself diminished very greatly in amount.

I estimated that one grain of ammonia evaporated in some seasons from every square foot per hour.

In taking sewage-water to the land, I think it very important that the movement should be as rapid as possible.

The water in its passage of 20 miles has lost its valuable ammonia, and that within two or three days. This is a sufficient proof that we must not trust to the ammonia as an indication of the amount of the organic matter which has been, as it is as rapidly removed as the organic matter is decomposed; that is to say, the length of time necessary for complete putrefaction is, under favourable circumstances, no greater than the time afterwards required for the removal of its products. In this water there was no life to be observed; but the estimation of the organic matter would have shown no difference, whether vitality had been present and the substance had been capable of entering into active and unwholesome forms, or had been ready to break up into instantaneous putrefaction, or had been preserved, like a mummy, in carbolic acid for a thousand years.

From this observation regarding the ammonia, we are clearly led to beware, in our schemes of irrigation by sewage-water, that the land shall be overflowed before the ammonia is thoroughly formed, or else, if the ammonia is formed, that it shall not be subjected to loss by long exposure to evaporation.

We see, also, that nature provides here for the complete obliteration of organic matter. It ceases altogether to be found in the water. It may be traced, either as such or as ammonia and carbonic acid, long after the bubbles of carburetted hydrogen have ceased to appear, until at last it dwindles down to an amount which is rather difficult to remove from water, and which, so far as we know, may be utterly disregarded.

In the passage of organic matter we may observe, from figures soon to be quoted, that the volatile and organic matter diminished from 9·33 grains per gallon down to 5·04, even when there was an increase of fixed matter, and that the decomposing matter in solution diminished still more rapidly, in the ratio of 283 to 17.

The organic matter having left the water, we may next inquire whether any trace of its existence remains behind. That trace we do find in the increased amount of alkalies, sulphates, and chlorides.

The alkalies and alkaline salts, but especially common salt, are prominent. The whole of the common salt which has been used as food, without the slightest loss, is found in water, when vegetation has not removed it. The sulphur has become partly oxidized, leaving sulphates, whilst some may have left in company with hydrogen. Phosphates have been increased for a time; but they rapidly fall, taking lime and some magnesia with them; and on the whole the great change to be observed in the water is connected chiefly with the alkaline salts. The most stable and most constant of these, without loss, is common salt.

On this subject I may quote what has been said elsewhere regarding the chlorides as an indication of the presence of animal matter, proper regard being had to the absence of other sources, which in this country are chiefly connected with artificial circumstances. "The absence of the chlorides, especially, may be considered as the certain absence of sewage matter, and their amount to be estimated as an indication of the comparative amount of sewage matter from day to day in cases where sewage only pollutes the river." I conclude from these data that the organic matter and the nitrogen in it are equally incapable of giving us a sufficient history of the water. We may draw another conclusion, that nature has provided a mode in certain conditions for its perfect removal.

It is worth while to ask whether this purification by putrefaction is not the most complete method; it seems to destroy that which we consider to be germs, which germs may be a beginning of any disease of the most terrible form. If the water were allowed to purify itself by standing, and producing whatever amount of organic life could be developed from it, the germs would be increased in number, instead of being diminished. The water above might remain very clear, but might be nevertheless very dangerous. In such a condition water seems to be best purified by filtration through the soil, which simply means extremely slow and careful filtration accompanied with partial oxidation; in that case the water coming from under the soil would be fit again for use, although it would contain, and does contain, the alkaline salts alluded to.

Waters from rich lands, unless from very deep drains, always contain more organic matter than water from hills, and more albumen, or matter so conditioned that it is capable of assuming a great variety of living forms with and without locomotion. Water from the hills of Scotland and Wales, Cumberland, and many parts of North England, gives rise to no variety of living forms; and in many of them there is scarcely anything to be remarked in the deposit. On studying this subject many years ago, I found that in some of those cases where the water was peaty there was also to be found a large amount of ammonia. The words I used were these:—"In a peat bog which is not well drained, and is therefore wet and cold, the acids of the peat do not become dissolved so as to form a very deeply coloured solution, but they form a solution of a pale yellow. But in grounds which are warmer, or, what is better, well drained, the amount of soluble matter is very great. The colour of such water is not to be confounded with the water which heavy showers bring down, filled with mud and bits of peat; it is often perfectly clear and bright, but brown like coffee. The acids in solution at such times are kept so by the presence of ammonia. Ammonia dissolves them in large quantities, and along with them also the salts which they form with lime, magnesia, soda, phosphates, &c.

"Plants begin to grow in warm weather; at this time ammonia is formed. It is at this time the organic matter decays, and in its approach to inorganic matter passes through the stage of ammonia.

"The peat mould Mulder has shown, but he has not mentioned this excess of ammonia."

I supposed at the time, as it appears, that the ammonia produced by the decomposition of vegetable substances was used for the solution of the ammoniacal humates of Mulder. The paper read was only a short note intended to call attention to an important and curious point—the amount of ammonia on those bare hills. I have had the subject before me frequently since that period, namely, 1847, but have never given it all the time required for its elucidation. Lately Professor Wanklyn and Messrs. Miles, Smith, & Chapman have examined the hill-waters, and have rather alarmed the public by informing them that they contain more albumenoid matter than waters coming from rich plains. The process they adopted has no doubt shown two conditions of the nitrogen; and it is possible that one was to be found as albumen; but it seems perfectly clear that it was not putrescible albumen. I had long ago found the nitrogen; it still remains to find out all the conditions of its existence. If waters containing nitrogen from the hills are allowed to stand, they neither show their nitrogen by the production of living forms in proportion to the amount of that element, nor by putrefaction; any fear, therefore, of such results may be entirely put out of our minds. The living forms, which are the most dangerous, are fewer than from pretty deeply filtered water of the plains; and putrefaction cannot exist when the matter to putrefy is so minute: when the solutions are extremely weak, oxidation overpowers it. Such, at least, is my experience.

And now let us consider what does take place during putrefaction. In all cases known to me, the first thing that occurs is the diminution of oxygen in the air absorbed by the water, and, to a more or less extent, the production of sulphuretted hydrogen, which, however, may exist in the water to a very small extent at a time, being rapidly converted into combined sulphuric acid when oxygen can be found for the purpose.

In Loch-Katrine water the oxygen found in the absorbed air by Dr. Penny was equal to 31·69 per cent.; that of the Manchester water is equal to 27 per cent. In the Liverpool water, at the bottom of the reservoir, the amount fell to 12 per cent., being equal on the surface to the Manchester. In the first case, as well as in the second and similar cases, the amount of oxygen is too great to allow of putrefaction; and in the third case it was very clear that the oxidation was a process of decolorization, at least in part, whatever else may have occurred. This decolorization is caused by the oxidation of the *humus* and its removal as carbonic or other acid.

Another important feature in putrefaction is the occurrence of carburetted hydrogen. The carbonaceous or organic compound, having lost the ties that bound it together, is broken up into fragments. The carbon, when it can find oxygen, goes off in its company, but otherwise it leaves with hydrogen; separate, however, it must, and that with considerable violence. When there is a great amount of oxygen these decompositions of organic matter do not take place; and if the Loch-Katrine and other water were largely supplied with organic matter, putrefaction would be entirely absent, as it now is, as long as the high amount of oxygen was kept up in continual presence.

That organic matter does exist in peaty water to a larger extent than in streams not peaty, but still offensive, may be taken for granted. A very brown water contains about four grains per gallon. It is extremely dark, indeed, if there are six; but in the one case putrefaction, or, what is still worse, the development of mischievous germs, may occur, whilst in the other they are stifled. How far this disinfecting and preserving quality of peaty matter will act in weak solutions I cannot tell; but in its greatest strength (that is, surrounded by the matter of the peat itself), preservation may exist for an indefinite time; and the non-occurrence, of putrefaction in weaker solutions may be considered sufficient to show that there is abundant preserving-power for the amount of matter to be preserved in these waters.

But the question still arises, what is this organic matter containing nitrogen? If it is albumen, I consider it is disinfected, if the peat is great in amount; and it is also incapable of putrefaction, if the oxygen is great.

We are not perfectly sure that these humates of Mulder's actually exist in the form he mentions. It may be that the nitrogen is otherwise combined, this giving another mode of explaining the conditions of the water. If they are combined, as Mulder believes, we have only to allow oxygen to enter the brown water and be absorbed, a process which readily occurs; the *humus* is bleached, and the ammonia is free to combine with carbonic acid, and readily to dissolve a fresh portion of the same salts of which it formed a part. If the nitrogen be otherwise combined, whether as albumen or any similar substance, the oxygen would still play a corresponding part.

By this view of the subject, the brown waters would be giving off carbonic acid, and freeing some carbonate of ammonia. When this takes place in nature, a certain acidity is found, coupled with a little bitterness, a quality possessed by great portions of the waters of the hills. This, I believe, occurs in those waters which have not been purified by descending deep into the earth (where thorough oxidation and sweetening take place), but which have passed only over the surface, and been partially oxidized. The complete oxidation produces carbonic acid; the restricted oxidation an acid that is not volatile, and is extremely bitter. I have prepared so little of this, that I am unable to give a good account of it. It was formed from extremely brown water, chiefly from the river Avon, in Lanarkshire. By its formation, the water became very bright; when boiled down, the acid became stronger to ordinary tests, and its bitterness became more intense.

I may remark, by the way, that in all the bottles in which this occurred there was a deposit of very fine flakes of silica, leading me to think that, by our usual modes of estimating that substance, we were apt to make the amount too small. I believe this acid to be the origin of that bitterness which I have so often observed on the hills; and even in the very brilliant water from the hills, the two processes have been going on at the same time—the formation of carbonic acid and the formation of the liquid acid.

Apparently there are times, perhaps flood-times, when the water does not become oxidized so much as usual; and then we find the bitterness as a result in a greater degree. Although I had observed this for many years, I had not known that the effects might be hurtful to those who drank the waters. I met this year, accidentally, at Oban, Professor Frankland, who told me of some gentlemen whom he had met, who were very much inconvenienced by the use of such water, and who actually took supplies of other water with them to Inverness, at which place they had found the river to cause diarrhœa. I am not aware that that disease is more frequent at Inverness than elsewhere; but the water, or at least the substance in it, to which I allude, seems to be unwholesome to those who have not been accustomed to use it.

Taking this view of the case, water collected in the mountains is very different from that which has run down into the plains. The age of the water also is a matter of importance, and if kept long in deep reservoirs or in shallow reservoirs.

If in shallow reservoirs, the water may be uniform throughout; if in deep reservoirs, the oxygen may never be able to permeate to the bottom. At a depth of twenty feet I have found more than twice as much organic matter than was to be found on the surface.

Water that comes down very rapidly may never be able, in a practical period, to have the organic matter thoroughly oxidized. Water which purls down rather slowly, falling, nevertheless, over many rocks, is always clearer, although the reason partly of this is, that such water is not surface only, but has drained through the soil.

Peat itself, if distilled with fixed alkalies, gives out ammonia. I suppose there is no doubt that some of this arises from the decay of vegetable albumen, some of which may be retained in the peat; but its mode of decomposition is still a problem; it seems to take place chiefly by oxidation, not putrefaction. I was inclined to think that a notable amount of this alkali may be taken by the acid waters direct from the atmosphere.

The smoke of peat, or rather the oily matter deposited by the smoke, contains nitrogen compounds, very probably the bases obtained by the decomposition of the vegetable albumen, which will not enter into fermentation, or expand itself into the lower and dreaded organized forms, although compelled to yield to the action of fire.

I give a few quotations from 'Mulder's Physiological Chemistry,' first German edition:—

"I repeat that the ammonia in the soil, as in the natural saltpetre-grottos of Ceylon, is formed from the atmospheric air, the oxygen of which, instead of forming nitric acid, changes the organic substances into ulmic acid, then into humic acid, geic acid, apocrenic acid, and into crenic acid."

P. 158. "This formation of ammonia from the constituents of the air and water is of the highest importance for the growth and success of the plant. It is the cause which converts the insoluble organic constituents of the soil into soluble substances, and thus presents them to plants as organic food, even when no ammoniacal manures are added besides to convert the five mentioned acids into soluble salts of ammonia."

P. 166. "Ulmic, humic, and geic acids, however formed, possess the power of absorbing ammonia and water to the extent of several per cent. The quantity given out (at 140° Centigrade) is between 8 and 16 per cent. It requires 195° to free them from water."

P. 167. "The power of ulmic, humic, and geic acids to condense ammonia is so strong, that the acid made by acting on sugar with hydrochloric acid almost always contains ammonia if air is not kept away."

P. 169. "These acids combine so intimately with ammonia, that they have the character of an organic compound of four elements by treatment with potash; at a higher temperature they lose the ammonia completely."

In the 'Handwörterbuch der Chemie,' under "Humus," it is said that by constant digestion with hydrochloric acid, ammonia was not removed. It is also added that boiled with an alkali the ammonia is not removed.

This accounts for the opinion held by Berzelius and others, that nitrogen existed in these substances; and certainly it is not clearly shown that they are only ammoniacal salts. At the same time they may certainly be something between the purely organic compound albumen and the alkali ammonia. Independently of this supposition, I am quite disposed to think that Professor Wanklyn is right in thinking that the substance may in some cases be albumen; but I am not disposed to think him right in calling it putrescible. Is there any albumen not putrescible? That is an open point; but we know that there are bodies beside which albumen will not putrefy, even if putrescible when alone.

Berzelius gives Hermann's old analysis of crenic acid as containing nitrogen; and until we can finally settle the composition of these substances, we may be allowed to doubt. I think we may find in these facts sufficient reason to believe that even the organic nitrogen is not a measure of the dangerous quality of water, whether that nitrogen exist in a non-putrescent condition or association of the albumen, or in a substance removed a step from albumen and passing downwards to a condition more allied to the idea of an inorganic body.

I have given my reasons for believing that neither the nitrogen of ammonia nor the nitrogen found in peaty water can be taken as the slightest indication of the amount of putrescible matter. At present, I believe, we have no idea of the actual relation in which it stands to health. It will be interesting to know now if the nitrogen of the nitrates and nitrites is at all similar. These salts are not formed when putrefaction is going on rapidly. The reason is very simple: the water is then deprived of its oxygen. We do not know all the conditions of the formation of these salts; but one is essential, that oxygen shall be present.

In the Thames water, at least two miles below and above Hungerford, there was nitric acid in considerable abundance in 1848. It is mentioned, in evidence given in a parliamentary inquiry twenty years before that, that red fumes rose on heating the deposit. I quote from memory.

In 1848 this was certainly the case. In later analyses, by Dr. Hofmann, the nitric acid was not mentioned. I considered this at the time a great mistake; and I examined the water again, finding extremely little nitric acid, and, I believe, in some cases none.

Having examined other putrid streams, and found no nitric acid, I conclude that it had disappeared from the Thames also when it became more impure than previously.

During the time that river was so offensive near the Houses of Parliament, the organic matter must have wholly, or nearly so, deprived of its oxygen the air in solution. Running streams, however, do not, so far as I know, contain much nitric acid; that substance is formed in greatest quantity by the action of porous substances.

The oxidation is assisted by the pressure to which the gas is exposed by being brought in contact with a great amount of surface, as well as of numerous surfaces contributing to the result. For these reasons nitrates are found best in soils through which azotized matter in solution is slowly passed.

An interesting question arises—Does the nitric acid indicate the amount of organic matter which was previously in solution?

If nitrates are put on land they are decomposed by vegetation, and the nitrogen is retained. These salts act both as food and air to plants. The water which flows from drained land may contain nitrates, but they are not a measure of the amount put on the land; and if they result from the organic matter there, they are still not in proportion to its amount, as, even after they are formed from vegetation, they may be decomposed by it.

Nitrogen, therefore, may be removed from water either as ammonia, or organic matter, or nitric acid, every trace of it disappearing. Those nitrates, however, which do remain indicate that at least an equivalent of albuminous matter or sewage-matter did exist.

Another interesting question occurs—Is the nitric acid removed without vegetation? I believe that it is so.

In water from drained fields vegetation is generally found, though frequently in very small quantities. In the soil around drains it is found in considerable quantities; but after putrefaction has occurred in the Medlock, at Manchester, I have not found it. The oxygen seems to be removed as the oxygen of the air is, probably leaving nitrogen to pass off as gas.

As the nitric acid indicates an equivalent of albumen, I have put it down as telling us of the previous presence of organic matter, in other words, of old organic matter. The word "old" has no relation to time except so far as to mean older than that from which the nitrites are formed, and the original organic matter. The nitrites, not being fully oxidized, are supposed to be in the process of becoming nitrates. They have been taken, therefore, as indicating recent organic matter,—the word "recent" meaning only that they stand between the organic matter and the nitrates. I do not know that nitrates are converted into nitrites in water.

In looking for organic matter, I think it quite unsafe to trust either to chemistry or the microscope, without giving time for the development of all possible germs; but this is a point which demands a good deal of inquiry. Microscopists have given us details of appearances; but these have not been sufficiently classed; and the conclusions drawn have, therefore, not been valuable to the desired extent. To trust to the microscope without time for development, is to believe that the germs of disease can be seen. The use of allowing it to stand is not that the germ of disease may be seen, as we do not know it if we see it, but to see if the matter is active. The value of this must be tested.

The gases of pure water contain nearly 34 per cent. of oxygen.

Dalton found cistern-water almost entirely deprived of its oxygen; and I have found every percentage of oxygen from 34 downwards.

I go further into this point in my chapter on water, which I hope to bring out soon. Meantime I may say that the examination for oxygen is a very important one.

The loss of the oxygen with peaty matter and no vegetation indicates, as already said, the formation of carbonic or a bitter acid. The loss of oxygen with the evolution of sulphuretted hydrogen indicates putrefaction; but there are two conditions which externally resemble each other very much, namely, the growth of vegetable matter with diminished oxygen, and the growth of vegetable matter with excess of oxygen. Water in the first of these conditions, too, may contain, as I imagine, the most dangerous ingredients: germs of all diseases may exist in such waters; we do not know to what extent; and, as we are very ignorant on the subject, it is well to be alarmed at the conditions, until we have examined them and made distinctions.

We do not know very much about the second of the two; and if I think it less dangerous, it is perhaps more from a prejudice in favour of the abundance of vital air*, and of those hill-waters which do not contain bitter peat.

"*Weighing the Organic Matter.*

"If nothing apparently organic is seen, or if there be no time to allow it to germinate, we may try the following method. About half a gallon is boiled down in a platinum vessel; or it may be boiled in porcelain and transferred with care, when only a few ounces are left, to a platinum or even to a small porcelain capsule. The residue is dried and weighed. It is then burnt so as to oxidize the carbon, and weighed again; the difference gives the organic and volatile matter of the residue. 212° F., or 100° Cent., is not sufficient to remove all water; but as it is a temperature so easily obtained, it is convenient to consider it enough, especially as we cannot obtain the organic matter by weighing with absolute exactness. We may obtain an excess of water by the use of 212°, on one side; but, again, organic matter begins to be given off from some residues even at that temperature; Professor Miller and others advise about 300° F. It is at any rate well to state the temperature in the account of the analysis.

"The burning must not be effected at a very high heat, or several salts will be decomposed or evaporated; but at best some will suffer; and the use of a little carbonic acid

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* The apparatus which I think most convenient for taking air out of water, so as to estimate it, was described in the Memoirs of this Society.

and water, or carbonate of ammonia, to restore the lost amount, is advisable. A little distilled water and a few expirations into it through a glass tube will be sufficient for most cases; of course after this the ash must be heated again.

"Professor Miller advises the addition of 0·3 gramme of carbonate of soda to a litre of water, or 20 to 25 grains to a gallon, making allowances in the weighing.

"The addition of carbonates is especially useful, so as to form nitrates of alkalies when chlorides of calcium or magnesium are in the water. Chloride of calcium is observable in water from the clay slates of the west of Scotland, at least in some places. It is found also in Loch-Katrine water, although I have not seen it mentioned.

"For some years I was inclined to believe that the bitter taste was owing to these salts, until I found it increased by oxidation. I am not prepared, however, to say that they are incapable of modifying the taste, although they exist in quantities very minute."

It is needless to remark that I consider Frankland's mode of obtaining carbon and nitrogen far superior to any weighing.

"*Use of Permanganate of Potash, or Chameleon.*

"It must be remembered that the amount of organic matter obtained in the above way may be equal to several grains in a gallon, and yet be quite innocent; for example, it may be a little peaty matter. One method of trying the quality is given above; another, of a very convenient kind, is by the use of permanganate of potash, or mineral chameleon. It will here be called chameleon, as the former term is very long. It is very highly coloured, and its decomposition is known by the disappearance of the colour.

"Chameleon is decomposed by putrid organic matter, and by several unwholesome gases, rapidly or instantaneously. It is decomposed by fresh organic matter, and especially organized matter, less rapidly. The putrid portion may be estimated readily; the latter is more uncertain.

"In using this test, it is well to take not less than 5000 grains of the water, or still better 1000 grammes, *i.e.* one litre; but if a small specimen only can be obtained, then 1000 grains may be used; and it is well always to give the figures for 1000 for uniformity, or (by multiplying by 70) to change them into the amount per gallon. Grains or grammes may be used indifferently, the solution being made to contain simply so much in 1000.

"A convenient plan is to use 2 grains of the pure crystallized chameleon to 1000 of water. This, of course, is the same as 2 grammes to a litre of water.

"The chameleon is poured conveniently from a Mohr's burette, or better still, when the water is very pure, from a dropping-tube in which 1 c. c. (cubic centimetre) is 6 to 8 inches long. Add a drop of the chameleon, and wait till the colour disappears; add another, and so on till the colour remains permanent. The organic matter which decomposes the chameleon in a minute or two must be noted carefully; but generally there is a greater quantity which decomposes very slowly; the result obtained from the latter is, I believe, of less value. By decomposing in a minute or two, it is meant that when a few drops are added, producing a slight colour, this undergoes a change in a minute or two. But generally considerable permanency is obtained in ten or fifteen minutes; then the slow decomposition begins, of quite another quality of organic matter, requiring hours or even days. This matter must be estimated either by the weighing process described, or by the further action of the chameleon, to be described. The amount decomposed instantly is a true measure of the putridity, it is believed. If there is very much organic matter, it is often very difficult to know when to stop, as the brown masks the red colour. But this must not bring discouragement, as experience will teach exactness; and if it does not allow exactness, it then will be no great loss in such cases, in a sanitary point of view, as in them there is decidedly too much organic matter, and the water may be condemned. From the same point of view, it is of less consequence whether the amount be a minute quantity more or less. It is well to make broad lines of distinction, and to condemn freely when there is rational hope of obtaining a purer water.

"The amount of available oxygen in one of the solutions of chameleon described is 0·0005, which may be either grammes or grains, or almost exactly one of oxygen in 2000 of the solution. If we wish to know the amount of oxygen used, we simply divide the amount of chameleon by 2000. It is probable that the use of the oxygen column is the only exact method of recording the results; and it is probable that the amount of oxygen required is the only exact measure of the impurity of the substances. We obtain in this number the amount of oxygen which is required for purification.

"Whilst looking over this plan in manuscript, I received Dr. Miller's paper, proposing also to use oxygen; he prefers a solution of chameleon containing 3·95 grains, equal to 1 grain of available oxygen to 10,000 of water, or 1 cub. cent. = 0·001 gramme, or say 1 milligramme of oxygen. This is a very convenient method; but it has been thought proper to keep the Tables as given, for various reasons, chiefly arising from this, that it has not been found so convenient to keep or to titre a very weak solution. In some cases a very strong solution is required; we can easily dilute it, but we cannot concentrate it readily. After estimating its stength, we dilute it (if we require a weak solution) to any amount we think proper;

and as the strength changes, the amount of water to be added will also change. If the amount of oxygen is given as the result of the analysis instead of the amount of chameleon solution, the strength of the latter does not require to be kept uniform. The analyist requires only to know its strength at the time of using it. For very bad water, the strong solution may be used; and when greater delicacy is wanted it may be diluted. The weak solution here used is made by adding nine of water to one of the strong solution.

"To make the chameleon solution, very pure water must be employed, and very pure crystals, as well as very pure vessels; it is better kept in considerable quantities, such as a quart or two. It must be tried occasionally, say every month, in a cool climate: calculation must be made as allowance for any change which it may undergo. For example, if it has lost 1 per cent., we must calculate how much the number would have been had there been no loss of strength. If calculation is not agreeable, a less quantity of solution may be made at a time, and that which is over may be thrown away, a fresh amount being made to the normal strength whenever the original weakens; or a certain amount of the crystals may be weighed and added to the solution; but this has been found less convenient.

"The estimation of the value of the chameleon is sometimes made with a solution of chloride of iron, sometimes oxalic acid. For ready reference, a Table is made showing the amount of a solution of iron which corresponds to a certain amount of chameleon. The sulphate of ammonia and iron has proved to be very valuable, keeping for several years in crystals, and acting instantaneously. The writer has not found oxalic acid equally sharp and quick; but eminent chemists use it—Mohr and Miller for example.

“ *Values of the Strong Solution.*

Permanganate of pot. or chameleon 2 grammes in 1000 c.c.	Oxygen.	Ferrum.	$\mathrm{FeO, SO_3 + NH_4O, SO_3 + 6\,Aq.}$	$\mathrm{KO, Mn_2O_7}$.
c.c.	gr.	gr.	gr.	gr.
1	0·000500	0·0035	0·0247	0·002
2	0·001000	0·0070	0·0494	0·004
3	0·001500	0·0106	0·0742	0·006
4	0·002000	0·0141	0·0989	0·008
5	0·002500	0·0176	0·1237	0·010
6	0·003000	0·0212	0·1484	0·012
7	0·003500	0·0247	0·1732	0·014
8	0·004000	0·0282	0·1979	0·016
9	0·004500	0·0318	0·2227	0·018
10	0·005000	0·03535	0·2474	0·020
100	0·050600	0·3535	2·4747	0·200
1000	0·506300	3·5353	24·747	2·000

“The numbers in the above Table run in the following proportions nearly :—

Chameleon Solution.	Oxygen.	Iron.	Sulphate of iron and ammonia.	$\mathrm{KO, Mn_2O_7}$ Crystals.
2000	1	7	49	4

“It is convenient to use six figures of decimals, as they may also be read as whole numbers, meaning so much in a million.

“ *Values of the Weak Solution.*

Permanganate of potash or chameleon solution. 0·2 gramme in 1000 c.c.	Oxygen.	Ferrum.	$\mathrm{FeO, SO_3 + NH_4O, SO_3 + 6\,Aq.}$	$\mathrm{KO, Mn_2O_7}$.
c.c.	gr.	gr.	gr.	gr.
1	0·000050	0·00035	0·00247	0·0002
2	0·000100	0·00070	0·00494	0·0004
3	0·000150	0·00106	0·00742	0·0006
4	0·000200	0·00141	0·00989	0·0008
5	0·000250	0·00176	0·01237	0·0010
6	0·000300	0·00212	0·01484	0·0012
7	0·000350	0·00247	0·01732	0·0014
8	0·000400	0·00282	0·01979	0·0016
9	0·000450	0·00318	0·02227	0·0018
10	0·000500	0·003535	0·02474	0·0020
100	0·005060	0·03535	0·24747	0·0200
1000	0·050630	0·35353	2·47470	0·2000

"In the oxygen column 6 and 3 may be left out.

"The first and under might be called the first quality of water; from 0·1 to 0·2, the second; 1 would be the 10th.

"It is considered better to acidify the water before adding the permanganate; this is done by adding three drops or water-grain measures of sulphuric acid to 1000 grains; some water will demand more; the object is to attain acidity equal to about three drops of sulphuric acid in 1000 grains of distilled water. I find Dr. Miller says 50 grains of diluted sulphuric acid (1 of acid to 3 of water) added to 8 ounces of water. Acid enables the oxygen to act on more matter and more rapidly; and the calculations are made on the supposition that acid is used; the proportions from different waters are not much changed by acid. Alkalies prevent the action, although they may prepare some of the matter to be more readily oxidized: when the colour has been difficult to see, the chameleon has been used with alkali instead of acid, in which case a green colour is obtained, which is more easily recognized in many cases, and serves as a corroboration."

As an illustration of the mode of examining water so as to obtain the first and the second conditions, if we may so call them, of organic matter, I will quote here a short paper, read at the Philosophical Society of Glasgow, on the water of the river Clyde. I think I there found the proper use of the permanganate of potash. I found at last a very sharp separation between those substances that decompose the permanganate at once and without acid, and those which do so on the addition of acid and after a short time. So decided is this that I consider them as indicating two different conditions of organic matter.

"I lately adopted a mode of examining water by which is seen, as I believe, the amount of organic matter readily decomposed, and the amount actually decomposed, or the amount of putrid gases resulting from decomposition and other matters. When this was done, I thought it would be interesting to test the Clyde by the new process, and for that purpose obtained specimens at various points, from the Broomielaw to the sea, afterwards continuing till the vessel came near Liverpool.

"The column to observe is number 1 of annexed Table of either series. This gives the amount of oxygen consumed instantly, and corresponds, as I believe, to the amount of decomposed matter which has left in solution putrid gases, whatever these gases may be. I shall suppose them to be sulphuretted hydrogen in minute quantities, or compounds of sulphur, or organic substances only. In the part of the Clyde near Glasgow, all these compounds will probably be found; as we go near the Firth the sulphur will be fully oxidized, and the organic compounds only will be the active agents in taking up the oxygen, as they oxidize more slowly.

"The amount of oxygen is calculated from the amount of permanganate employed. The method is an extension of the original idea given out by the late Forchammer many years ago; the amount of water used was 100 cubic centimetres or 1544 grains. The permanganate required to give perfectly pure water perceptible colour was in every case subtracted from the number obtained.

"The examination of the Clyde is not perfect, but there is much to be seen that is interesting. Specimens ought to have been taken higher than any town on the river. They began below Bothwell, or 1½ mile above Cambuslang, about 6½ miles above the Quay at Glasgow. We there find the oxygen required for the water to be 0·000045 per cent. by weight, or equal to 3·15 grains per 100 gallons. When we come to the Broomielaw, at Glasgow, we find the amount of oxygen demanded had risen to 0·00022 per cent. by weight, or 15·40 grains per 100 gallons.

"We then see it steadily diminishing; and when we arrive at Port Glasgow, 20 miles down, it has become equal to that at 6½ miles above Glasgow. If we had taken the north side, below Dumbarton, instead of the south, on which the vessels sail, the difference would have been greater.

"Leaving Greenock and turning the point, we have a rapid change; and when off the west of Gourock Bay, at the distance kept by the Liverpool vessels, the amount of oxygen demanded fell to 1·05. Off Wemyss Bay the fall is remarkably low; but it is not always so, as it depends on tide and wind. The current seems to run down the centre, whilst the ocean-water comes up the coast. The winds and floods may modify this.

"We have to observe a complete change about Gourock or the Cloch, if we keep to the south and east side; but if we keep to the north, the greatest change is perceptible not far off Helensburgh.

"This wide space of water reduces the amount of oxygen required to 0·00001 per cent. by weight, or 0·7 grain per 100 gallons.

"We are now led to ask if the purest state of the water is attained here. We may go further towards the open sea, and find that the amount required falls to 0·000005 per cent. by weight, or 0·35 grain per 100 gallons. This is at Arran, where the sea is more extensive, and oxidation, even of the peaty matter from the hills, is completed. We observe this to continue till the entrance of the North Atlantic is passed; and as we come south, to a narrower channel, the amount of oxidizable matter increases and keeps up steadily till we arrive at Liverpool. No specimen was taken nearer than 17 miles from Liverpool. The point 0·00001 per cent. oxygen by weight, or 0·7 grain per 100 gallons, is an important one to examine. We find that at Dunoon, Innellan, and Holy Loch, this amount may be called permanent. We may ask, Why does it not fall lower? and is it true that the sewage of Glasgow affects the Firth until we arrive at Lamlash and obtain the number 0·000005 per cent. oxygen by weight, or 0·35 grain per 100 gallons?

"We observe that the amount of purification is remarkably rapid whenever the Clyde widens, as at Greenock to Gourock; so that, even near Helensburgh, the number has fallen to 0·0000125 per cent. oxygen by weight, or 0·875 grain per 100 gallons. This change, from 0·000045 or 3·15 grains per gallon, has been effected in the space of about 2 or 3 miles. Is it possible, then, that the slight further change required demands all the wide distance between Gourock and Arran? I think not. The reason we do not find the number for oxygen falling below 0·00001 per cent. by weight, or 0·7 grain per 100 gallons at Holy Loch, Strone, Dunoon, and Cove, may be seen by examining the Lochs. We there find that they themselves send in water with the oxygen number 0·00001 per cent. by weight, or 0·7 grain per 100 gallons.

"At this point we must remember that the oxygen may not all be required for putrid matter, and that the streams contain some peaty matter which may also instantly require oxygen. At any rate we know this, that, for whatever they do require it, it can be for the destruction of no very unwholesome matter in the water, as the highland streams must be considered free from such taints. There is a little of the soil of cattle when the weather is wet, but perhaps none when the weather is so dry that the water must pass through the earth before reaching the bed of the stream.

"We cannot, then, expect the sea-water to demand less oxygen than the streams and lochs that affect it here. In other words we discover the influence of the land on the drainage from the hills, even when all trace of the influence of Glasgow has gone. This is, I believe, the reason that, according to circumstances, such as currents and weather, we observe the water of the Firth sometimes as pure as the deep sea, and at other times less so. At Wemyss Bay, for example, the Table does not show this very well. This will happen at other parts of the coast when the streams enter purer than the usual sea-water there, as, for example, at Dunoon and Innellan, where the water flows from streams equal, when not in flood, to the deep Firth water.

We see the influence of the coast also as we go on towards Liverpool, and between the Isle of Man and Lancashire.

"I consider that 0·00001 per cent. oxygen by weight, or 0·7 grain per 100 gallons, marked the limits of the Glasgow sewage. If the sewage were not all expended, the amount would be greater, because the lochs and their streams do not show a higher number. Whenever the water of the Firth falls down to that of the streams, we are, as I suppose, receiving the air at least as pure as the air above the streams on the hills, and may be contented.

"Some people will perhaps demand more than this number. I have not made examinations far enough towards the ocean to enable me to speak of it; but it seems to me that the numbers 0·00001 per cent. by weight, or 0·7 grain per 100 gallons, of the streams, and 0·00005 or 0·35 grain per 100 gallons, mark two very important points. The first characterizes air which is certainly most wholesome and agreeable; the second is that ocean-air which some persons require, but which to others is said to be too strong. On the meaning of this expression "too strong," I will not attempt to speak; but it is very much used by a great public, and it must have a wide foundation. It is apparently certain that to some it is important not to have the extremely strong or perhaps purest air.

"These observations seem most curiously to coincide with general observation. The current of the Clyde keeps to the south; and we see that the water changes less rapidly there. At the north the direct current is not much observed, and the influence of the lochs on fiords is seen. The water from them presses against the Clyde current, and aids in keeping it to the south-east. This had influenced the building of the numerous residences on the Argyle side, lining all the coast. The population had not found it needful to make a chemical analysis, but had seen where the purest water was with the unaided eye. A mode of observation, apparently quite as good as chemical analysis, is the examination of the rocks on the coast, the dark and dirty aspects of these in impure water being very striking.

"I am not, however, inclined to look on my work here as useless, although common observation has done its part so well. I believe it to be of great importance to us to be able to apply tests so as to prove the truth of popular belief, which is formed by long and tedious processes. With these little chemical experiments we may do the work in an hour, which the instincts and expensive experience of whole generations were required to perform.

"I believe that in these results we have a method of examining the coasts which will be valuable to us as a sanitary police. It can be applied wherever there are sheets of water.

"I will not say that these numbers represent the relative wholesomeness of places on the land and on the sea; but I think it probable that they may fairly be used as comparisons of places on the sea only. When the land is in question, there are many sources of emanations to be considered.

"To be complete, the examination ought to be continued for several years. Last year was not an average one; there was much rain.

"There are floods in the Clyde which bear down so much water that the brown peaty matter is readily seen below Dunoon; but on these occasions there is no fear of putrid matter, as the water carried down is so abundant. At the same time there will be a certain degree of purity less at the sea-entrance, corresponding to the greater degree of purity caused by the flood at Glasgow. By this it is not meant that the water as low as Cloch or Dunoon is affected by more than an infinitesimally small amount of the sewage matter which is washed past Glasgow in floods, whilst at other times the effect has ceased, as before said, at least on the north, below Helensburgh. There is, however, a little muddiness observable for many miles, and this will be found even in places where the oxidation of all the readily transported matter has taken place.

"Even where total oxidation has occurred, a little muddy matter remains. It consists chiefly of earthy bodies, which may float until they obtain a suitable place for deposit.

"It would be interesting to find the time needful to perform this change on the organic matter of Glasgow. We see that the great bulk of the purification is performed in the fresh water. At Dumbarton it must be rare indeed that anything can be known to exist in the water from its smell. When the Clyde passes into the estuary, which may be said to begin at the Rock of Dumbarton, the change is effected by spreading the water over many square miles, and preparing it effectually for those who enter the sea- and Loch-regions below Helensburgh. Below this point it is not a river on which we are, but an arm of the sea, protected on all sides from oceanic waves.

"The water bears the qualities of coast-water in proportion to its excessive length of coast. This quality must exist more or less on all coasts which have fresh springs running into them. In cases where the rivers are large or numerous, the evil of the meeting of fresh and salt water is well known. On the Firth of Clyde no large bodies of fresh water are found after we leave the mouth of the river portion of the Clyde; and, as observed, these streams are remarkably pure, or, if coloured, it is only by peat-water. The absence of great rivers is therefore an advantage of an important kind to this coast.

"I was extremely glad to be able to speak thus favourably of the districts which are so agreeable to me; but I am certainly desirous of speaking still more favourably of some which are too much affected by Glasgow. They may be constantly deteriorating. This is a very serious matter, not merely for owners of property on the Firth, but for all the inhabitants of Glasgow, who are favoured more than those of any other large city in the Kingdom by a ready access to this wonderful sanatorium.

"In making these remarks, I take for granted that the air above the water will be pure in inverse proportion to the amount of volatile oxidizable matter.

"We must not, however, forget that if the river is rendered impure, the town is proportionately purified. And we must also remember that the work done in the water is enormous. If the refuse matter were allowed to evaporate into the air and so collect its oxygen, the act of purification would go on in the air we breathed; but now the water draws in oxygen, and as soon as it is consumed it draws in more. Even the air of the river is, at the distance of a few miles down, purer than the air of many towns; and when the spaces widen, the case is stronger still.

"I have sometimes inquired if we obtained in fish an equivalent of the manure sent out, and if it is not easier to gather the crop in boats than to put the manure laboriously on land. I find, however, that the loss by putrefaction, oxidation, and evaporation is so rapid that there was no hope of it being used profitably. In one current examined, a very deep one, and much stronger in sewer matter than the Clyde ever is, the whole nitrogen had disapeared in three days,—the greater part in much less.

"Whilst the chief object of my paper is to show a new mode of examining climate as well as streams, I am glad to be able to say that Glasgow has failed to destroy the purity of the Firth beyond the points indicated, and that we may still rejoice in our summer dwellings. It is, however, not to be forgotten that the effects are found too far away, and that twenty-two miles of mischief is more than abundance. It is to be hoped that the cautious policy of the city in waiting for the best method to be adopted for using its refuse will enable it to carry out that future work, whatever it may be, with the greatest success.

"I have referred only to the 1st column. It indicates the amount of permanganate decomposed instantly; this represents the gases of decomposition, such as most readily pass into the atmosphere.

"It will not be supposed that this is the only offensive matter.

"The 2nd column represents the amount when acid is

	Time.	Oxygen per 100 gallons required to oxidize		Oxygen per cent. by weight required to oxidize	
		I. The decomposed organic matter.	II. The easily decomposable organic matter.	I. The decomposed organic matter.	II. The easily decomposable organic matter.
6½ miles above Glasgow	Aug. 15, '66. 3.30 p.m.	3·15	11·55	0·000045	0·000165
5½ miles above Glasgow	3.15 „	4·55	9·45	0·000065	0·000135
5 miles above Glasgow, Cambuslang	3 „	5·95	11·20	0·000085	0·000160
Broomielaw	1.30 „	15·40	22·05	0·000220	0·000315
Mouth of Kelvin	1.20 „	11·55	18·20	0·000165	0·000260
100 yds. below Renfrew	1 „	10·85	25·55	0·000155	0·000365
100 yds. above Bowling	12.45 „	4·55	8·05	0·000065	0·000115
At Dumbarton	12.30 „	4·55	6·65	0·000065	0·000095
Between Greenock and Port Glasgow	12.10 a.m.	3·15	4·20	0·000045	0·000060
Gourock	July 25, '66. 12.55 p.m.	1.05	1·75	0·000015	0·000025
Cloch	12.45 „	1·05	1·57	0·000015	0·0000225
Inverkip	12.30 „	0·70	1·40	0.000010	0·000020
Wemyss Bay	12.15 „	0·35	0·87	0·000005	0·0000125
Opp. Toward	12 noon.	0·70	1.05	0.000010	0·000015
Between Toward and Cumbrae	11.30 a.m.	0·70	1·05	0·000010	0·000015
Opp. Cumbrae Lighth.	11 „	0·70	0·87	0·000010	0·0000125
Opp. Lamlash	10.15 „	0·35	0·70	0·000005	0·000010
188.	July 25, '66. 10 a.m.	0·35	0·87	0·000005	0·0000125
176. Ailsa Craig	9 „	0·35	0·70	0·000005	0·000010
160. From Liverpool	8 „	0·35	0·87	0·0000075	0·0000125
131. „ „	6 „	0·52	0·87	0·0000075	0·0000125
103. „ „	4 „	0·52	0·87	0·000010	0·0000125
74. N. of Isle of Man	2 „	0·70	1·05	0·0000125	0·000015
46. From Liverpool	July 24, '66. 12 night.	0·87	1·22	0.0000125	0·0000175
32. „ „	11 p.m.	0·87	1·40	0·0000125	0·000020
17. „ „	10 „	1·05	1·75	0.000015	0·000025
Off Greenock	Aug. 13, '66. 5.10 p.m.	1·40	1·57	0·000020	0·0000225
Off Helensburgh	Aug. 15, '66. 6.35 p.m.	0·875	1·22	0·0000125	0·0000175
Off Gourock	Aug. 13, '66. 5 p.m.	1·22	1·57	0·0000175	0·0000225
Between Kirn and Gourock	Aug. 13, '66. 6 p.m.	1·22	1·40	0·0000175	0·000020
Arrowchar (Loch Long)	July 30, '66. 1.30 p.m.	0·70	1·05	0·000010	0·000015
Off mouth of Loch Long	Aug. 15, '66, 6.55 p.m.	0·875	1·40	0·0000125	0·000020
Head of Loch Goil	Aug. 17, '66. 1.5 p.m.	0·875	1·05	0·0000125	0·000015
Entrance of Loch Goil	4.30 „	1·05	1·22	0·000015	0·0000175

	Time.	Oxygen per 100 gallons required to oxidize		Oxygen per cent. by weight required to oxidize	
		I. The decomposed organic matter.	II. The easily decomposable organic matter.	I. The decomposed organic matter.	II. The easily decomposable organic matter.
Head of Holy Loch	Aug. 1, '66. 2.45 p.m.	0·70	1·57	0·000010	0·0000225
Middle of Holy Loch	2.30 ,,	0·52	1·05	0·0000075	0·0000150
Dunoon (several 100 yds. from shore)	July 31, '66. 1 p.m.	0·70	1·22	0·000010	0·0000175
Dunoon (about 800 yds. from shore)	1.15 ,,	0·70	1·22	0·000010	0·0000175
Off Dunoon	Aug. 16, '66. 1 p.m.	0·70	0·875	0·000010	0·0000125
Stream at West Bay, Dunoon	Aug. 20, '66. 1 ,,	0·52	0·70	0·0000075	0·000010
Stream between Innellan and Dunoon	12 noon.	0·35	0·52	0·000005	0·0000075
Between Innellan and Dunoon	Aug. 16, '66, 1·8 p.m.	0·875	1·05	0·0000125	0·000015
Off Innellan	1.15 ,,	0·70	0·875	0·000010	0·0000125
Off Wemyss Bay (high tide)	Aug. 9, '66. 12 noon.	1·05	1·22	0·000015	0·0000175
Off Wemyss Bay (low tide)	4.30 p.m.	0·875	1·22	0·0000125	0·0000175

used to assist the oxidation. I suppose this to represent the amount easily decomposed.

"The 3rd column represents the amount of oxygen used by the organic matter in twenty-four hours. I do not at present see its use. I do not think this at all a fair mode of representing the impurity of water, unless we are sure that the quality does not change. We see this by the numbers above, and at, Glasgow bridge. The purer water has, by this mode of representing it, the appearance of being equal to the worst. The only column that gives the continuous increase and decrease, exactly as the senses find the case to be, is the column No. 1.

"It is not, however, meant that the gases of decomposition are the only evils to be apprehended from impure water; but, so far as I know, they are those that affect the air principally. The most dangerous bodies are probably in the water itself, and it is possible that they may not be affected by the process of Column 1.

"I am not inclined to draw any important conclusion from the total organic matter without inquiring into its character. At present, I believe, we are quite safe in attending to the conclusions here given in Column 1, with such precautions as I have elsewhere indicated—when, for example, nitrites, sulphites, &c. are present. By and by we shall learn the true mode of dealing with the other columns."

Having had occasion to examine another stream receiving sewage and flowing about twenty miles, the following results were obtained with the chameleon :—

Oxygen required by the decomposed and decomposing Matters.

Grains per gallon.	
2·835	1·205
1·249	1·630
1·949	1·205
1·170	1·453
0·425	0·425
0·815	0·177

The gradual diminution of the decomposed organic matter is here seen very plainly. As new sewage comes in in certain places, the amount rises.

These numbers correspond entirely with the apparent condition of the stream. They do not, however, correspond with the total volatile matter, which was as follows :—

Volatile matter.	Fixed matter.
9·33	44·27
6·23	28·91
3·99	24·08
3·08	36·75
3·43	31·85
4·16	30·31
5·22	29·92
5·04	69·30

I believe the results obtained by the chameleon are more useful in a sanitary point of view than the other results.

Let us take another case.

When heavy rains drove the impure water forward, the chameleon gave the following:—

Oxygen in Grains per Gallon required for the decomposed and decomposing Matter.

0·992	1·106
0·284	1·134
0·638	1·276
0·709	1·347
0·992	

In such a case the ordinary analysis gave

Volatile matter.	Fixed matter.
4·20	20·72
4·48	21·21
7·14	26·39
6·02	34·02
4·27	28·28
4·69	32·27

These latter do not at all show the fine distinctions made by the permanganate, which agrees entirely with our senses, and may be used instead of them for putrid substances.

The permanganate must be used with consideration, like all other chemical tests. We must be careful that there are no lower metallic oxides in the water, which will remove the colour as suddenly as the sulphuretted hydrogen from putrid matter. Much has been said against the permanganate; but nothing even pretends to do what it does. Those who neglect it, leave out the putrid matter or drive it off in boiling.

Nitrites decompose chameleon instantly. They are the result of the oxidation of azotized matter, and must be estimated.

"The decomposition of the chameleon may be caused by the organic matter present, or by nitrites &c. The presence of nitrites or nitrous acid will show if the organic matter is only recently oxidized.

"Generally there may be found organic matter and nitrites together, showing that some is oxidized and some ready to become so.

"Schönbein has found traces of nitrous acid in the efflorescence of walls. In judging of the time, we must make allowance for this, and not judge from small traces such as he alludes to.

"When nitrites and organic matter are found together, they may be estimated separately. The chameleon will be decomposed by both. 158 of chameleon solid is equivalent to 95 of nitrous acid, or 1000=606. It will be necessary to find one of the substances separately, and to subtract it from the whole, in order to find the amount of the second.

"The amount of nitrous salts may be found by using ozone-paper, that is, paper with iodide of potassium and starch. The water to be tried is made acid with a little dilute sulphuric acid, not more than three drops to 1000, and a drop put upon the paper. If nitrous acid is present, it becomes blue. This blue is obtained immediately by putting a drop of the solution on ozone-paper when there is 1 of NO_3 in 30,000 of water; and by waiting patiently, and giving time, it may be seen with much less.

"In order to find how much nitrite is in water, the solution may be diluted until the reaction ceases to be distinct even after waiting. The amount in the water will then be 1 in 100,000. If, in another case, when we find the reaction distinct in water, we take 100 grains and add 900 to it, and find it beginning to be indistinct, the undiluted quantity must have been 10 times as strong as the diluted. The amount in the 1000 grains is 1 in 100,000; the amount in the 100 grains is 10 times as great, or 1 in 10,000; and thus we may arrive sufficiently near to the total amount. The amount of water added in order to bring the reaction to the adopted minimum is the measure of the strength of the solution.

"A test like this depends partly on the eye and partly on the delicacy of the test-paper. The same result will be attained if the same eye and tests are regularly used.

"The method of testing with paper is not so refined as the use of a larger amount of water, say 1000 grains; with this amount, the presence of 1 of NO_3 in 3¼ millions of water may be detected on adding starch and iodide of potassium; 3 drops of sulphuric acid are also added to the 1000 grains, *i.e.* 3 grains by measure. If no nitrous acid is present, no blue colour will be seen with this amount. If nitrous acid is present, the colour will begin in a few seconds. Some may prefer one way, and some the other."

The methods given of estimating the amount of nitrous acid are minimetric, proceeding by dilution instead of concentration. The value with gases is better known than in liquids; but it is believed that it will be sufficiently exact with the latter in cases where pure scientific accuracy is not attainable and not necessary, and where it is important to save time, labour, expense, and patience.

Suppose we find that a specimen of water contains 0·001 gramme of nitrous acid in 1000 grms., or in the quantity used, we find by calculation that this is equal to 0·000421 of oxygen, or, as the Table shows, to 0·842 of the solution of chameleon used.

Now suppose 1000 of the water decolorize 5 of chameleon, we must subtract from 5 the amount which would be due to the NO_3, $5·0-0·842=4·158$, which then is the amount in c.c. of chameleon solution decomposed by the organic matter. 1 of NO_3 requires 0·421 of O to become NO_5.

0·001 of nitrous acid = 0·000421 of oxygen,
or 0·001684 solid chameleon,
or 0·842 of the solution of chameleon of ·2 in 1000.

It will be seen that if we find the amount of O to which

NO_3 is equal, we require only to multiply by 2000 to obtain the amount of solution of chameleon. But we may do it still more easily by simply multiplying the amount of NO_3 obtained by 842 as a whole number. This gives the amount of chameleon solution to which it is equal.

"The estimation of both is interesting; but it is much more important to obtain the total chameleon used, as the presence of NO_3 must be considered a great objection to water, partly on its own account, and partly because of its origin.

"It is to be observed that water containing much animal matter becomes extremely acid. It is a common thing to find water extremely clear, with no apparent organic matter, even on standing, also extremely acid, and retaining nitrates. I have found it so filled that it appeared to flow less readily than pure water, and had a most nauseous taste. It was close to a churchyard, and was considered excellent water. This perversion of taste is difficult to understand, but it must be combated; it is not natural, and causes illness and death. This acid water is an excellent solvent of metals; and if lead is present, the solution becomes rapidly so strong as to taste of the metal. This acidity, in conjunction with nitrates, does not, so far as I know, exist when the organic matter is very old, probably because the organic acid is oxidized. Some of it, probably all, is caused by the formation of organic acids. Similar organic and entirely colourless solutions, acid, but free from nitric acid, may be found by allowing peaty water to stand without evaporation, but in contact with air, for some years.

"In the nitrous waters no organic matter will be apparent on burning unless there is more than the acid can oxidize. A very white ash is, therefore, a suspicious circumstance; and unless the matter is extremely free of organic matter, this white ash is a certain indication of nitric acid. If there is a great deal, the ash melts readily. By white is meant white as soon as it is incinerated.

"These acid nitrous waters contain phosphates generally. Phosphate of lime, or even alumina and iron, may be precipitated from them by ammonia, if they are direct from animal matter, and have not passed through porous matter sufficient to deprive them of some of the less soluble substances.

"There are many interesting questions to be asked regarding nitrates. I am inclined to think that their presence shows that the most dangerous state of the organic matter is past. When they appear in any solution, the chief escape of putrid gas seems to have ceased; the water may, however, be still dangerous to use, and of course is revolting to the imagination. It will be well to examine how far these suspicions are correct. When complete nitrification has occurred, all that class of evils arising from organic matter direct are prevented.

"This paper was written for a special purpose, and does not pretend to say all that may be said regarding organic matter in water. As to the purification, there are many points to be observed. It has been generally held that nitric acid is the ultimate form which nitrogenous substances of organic origin assumes; but we must remember that plants have the power of decomposing nitrates, and of using the nitrogen for their own purposes. There are conditions, therefore, in which the organic matter may be entirely removed from water; and when we remember how readily soils absorb phosphates and potash, we easily see why common salt should chiefly be left. The oxygen of the nitrates may even be used for the purification of water, whilst the earthy salts and phosphoric acid are precipitated. This operation of decomposing the nitrates seems to be the final one which is at hand for purifying water, and probably explains the marvellous results we frequently see. This decomposition is performed by living plants apparently; but some observations seem to indicate that the effect may be produced by the organic dead matter in water. The nitrites found in plants by Schönbein may possibly have been formed from the nitrates by deoxidation."

Animal and Vegetable Matter—The Common Salt of Sewage.

This subject deserves a further separate treatment.

As animals are formed out of vegetables, the elements which compose each are of course the same; and even the proximate organic principles are difficult to distinguish, especially in a state of decomposition. Still it is remarkable what a clear insight is given into the quality of water by simply boiling down a few thousand grains and burning the residue. We can by the eye and the smell detect humous or peaty acids, nitrogenous organic substances and nitrates, and estimate their amount to a very useful point of accuracy. There may be times when this is the only experiment that can be made. After doing this, and trying other methods many hundred times, I still return to it as delicate and little liable to fail. The want of numerical results is a serious objection, rendering the plan unfit for use when public reports are to be made of the analyses; but it is an excellent guide for the chemist in his laboratory. We may even decide by it the animal or vegetable origin of the matter.

This, however, is not enough; we require to obtain some knowledge of the quantity as well as of the quality, and we even require to know something of the previous history, of the water.

Supposing decomposition to have gone so far that nothing definite can be affirmed as to the organic substances. It has already been shown that the carbon and the nitrogen may entirely disappear, the phosphorus may be precipitated, and the sulphur be, to a large extent, removed, nothing whatever remaining of the original substances except alkalies, some of the alkaline earths, and chloride of sodium, or common salt. The latter in reality is the only substance which, so far as I know, is neither dissipated nor precipitated in whole or in part, potash excepted. The common salt, therefore, may be taken as an indication of the amount of original or organic matter, if we subtract from it any portion which we know to have appeared originally connected only with inorganic substances.

Perhaps all natural waters contain common salt, although rain in a quiet day, and in a clear district, is nearly free from it, perhaps in some places entirely so. In passing through the soil it always collects some chlorides; some of these are the measure of the amount of vegetable matter decomposing in the soil. If we were to estimate the amount of chlorides dissolved by the salt alone out of the inorganic soil, and subtract that from the total, we should obtain the amount dissolved by the action of vegetation breaking down the rocks or soil. If the organic matter were decomposed, the common salt would still flow down the streams and tell its tale. These quantities, however, vary excessively on every soil and in every climate, and with every wind and degree of wind.

The common salt which comes down our streams comes from the wear and tear both of animals and vegetables in the process of living. We cannot tell which came from the animals and which from the vegetables.

From pasture-land and land generally, the greatest amount is from vegetation; and if we collected the organic matter, we should find also there that vegetation gave the greatest amount of matter, except from recently manured fields, or when the water comes from shallow or surface drains, where the land had upon it a good deal of animal matter.

When, however, we come to towns or great collections of animals, the chlorides, as well as the organic substances with which they are allied, are the products mainly of changes taking place in animal life. Let us compare the amount of chlorides to be found in plants and in animals.

Chlorides in the blood of animals (Nasse and Poggiale in Gmelin) as chloride of sodium in 1000 parts.

Dog	4·490
Cat	5·274
Horse	4·659
Calf	4·804
Goat	5·175
Sheep	4·895
Rabbit	4·092
Swine	4·281
Goose	4·246
Hen	5·396

If we take the ashes, the amount is, according to Stölzel,

For ox-blood	51·19
For ox-flesh	4·86
Ashes of the serum in man *—	
Chloride of sodium	61·087
„ potassium	4·085
The ashes of the blood in man	57·641
„ „ of the horse, according to Lehmann	67·105
Urine	22·972

But as the latter is produced so rapidly and frequently, it is the greatest and most constant source of common salt. No plant can be compared with animals in this respect.

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* Robin and Verdeil's 'Traité de Chimie Anatomique.'

Chlorides in Vegetable Matter.

Ashes. Name of Plant.	Chloride of Sodium.	Chloride of Potassium.
Wheat grain	0·27 0·55 0·64 1·60 0·34 Way and Ogston.	
In the whole ash	10·00 Weber.	
Ripe oats. Grains ...	0·92 0·07 0·45 1·24 Way and Ogston.	
Buckwheat straw	(1) 4·55 (2) 3·01 (3) 3·19 (4) 3·75 (5) 3·38 (6) 1·69 E. Wolff.	(1) 7·41 (2) 26·93 (3) 0·77 (4) 3·13 (5) 6·85 (6) 9·70 E. Wolff.
Barley straw with chaff	3·07 5·68 2·14 4·37 5·56 Way and Ogston.	...... 0·90
Barley grains	0·56 1·44 0·41 0·61 0·72 1·59 ... 2·01	...... 1·29 ...... 1·93 Way and Ogston.
Ripe oat straw, Middle	3·03	
Bottom	15·36 J. P. Norton.	
Radishes. Root	10·89 Herepath.	
Chestnut.		
Young wood		9·67
Green shell		15·42 2·20 Wolff.
Asparagus	7·97 Schlienkamp. 12·94 T. Richardson.	
Tea	2·40 2·15 Spooner. 2·25 Twis. 4·66 Hague. 3·25 Homes.	
Orange-tree. Root ...	1·18	
Stem ...	0·25 Rowney and Blow.	
Beech.....................	6·68 0·62 Witting.	
Birch.....................	7·17 9·84 Witting.	

When animals decay, they must, according to the above analyses, give out much more common salt than the same amount of vegetable matter; and this we find to be the case in the drainage of grave-yards. But it is from living animals we find the greater amount of common salt: that which goes into the blood is rapidly thrown out; and as we take with our food amounts varying from 100 to 250 grains of common salt per day, let us say 150, an equal amount must be abstracted from us. It is for this reason sewage-water constantly is found to contain common salt, increasing according to the amount of sewage. For this reason common salt is found in the saltpetre of all countries. It is a part taken from the blood of the dead or the living animal remaining with the nitrogen unchanged, even when the organic matter has been so highly oxidized that nitric acid takes the place of albumen.

Pure spring-, or very fine stream-water gives only a slight precipitate with nitrate of silver. This has a bluish tinge, not being dense enough to become white. When there is more than this slight precipitate, nitric acid is generally found, especially if the water has passed through porous materials.

These tests must be used and the inferences drawn with great discretion. We know that near the sea there may be found a certain amount of chlorides in the springs, rising according to circumstances, until the water becomes brine. The same thing occurs near great deposits of salt, such as in Cheshire and elsewhere. There are also saline wells scattered over most countries, the origin of whose chlorides is quite unknown to us. There is in the rain driven violently from the sea an amount of common salt sufficient, on crystallizing, to dim the windows of houses many miles inland. In manufacturing-towns where coal is burnt, the rain contains more chlorides than rain in the country. Many large districts of tropical countries contain nitrates and common salt in the soil; but these salts in all probability result from oxidized animal matter. Chlorides are found in some districts in this country rather in excess of the average from superficial causes. But notwithstanding all these exceptions, which appear for the moment numerous, I must still consider that the test is one which may be generally used.

In England it is almost universally the case that the presence of much chlorine in drainage-water indicates drainage from animal matter; and no water containing chlorides to a great extent ought to be used without careful examination as to the source. One grain per gallon is too much, and is, in many places, to be suspected of being caused by impure drainage. Of course we must in this case, as in all inquiries, be careful that no disturbing causes intervene. In this country chlorides may be given out from manufactories, in which are constantly made chemical experiments sufficiently large to interfere with our accuracy, if we are not very careful.

It may be supposed here that I am adding many qualifications; but the same may be done in the case of nearly all experiments. No experiment is of value unless it is viewed on all sides to prevent the admission of errors; and I do not know that more care is required in this case than in many others. In the case of chlorides we learn readily the average amount in a district, either in the rain or the drainage, and we detect the smallest increase. Any amount of common salt above the average of the district obtained in a well in a city or camp, or near habitations of men or animals, is an almost certain proof of impure drainage; when the clue has been followed up, I have found the origin in a sewer or some such spot, times without number and for many years. The presence of the sea or manufactories, or of disturbed strata with mineral waters, will seldom, after all, cause any error with a careful person.

Nitrates are very common in small quantities. They are found in water from manured land, in gardens, wells near houses, and, as a consequence, in nearly all town wells; in great abundance near churchyards, if the drainage is direct. They are not necessarily found in sewage which has not flowed through strata. The amount from the atmospheric sources is so minute that it will not interfere with any inquiry regarding the wholesomeness of water at present.

Although caution must be exercised in drawing conclusions from the presence of the chlorides, their absence may be held as conclusive against the presence of decomposed animal matter and excretions of animals in large quantities.

If chlorides and nitrates are found together in water, we may take it for granted that animal matter has existed there or does exist in the water. Of course a very rigid scientific inquiry shows at once that vegetable matter may be present, especially from grain or seeds; but practically this need not affect the question, especially in a sanitary point of view, because, if this accumulation of vegetable matter occurred capable of giving as much nitrogen as animal matter, it would be sufficiently and perhaps equally hurtful when putrid.

If chlorides and nitrates are found in water still capable of decomposing chameleon, the presence of animal matter, or injurious gases or nitrites, may be assumed, a part only being oxidized.

If the chlorides and nitrates are present, but the power of decomposing chameleon absent, then animal matter in a putrefying state is absent; but whether it disappeared minutes or ages before is not shown by chemistry.

The chloride of sodium in a sewage stream of a very sluggish nature was estimated. The following numbers were obtained in the course of 14 miles :—

8·97
7·53
6·25
7·78
7·93
7·78
6·62
40·89

The numbers are pretty uniform, although they rise and fall with evaporation and the additions made to the stream; but whenever certain manufactures come in, a sudden rise is observed. Before this occurs, I believe the chloride of sodium is a perfectly exact comparative measure of the amount of sewage. It may in many cases also be made into a positive measure. This will, however, depend on the mode of managing the sewers.

The conclusion simply is, that chloride of sodium is the most abiding constituent of sewer-water, and, with due precaution, the best index of comparative amounts in the original sewage. When decomposition has not taken place, it may also be used for indicating the comparative strength or value of sewage-water.

Perhaps it will be thought that in here speaking of the impurities of water, when the River Commissioners are making their inquiry in Lancashire, it would be better if I spoke less of the laboratory side of the question, and more of the great evils which have destroyed the beauty and use of our rivers. On one important point I am of the same opinion which years ago I expressed in this Society, that we waste an enormous amount of water. We throw into a stream without compunction impurities which will destroy the value of many hundred times, or perhaps thousand times, their bulk; and this evil may, to a certain extent, be cured with our present knowledge. We try in vain to take out that which we have thrown in. A large part of the evil can be cured by little trouble, although I confess that there is much remaining which demands more time and space than is everywhere conveniently found. Time and space, although by many treated as mere forms of thought, are expensive in Lancashire.

The following may be regarded as a summary of the results required for sanitary purposes:—

1. Quality of the organic matter.
2. Condition of the gases of decomposition.
3. Organic matter: easily decomposed organic matter, and slow to decompose.
4. Nitrates as remnants of organic matter.
5. Nitrites.
6. Chlorides, with precautions as indicating animal sources when greater accuracy is wanted.
7. Oxygen of the dissolved air, as indicating the activity of the decomposition.
8. Total organic matter.
   This the author found by weighing.
9. The usual inorganic analysis, to be spoken of in a second paper.

The author considered that for purely sanitary purposes the Nos. 1, 2, 3, and 6 were the most important.

The organic matter may be divided into many parts—nitrogenous into several parts, and carbonaceous into several. The total nitrogen has been taken by Dr. Frankland very carefully, and the mode of taking oxides of nitrogen improved. The division into separate parts has been begun by Wanklyn. The carbonaceous has not been much studied of late years.

III. *Description of a Dolerite at Gleaston, in Low Furness.*

By E. W. Binney, F.R.S., F.G.S.

Read March 31st, 1868.

During the last 30 years the tract of land known as the Hundred of Low Furness has been investigated and described by several geologists. It was one of the earliest fields investigated by the venerable Sedgwick, who has left us a most valuable memoir of his labours in that district. Since then, Mr. Jopling, myself, and Sir R. I. Murchison and Professor Harkness have published descriptions of the Silurian mountain-limestone and Permian formations of the country. Miss E. Hodgson has also given us information as to the drift-deposits overlying the palæozoic strata. Still, notwithstanding what has been done, it may confidently be asserted that the peninsula comprising the southern part of the Hundred of Low Furness has yet to be carefully examined before its geology can be said to be thoroughly known.

None of the above-named persons appear to have been aware of the occurrence of any trap-dykes in that district, judging from their published writings, with the exception of Mr. Jopling, who, in his sketch of the geology of Low Furness and Cartmel, comprehending the Hundred of Lonsdale north of the sands, published in 1843, when speaking of the geology of Gleaston, says:—"Carboniferous limestone abounds, and in the quarries near the castle are many fossils beautifully preserved in the shale-beds between those of the limestone; there is also a vein of trap." At page 72, the same author says, "There are also appearances of trap near Gleaston, associated with limestone breccia."

In the month of October last, Miss E. Hodgson was so kind as to send me some specimens of rocks from Gleaston which puzzled her a good deal. Some of the parties to whom she had sent them called them dolomites, whilst others named them traps and greenstones. To the latter opinion Miss Hodgson, I believe, was inclined to add the weight of her sanction. Not having previously seen, or even heard of, the occurrence of any such rocks in the district where they were said to be met with, I went over to examine them, and, having been furnished with information by Miss Hodgson, easily found the place where they are exposed at Gleaston Green. At that time Mr. Jopling's book had not been seen by me. The space occupied by these singular rocks, at least so far as at present exposed, is so limited that all that can be seen is very soon ascertained. Specimens were collected, and a few observations made. The former, by the kindness of my friend Professor Roscoe, F.R.S., were analyzed for me in the laboratory of Owens College. It is only by the labours of the chemist that geologists can with any certainty decide upon the age and origin of such rocks as those which are met with at Gleaston.

On approaching Gleaston Green from Scales, the mountain-limestone appears to occupy the country so far as it can be seen. In a quarry below the old castle on the roadside, this rock in the northern part is very hard, and dips to the west at an angle of 25°, whilst in the southern part, where it is softer, it dips in the same direction at an angle of 16°. Owing to the covering of drift, the limestone is not seen nearer to the mill; but it probably extends further in that direction. At a short distance below the mill, dark-coloured laminated shales are seen in the bank on the roadside, dipping apparently at an angle to the N.N.W. We then come to the rocks at the end of the Green. They appear to run in an east and west direction, and are not now exposed for more than twenty yards.

From north to south they may probably extend about forty yards, but certainly for more than half of that distance, towards the beck, they are not now seen until the land rises on the bluff south of the beck, where they reappear as a reddish and bedded trap ash, having an east and west direction, and dipping N.N.W. at an angle of about 60°. This ash is succeeded by a coarse breccia of a few yards in thickness, so far as exposed, which dips slightly north of west, at an angle of 25°, and then is covered up by grass so as not to be seen; but no doubt, from sections in the adjoining lane and borings made on the rise of the strata, dark-coloured shales occur again; and the dyke most probably intrudes through these shales, which are in every respect like limestone shales; but no organic remains were observed in them, so as to make us certain of their geological age.

Returning to the north side of the beck, nothing is exposed of the district west of the hard rocks seen on the Green, owing to the thick covering of drift in that direction; but Mr. Hodgson has proved, by a series of boreholes, the occurrence of upper Permian sandstone, red shale, and limestone shale—the first to the S.W., the second to the west, and the third to the N.W. of Gleaston; and Mr. Ashburner has proved the occurrence of limestone shale and limestone in bores to the E. and N.E. of the locality where the rock is found.

In the bluff on the south side of the stream, as previously stated, the rock appears more like a trap ash of a reddish-brown colour, and exhibits traces of bedding and white lines like carbonate of lime. Immediately adjoining the trap ash, and on its rise, occurs the coarse breccia, composed of fine-grained siliceous rocks, cemented together with quartz, and like a Permian breccia; but although the beds are near together, there was not evidence to show whether the trap ash gradually passed into the breccia or intruded through it; still the breccia appeared to dip in the same direction, but at a much less angle, namely 25°, to a little west of north. This is a very interesting fact to prove; for if the rock graduates into the breccia, it would appear to be of Permian age, and most probably a melaphyr; but if it is intrusive, as the evidence on the whole appears to prove, all we can say is that it is of later date.

This breccia is composed of angular pieces of a fine siliceous stone, of a pink colour, more resembling quartzite than anything else, cemented together by small quartz crystals, and containing minute quantities of protoxide of manganese. The form of the fragments is very like that of the rocks in the Permian breccia of Rougham Point, near the mountain-limestone of Humfray Head, and there consisting for the most part of the neighbouring mountain-limestone; but no limestone has yet been met with in the Gleaston breccia as might have been reasonably expected; and the pink quartzite is a rock hitherto unknown in the district. The Permian brecciæ, so far as my experience goes, although sometimes containing volcanic ash, are composed of the rocks now found in the neighbourhood where they occur, and nearly always vary with the older geological rocks of the district. The composition of the Gleaston breccia makes me hesitate in designating it as Permian, as it may be some rock altered by the dyke.

The rock, in its best state of preservation, is remarkably hard, of a reddish brown colour, has a moderately straight fracture, and a pinkish white streak, and its specific gravity is 2·92.

Three average samples of the rock were taken, two from the north and one from the south side of the beck. All of them were more or less decomposed by exposure to air and moisture, but No. 26 much less than Nos. 23 and 24. Professor Roscoe, on analysis, found their present chemical composition to be as follows:—

	South of stream, No. 23.	North of stream, No. 24.	North of stream, No. 26.
Silica	45·54	50·96	51·10
Peroxide of iron	24·76	24·20	21·58
Alumina	7·70	14·48	9·40
Lime	13·84	7·32	6·24
Magnesia	0·57	0·55	1·33
Carbonic acid	2·78	1·90	2·70
Alkalies, Water (by difference)	4·82	0·59	7·65

The only rock which I know of a similar composition is a probable variety of green earth, resembling a decomposed pyroxene, described by Macfarlane, in the 'Canadian Naturalist,' New Series, vol. iii. No. 1, page 5, in a paper on the cupriferous bed of Portage, Lake Michigan, which consists of—

Silica	46·48
Alumina	17·71
Protoxide of iron	21·17
Lime	9·89
Magnesia (trace).	
Alkalies (by difference)	1·97
Water	2·78

Mr. David Forbes, F.R.S., to whom were forwarded small specimens of the rocks and the above analyses, kindly informed me that the rocks were so much decomposed that it was difficult to pronounce with certainty as to what they were, but he was inclined to think that they were an intrusive dolerite of carboniferous age rather than a melaphyr. The iron had been changed from a protoxide into a peroxide, and the lime had resulted from the decomposition of a lime felspar. As Mr. Forbes had found the presence of titanium united with iron in all the carboniferous dolerites he had examined, I took several ounces of the three samples above given, and having heated them in a crucible, so as to convert the iron from a per- into a protoxide, extracted it by a magnet. About half an ounce of this iron was very carefully examined by Mr.

Thorpe in Dr. Roscoe's laboratory, especially for titanic acid, and no trace of that substance was found. He used the test with microcosmic salt, having separated iron and silica. The absence of titanium in the rock would lead us to believe that it was of later origin than the carboniferous age; but if traces of that metal had been found, it would not only have settled the question as to the age, but it would have shown a connexion with the hæmatite iron-ores of Whitehaven and Ulverston, all of which contain more or less of titanium, as proved by the deposits of that metal found on the sides of old furnaces where hæmatite has been smelted. Is the rock of Permian age? It is certainly not much unlike the melaphyr of the German geologists; and the breccia near the dolerite is not greatly different from that of Ballochmyle, described by Mr. A. Geikie, F.R.S., in the 'Geological Magazine' for December 1866; but we could not obtain direct evidence that the breccia gradually passed into the trap; the latter appeared to protrude through it; but certainly the trap and the breccia dipped in the same direction, the one at about 60° and the other at 25°, a little west of north. This point can only be satisfactorily determined by cutting a trench and showing the contact of the breccia with the trap. The extent of the dyke can only be traced for a few yards east and west, as previously stated; and none of the hæmatite iron deposits, so far as known, have been found south of it. Its age also appears to be more recent, even supposing it to be Permian, than those deposits, which for the most part must be considered of carboniferous age. The occurrence of this trap might have been considered to have some connexion with the deposition of the iron, had it been of carboniferous age; but it is evidently more recent, and therefore could have had nothing to do with it further than to disturb or displace it.

IV. *On some Constituents of Cotton-fibre.*

By Edward Schunck, Ph.D., F.R.S.

Read February 4th, 1868.

It is generally supposed that cotton-fibre, when quite pure, consists entirely of woody fibre, or cellulose, and that its composition is consequently represented by the formula $C_{12}H_{10}O_{10}$. It is certain, however, that in the raw state, as furnished by commerce, it contains a number of other substances, some of which occur so constantly that they may be considered essential constituents of cotton, viewed as a vegetable product. The object of the bleaching process to which most cotton fabrics are subjected, is to deprive them of the impurities which are either natural to the fibre itself or have been introduced, accidentally or designedly, during the process of manufacture. Notwithstanding the importance of an accurate knowledge of everything connected with this staple, from an industrial point of view, I cannot find that the substances existing along with cellulose in cotton-fibre have ever been subjected to a special chemical examination; and all that is known about them may therefore be stated in a few words. Persoz* says that the woody fibre constituting the tissues of cotton, hemp, linen, &c. is not pure; it contains :—

1. A certain quantity of colouring-matter, which is more or less protected from the action of decolourizing agents by the bodies which accompany it, naturally or accidentally.

2. A peculiar resin, insoluble in water and not easily soluble in alkalies, which plays the part of a reserve, and protects the colouring-matters inherent in the fibre from the action of the agents which ought to destroy and remove them.

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* Traité de l'Impression des Tissus, t. ii. p. 20.

3. A certain quantity of fatty matter, a very small portion of which is inherent in the fibre, the greatest part being derived from the operations of spinning and weaving.

4. A neutral substance, which consists either of flour, starch, or glue, according as one or the other has been employed in sizing the warp before weaving.

5. Inorganic salts, some of which are peculiar to the fibre, while the others are derived from the water and the matters used in preparing the dressing of the warp.

In his excellent article on bleaching, in the last edition of 'Ure's Dictionary of Arts,' Dr. R. A. Smith says, "The substances present in cotton goods, and to be treated in bleaching, are as follows:—*a.* the resinous matter natural to the filaments; *b.* the colouring matter of the plant; *c.* the paste of the weaver; *d.* a fatty matter; *e.* a cupreous soap; *f.* a calcareous soap; *g.* the filth of the hands; *h.* iron rust, earthy matters, and dust; *i.* the cotton-fibre itself; *j.* the carbonaceous matter caused by singeing; *k.* the seed-vessels." And he then proceeds to give a short account of these various substances, containing all that was known regarding them at the time the article was written.

My object in undertaking the investigation, the results of which I am about to describe, was to endeavour to throw a little more light on the nature of the substances which are contained in, or attached to, the framework of cellulose of which cotton-fibre mainly consists, and which are, together with the latter, produced by the plant, without taking into consideration the foreign and extraneous matters introduced during the process of manufacture. It is well known that these substances are almost insoluble in water, but soluble in hot alkaline lye. Indeed the principal operation in the bleaching of cotton goods consists in subjecting them for some time to the action of boiling solutions of soda or some other alkali—chlorine or its compounds being only used to impart to them the highest degree of whiteness. Now, I have confined myself in this investigation to those natural constituents of cotton-fibre which are insoluble in water, but soluble in alkaline lye, and which are afterwards precipitated from the alkaline solution by acid. Whether cotton contains naturally any substance soluble in water, or which, being originally insoluble, is rendered soluble therein by the prolonged action of alkalies, is a question on which I pronounce no decided opinion, though I am inclined to believe that there exists in it at least one such body.

For the purpose of procuring the substances which I proposed to myself to examine, I might have taken raw cotton, treated it to exhaustion with alkaline lye, and then operated on the liquor thus obtained. But to this apparently simple course serious objections presented themselves. In the first place, unspun cotton is a very bulky article, difficult to treat in vessels of ordinary size. Secondly, after treatment with alkaline liquids, the cotton might have been rendered useless as an article of manufacture, and consequently unsaleable, which, considering its high price at the time I commenced my experiments, and the very large quantity required in order to obtain definite results, would have entailed a very heavy expense. Thirdly, raw cotton generally contains a quantity of mechanical impurities, chiefly fragments of seed-vessels, which might have yielded up to the alkali substances not properly belonging to the fibre. I therefore preferred employing for my purpose cotton-yarn made from definite unmixed kinds of cotton. Apart from financial considerations, yarn presents certain advantages as compared with raw cotton. It is much freer from mechanical impurities, which are to a great extent removed during, or rather previously to, the operations of spinning; while, on the other hand, in a well-ordered manufactory, little or nothing of a foreign nature is added to the cotton to render it impure. It can also be treated without any trouble, and in large quantities, in the ordinary vessels used by bleachers, without rendering it necessary to set up special apparatus for the purpose.

In the first experiment which I made, 450 lbs. of No. 20 yarn, carefully spun from East-Indian cotton, of the variety called "Dhollerah," was treated, in an ordinary bleacher's kier heated by steam, with boiling water containing 13½ lbs. of soda-ash, for 7½ hours. The resulting dark-brown liquor, after the yarn had been taken out, drained, and slightly washed, was removed into another vessel and mixed with an excess of sulphuric acid, which produced a copious, light-brown, flocculent precipitate, while the liquid became nearly colourless. This precipitate was allowed to settle; the liquid was poured off, and, after being washed with cold water to remove the sulphate of soda and excess of acid, the precipitate was put on strainers of calico and allowed to drain—an operation which, in consequence of its gelatinous nature, occupied some time. A thick pulp was thus obtained, which was found to weigh 60 lbs. Of this 3 lbs. was taken and dried completely, at first in a stove and then in a water-bath, when it left 531 grs. of a brown, brittle, horn-like substance, translucent at the edges. The whole of the precipitate, if dried, would therefore have weighed 10,620 grs., which is equal to 0·337 per cent. of the weight of the cotton; that is to say, 1000 lbs. of cotton-yarn would have yielded nearly 3½ lbs. of matter insoluble in water, but soluble in alkali. This result is, of course, only approximative.

In a second experiment 2400 lbs. of yarn, made from the same kind of cotton, but of rather inferior quality, was treated in the same way; but the quantity of precipitate obtained was not determined.

The third experiment was made with 500 lbs. No. 20 yarn, spun from American cotton of the kind called in commerce "Middling Orleans." It was treated in the same manner as the other two lots, and yielded a precipitate of exactly the same appearance as that from East-Indian cotton, and amounting, when dry, to 0·48 per cent. of the weight of the yarn employed.

The precipitate, in all three cases, consisted for the most part of organic matter. In addition to the matter extracted from the cotton by the alkali, it contained a small quantity of cotton filaments, which had been detached from the yarn during the process of boiling. When incinerated, it left from 2·3 to 6·9 per cent. of a light-yellow non-alkaline ash. This ash consisted chiefly of oxide of iron, alumina, and silicate of alumina, the remainder being sulphate of lime and sulphate of soda.

The three lots of precipitate obtained were treated separately; but as the products which they yielded were essentially the same, the same process of treatment was applied to all. This process was as follows:—

The precipitate was in the first instance treated, while still moist, with boiling alcohol, and the boiling liquid was filtered through a large tin funnel, surrounded by hot water. The residue on the filter was treated again with boiling alcohol, and the process was repeated until nothing more was dissolved. More than half of the precipitate remained undissolved. The undissolved residue was much paler than the original precipitate, the colouring-material of which had, for the most part, passed into solution. The alcoholic liquid, which was of a dark-brown colour, deposited, on cooling, a quantity of dirty white flocks, which were filtered off, washed with alcohol, and then redissolved in boiling alcohol. On adding to this solution a little acetate of lead, a dark-brown precipitate was produced, consisting of a compound of colouring-matter with lead; and the liquid, after being filtered as before through a hot-water funnel, and allowed to cool, deposited a quantity of nearly white flocks. These were filtered off, washed with alcohol to remove the excess of lead-salt, and then treated with boiling dilute caustic soda-lye, in which they melted like wax or fat, without dissolving. The mass, after cooling, was filtered off, washed with water, and then dissolved again in boiling alcohol, to which a little animal charcoal was added. The solution, which was quite colourless, deposited, after filtration and cooling, a quantity of crystalline scales, of a beautiful pearly lustre, and generally in such abundance as to convert the liquid into a thick jelly. This deposit, which was at first very bulky, on being filtered off and dried, shrank considerably, and yielded at last a white or faintly yellow wax-like cake, consisting of a body which I propose to call *Cotton-Wax.*

The alcoholic liquid from which this substance was deposited was of a dark-brown colour. It contained two bodies, which, for want of a better term, may be called colouring-matters, and also a small quantity of a crystalline fatty acid having the properties and composition of margaric acid. The two colouring-matters resemble one another in most of their properties, but may be distinguished by their different degrees of solubility in alcohol—one being easily soluble in cold alcohol, the other soluble in boiling, but very little soluble in cold alcohol. As these substances possess very few characteristic properties, and it is indeed doubtful whether they are peculiar to cotton or not, I will, instead of giving them peculiar names, call the one which is most easily soluble in alcohol simply *Colouring-matter A,* the other *Colouring-matter B.* These bodies were separated from one another and from the fatty acid in the following manner:—The liquid having been evaporated, left a brown, semisolid, resinous mass, which was treated with a small quantity of warm, or boiling alcohol. This dissolved only a part of the mass, the remainder being left undissolved as a brown powder, consisting for the most part of the colouring-matter B. The liquid having been filtered, the powder was treated with a large quantity of boiling alcohol, in which it usually dissolved almost entirely. A small quantity, however, of a brown flocculent substance was generally left undissolved; and this substance, which consisted of impure pectic acid, having been filtered off while the liquid was hot, the latter, on cooling, deposited the greatest part of the colouring-matter as a brown powder, which only required to be filtered off, washed with alcohol, and dried. The dark-brown alcoholic liquid, containing the more soluble colouring-matter A, as well as the fatty acid and a portion of the colouring-matter B, was now mixed with ammonia and chloride of barium, which gave an abundant greyish-brown precipitate, consisting of all the organic matter previously contained in the liquid in combination with baryta. This precipitate was filtered off, and then treated with boiling alcohol. The latter being filtered boiling hot, afforded, on cooling, a small quantity of a brownish crystalline deposit. This process was repeated as long as the filtered liquid deposited anything, and until, therefore, the precipitate was thoroughly deprived of everything soluble in alcohol. The crystalline matter deposited from the alcoholic liquid consisted almost entirely of the baryta-salt of a fatty acid. After being filtered off, it was treated with warm dilute hydrochloric acid, by which it was decomposed, the fatty acid rising to the surface as a light-brown oil, which became solid on cooling. The latter, after being well washed with hot water, was dissolved in warm alcohol, and the solution, having been decolourized with animal charcoal and filtered, was evaporated spontaneously, when it left a perfectly white mass of crystalline needles, consisting of the fatty acid in a state of purity.

The baryta-precipitate, after treatment with boiling alcohol, was decomposed with warm dilute hydrochloric acid, which left undissolved a dark-brown, semifused mass. This mass, after being kneaded in hot water and thoroughly freed from barium-salt, was treated with boiling alcohol. The alcohol generally left some brown powder undissolved, consisting of colouring-matter B; and an additional quantity was usually deposited from the alcohol on cooling. Having been filtered off, it was purified by dissolving it in a sufficient quantity of boiling alcohol, as above described. The filtered liquid left, on evaporation, a dark-brown resinous mass, which was reduced to a fine powder, and then agitated in a flask with ether. The ether dissolved only a very small portion of the mass, but it served to remove a trace of fatty acid contained in it. The ether, on evaporation, left a brown fatty residue, which, after combination with baryta and treatment of the compound with boiling alcohol, as just described, yielded a quantity of pure fatty acid. The portion left undissolved by the ether was treated with cold alcohol, which dissolved the greatest part of it. The dark-brown solution, after being filtered from a small quantity of colouring-matter B, which was usually present, was evaporated to dryness, when it left a dark-brown, shining, resin-like residue, consisting of the colouring-matter A.

The greatest part of the precipitate produced by acid in an alkaline extract of cotton is insoluble in alcohol. This portion, when dry, has the appearance of a brown, friable, earthy mass, among which cotton filaments, particles of woody fibre, and other impurities derived from the cotton may be seen. When burnt, it leaves a considerable quantity of ash. Its weight, when dry, amounted, in the case of East-Indian cotton, to 77 per cent. of that of the entire precipitate, in that of American cotton to 66 per cent. Though much lighter in colour than the precipitate before treatment with alcohol, it still contains a quantity of colouring-matter, which cannot be extracted by means of boiling alcohol, not even when ammonia is added—a circumstance which is due either to the presence of some colouring-matter distinct from those just referred to, or perhaps to the latter being intimately combined with, or firmly attached to, some other constituent of the mass. I have reason to suppose that it contains also a small quantity of some albuminous substance, the presence of some such substance being rendered probable by an examination of the products of decomposition with caustic alkali, as will be explained presently. Its chief constituent, however, is a body belonging to the pectine class, generally either pectic or parapectic acid, or a mixture of both. The presence of some such body is indicated by the gelatinous appearance of the precipitate with acid, its increasing in bulk and partially dissolving in water after the precipitating acid has been removed, and its shrinking and curdling on the renewed addition of acid or of salts or alcohol. Nevertheless the isolation and preparation, in a state of purity, of this substance cannot be effected without considerable difficulty, in consequence of the pertinacity with which the colouring-matter adheres to, and accompanies it. If the precipitate, after exhaustion with boiling alcohol, is treated with boiling water, the latter dissolves a considerable quantity of the body in question, but on evaporation it leaves a dark-brown residue, which, on analysis, is found to contain no inconsiderable amount of nitrogen, a proof of the presence of one or both of the colouring-matters of cotton. I was unsuccessful in all my attempts to remove this impurity by means of animal charcoal, earths, metallic oxides, or salts of any kind. I succeeded, however, in devising two methods of preparing, in a state of tolerable purity, a body soluble in water having the properties and composition of Frémy's parapectic acid, and thus proving that cotton contains a substance belonging to the pectine series. I was unable to procure pectine itself, though there can be little doubt that it is pectine which exists originally in the fibre and gives rise to the formation of the pectic and parapectic acids which are contained in the alkaline extract. One of the methods which I employed for preparing this acid, founded on the insolubility of its alkaline compounds or salts in an excess of caustic alkali, was as follows :—

The precipitate from the alkaline extract of cotton, after exhaustion with boiling alcohol, was, without being previously dried, dissolved in dilute caustic soda-lye. The solution, after being filtered in order to separate the filaments of cotton and other impurities which were present, was mixed with alcohol, which produced an abundant light-brown flocculent precipitate, consisting of impure pectate and parapectate of soda. The liquid, which was brown, contained a considerable quantity of colouring-matter, and was filtered. The precipitate, after being washed with alcohol, was again dissolved in water, and to the solution there was added a quantity of strong caustic soda-lye, which produced a precipitate of a much paler colour than that with alcohol, while the liquid retained in solution another portion of colouring-matter. The precipitate having been allowed to settle, the liquid was decanted, and the precipitate was stirred up with a fresh quantity of caustic soda-lye, the process being repeated until the supernatant liquid had become colourless. The precipitate was now treated with warm dilute hydrochloric acid, which took up the alkali, leaving a quantity of pale-brown flocks undissolved. These were filtered off and washed for some time with cold water, the latter being exchanged for alcohol as soon as the principal part of the acid and chloride of sodium had been removed, in order to prevent a loss of parapectic acid, which is soluble in pure water, though insoluble in water containing strong acids or salts.

As soon as the liquid ceased to give a precipitate with nitrate of silver, the mass was taken from the filter, pressed between folds of blotting-paper in order to take up the alcohol, and then treated with boiling water for some time. A milky liquid was thus obtained, which was set aside for some time in order to allow the flocculent matter suspended in it, which prevented it from filtering readily, to settle. The liquid having been decanted from the deposit, was filtered and evaporated, when it left an amorphous light-brown residue, resembling gum or gelatine, and having the properties and composition of parapectic acid. The flocculent deposit consisted of pectic acid; but it was contaminated with a considerable quantity of colouring-matter, from which it could, in most cases, not be separated without undergoing a change. If, for instance, it was dissolved again in caustic soda, and the process just described was repeated, it was usually converted for the most part into a substance soluble in water—that is, into parapectic acid.

The second method of preparing the acid is far less tedious, and yields a product much freer from colour than the one just described. It differs, however, from the latter in one particular only, viz. in the use of a bleaching agent, instead of caustic soda as a precipitant. To the watery solution of impure pectate and parapectate of soda obtained in the same manner as before, there was added a clear solution of chloride of lime, which produced an abundant precipitate, consisting mostly of pectate and parapectate of lime. By adding an excess of the bleaching-solution, the colour of the precipitate, which was at first brown, became gradually lighter. As soon as it had lost all its colour, or retained only a yellowish tinge, it was filtered off, washed with water, and treated with dilute hydrochloric acid. The white flocks left undissolved by the acid were filtered off, washed at first with water and then with alcohol, and lastly treated as before with boiling water, which left the pectic acid undissolved. The filtered liquid left, on evaporation, a residue of parapectic acid, having only a faint yellowish tinge, like that of the purest gum. It did not differ in composition from that prepared by the process first described, proving that the chloride of lime produced no decomposing effect, but merely served to destroy and remove the colouring-matter.

I shall now proceed to give a short account of the properties and composition of the various substances the preparation of which has been just described.

Cotton-Wax.

There can be no doubt that this substance must be classed with the waxes, bodies which are distinguished by their insolubility in water and alkaline liquids, their sparing solubility in alcohol and ether, and their high melting-point. Indeed it so closely resembles in many respects the better-known vegetable waxes, such as the cerosine prepared by Avequin from the leaves of the sugar-cane, and the wax from the leaves of the Carnauba palm (*Corypha cerifera*), examined by Brande* and Lewy†, that its identity with one of these may be suspected. Until it has been ascertained whether it is really a distinct member of the class to which it belongs, or not, I think the name Cotton-Wax which I have given it will suffice to distinguish it from other nearly allied bodies. It has the following properties:—

It is insoluble in water, but soluble in alcohol and ether. If a concentrated solution in boiling alcohol be allowed to cool, the greatest part of the substance is deposited, causing the liquid to assume the appearance of a thick white jelly, like starch-paste, which, when examined under the micro-

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* Philosophical Transactions for 1811, p. 261.

† Journ. f. prakt. Chemie, B. xxxvi. S. 65.

scope, is found to consist of minute scales and needles suspended in the alcoholic liquid. When the mass is filtered off and dried, it shrinks very much, and is converted into a coherent cake, which has a waxy lustre, is translucent, friable, and lighter than water, and does not soften when kneaded between the fingers. A specimen of the substance from East-Indian cotton, when heated in the usual manner in a capillary tube, was found to melt at 86° C. to a transparent liquid, which solidified again at 81° C. Another specimen, from American cotton, fused at 86° C., and became solid again at 82° C. According to Avequin, cerosine fuses at 82° and solidifies at 80°. Carnauba wax, according to Lewy, melts at 83°.5. When heated on platinum-foil, cotton-wax gives off an odour resembling that of burning fat, and then burns with a bright flame, without leaving any ash. If heated in a tube it melts, emits a penetrating odour, and then volatilizes completely, yielding an oily sublimate which soon becomes solid and crystalline, and seems to consist of unchanged substance. Singular to say, cotton-wax, when pure, is quite insoluble in caustic alkalies; and it is therefore difficult to account for its presence among the products extracted from cotton by soda-lye, unless it be assumed that it exists originally, not within the fibre, but on its surface, and is merely fused and mechanically detached by the action of the hot liquid. When treated with boiling dilute caustic soda-lye, it melts without dissolving, and the filtered liquid gives only a trifling precipitate with acid; but when carefully heated with hydrate of soda, it yields a compound which is entirely soluble in water; and the solution now gives, with acid, a copious white flocculent precipitate, which consists of a true fatty acid, formed by a process similar to that of ordinary saponification, or more so perhaps to that by which alcohols are converted into the corresponding acids. When cotton-wax is treated with fuming nitric acid, it does not dissolve, even when the acid is boiled for some time; it merely melts, and solidifies on cooling. But a change has nevertheless taken place; for if the excess of acid be poured off, and the undissolved fatty matter, after being washed, be treated with boiling caustic soda-lye, it dissolves entirely, and the solution gives, with acid, a white flocculent precipitate. By the action of nitric acid, therefore, the substance is probably transformed in the same manner as by the action of dry caustic soda.

In order to prepare the substance for analysis, it was kept in a state of fusion in the waterbath for several hours, then reduced to powder and placed in a bell over sulphuric acid. Its analysis yielded the following results.

I. 0·2605 grm. obtained from East-Indian cotton gave 0·7675 grm. carbonic acid and 0·3365 grm. water.

II. 0·3130 grm. of the same lot gave 0·9215 grm. carbonic acid and 0·4010 grm. water.

III. 0·1615 grm. prepared from American cotton gave 0·4760 grm. carbonic acid and 0·2110 grm. water.

These numbers correspond in 100 parts to

	I.	II.	III.
C	80·35	80·29	80·38
H	14·35	14·23	14·51
O	5·30	5·48	5·11
	100·00	100·00	100·00

It would have been easy to devise a formula corresponding to these numbers; but as the quantity of material at my disposal was not sufficient to enable me make any experiments to determine the atomic weight of the substance, the accuracy of any such formula would have been a mere matter of probability. That its composition is very similar to that of other vegetable waxes will be seen by comparing the above numbers with those obtained by Dumas*

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* Annales de Chim. et de Phys. t. lxxv. p. 223.

in the analysis of cerosine, and by Lewy * in that of Carnauba wax, which were as follows :

	Cerosine.	Carnauba wax.
C	81·00	80·36
H	14·16	13·07
O	4·84	6·57
	100·00	100·00

Its composition differs also but little from that of the wax-like body derived from the *Ceroxylon andicola.*

For the purpose of examining the product of decomposition which is obtained from cotton-wax by the action of dry caustic alkalies, I took some of the substance, added caustic soda-lye, evaporated to dryness, and heated the dry residue rather strongly, then added water and evaporated again, repeating the process several times, until the product was found to be entirely soluble in water. An excess of acid was then added; the matter left undissolved was filtered off, washed with water, and dissolved in a little boiling alcohol. The solution on cooling deposited white crystalline flocks, which after being filtered off and dried had the appearance of a white opaque mass resembling stearic acid. This mass, when examined under the microscope, was found to consist of minute star-shaped aggregates of needles. When heated it was easily volatilized, yielding an oily sublimate which became crystalline on cooling. It dissolved easily in alkaline liquids, and the solutions frothed on being boiled. The solution in ammonia gave with chloride of calcium a white curdy precipitate. In boiling water it melted, forming transparent oily drops, which became solid and crystalline on cooling. In a capillary tube it fused at 85° C. and solidified at 77° C. The quantity of substance at my disposal was just sufficient for one analysis, which yielded the following results.

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* Journ. f. prakt. Chemie, B. xxxvi. S. 74.

0·1815 grm. gave 0·5315 grm. carbonic acid and 0·2295 grm. water.

In 100 parts it contained therefore

C	79·86
H	14·04
O	6·10
	100·00

The composition of this substance does not differ very widely from that of cerosic acid, the acid formed by the action of dry potash and lime on cerosine, which, according to Lewy, consists of

C	80·11
H	13·55
O	6·34
	100·00

There can be little doubt, I think, that cotton-wax is identical with the "resin" which, according to Persoz and others, is peculiar to cotton, and which is said to protect the filaments from the action of external agents. I incline to the belief that it is formed on the exterior of the fibres, and clothes them with a thin waxy covering (like the coating of a similar material sometimes found on leaves and fruit), and thus imparts to them their well-known property of resisting water. If it be supposed to be contained solely in the interior of the cells forming the fibres of cotton, it is difficult to conceive by what means any portion of it comes to be dissolved in the alkaline lye, in which when pure it is quite insoluble. Its solubility in alkalies is not promoted in any appreciable degree by admixture with the colouring-matters of cotton; for on adding to it a quantity of either of the two colouring-matters and acting on the mixture with alkali, the colouring-matter is simply dissolved, leaving the wax behind.

The quantity of cotton-wax obtained in my experiments was exceedingly small. It amounted to about 1 per cent. of the weight of the brown precipitate thrown down by acid from the alkaline extract. It is by no means certain, however, that this was the total quantity contained in the cotton.

Fatty Acid.

This substance when prepared in the manner above described has the appearance of a white mass consisting of microscopic needles arranged in spheres. It fuses at 55°·5 C. and solidifies again at 50°·5 C. When heated on platinum it melts and then burns with a highly luminous flame. Heated in a tube it is volatilized, leaving hardly any residue and furnishing an oily sublimate which soon becomes solid. It dissolves readily in alcohol and ether. The alcoholic solution reddens litmus-paper slightly. It dissolves in warm caustic potash and soda-lye, as well as in liquid ammonia; and the solutions froth on being boiled. The solution in potash yields, on cooling and standing, a quantity of crystalline needles, while the solution in soda gives immediately a thick soap which fills the whole liquid. The solution in ammonia deposits on cooling shining crystalline scales. The compound with soda is obtained in a state of purity by adding carbonate of soda to the alcoholic solution of the acid, evaporating to dryness, treating the residue with boiling absolute alcohol, filtering and evaporating. When this compound is dissolved in boiling water, and the solution is allowed to cool, it is deposited again as a gelatinous mass, which when examined under the microscope is found to consist of small needles, arranged in star-shaped or fan-shaped masses. The watery solution gives with the chlorides of barium and calcium white flocculent precipitates, with acetate of lead an abundant white precipitate, and with nitrate of silver a white flocculent precipitate which becomes only slightly discoloured on exposure to light or on heating the liquid. The alcoholic solution of the acid gives with acetate of baryta a white granular precipitate, which is soluble, though with difficulty, in boiling alcohol. With acetate of magnesia it gives at first no precipitate; but after some time a white crystalline deposit is formed, consisting of the magnesia-compound. These reactions belong to the group of fatty acids of which stearic and palmitic acids are members. In order to ascertain the exact place in the series occupied by the acid from cotton, it was submitted to analysis, the following being the results obtained.

I. 0·2895 grm. from East-Indian cotton, after being kept in a state of fusion for several hours in the water-bath, gave 0·7990 grm. carbonic acid and 0·3365 grm. water.

II. 0·2270 grm. from American cotton gave 0·6280 grm. carbonic acid and 0·2700 grm. water.

These numbers lead to the following composition:—

	Calculation.		I.	II.
C_{34}	204	75·55	75·27	75·45
H_{34}	34	12·59	12·91	13·21
O_4	32	11·86	11·82	11·34
	270	100·00	100·00	100·00

The formula $C_{34} H_{34} O_4$ belongs to margaric acid, one of the products derived from ordinary fats. Modern researches have rendered it almost certain that what was formerly called margaric acid is in fact a mixture of stearic and palmitic acids. In consequence, however, of the minute quantity of the substance obtained from cotton, I was unable to undertake any experiments for the purpose of separating its constituents from one another, but was obliged to content myself with proving the presence of one of the ordinary products of the saponification of fats and oils among the bodies extracted from cotton by alkalies. The quantity procured from American cotton was, indeed, so small that I was hardly able to purify it sufficiently for the purposes of analysis, and the amount of hydrogen which it was found to contain differed accordingly rather widely from that demanded by theory; the presence of some impurity or other in this specimen was also indicated by its rather lower melting-point.

As regards this fatty acid, the question will naturally arise, whether it is to be considered a natural constituent of the fibre, or whether it is a foreign body introduced subsequently to the gathering of the cotton, either before or during the process of manufacture; but this is a question to which it is not easy to give a satisfactory reply. I am assured by persons practically conversant with cotton-spinning that it is impossible for the cotton to be contaminated with any substance of a fatty nature during the process of its conversion into yarn, provided ordinary care be taken, since it can only in consequence of gross carelessness come into contact with the oil or fat used in greasing the machinery. On the other hand, it is quite possible that after the cotton has been gathered, especially during the process of ginning, a portion of the oil of the seed may escape, diffuse itself among the fibres, and give rise to the formation of fatty acid in consequence of the action on it of the alkali. Be this as it may, I have not failed in any of my experiments, whether made with East-Indian or American cotton, to obtain a small quantity of a solid crystalline fatty acid. I have also searched for oleic acid, but without success—though traces of a dark-brown oily substance always presented themselves when the ether with which the colouring-matter A had been treated was evaporated. This oily matter I found it impossible to purify.

Colouring-Matter A.

This substance is easily soluble in alcohol and is left on evaporating the solution as a dark-brown, shining, brittle, amorphous resin, which is transparent in thin layers. In boiling water it softens and melts to a pasty mass, which becomes hard and brittle again on cooling. It is insoluble in ether, and is accordingly precipitated on the addition of ether to the alcoholic solution. When heated on platinum it melts, swells up considerably, and burns with a bright, but smokeless, flame, leaving a very voluminous coal which gradually burns away, only a slight trace of ash being left. It contains nitrogen, and when heated with soda-lime gives off ammonia in abundance. It dissolves in concentrated sulphuric acid and in glacial acetic acid, giving dark-brown solutions, from which it is reprecipitated by water in brown flocks. It is easily decomposed by boiling nitric acid, yielding a yellow solution, which on evaporation leaves an abundance of oxalic acid, but no picric acid. It is easily soluble in caustic and carbonated alkalies, giving dark yellowish-brown solutions, from which it is reprecipitated by acids in light-brown flocks. The ammoniacal solution leaves, on evaporation, a dark-brown, amorphous residue, which does not dissolve again entirely in water, in consequence of a loss of a part of its ammonia during evaporation. The ammoniacal solution gives brown precipitates with the chlorides of barium and calcium, but none with sulphate of magnesia. With nitrate of silver it yields a dark reddish-brown precipitate, which dissolves completely in an excess of ammonia, giving a yellowish-brown solution, which remains unchanged on being boiled. The alcoholic solution of the substance gives brown precipitates with the acetates of baryta, lead, and copper. On the addition of an alcoholic solution of potash it gives a brown precipitate, which sinks rapidly, forming a glutinous deposit. A similar effect takes place when an alcoholic solution of soda is employed. The substance is easily soluble in a boiling solution of acetate of soda, and is reprecipitated by acids in brown flocks. When the substance in a finely divided state, as obtained by precipitation from its alkaline solution with acid, is exposed for some time to the action of chlorine, its colour changes gradually from brown to pale yellow. After being filtered off and washed with water, the product of the action dissolves easily in alcohol, and is left on evaporation as a brown transparent resin, which contains chlorine, but in other respects closely resembles the original substance.

The analysis of this colouring matter led to the following results :

I. 0·3735 grm. prepared from East-Indian cotton and dried at 100° C. gave 0·7980 grm. carbonic acid and 0·2160 grm. water.

0·5200 grm. burnt with soda-lime gave 0·6655 grm. double chloride of platinum and ammonium.

II. 0·3745 grm. of the same specimen gave 0·7985 grm. carbonic acid and 0·2160 grm. water.

III. 0·3925 grm. obtained from American cotton gave 0·8400 grm. carbonic acid and 0·2085 grm. water.

0·5800 grm. gave 0·4820 grm. chloride of platinum and ammonium.

IV. 0·3845 grm. of the same gave 0·8245 grm. carbonic acid and 0·2010 grm. water.

0·7000 grm. gave 0·5920 grm. chloride of platinum and ammonium.

These numbers correspond in 100 parts to

	I.	II.	III.	IV.
C	58·27	58·11	58·36	58·48
H	6·42	6·40	5·90	5·80
N	8·03	......	5·22	5·31
O	27·28	......	30·52	30·41
	100·00		100·00	100·00

It will be seeen that the composition of the substance varied, especially as regards the nitrogen, much more than it ought to have done, supposing it to have been perfectly pure. In consequence of the amorphous nature of the product it is difficult to determine whether Indian and American cotton contain two distinct colouring-matters, both easily soluble in alcohol and having the same general physical properties, or whether in one or both cases the specimens submitted to analysis, though essentially the same substance, were not chemically pure. It is, I believe, a difficult task to obtain, in a state of purity, an uncrystallizable resinous body having few characteristic properties; and the results arrived at by examining the composition of such bodies are seldom satisfactory.

Colouring-Matter B.

This substance is deposited from its solution in boiling alcohol as a brown powder, which, after being filtered off and dried, forms coherent masses of a colour varying from light to dark brown, which may be easily broken, the fracture being dull and earthy. In boiling water it softens and yields a dark-brown cake. It is almost insoluble in cold alcohol, and when it has once been dried it dissolves with great difficulty even in boiling alcohol. By this property it may easily be distinguished from the other colouring-matter, which it closely resembles in most other respects. When heated on platinum, it burns without previously melting; and the carbonaceous residue burns away with difficulty, leaving at last a bulky white or yellowish ash. This ash is not alkaline, and consists principally of alumina and sulphate of lime. The ash was in most cases so considerable that I was led to suspect that this colouring-matter might possibly be a compound of the other with some earthy base, in which case the striking similarity in the properties and reactions of the two substances would have admitted of an easy explanation. In order to submit this supposition to the test of experiment, I took some of the colouring-matter B, pounded it very fine, added a little concentrated sulphuric acid and then absolute alcohol, after which the whole was well stirred in a mortar and left to stand for some time. The liquid, which had a brown colour, was filtered and mixed with water, which gave a brown precipitate, This was filtered off, washed with water, and dissolved in boiling alcohol. The solution left, on evaporation, a brown resinous residue, which was almost insoluble in cold alcohol, and contained, therefore, none of the colouring-matter A.

In determining the composition of this substance the following results were obtained :

I. 0·4030 grm. prepared from East-Indian cotton and dried at 100° C. gave 0·8370 grm. carbonic acid and 0·2245 grm. water.

0·6920 grm. gave 1·0240 grm. chloride of platinum and ammonium.

II. 0·4000 grm of the same specimen gave 0·8345 grm. carbonic acid and 0·2230 grm. water.

0·6970 grm. gave 1·0535 grm. chloride of platinum and ammonium.

1·2005 grm. left on being incinerated 0·0135 grm. of ash = 1·12 per cent.

III. 0·4005 grm. of another specimen from East-Indian cotton gave 0·8355 grm. carbonic acid and 0·2265 grm. water.

0·7015 grm. gave 1·0575 grm. chloride of platinum and ammonium.

0·5870 grm. left 0·0045 grm. of ash = 0·76 per cent.

IV. 0·4015 grm. from American cotton gave 0·8505 grm. carbonic acid and 0·2060 grm. water.

0·7045 grm. gave 0·8615 grm. chloride of platinum and ammonium.

V. 0·4230 grm. of the same gave 0·8930 grm. carbonic acid and 0·2130 grm. water.

0·7035 grm. gave 0·8240 grm. chloride of platinum and ammonium.

0·7910 grm. left 0·0095 grm. of ash = 1·20 per cent.

After deducting the ash, these numbers correspond in 100 parts to—

	I.	II.	III.	IV.	V.	Mean.
C	57·28	57·53	57·32	58·47	58·26	57·77
H	6·25	6·26	6·32	5·77	5·65	6·05
N	9·39	9·59	9·53	7·77	7·43	8·74
O	27·08	26·62	26·83	27·99	28·66	27·44
	100·00	100·00	100·00	100·00	100·00	

It will be seen that in this case, as in that of colouring-matter A, there is a wider discrepancy in the numbers yielded by analysis than ever takes place with a perfectly pure substance. Nevertheless the composition of colouring-matter B, as represented by the mean of the numbers just given, approaches so closely that of colouring-matter A from East-Indian cotton as to make it probable that, when pure, the two bodies do not differ in composition from one another *.

From what has just been stated it may be inferred that, as regards their chemical properties, these colouring-matters possess very little interest. It is simply the fact of their being the cause of the yellow or brownish tinge natural to raw cotton which gives them any importance, and makes a knowledge of their properties desirable from a practical point of view. The darker shade of colour seen in the so-called "nankin" cotton is probably due to a great excess of these colouring-matters existing in the fibre. It is certainly not caused by oxide of iron, since the ash of this kind of cotton contains no more iron than that of or-

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* It is quite possible that these colouring-matters may be products of decomposition derived from some other substance existing in the fibre, and that they may consequently vary in composition according to the strength of the solvent used for extraction, the time during which it acts, and other circumstances.

dinary kinds, and the colour is for the most part removed by treatment with caustic alkali.

Pectic Acid.

A considerable portion of the organic matter extracted from cotton by caustic alkali consists of a body belonging to the pectine class. Which member of this class it is that exists originally in the fibre is a question on which I express no opinion, though there can be no doubt that the precipitate produced by acid in the alkaline extract of cotton contains pectic acid itself. This acid, which is insoluble in water, is almost entirely converted, during the process of purification which I adopt, into an acid soluble in water, which seems to be identical with Frémy's parapectic acid. The chief properties of this soluble acid are as follows :—

On evaporation it is left as a light-yellow, amorphous, translucent substance, resembling gum or gelatine. When heated on platinum it burns with a slight flame, without previously melting, leaving a little ash, which is yellow or brown and only slightly alkaline, and consists chiefly of alumina, together with oxide of iron and lime. The watery solution is clear and colourless and reddens litmus-paper. It gives abundant, white, flocculent precipitates with sulphuric, nitric, hydrochloric, and acetic acids, as well as with baryta-water and acetate of lead. When the watery solution is mixed with several times its volume of alcohol the liquid gelatinizes, the jelly being perfectly clear and transparent. The substance is decomposed with difficulty by boiling nitric acid; and no oxalic acid can be discovered among the products of decomposition. When treated with strong caustic potash or soda-lye it turns yellow; and on boiling, the liquid assumes a bright-yellow colour similar to that of chromate-of-potash solution. On continuing the action of the caustic alkali the substance is gradually dissolved; but on adding an excess of acid no precipitate is produced, showing that a complete change has taken place. In very dilute potash or soda-lye, as well as in liquid ammonia, it dissolves readily on boiling. The solutions when mixed with alcohol give colourless jellies, consisting of the salts of the respective bases. If the precipitated ammonia-compound, after being filtered off and washed with alcohol until the excess of ammonia is removed, be treated with boiling water, it dissolves and the solution leaves, on evaporation, a transparent, amorphous, nearly colourless residue, which separates from the sides of the vessel in shining scales. This residue dissolves again entirely in water. The solution is neutral to test-paper, and gives flocculent precipitates with alkaline salts, such as chloride of sodium and chloride of ammonium, as well as with all earthy and metallic salts which I have tried, except perchloride of mercury and chloride of gold. The precipitate with nitrate of silver, which is white, does not change much when exposed for several days to light, and when dissolved in ammonia yields a solution which on being boiled becomes yellow but deposits no metallic silver. When a solution of the acid in a sealed tube is heated for some time in the water-bath, it is found to have undergone a complete change. The liquid leaves, on evaporation, a brown, slightly deliquescent residue, which dissolves again with ease in cold water. The solution has a strong acid reaction. With caustic alkalies it strikes a deep yellow colour. It gives flocculent precipitates with baryta-water and acetate of lead, but none with hydrochloric acid or chloride of sodium. With nitrate of silver it gives a precipitate which blackens on exposure to light, and when made alkaline it reduces oxide of copper at the boiling-heat. The acid has, in fact, been converted into the substance, or mixture of substances, to which Frémy has given the name of metapectic acid.

The reactions just described are of themselves almost sufficient to prove that the acid obtained from cotton is one of the derivatives of pectine. All uncertainty on this point, however, was removed by an examination of its composition, which led to the following results :—

I. 0·4100 grm. obtained from East-Indian cotton, and purified by the chloride-of-lime process, gave, after being dried at 100° C., 0·6070 grm. carbonic acid and 0·1755 grm. water.

0·7170 grm., burnt with soda-lime, gave 0·0385 grm. chloride of platinum and ammonium, containing 0·0024 grm. nitrogen = 0·33 per cent.

1·0210 grm. left 0·0135 grm. of ash = 1·32 per cent.

II. 0·4635 grm. from American cotton, purified by the same process and dried at 100° C., gave 0·6660 grm. carbonic acid and 0·1945 grm. water.

0·8375 grm. gave 0·0370 grm. chloride of platinum and ammonium, containing 0·0023 nitrogen = 0·27 per cent.

0·3215 grm. left 0·0065 grm. of ash = 2·02 per cent.

III. 0·5000 grm. from American cotton purified by means of caustic soda, in the manner above described, gave 0·6675 grm. carbonic acid and 0·2015 grm. water.

0·7545 grm. gave 0·0525 grm. chloride of platinum and ammonium, containing 0·0033 nitrogen = 0·43 per cent.

0·5125 grm. left. 0·0440 grm. of ash = 8·58 per cent.

If the ash be deducted and the small amount of nitrogen, which evidently belonged to some slight impurity, be neglected, these numbers lead to the following composition:

	I.	II.	III.
C........	40·91	39·98	39·82
H	4·81	4·75	4·88
O	54·28	55·27	55·30
	100·00	100·00	100·00

The results arrived at by different chemists in examining the composition of pectine and its derivatives vary so much that it is difficult to identify any member of the class by means of analysis only. The composition just given approaches that of Frémy's parapectic acid, which, according to him, consists of

C	41·67
H	4·97
O	53·36
	100·00

I have mentioned that the acid originally contained in the alkaline extract of cotton is insoluble in water, but is gradually converted during the process of purification into a soluble acid. Of the insoluble acid, the true pectic acid, I obtained on one occasion only a sufficient quantity for the purposes of analysis. It was left undissolved by boiling water in the preparation of the specimen of soluble acid employed for the second of the above analyses, and when dry was dark-grey and horn-like, and was with difficulty reduced to powder.

0·5270 grm. of this substance gave 0·7880 grm. carbonic acid and 0·2315 grm. water.

0·6400 grm. left 0·0200 grm. of ash=3·12 per cent.

After deducting the ash, these numbers correspond in 100 parts to

C	42·09
H	5·03

According to Chodnew *, pectic acid contains in 100 parts

C	42·22
H	5·24
O	52·54

Frémy's experiments led him to infer that there exists in plants a body insoluble in water, which he calls pectose, and which, by the action of acids, alkalies, or the pectic ferment (pectase), is converted first into pectine, then into pectic acid, and other products in succession. It is not

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* Annalen d. Chem. u. Pharm. B. li. S. 363.

improbable that this may be the case with cotton-fibre also; but I have as yet made no experiments to decide this point. The mere fact of the presence of pectine in cotton-fibre need not excite surprise, since it is a common constituent of various parts of plants, and probably stands in some close relation to cellulose, from which it is perhaps derived by decomposition, or which it may, on the contrary, serve to form by that process of building up constantly going on in the vegetable cell.

Pectic, parapectic, and metapectic acids have recently been discovered by Divers and Abel among the products of the spontaneous decomposition of gun-cotton; and the latter maintains that "these substances have been so frequently obtained, that they must be regarded as general products of the gradual decomposition of gun-cotton." If from this we are to infer that the acids named are derived from nitro-cellulose itself, I think it will be granted that there is little probability in the supposition, and that it is safer to assume that these acids preexisted in the cotton-fibre, in the form of pectine or pectic acid, before the action of nitro-sulphuric acid on it.

According to Fremy, the pectic acid of plants is always accompanied by a small quantity of an albuminous substance. It seemed to me not improbable that this might also be the case with the pectic acid of cotton; but as the preparation in a state of purity of a body of this class when occurring along with the other constituents of cotton-fibre would without doubt have been a very difficult task, I determined to ascertain whether, by the action of caustic alkalies on the impure pectic acid of cotton, I could procure any of the products of decomposition of albumen, some of which are bodies of very characteristic properties. For this purpose I took a quantity of the brown precipitate thrown down by acid from an alkaline extract of cotton; and after having exhausted it as far as possible with boiling alcohol, I dried it, added a quantity of hydrate of soda, equal in weight to that of the dry residue, together with a little water, and then heated the mixture over the fire in an iron ladle, with the precautions which are usually observed in the preparation of leucine and tyrosine from animal matters. The decomposition was accompanied by a copious evolution of gas, and a strong smell of ammonia, the latter being probably derived for the most part from the colouring-matter present, which had not been completely removed by the alcohol. The disengagement of gas having ceased, the mass was treated with boiling water, in which it dissolved almost entirely, yielding a dark-brown solution. To this there was added an excess of acetic acid, which gave a brown flocculent precipitate. This having been filtered off, the liquid was evaporated. During evaporation it deposited a considerable quantity of a brownish-white crystalline powder, consisting of oxalate of soda. This was filtered off, purified by recrystallization from boiling water, and converted into oxalate of lead, from which there was obtained, in the usual manner, a quantity of pure crystallized oxalic acid, weighing 18·3 grms. The mother-liquor of the oxalate of soda was evaporated. It left a dark-brown syrup, which was dissolved in boiling alcohol, and mixed with concentrated sulphuric acid as long as any sulphate of soda was precipitated. The latter having been filtered off, acetate of lead was added, and the liquid, after being filtered from the precipitated sulphate of lead, was deprived of its excess of lead by means of sulphuretted hydrogen, filtered again, and evaporated. The brown syrup which remained was mixed with a large quantity of alcohol and left to stand for several weeks. During this period the liquid gradually deposited a quantity of white crystalline needles, which, after being filtered off, washed, and dried, weighed 0·6 grm. These crystals possessed the properties of tyrosine, and were quite free from leucine.

Now, as tyrosine is only formed, as far as we know, from albumen and bodies of the same class, it is almost certain that cotton contains a small quantity of an albuminous substance. The oxalic acid obtained in this experiment was doubtless derived from the pectic acid. Of the ratio in which the latter stands to the albuminous matter, some estimate may be formed by that subsisting between their respective products of decomposition.

The precipitate produced by acid in an alkaline extract of cotton contains, then, the following organic substances:—

1. Cotton wax.
2. Margaric acid.
3. A colouring-matter easily soluble in alcohol.
4. A colouring-matter sparingly soluble in alcohol.
5. Pectic acid.
6. Albuminous matter.

Of these various bodies the pectic acid far exceeds the others in quantity. Then follow the colouring-matters. The three other constituents are present in extremely minute quantities only. Now I am far from asserting that cotton-fibre does not contain, besides these, other organic substances which are soluble in water or alkali, but are not afterwards precipitated by acid. Indeed it is quite possible that such may be the case; for it is well known that cotton, during the process of bleaching, loses about five per cent. of its weight, whereas the total weight of all the substances obtained in my experiments amounts to hardly one-half per cent. It is, however, not improbable that a portion of the matter which escapes observation when my mode of proceeding is adopted, may consist in great part of parapectic acid. This body, though insoluble in tolerably strong acids or saline solutions, is soluble in pure water. When, therefore, it is precipitated from an alkaline solution, the precipitate, on being filtered off and washed, begins gradually to dissolve as soon as the greatest part of the precipitant has been removed. I have observed this taking place on washing the precipitate thrown down by acid from the alkaline extract of cotton. When the acid has to some extent been removed, the wash-water begins to become thick and slimy, and runs through very slowly, in consequence of its dissolving a portion of the parapectic acid of the precipitate. A part also of the pectic acid, originally present, may undergo a further change by the action of the alkali, and be converted into the metapectic acid of Frémy, which is very soluble in water, and is not precipitated by stronger acids. The loss of weight sustained by cotton during its treatment with alkali, and not accounted for by my experiments, may therefore be due to such derivatives of pectine as are not precipitated by acid, or are subsequently removed by washing the precipitate with water.

I have a few remarks to make in conclusion in regard to the part which the bodies naturally accompanying the cellulose of cotton may be supposed to play during the process of manufacturing gun-cotton, and their influence on the quality of the product. In his elaborate memoir on gun-cotton, Mr. Abel attaches some importance to the resinous and other organic impurities of the fibre, and is inclined to attribute the instability occasionally observed in the product to their forming by the action of the acid bodies which are decomposed spontaneously at the ordinary, or a slightly elevated, temperature. It seemed to be, therefore, a matter of some interest to ascertain the nature of the products formed by the action of the mixture of the nitric and sulphuric acids, of the strength employed in the preparation of gun-cotton, on the various bodies from cotton which I have described. For this purpose I took, in the first place, a quantity of colouring-matter A, and allowed a mixture of 1 part nitric acid, of sp. gr. 1·52, and 3 parts concentrated sulphuric acid to act on it in the cold. The substance first caked together, and then gradually dissolved, forming a clear yellow solution, which, when mixed with water, gave an abundant yellow flocculent precipitate. This precipitate, after being filtered off and completely washed, was found to consist of a substance which could hardly be distinguished from the original colouring-matter. After being dried, it appeared brown and resin-like; and on being heated, it burned away easily, but without the least explosion or deflagration. It was insoluble in boiling water, in which it merely melted to a resinous cake. It dissolved easily in alcohol and alkalies, giving yellow solutions, but was insoluble in ether. The alcoholic solution left, on evaporation, a brown, brittle, resin-like residue, just like the original substance. Colouring-matter B, when treated in the same way, behaved similarly, and yielded a product which could not be distinguished from the colouring-matter itself, and was also decomposed without any explosive action on being heated. It should be mentioned, however, that in each case the product, when heated in a sealed tube at 100° C. for several hours, seemed to undergo a slight decomposition, resulting in the formation of a small quantity of acid, the nature of which could not be determined; but in other respects no marked change of properties could be discovered. Parapectic acid, when treated with the acid mixture, behaved very differently. It swelled up at first, and sank down in thick flocks, which, after prolonged contact with the acid, remained undissolved, and apparently unchanged; but on adding a large quantity of water they dissolved completely, showing that a conversion into metapectic acid had probably been effected. It is not probable, therefore, that these impurities of the fibre, even when their amount is considerable, exert any influence on the quality of the gun-cotton made from it. The colouring-matters are dissolved by the acid, and thus removed; while the products derived from pectine or pectic acid are extracted by the copious washings with water to which gun-cotton is always subjected after the action of the acid is completed. The quantity of wax and fatty acid contained in the fibre is too minute to produce any appreciable effect after its conversion into gun-cotton.

V. *On Solar Radiation.* Part I.

By Joseph Baxendell, F.R.A.S.

[Read before the Physical and Mathematical Section, October 10th, 1867.]

Although observations of solar radiation have now been regularly made for several years at various public observatories, and by many amateur meteorologists, I am not aware that any useful or important result has yet been deduced from them. It seems to be generally supposed that the disturbing influences which affect the indications of the black-bulb thermometer are so uncertain and irregular in their action as to render it almost hopeless to expect that any new and valuable result can be obtained from them. On comparing sets of observations made by different observers, the most startling and discouraging discrepancies are often found to exist, for which, in the absence of any information as to the exact circumstances under which the observations were made, it is impossible to account satisfactorily. A few years ago an inquiry in which I was engaged led me to undertake a discussion of the Greenwich solar-radiation observations; but the results proved so perplexingly anomalous and unsatisfactory, that I could not venture to place any reliance upon them. Having, however, recently become possessed, through the kindness of the Rev. Robert Main, F.R.S., and the Trustees of the Radcliffe Observatory, Oxford, of copies of the volumes of Radcliffe Observations for the years 1858 to 1864, and finding that they contained a valuable series of solar-radiation observations, I have been led to resume the subject; and although the inquiry is still incomplete, I have thought that some of the results already obtained are sufficiently curious and remarkable to render it desirable to bring them before the Society, in order that attention may be drawn to a much neglected but highly interesting branch of inquiry, and to the necessity of devising and adopting a more reliable and systematic method of determining the intensity of solar radiation than the one at present in use. It is much to be regretted that no regular and long-continued series of observations has ever been made with Sir John Herschel's actinometer, since there can hardly be any doubt that such a series would yield much more accurate and reliable results than can be obtained by the use of either the ordinary or the *vacuo* black-bulb thermometer.

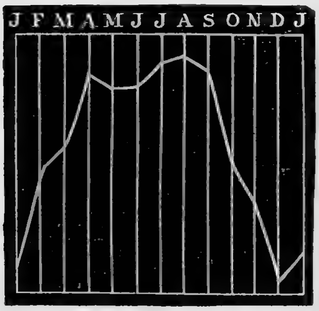
The Oxford solar-radiation observations commenced on the 2nd of January 1856, and were made with a thermometer supplied by Negretti and Zambra. On the 3rd of November 1858, this thermometer was unfortunately broken; but a new thermometer being obtained, the observations were resumed on the 1st of December 1858, and continued uninterruptedly till the 11th of September 1864, when the second thermometer was also broken: a third thermometer was obtained and brought into use on the 17th of September, and the observations continued with it to the end of the year. The entire series of observations during the nine years 1856–1864 must therefore be regarded as consisting of two distinct series, made with different thermometers, and therefore not strictly comparable with each other. I have, in consequence, thought it desirable to confine the discussion, in the first instance, to the observations made with the second thermometer during the years 1859–1864, more especially as in 1857 the readings of the first thermometer were only to the nearest whole degree, and in 1858 the month of November was omitted; and in order to make six complete years, I have included the observations made with the third thermometer from the 17th September to the end of 1864.

The following Table shows the mean monthly and annual differences between the maximum temperature in the shade and that in the sun during the six years 1859–1864, and the monthly and annual means of the entire series :—

	1859.	1860.	1861.	1862.	1863.	1864.	Monthly means.
	°	°	°	°	°	°	°
January............	7·3	5·7	5·7	4·2	4·9	5·3	5·51
February	10·9	11·5	9·0	6·8	9·2	7·6	9·16
March	11·7	11·2	10·4	7·6	10·2	9·4	10·08
April...............	13·2	16·1	15·4	11·6	11·8	11·3	13·23
May	15·0	13·5	13·5	11·6	11·5	12·3	12·90
June	16·0	12·3	14·4	10·1	13·2	11·6	12·93
July	17·8	12·8	12·4	13·4	14·2	12·3	13·81
August	16·3	12·5	15·1	13·3	12·5	14·5	14·03
September.........	16·3	11·8	14·1	12·5	12·8	14·8	13·71
October............	11·1	8·7	11·0	8·8	8·4	10·0	9·66
November.........	12·4	8·4	7·9	8·2	5·4	6·5	8·00
December	6·2	4·9	6·0	4·4	3·3	2·2	4·50
Annual means.	12·85	10·78	11·24	9·37	9·78	9·81	

A projection of the mean monthly values gives the curve in diagram A, from which it will be seen that the maximum amount of radiation occurs in August, or about a month later than the time of maximum temperature; whilst the minimum occurs in December, about a month earlier than the time of minimum temperature. There is also a slight secondary maximum in April. This curve, therefore, differs from that of any other ele-

A



ment of temperature; but observing that the time of minimum corresponded exactly with that of maximum in the curve laid down from the numbers given in a table which I communicated to the Physical Section on the 5th of March 1863, 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, and that the time of maximum agreed nearly with that of minimum disturbance of mean daily temperature, it occurred to me that the two phenomena might be closely connected with and dependent upon each other, and that the annual values of the solar radiation might bear a constant ratio to the corresponding values of the oscillations of mean daily temperature. The annual sums and mean daily values of the oscillations of mean daily temperature at Oxford for the six years, and the corresponding ratios, were therefore calculated, and found to be as follows:—

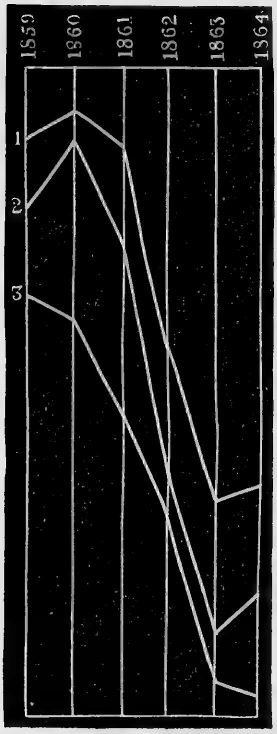
	Annual sums.	Daily means.	Ratios.
	°	°	
1859	1182·3	3·24	3·96
1860	953·0	2·60	4·14
1861	1054·9	2·89	3·88
1862	1043·4	2·85	3·28
1863	1198·4	3·28	2·98
1864	1158·0	3·17	3·09

The numbers in the last column showed that the ratio was not even approximately constant, but that, on the contrary, it was subject to considerable change. I therefore concluded that the calorific intensity of the sun's light was also subject to variations; and a glance at the course of the numbers at once suggested that this variation would be found to follow that of solar-spot frequency. Referring to Schwabe's observations of the solar spots, we have the following numbers of groups observed by him:—

1859	205	1861	204	1863	124
1860	211	1862	160	1864	130

A projection of these numbers, and of the corresponding ratios of solar radiation, to the oscillation of mean daily temperature is shown in diagram B: No. 1 is the curve of sun-spot frequency, and No. 2 that of the ratios; and it will be seen that the similarity in form of the two curves is remarkably striking, and apparently conclusive, as to the connexion between the two classes of phenomena.

B



Assuming that changes in the heating power of the sun's rays follow the course of the changes in solar-spot frequency, it seemed probable that the ratio of the difference between the maximum temperature in the shade and in the sun, to the difference between the mean daily temperature (or better, perhaps, the temperature of evaporation) and the maximum temperature in the shade, would also exhibit corresponding changes. It will be seen that the following results strongly support this view:—

	Max. temp. in sun, less max. temp. in shade.	Max. temp. in shade, less mean temp. of evaporation.	Ratio.
	°	°	
1859	12·85	10·06	1·27
1860	10·78	8·63	1·25
1861	11·24	9·52	1·18
1862	9·37	9·28	1·09
1863	9·78	10·38	0·94
1864	9·81	10·57	0·93

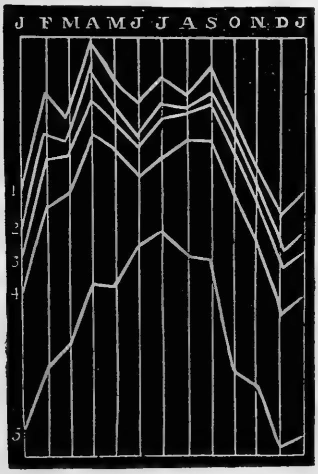
The line No. 3 in diagram B is a projection of these ratios. It would probably be better to employ the maximum instead of the mean temperature of evaporation, but this is not given in the printed observations.

The mean values of solar radiation given above in the first Table are deduced from observations made on every day in the year, and therefore in every possible state of the atmosphere—clear, cloudy, rainy, foggy, calm, stormy, &c.; but it was evidently desirable to determine the calorific intensity of the sun's rays on days when the sky was cloudless at the time of maximum temperature. The printed observations, however, do not always show when this was the case, and it became necessary to adopt some arbitrary principle of selection. As the one which appeared to me to be least open to objection, I selected, in the first instance, the highest value in each month of the six years, and taking the means obtained the following numbers, a projection of which is shown in curve No. 1 of diagram C:—

	°		°		°
January	16·4	May	20·9	September	21·8
February	20·5	June	20·1	October	19·6
March	19·3	July	21·1	November	17·2
April	22·6	August	20·3	December	15·3

This curve exhibits two principal maxima in April and September, and a low minimum in December.

C



Taking next the means derived from the three highest values in each month, and then those of the five highest, we have curves No. 2 and No. 3. The slight irregularities in the first curve have now disappeared, and curve 3 has two well-defined maxima in April and September, a principal minimum in December, and secondary minimum in June.

Proceeding, now, a step further, and taking the ten highest values in each month, we have curve No. 4. The first maximum still occurs in April, but a slight change has taken place in the time of the second, which now occurs in the latter part of August instead of the middle of September; but no change has taken place in the times of the two minima.

Finally, taking the ten lowest values in each month, we have curve No. 5. Here the first maximum has almost disappeared, though still occurring in April, and the second maximum has advanced into July. The principal minimum is still in December, but the secondary minimum occurs in May instead of June, though, like the first maximum, it has almost disappeared.

It seems, therefore, from a comparison of these five curves, that the general curve in diagram A, which is laid down from the monthly means of all the observations, may be regarded as compounded of two primary curves,—one having two well-marked maxima in April and September and two minima in December and June, and the other having only one maximum in July and one minimum in December. From curves 3 and 4 it appears that the influence of the second primary curve upon the general features of the first does not become apparent while the number of selected days is limited to five per month. The first primary curve therefore represents the monthly changes in the calorific intensity of the sun's direct rays on cloudless or nearly cloudless days, and it leads us to this remarkable conclusion, that the heating power of direct sunlight on clear days in the latitude of the British Islands is greater in the months of April and September than in the month of June, when the sun attains his greatest meridian altitude.

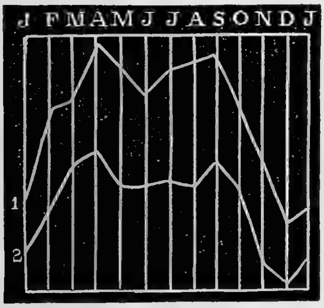
The second primary curve presents the intensities when the solar rays are more or less intercepted and dispersed by clouds and haze, and it approaches in form the annual curve of temperature; but the first curve, of which curve

3, diagram C, may be taken to be a fair representation, is unlike that of any other thermometric element. It has, however, a remarkably close resemblance to the curve representing the monthly changes of one of the magnetic elements, namely, that of the monthly means of the diurnal ranges of the magnetic needle. In the volume of the Greenwich Observations for 1859, the Astronomer Royal has given a table showing the monthly means of the diurnal ranges of the magnetometer at Greenwich from ten years' observations, 1848–1857. The numbers in this table are as follow:—

	′		′		′
January	9·5	May	12·7	September	13·5
February	11·3	June	12·6	October	12·2
March............	13·1	July	12·7	November	9·3
April	14·0	August	12·6	December	8·2

The line No. 2, in diagram D, is a projection of these numbers, and the line No. 1 is a repetition of curve 3, diagram C. It will be seen that the maxima and minima of the one correspond exactly with those of the other; and we are therefore entitled to conclude that the two phenomena are intimately connected, and that the causes which produce variations in the intensity of solar radiation also affect the diurnal oscillations of the magnetic needle.

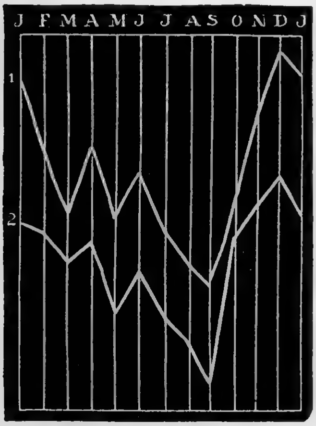
D



It was supposed by Sir William Herschel that the emission of heat from the sun varied according to the greater or less frequency of solar spots; and he attempted to support this view by a comparison of the prices of grain in years when the solar spots were numerous with those in years when few or no spots were seen; but this kind of evidence was generally regarded as unsatisfactory, and it is, I believe, now generally admitted by meteorologists that the many valuable series of thermometrical observations which have since been made in various localities have hitherto failed to afford any decided indication of a periodical change in the element of mean annual temperature. The question therefore naturally arises, if the intensity of solar radiation varies in a period corresponding with the period of solar-spot frequency, why does not the element of mean annual temperature exhibit a similar periodical change? For the present, I will merely suggest that a clue to the correct explanation of this apparent anomaly may perhaps be found in the conclusion at which Professor Forbes arrived from a discussion of his observations made in Switzerland, in conjunction with Professor Kaemtz, on the intensity of solar radiation at different elevations in the atmosphere, namely, that the heating rays of the sun consist of two kinds,—one kind of high intensity, which suffers little or no loss in passing through the atmosphere; and the other of much lower intensity, and therefore much more absorbable. Now, if we suppose that the relative quantities of the two kinds of rays undergo periodical changes, such that the maximum of the one corresponds to the minimum of the other, it will be evident that when the lower strata of the atmosphere receive less heat by absorption, owing to a diminished supply of the rays of low intensity, the ground will receive more from the simultaneous increase in the quantity of the rays of high intensity, and this being communicated to the lower atmosphere by conduction and convection, as well as by radiation upwards, will restore the equilibrium, and tend to produce uniformity in the mean annual temperatures.

On a former occasion I urged the desirability of giving more attention than has hitherto been done to the oscillations of mean daily temperature, and have shown, in the present discussion, their importance in connexion with the subject of solar radiation. I may now add that they appear to have an intimate connexion with magnetic phenomena. Among the tables given by the Astronomer Royal in the volume of Greenwich Observations for 1859, is one showing the monthly mean horizontal magnetic force at Greenwich for the nine years 1849–1857, corrected for secular variation. The line No. 2 in diagram E is a projection of the numbers in this table, and the line No. 1 in the same diagram shows the monthly mean daily oscillation of temperature. It will be seen that every rise or fall in one curve has a corresponding rise or fall in the other. It is, in fact, rare to meet with so close an agreement between curves representing phenomena so widely different as the earth's horizontal magnetic intensity and the mean daily temperature of the atmosphere.

E



The conclusions arrived at from this discussion may be briefly recapitulated as follows :—

1st. That the calorific intensity of the sun's light is subject to periodical changes, the maxima and minima of which correspond respectively with those of solar-spot frequency.

2nd. That the intensity of a ray of direct sunlight on its arrival at the earth's surface, in the latitude of Oxford, is greater in April and September than in June, when the sun's meridian altitude is greatest.

3rd. That the curve representing the mean monthly values of solar radiation on cloudless days has its times of maxima and minima corresponding with those of the curve representing the mean monthly diurnal ranges of the magnetometer.

4th. It seems probable that the heating rays of the sun consist of two kinds, differing considerably in intensity, and being subject to periodical changes, the times of maximum of one kind, and those of minimum of the other, corresponding respectively to the times of maximum frequency of solar spots.

5th. That the oscillations of mean daily temperature are intimately connected with the changes which take place in the earth's horizontal magnetic intensity.

I have said that the results derived from the Greenwich observations of solar radiation were anomalous and unsatisfactory. It may, therefore, be necessary to state that these observations appear to have been made under circumstances not favourable to the accurate determination of the intensity of solar radiation at all seasons of the year. The values derived from them appear to be too high in summer, and too low in winter. Take, for instance, any winter month, say December 1857. The mean difference for the month between the maximum in sun and the maximum in shade was only 1°·7; at Oxford it was 6°·1. The highest value during the month was 4°·5 at Greenwich; at Oxford it was 19°. At Greenwich there were only seven days on which the difference exceeded 3°; at Oxford there were seventeen: and yet at Greenwich the month was unusually fine and dry, only 0·36 of an inch of rain fell, and several days appear to have been nearly, if not quite, cloudless. Under these circumstances it is difficult to understand why the black-bulb thermometer, if properly exposed, did not register much greater differences. In 1859 there was a remarkable and unaccountable falling off in the summer values, and the mean for the year was decidedly lower than the mean of any of the three preceding years; but at Oxford it was higher. I can only account for this remarkable difference by supposing that some change was made in the position of the solar-radiation thermometer at Greenwich in the early part of the year.

From the beginning of 1860 to the end of 1864 the observations at Greenwich were made with a black-bulb thermometer *in vacuo,* and it is satisfactory to find that the course of the annual means is in tolerably fair agreement with that of the results obtained at Oxford with the ordinary black-bulb thermometer.

VI. *Solar-radiation Observations, made at Old Trafford, Manchester.* By G. V. Vernon, F.R.A.S., F.M.S.

Read November 26th, 1867.

Mr. Joseph Baxendell, F.R.A.S., having, at a recent meeting of the Physical Section, read a paper on this subject, showing some peculiarities of the distribution of solar radiation throughout the year, I have at his suggestion reduced and tabulated my observations bearing upon this point down to the end of 1866, making eleven years in all. Unfortunately the month of August is deficient in six years out of the eleven. The observations were made with Negretti and Zambra's patent maximum thermometers, neither thermometer having any index error, but reading correctly with the standard throughout the scale.

Taking the differences between the mean maximum black-bulb reading in the sun, and the mean maximum reading in the shade for each month, we find a minimum in December and a maximum in July. The maximum occurs in the warmest month, but the minimum appears to occur some time before the coldest month arrives, viz. January. If we take the differences between one month and the succeeding one, we find the following values:—

January to February	+2°·02
February to March	+4·64
March to April	+4·06
April to May	+1·67
May to June	−0·09
June to July	+0·56
July to August	−2·39
August to September	−2·07
September to October	−3·46
October to November	−3·21
November to December	−1·32
December to January	+0·59

From my observations the greatest increase in the amount of solar radiation appears to take place in March, and nearly as much in April; afterwards, up to July, the warmest month, the increase is remarkably small, and in June shows a slight amount of decrease. These figures do not quite agree with those deduced from the Oxford observations by Mr. Baxendell, but they very clearly confirm the fact that a large amount of solar heat is dispersed in some way, and without a corresponding effect upon the thermometer, or the difference between the maximum in the sun and the maximum in the shade would show a large increase in the months of maximum temperature.

The greatest effect of solar radiation, therefore, appears to occur in the spring, and this is in accordance with the very rapid growth of vegetation we often see suddenly take place in early spring.

Old Trafford, Manchester.—Solar Radiation.

Year.	January.			February.			March.		
	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.
	°	°	°	°	°	°	°	°	°
1856	45·4	43·9	1·5	49·1	46·9	2·2	54·6	48·2	6·4
1857	43·1	41·7	1·4	49·9	46·4	3·5	54·4	47·7	6·7
1858	49·8	44·8	5·0	47·9	44·1	3·8	56·3	50·7	5·6
1859	51·1	46·0	5·1	53·6	47·4	6·2	57·6	50·8	6·8
1860	46·4	42·9	3·5	47·1	41·9	5·2	54·3	46·4	7·9
1861	41·3	40·4	0·9	52·2	46·8	5·4	60·4	49·6	10·8
1862	44·6	43·2	1·4	49·7	46·3	3·4	56·8	47·9	8·9
1863	...	...	...	55·2	49·6	5·6	62·8	51·6	11·2
1864	43·4	41·7	1·7	45·5	41·6	3·9	59·8	47·9	11·9
1865	43·6	41·3	2·3	46·8	42·3	4·5	57·0	44·1	12·9
1866	49·0	47·3	1·7	52·9	46·4	6·5	58·7	47·6	11·1
Means	45·77	43·32	2·45	49·90	45·43	4·47	57·52	48·41	9·11

	April.			May.			June.		
1856	66·3	55·9	10·4	70·6	59·7	10·9	76·4	64·3	10·1
1857	65·6	53·8	11·8	75·2	62·2	13·0	82·6	73·6	8·0
1858	66·5	57·9	8·6	72·6	59·5	13·1	82·7	73·8	8·9
1859	61·9	53·9	8·0	79·4	65·7	13·7	83·5	68·8	14·7
1860	63·0	54·4	8·6	81·7	65·4	16·3	81·3	63·9	17·4
1861	72·6	55·5	17·1	73·2	59·4	13·8	86·2	68·6	17·6
1862	70·8	56·0	14·8	81·2	62·7	18·5	...	...	...
1863	69·9	56·2	13·7	74·8	59·5	15·3	81·4	65·0	16·4
1864	76·0	59·0	17·0	81·4	66·5	14·9	81·2	66·2	15·0
1865	81·5	63·5	18·0	79·3	65·0	14·3	93·0	72·8	20·2
1866	74·1	57·3	16·8	82·1	62·2	19·9	88·0	71·8	16·2
Means	69·84	56·67	13·17	77·37	62·53	14·84	83·63	68·88	14·75

	July.			August.			September.		
1856	80·7	68·0	12·7	...	...	...	74·3	63·9	10·4
1857	83·6	69·6	14·0	82·5	72·0	10·5	75·8	67·4	8·4
1858	79·6	66·8	12·8	...	...	...	79·7	66·3	13·4
1859	86·7	74·2	12·5	...	...	...	76·2	63·7	12·5
1860	83·8	66·8	17·0	...	...	...	71·8	59·5	12·3
1861	85·8	67·1	18·7	...	...	...	78·5	63·9	14·6
1862	80·3	64·8	15·5	...	...	...	74·2	62·6	11·6
1863	87·3	68·9	18·4	83·4	66·0	17·4	70·1	58·5	11·6
1864	86·0	70·9	15·1	80·9	68·0	12·9	73·3	65·2	8·1
1865	94·4	76·6	17·8	81·7	68·7	13·0	82·3	73·4	8·9
1866	84·2	70·3	13·9	77·8	67·0	10·8	70·0	62·4	7·6
Means	84·76	69·45	15·31	81·26	68·34	12·92	75·11	64·26	10·85

Solar Radiation (continued).

Year	October.			November.			December.		
	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.
	°	°	°	°	°	°	°	°	°
1856	66·3	59·4	6·9	47·6	45·7	1·9	45·4	44·8	0·6
1857	66·9	59·8	7·1	56·5	50·1	6·4	57·5	50·8	6·7
1858	63·8	55·0	8·8	51·8	46·6	5·2	50·2	45·3	4·9
1859	66·1	56·5	9·6	51·5	46·9	4·6	42·6	39·8	2·8
1860	64·1	55·4	8·7	49·0	45·3	3·7	40·8	39·5	1·3
1861	64·8	60·6	4·2	47·2	45·9	1·3	44·0	44·8	—0·8
1862	61·9	56·5	5·4	44·9	43·7	1·2	48·2	48·0	0·2
1863	62·0	55·9	6·1	52·3	50·7	1·6	48·6	48·3	0·3
1864	61·7	57·0	4·7	49·1	48·2	0·9	41·9	43·2	1·3
1865	62·4	58·6	3·8	51·7	50·5	1·2	47·8	47·4	0·4
1866	62·5	58·1	4·4	56·4	49·5	6·9	52·8	47·5	5·3
Means	63·86	57·47	6·39	50·73	47·55	3·18	47·26	45·40	1·86

Monthly values.

	Max. in sun.	Max. in shade.	Diff.
	°	°	°
January	45·77	43·32	2·45
February	49·90	45·43	4·47
March	57·52	48·41	9·11
April	69·84	56·67	13·17
May	77·37	62·53	14·84
June	83·63	68·88	14·75
July	84·76	69·45	15·31
August	81·26	68·34	12·92
September	75·11	64·26	10·85
October	63·86	57·47	6·39
November	50·73	47·55	3·18
December	47·26	45·40	1·86

VII. *A Comparison of Solar Radiation on the Grass and at Six Feet from the Ground.* By THOMAS MACKERETH, F.R.A.S., F.M.S.

Read November 26th, 1867.

As the Corporation of the Borough of Salford has recently erected a structure in front of the Town Hall for meteorological purposes, and as no better position could be found for the solar-radiation thermometer than on the apex of the roof of the shade-stand, which is six feet from the ground, I fixed a similar thermometer in a similar position at Eccles, in order that I might institute a comparison of solar radiation between Eccles and the Borough. Though I anticipated very different results from a thermometer thus placed than I had found from a similar thermometer placed upon the grass, I was not prepared to find them so widely different as I shall here present them. The instruments used are the ordinary exposed black-bulb self-registering thermometers duly compared. I have also added a column of comparison from results obtained by a self-registering black-bulb thermometer placed *in vacuo* on the grass. The observations are for the month of October only, the only complete month in which all the instruments have been used.

It appears therefore that for the month, at a position six feet above the ground, there is a mean difference of solar influence of nearly 4 degrees above that upon the grass; and in some extreme instances the difference has been from 10 to nearly 12 degrees. A thermometer placed *in vacuo* on the grass has only exceeded this difference by 1·7 degree. These results show how important it is that some definite principle should be adopted in the placing of solar thermometers, as certainly no comparison can be made between the amount of solar radiation at any two or more places, unless some common plan of placing the instruments be adopted.

Date.	Exposed bk. bulb therm. on grass.	Exposed bk. bulb therm. 6 feet from ground.	Difference.	Black bulb therm. *in vacuo* on grass.	Difference from therm. 6 feet from ground.
1867.	°	°	°	°	°
Oct. 1	69·2	73·0	+ 3·8	83·2	+10·2
„ 2	57·5	57·6	+ 0·1	62·1	+ 4·5
„ 3	61·4	62·7	+ 1·3	71·0	+ 8·3
„ 4	52·5	57·0	+ 4·5	60·0	+ 3·0
„ 5	66·5	68·2	+ 1·7	72·2	+ 4·0
„ 6	64·0	68·0	+ 4·0	67·7	− 0·3
„ 7	60·7	67·2	+ 6·5	77·4	+10·2
„ 8	54·9	60·1	+ 5·2	68·0	+ 7·9
„ 9	46·7	45·8	− 0·9	48·9	+ 3·1
„ 10	62·0	67·6	+ 5·6	75·8	+ 8·2
„ 11	51·3	52·5	+ 1·2	54·0	+ 1·5
„ 12	56·2	56·5	+ 0·3	56·5	0·0
„ 13	53·4	52·7	− 0·7	57·0	+ 4·3
„ 14	60·0	61·5	+ 1·5	61·2	− 0·3
„ 15	61·0	63·6	+ 2·6	65·0	+ 1·4
„ 16	64·8	74·3	+ 9·5	71·0	− 3·3
„ 17	63·0	74·8	+11·8	71·1	− 3·7
„ 18	62·5	70·7	+ 8·2	67·5	− 3·2
„ 19	57·3	68·2	+10·9	62·0	− 6·2
„ 20	60·5	68·9	+ 8·4	64·4	− 4·5
„ 21	60·0	63·8	+ 3·8	63·1	− 0·7
„ 22	72·8	82·1	+ 9·3	79·8	− 2·3
„ 23	64·0	64·8	+ 0·8	65·6	+ 0·8
„ 24	54·2	55·4	+ 1·2	56·0	+ 0·6
„ 25	51·8	52·5	+ 0·7	53·0	+ 0·5
„ 26	62·0	69·3	+ 7·3	65·3	− 4·0
„ 27	50·3	51·4	+ 1·1	53·7	+ 2·3
„ 28	52·6	61·9	+ 9·3	58·7	− 3·2
„ 29	59·4	65·4	+ 6·0	62·0	− 3·4
„ 30	61·0	68·8	+ 7·8	67·0	− 1·8
„ 31	58·3	64·8	+ 6·5	64·2	− 0·6
Mean ...	59·0	62·9	+ 3·9	64·6	+ 1·7

VIII. *Solar-radiation Observations, made at Eccles, near Manchester.* By Thomas Mackereth, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, December 31st, 1867.

Feeling considerable interest in the paper read by Mr. Baxendell, F.R.A.S., at a recent meeting of the Physical Section, and at a General Meeting of the Society, on this subject, I have reduced five years' observations which I had made at Eccles with an ordinary black-bulb thermometer placed on the south side of the shade-stand, four feet above the ground. The thermometer had been duly compared at the Kew Observatory. I was chiefly induced to make these reductions because the discussions of Mr. Baxendell on the subject were from observations made at Oxford by means of a thermometer placed somewhat similarly to mine, at least as mine was placed during the five years I have reduced.

Taking the differences between the mean maximum black-bulb reading in the sun, and the mean maximum reading in the shade, for each month, we find two maxima (one in May, and another in August) and two minima (one in June, and another in December).

In order to project the gradual rise and fall of solar radiation for each month, I have adopted the form of table presented by Mr. Vernon in his reductions of similar observations. Thus, if we take the differences between one month and the succeeding one, we find the following values :—

	°
January to February	+2·80
February to March	+2·98
March to April	+3·16
April to May	+1·14
May to June	−0·80
June to July	+0·38
July to August	+0·56
August to September	−1·48
September to October	−5·28
October to November	−2·26
November to December	−3·06
December to January	+1·86

The foregoing Table shows a very regular though rapid increase of the amount of solar radiation from January till April, which is only slightly exceeded in May. We have then a slight decrease, and afterwards another maximum in August. Then follows a very rapid decrease to October, which continues, though not so rapidly, to the end of the year. This Table shows that the observations at Eccles have a projection very similar to the one presented by Mr. Baxendell on page 38, vol. vii., of the 'Proceedings;' and I have no doubt that if the observations had extended over a longer period, the similarity would have been more striking.

Below I present my monthly means as they have been reduced.

Year.	January.			February.			March.		
	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.	Max. bk. bulb in sun.	Max. in shade.	Diff.
	°	°	°	°	°	°	°	°	°
1862.	48·0	42·5	5·5	52·7	45·4	7·3	55·1	46·7	8·4
1863.	49·8	44·3	5·5	55·5	47·4	8·1	62·4	49·9	12·5
1864.	44·9	40·0	4·9	50·5	40·4	10·1	57·8	45·6	12·2
1865.	44·6	40·3	4·3	44·8	40·0	4·8	51·8	42·1	9·7
1866.	49·9	46·7	3·2	52·1	45·0	7·1	54·4	44·9	9·5
Means	47·44	42·76	4·68	51·12	43·64	7·48	56·30	45·84	10·46

	April.			May.			June.		
1862	66·9	53·3	13·6	77·5	61·6	15·9	73·2	60·8	12·4
1863	68·6	53·5	15·1	72·8	57·3	15·5	80·5	63·9	16·6
1864	69·1	54·9	14·2	80·8	62·4	18·4	78·8	62·6	16·2
1865	73·8	60·3	13·5	71·9	60·5	11·4	79·8	67·4	12·4
1866	65·4	53·7	11·7	69·8	57·2	12·6	79·0	66·8	12·2
Means	68·76	55·14	13·62	74·56	59·80	14·76	78·26	64·30	13·96

	July.			August.			September.		
1862	78·0	63·4	14·6	79·1	64·5	14·6	75·0	60·7	14·3
1863	82·4	66·7	15·7	82·2	65·4	16·8	69·8	56·9	12·9
1864	84·1	67·8	16·3	79·5	64·4	15·1	76·3	61·8	14·5
1865	81·9	68·8	13·1	78·1	64·7	13·4	85·3	70·2	15·1
1866	78·0	66·0	12·0	78·4	63·8	14·6	69·8	59·5	10·3
Means	80·88	66·54	14·34	79·46	64·56	14·90	75·24	61·82	13·42

	October.			November.			December.		
1862	65·3	54·7	10·6	51·8	42·4	9·4	51·8	47·5	4·3
1863	62·4	53·5	8·9	55·4	49·1	6·3	51·5	47·7	3·8
1864	62·2	54·2	8·0	53·1	47·4	5·7	45·0	42·6	2·4
1865	65·9	56·3	9·6	55·5	49·3	6·2	50·4	47·2	3·2
1866	60·9	57·3	3·6	51·5	49·7	1·8	47·5	47·1	0·4
Means	63·34	55·20	8·14	53·46	47·58	5·88	49·24	46·42	2·82

Mean Monthly Values.

	Max. in sun.	Max. in shade.	Diff.
	°	°	°
January	47·44	42·76	4·68
February	51·12	43·64	7·48
March	56·30	45·84	10·46
April	68·76	55·14	13·62
May	74·56	59·80	14·76
June	78·26	64·30	13·96
July	80·88	66·54	14·34
August	79·46	64·56	14·90
September	75·24	61·82	13·42
October	63·34	55·20	8·14
November	53·46	47·58	5·88
December	49·24	46·42	2·82

IX. *On Solar Radiation.* Part. II.

By Joseph Baxendell, F.R.A.S.

Read before the Physical and Mathematical Section, Jan. 28th, 1868.

It has been shown in the first part of this paper that the curve laid down from the monthly means of solar radiation derived from the five highest values in each month, or those corresponding to the five clearest days, exhibits two maxima and two minima, occurring respectively in April and September, and June and December. In order, however, to determine the influence of the seasons upon the amount of solar radiation on clear days, it will be necessary to correct these means for the difference of meridian altitude of the sun in the different months of the year. For this purpose I have employed the Table given in the article on Climate in the 'Encyclopædia Britannica,' referred to by Sir John Herschel in a note at foot of page 11 of his admirable 'Treatise on Meteorology;' and I have neglected, as being immaterial for our present purpose, any correction which might be necessary in a more exact inquiry in consequence of equal increments in the difference between sun and shade temperatures not corresponding exactly to equal increments of calorific intensity of the sun's rays. This Table gives for every fifth degree of altitude the calorific intensity of sunlight, the intensity of the light on entering the earth's atmosphere being taken = 1. If the rate of emission of heat from the sun were constant, and change of season exercised no influence on the absorptive power of the atmosphere, the quotient obtained by dividing the mean value of solar radiation on clear days in any month, by the number in this Table corresponding to the sun's meridian altitude in the middle of the month, would be a constant quantity. The results given in the following Table will, however, show that, according to the Oxford observations, this is by no means the case:—

	Mean amount of solar radiation on clear days.	Meridian altitude of the sun on the 15th of the month.	Proportion of light transmitted.	Number in first column divided by number in third column.
	°	° ′		
January	13·5	17 6	·37	36·5
February ...	17·6	25 32	·51	34·5
March.........	17·8	36 3	·61	29·2
April	20·3	47 57	·68	29·8
May	19·2	57 4	·71	27·0
June	18·1	61 34	·72	25·1
July	19·2	59 50	·72	26·7
August	19·6	52 23	·69	28·4
September ...	20·0	41 22	·65	30·8
October	18·1	29 48	·56	32·3
November ...	15·6	19 48	·43	36·3
December ...	13·0	14 59	·33	39·4

The numbers in the last column show that the power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, the maximum effect taking place in the month of June, when the sun attains his greatest meridian altitude, and the minimum in December, when his meridian altitude is least. As the amount of aqueous vapour in the atmosphere is much greater in summer than in winter, this result tends strongly to confirm the view taken in my paper "On the Theory of Rain," and since held to be established by Professor Tyndall's experimental investigations, that air charged with aqueous vapour has a much greater power of absorbing and radiating heat than dry air. If, however, we use the Greenwich observations in this investigation, we shall arrive at a totally different result. Taking, for instance, those made in 1857, and treating them by the method employed in discussing the Oxford observations, we have the results shown in the following Table :—

	Mean amount of solar radiation on clear days.	Meridian altitude of the sun on the 15th of the month.	Proportion of light transmitted.	Number in first column divided by number in third column.
	°	° ′		
January	9·0	17 19	·38	23·6
February ...	16·6	25 42	·51	32·6
March	18·2	36 37	·62	29·3
April	23·8	48 29	·68	35·0
May	24·8	57 31	·71	34·9
June	26·9	61 52	·72	37·3
July	26·8	59 59	·72	37·2
August	24·8	52 25	·69	35·9
September ...	22·9	41 21	·05	35·2
October	20·2	29 47	·56	36·1
November ...	9·6	19 52	·43	22·4
December ...	3·9	15 12	·33	11·9

The numbers in the last column of this Table are considerably greater in the summer than in the winter months, thus indicating a greatly reduced absorptive action of the atmosphere in the sun's heating rays in the warmest half of the year, when the quantity of aqueous vapour in the air attains a maximum, a result directly opposed to that derived from the Oxford observations, and to the conclusions drawn by Professor Tyndall from his experiments. If, however, we examine the conditions under which the observations were made at Greenwich, we find that while the ordinary thermometers on the shade-stand were placed with their bulbs about 4 feet from the ground, the solar-radiation thermometer was placed in an open box about 13 inches high, with its bulb about 10 inches above the bottom of the box. Now a little consideration will show that on a clear calm day in summer the air in this box will be heated to a temperature several degrees above that of the air at 4 feet from the ground; while on the other hand in winter it will often be several degrees colder. The readings of a thermometer placed in it will therefore be too high in summer and too low in winter; and the magnitude of the errors may well be sufficient not only to mask the true action of the varying amount of the aqueous vapour in the atmosphere, but to lead to conclusions directly at variance with the truth.

The heating effect of the sun's rays at the surface of the earth during a given interval obviously depends upon the greater or less prevalence of cloud or haze, and it is also evident that the amount of the latter will in general depend upon the degree of humidity of the air as determined from observations taken with the dry- and wet-bulb thermometers. Any alterations, therefore, which may take place in the calorific intensity of the sun's light ought to be indicated by a comparison of the differences between the mean temperatures of the air and of evaporation, and those between the maximum temperatures in the sun and in the shade. The values of these elements for the years 1859–64 at Oxford, and their ratios, are as follows :—

	Maximum in sun less maximum in shade.	Temperature of air less temperature of evaporation.	Ratio.
1859	12·85	2·67	4·81
1860	10·78	1·98	5·44
1861	11·24	2·49	4·51
1862	9·37	2·72	3·44
1863	9·78	2·87	3·40
1864	9·81	2·73	3·59

A projection of the ratios gives a curve closely resembling the curve No. 1, diagram B (p. 132), and therefore strongly supporting the view that the calorific intensity of the sun's rays varies in a period corresponding with the solar-spot period.

If instead of the difference between the mean temperatures of the air and of evaporation we employ the difference between the mean temperatures of the air and of the dew-point we obtain the following ratios :—

1859	2·33	1862	1·66
1860	2·57	1863	1·65
1861	2·17	1864	1·72

It will be seen that the curve representing these ratios is identical in form with that representing the last series of ratios, as indeed might have been expected.

Proceeding, now, to apply the same method to the treatment of the mean monthly values of solar radiation we have the following Table :—

	Mean monthly solar radiation.	Mean temperature of air less mean temperature of evaporation.	Ratio.
	°	°	
January	5·51	1·31	4·20
February	9·16	1·75	5·23
March	10·08	2·36	4·27
April	13·23	3·00	4·41
May	12·90	3·78	3·41
June	12·93	3·43	3·76
July	13·81	4·15	3·32
August	14·03	3·73	3·76
September	13·71	2·81	4·88
October	9·66	2·05	4·71
November	8·00	1·41	5·67
December	4·50	1·15	3·91

Before the true import of the numbers in the last column can be clearly understood, it will be necessary to apply corrections for the changes of meridian altitude of the sun in the different months of the year; and employing for this purpose the table in the article on climate in the 'Encyclopædia Britannica' already referred to, we obtain the following corrected numbers :—

January	1·59	July	2·39
February	2·66	August	2·59
March	2·60	September	3·17
April	3·00	October	2·63
May	2·42	November	2·44
June	2·70	December	1·29

The highest values occur in April and September, the months in which the diurnal oscillations of the declination magnetometer are greatest, and the general course of the numbers indicates that clouds and haze are less prevalent, or less dense, or their power of absorbing the heating rays of the sun less active in the spring and autumn than in the winter and summer months; and in connexion with this it may be remarked that the rate of change in the difference between the temperature of the air and the temperature of evaporation is greatest in the months of April and September.

Since the first part of this paper was printed in the Society's 'Proceedings,' Mr. Mackereth, F.R.A.S., has communicated to the Physical and Mathematical Section the monthly results of his Solar-radiation Observations made at Eccles during the five years 1862–66. As these observations were made with a black-bulb thermometer placed somewhat similarly to that used at Oxford, and as the series extends two years beyond the Oxford series, it was evidently very desirable to examine how far they confirmed the conclusions derived from the Oxford observations. The mean annual values of solar radiation having been deduced from the monthly means, and Mr. Mackereth having kindly supplied me with the annual mean temperatures of the air and of the dew-point, they were treated on the plan employed in discussing the Oxford observations.

These data and the results thus derived from them are shown in the following Table :—

	Mean temperature of air.	Mean temperature of dew-point.	Difference.	Mean amount of solar radiation.	Ratio.
	°	°	°		
1862	47·3	42·3	5·0	10·90	2·18
1863	48·3	42·3	6·0	11·47	1·91
1864	47·0	40·5	6·5	11·50	1·77
1865	48·6	43·3	5·3	9·72	1·83
1866	48·3	42·3	6·0	8·25	1·37

The mean ratio for the two years 1865–66 is 1·60, while that for the years 1862–64 amounts to 1·95. It appears, therefore, that the calorific intensity of the sun's rays continued to diminish for two years after the termination of the Oxford series; and as the observations of Schwabe, Wolf, Balfour Stewart, and others have shown that the frequency of solar spots also diminished during these two years, the probability that a close connexion exists between the two phenomena is considerably increased by the results of Mr. Mackereth's short but valuable series of observations.

On comparing the Oxford and the Eccles series of results it will be seen that the values for the years which are common to both (1862–64) are greater at Eccles than at Oxford in the ratio of 1·17 to 1·00, thus indicating that the blackened bulb of the thermometer at Eccles absorbs radiant heat more readily than that at Oxford. Dividing the Eccles mean annual values by 1·17 in order to reduce them to the Oxford scale, and calculating the ratios, we have,—

	Reduced values of solar radiation.	Ratios
1862	9·33	1·82
1863	9·82	1·63
1864	9·84	1·51
1865	8·32	1·57
1866	7·06	1·17

Combining, now, the two series of ratios, and taking the means of the two values given for each of the three years 1862-64, we have, finally, the following series of corrected ratios all referred to the same scale, and therefore strictly comparable with each other. I have added for comparison the number of groups of solar spots observed in each year by Schwabe:—

	Ratios.	Number of groups of solar spots.
1859	2·33	205
1860	2·57	211
1861	2·17	204
1862	1·74	160
1863	1·64	124
1864	1·61	130
1865	1·57	93
1866	1·17	45

The curves laid down from these two series of numbers present so close a resemblance in their general form, that it seems impossible to resist the conclusion that the causes which influence the frequency of solar spots also produce corresponding changes in the calorific intensity of the sun's rays.

In addition to the conclusions drawn from the discussion in the first part of this paper we have now the following:—

1. The power of the atmosphere to absorb the heating rays of the sun is much greater in the summer than in the winter months, and depends apparently upon the amount of aqueous vapour which it contains.

2. Clouds and haze are less prevalent during the day, or their power to intercept the heating rays of the sun is less active in the spring and autumn than in the winter and summer months.

3. Observations of solar radiation made with a black-bulb thermometer, to be of value, ought to be taken with

the radiation thermometer placed at the same height above the ground as the shaded maximum thermometer with which it is compared; but while freely exposed at all times to direct sunlight, it ought to be protected as much as possible from disturbing influences.

4. Solar-radiation observations made on a plan similar to that adopted at Oxford show that the calorific intensity of the sun's light continued to diminish during the years 1865–66, when the frequency of solar spots was also diminishing, thus giving additional weight to the probability that changes in the heating power of the sun's rays are intimately connected with variations in solar-spot frequency.

X. *On the Structure of the Woody Zone of an undescribed form of Calamite.* By W. C. Williamson, F.R.S., Professor of Natural History in Owens College, Manchester.

Read November 3rd, 1868.

Whilst engaged, some years ago, on an inquiry into the nature of the fossil coal-plants known as *Sternbergiæ*, my attention was arrested by some structures allied to those found in *Dadoxylon*, but occurring in some stems of Calamites. At the same time, the curious specimen represented in fig 1, of which a woodcut was published in the 5th edition of 'Lyell's Manual of Geology' (fig. 478), fell into my hands, and threw new light upon the nature of the small round cicatrices seen at the upper extremity of the longitudinal ridges of each node in many Calamodendra. These circumstances led me, in 1852, to prepare numerous sections of these plants from the specimen in my cabinet, represented in fig. 2, in which the structure was preserved; but as the example only contained the innermost portion of the woody zone, I put the subject aside for a season, in the hope of meeting with further illustrative specimens.

Recently my attention has again been called to the subject by a correspondence with M. Cyrille Grand-Eury, of St. Etienne, who has obtained forms of Calamite altogether different from mine. Other, apparently different, types are in the possession of M. Adolphe Brongniart, of M. Schimper, and of my friend Mr. Binney. It thus becomes probable that several distinct forms of Calamites exist, and that a large amount of combined labour will be required to elucidate their varied aspects. Hence, though my present researches have been chiefly directed to one portion of the structure of one type, it appears desirable that what I have ascertained respecting that type should be recorded for the benefit of others labouring in the same field.

The publication by Sir Charles Lyell of the figure referred to has elicited various opinions respecting the fossil represented. The conclusion at which I arrived was, that the central portion (*a*) was a cast of part of the pith from the base of the plant; that the verticils of radii (*b*), which I would term verticillate medullary *radii,* in contradistinction to medullary *rays,* were prolongations of the pith passing through the woody zone to connect the pith with the bark; that *c* represented the woody zone of the lowermost portion of the stem, whilst *d* represented the exterior of a single articulation of the outer, or cortical layer of the plant.

But several difficulties opposed themselves to this explanation. 1st. The supposition that any *Calamodendron* consisted of two Calamites, one within the other, the one representing the exterior of the pith, and the other the exterior of the bark, was novel, and unsupported by other testimony. 2nd. That the inner structure (*a*) and the outer articulation (*d*) could not belong to each other, since the former consisted of at least seven or eight internodes, or joints, whilst the outer structure (*d*) was a single joint. It was therefore supposed by some that the central portions (*a, b,* and *c*) represented the pith and ligneous zone of one Calamite, which had been accidentally introduced into the interior of the cast of another, when foreign arenaceous material replaced the original vegetable tissues.

In reply to these objections I would urge the following arguments:—That the central pith (*a*) is a true Calamite of the type of *C. Suckowii, Steinhauerii,* and others, is unmistakable; that the medullary radii and investing ligneous zone belong to this pith is equally obvious, both the latter facts being demonstrated by specimens to be described in the following pages. The only doubtful point is the relation which these inner structures bear to the supposed bark (*d, e*). Now, though the latter consists of one articulation of about two inches in length, it does not follow that it did not belong, like the central pith, to a portion near the *base* of the stem, because we well know how rapidly these nodes increase in size as we ascend from the basal one. The aspect of the woody layer (*c*) demonstrates that it has been prolonged considerably below the base of the pith, or, in a word, that the pith has *not* extended so far downwards as to reach the lowest articulations of the stem; as if, reasoning from living exogenous types, we might regard the latter portion as a pithless root rather than a true stem.

But even were these explanations not sufficient, there yet remains another. The specimen shows that the carbonaceous matter of the ligneous zone (*c*) has been preserved after the cellular structures of the medulla (*a*) and of the verticils of medullary radii (*b*) had been replaced by inorganic sand. I deem it possible that the latter change may have taken place before the woody substance of the supposed bark (*e*) disappeared and was similarly replaced. In this case the small portion of ligneous zone thus preserved at the base of the plant might have become slightly detached from its original position, and floated upwards into an internode occupying a higher position in the outer stem than those of which it was originally the centre. The specimen indicates that only about two inches of the lowermost portion of the woody zone had been thus preserved, the external lineaments of the pith and medullary radii having been preserved along with it. I presume that after the base of the plant became imbedded in the stratified sandstone, according to the fashion common amongst the coal-measure plants, the greater portion of the woody contents decayed and floated out, this fragment at its extreme base alone escaping the general decomposition, and being permanently fossilized*.

Which of the above explanations may prove the true one can only be determined by future discoveries; but that the Calamite-like medulla, with its verticils of medullary radii, belongs to the black ligneous zone surrounding it, I shall now proceed to demonstrate from the structure of the specimen represented in fig. 2. This drawing exhibits the appearance of the specimen previous to my cutting it up into sections. It will at once be recognized as consisting of portions of three internodes from the lower part of a stem; owing to the preservation of the innermost portion of the ligneous cylinder, the medullary cast, representing the common type of Calamite, is not seen in its usual form, the transverse constrictions of the latter being here replaced by a projecting mass of organized carbonaceous substance (*b*), whilst the longitudinal grooves usually furrowing the several internodes are represented by sharply defined

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* My friend Mr. Binney, whose extensive experience of Calamites gives weight to his opinion, authorizes me to state that he entirely concurs in the above conclusions.

projecting ridges (*c*) ; these, which lose themselves in the nodes, are separated from each other by more depressed excavated grooves (*d*) corresponding with the elevated longitudinal ridges of the common Calamites. A slight microscopic examination demonstrates that these two features (*c* and *d*) owe their existence to two very different elementary tissues, arranged in vertical laminæ, or wedges, which radiate in alternating series from the medulla to the periphery of the woody zone. It will be remembered that in 1841, Unger, in a work of Petzholdt's ('Ueber Kalamiten- und Steinkohlen-Bildung,' Dresd. u. Leipz. 1841, Tabs. 7 and 8), called attention to a similar arrangement of tissues in the *Calamitea striata* of Cotta, in which one of the alternating structures consisted of *transversely barred fibres,* such as are seen in *Stigmaria,* traversed by medullary rays, and of intermediate tissues composed of smaller and more numerous woody fibres, each radiating series of which had one large central medullary ray. The general type just described resembles, in its broad outlines, what I find in my specimen; but in their minute details the two plants are different.

As already mentioned, the raised ridges and the intermediate depressions in fig. 2 consist of two very distinct structures, each of the former (*c*) being composed of numerous longitudinally disposed vessels of a reticulated type, whilst the latter consist of oblong prosenchymatous cells. In the transverse section, both these structures are seen arranged in the same manner, radiating in *equally regular* parallel lines from centre to circumference. I shall henceforth speak of these alternating structures as composing the *vascular* and the *prosenchymatous* tracts.

Fig. 3 represents a portion of a transverse section made in the centre of an internode, as at 2 *a*, the figure being almost limited to two of the vascular tracts and an intervening prosenchymatous one. As the crenulated outline marking the junction of the woody zone with the pith (fig. 3 *a*) indicates, the portions *c*, *c* radiate from two of the longitudinal furrows characteristic of an ordinary Calamite, whilst *d* corresponds with one of the intervening prominent ridges. The portion *c* consists of the transversely divided mouth of vessels, having a diameter of from $\frac{1}{600}$ to $\frac{1}{700}$ of an inch; they are arranged in from 20 to 25 regular rows, radiating from the pith (*a*) to the periphery of the woody zone. At their medullary extremity these rows of vessels combine to form a sharp woody wedge (*c*), which fits into one of the longitudinal grooves of an ordinary Calamite.

The appearance presented by these vessels when more highly magnified is seen in fig. 4, where portions of two tracts are represented. The walls of each tube do not appear to have been very thick; but it is difficult to determine exactly how much of the substance represents the original woody tissue, and how much is due to subsequent mineral infiltration. There is now no hollow cavity within each vessel.

The prosenchymatous cells forming the intermediate tracts (fig. 3 *d*) have a larger diameter than the cells, averaging about $\frac{1}{400}$ of an inch. They are also more symmetrically arranged in linear series; but in other respects their distribution resembles that of the vessels just described. They radiate from within outwards in from 30 to 35 regular lines. When more highly magnified (fig. 5), each cell appears to have thick walls, like those of recent woody fibre, which I at first believed these tissues to be; but I think that the appearance in question is due to mineral infiltration, and that the true walls of these cells were thin. It will be observed that, in this section, their regular radiating arrangement is that of the pleurenchyma or true woody fibre of coniferous stems, rather than that of ordinary cellular tissue, or parenchyma.

Fig. 6 represents part of a tangential section, the letters *c* and *d* being used to indicate the same parts as in the last figure. We here see that the vessels (fig. 3 *c*) run from one articulation or internode to the other, in the shape of elongated tubes, arranged like the fibres of living Conifers, and separated at intervals by numerous medullary rays (fig. 7 *e*) consisting of vertical layers of cells, arranged in single series. The surfaces of the vessels in this section often display no trace of structure; but here and there we obtained distinct evidence that their walls were strengthened internally by woody reticulations. The intermediate prosenchymatous tracts (fig. 6 *d*) consist, as already stated, of oblong cells (fig. 8) of fusiform shape, but which do not exhibit, *in this aspect*, the regular serial arrangement that is so conspicuous in the transverse section; medullary rays appear almost, if not wholly, absent from this part of the structure. In only two instances have I detected anything that could be mistaken for such a ray; and these were possibly nothing more than a few linearly arranged cells, shorter than the rest.

On making a vertical section of a vascular tract (figs. 2 and 3 *c*) in the plane of a medullary ray (fig. 9), we discover that the vessels (9 *c*) are still regularly arranged in parallel series; and, on applying a high magnifying-power, we find their surfaces to be covered with small reticulations, arranged in an irregular order (fig. 12), but usually with from three to four contiguous areolæ between the two sides of each vessel. These reticulated vessels very closely resemble those seen in some varieties of *Dadoxylon.* The reticulations have no central dot, consequently they must not (as Mr. Carruthers has already pointed out) be confounded with the disks of true glandular fibre. This section reveals the form and arrangement of the cells constituting the medullary rays (9 *e*). They have thin walls, and are arranged in a muriform manner; only the long axis of each cell is often vertical instead of horizontal as is common in living plants. In other respects they appear to be distributed as is usual amongst Coniferæ, strongly reminding us of their arrangement in the carboniferous genus *Dadoxylon*.

Thus far I have confined myself to a description of the woody zone in its undisturbed form, as it appears in the middle of the internodes; but at and in the neighbourhood of the nodes or articulations (fig. 2 *b*) a remarkable modification occurs. As is well known, each of these nodes is represented in the common type of Calamite by a circular transverse constriction; but in the living plant it was merely an indentation of the pith, occasioned by a projecting lenticular ring of woody tissue, of which the medullary margin was somewhat wavy, or projecting at points corresponding with the longitudinal grooves into a series of small irregular teeth.

Fig. 10 represents a vertical section of this structure, as seen under a very low magnifier, *a* indicating the pith, and *b* the woody lenticular ring.

Fig. 11 represents a more highly magnified view of one of the tooth-like projections from this ring, as seen in its horizontal section. The cellular clusters at its internal angles (*i*, *i*) probably belonged to the pith.

I have already called attention to the remarkable series of horizontal verticils of cylindrical prolongations of the pith seen in fig. 1. It is well known that in many fossil Calamites, at the uppermost part of each longitudinal ridge of the several joints there is a small round mark or cicatrix, to which various functions have been assigned. The example figured indicates that wherever such marks occur, the specimen bearing them is a pith, and that the marks are merely the points from which what I have termed the verticillate medullary radii spring. In nearly every specimen hitherto found these radii have disappeared along with the woody zone which they penetrated, the example figured being the only one of its kind that I have either seen or heard of during thirty years of association with the plants of the coal-measures. These radii appear to have been composed of the same tissue as the medulla itself, judging from the circumstance that the inorganic material with which they are filled is identical with that replacing the pith*; they have most probably united the pith with the bark. As this function was amply performed by means of the medullary rays in the fibrous tracts, we must assume that the *radii* had in addition some undiscovered special functions of their own. On turning to the tangential section (fig. 6), we find that a radius (6 *f*) penetrates the centre of the upper extremity of each cellular tract (*d*), in which portion it will be remembered there are no true medullary rays—a circumstance which indicates that the prosenchymatous tracts are not merely prolongations of the pith, since true prolongations of the latter passing through them have retained their separate forms. Each radius is cylindrical, somewhat compressed laterally, and occasionally, but not often, rather triangular. In figs. 10 *f* and 17 *f* we see its position in reference to the node of the woody zone, having a mass of vascular tissue (*c*) above, and the prosenchymatous tissue (*d*) below it. A very limited portion of the latter tissue, peculiarly deflected, interposes between the upper surface of the radius and the remarkable articular or nodal arrangement of the vascular elements next to be described.

It must be remembered that the longitudinal grooves of Calamites usually alternate in contiguous joints or internodes, the elevated ridges of one joint being continuous

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* My friend Mr. Carruthers has suggested that the scars left when these radii are broken off represent the openings of meshes in the woody tissue through which *vascular* bundles passed to whorls of leaves or branches produced at the nodes (Geol. Journal, July 1868, p. 332); but my specimens do not sustain this opinion for the reasons given in the text.

with the furrows of the adjoining ones, though occasionally no such alternation takes place. In the former case, one of the radiating vascular tracts would, if prolonged straight upwards through the node, run into a prosenchymatous tract of the joint above. In the exceptional instances, the vessels are prolonged through the node with little disturbance, and continued into the corresponding vascular tract of the next joint. In both cases the vessels of the various articulations are shown to be continuous. This alternation in the arrangement of the tracts causes peculiar modifications in the disposition of the vessels on crossing the node.

Three disturbing elements exist at the articulation,—1st, the verticillate medullary radii; 2nd, the verticils of vascular bundles; and, 3rd, the alternation of the tracts.

Fig. 13 represents a transverse section almost in the plane of the verticillate radii; exactly so on the left of the figure, but traversing their upper surface to its right: *a* represents the pith, *f* the several medullary radii, and *c* the woody wedges which separate the latter. In fig. 14 one of these wedges is shown more highly magnified. It consists of from 30 to 40 radiating series of vessels, arranged like the fibres of most recent Coniferæ; but at its two outer margins are a very few rows of larger structures (*d*), which we find to be sections of the outermost rows of prosenchymatous cells seen in fig. 3 *d*, some of which, as already shown in fig. 6, separate each medullary radius from the nearest vascular tracts. At its inner extremity each wedge is pointed, having its two sides slightly excavated; hence across the centre of fig. 13 we have a crenulated outline of the pith, readily recognized as representing a section of the exterior of an ordinary Calamite. At this point of the stem the regular arrangement of the vessels has undergone little disturbance, space for the passage of the medullary radii being obtained at the expense of the prosenchymatous cells. This is shown in figs. 6 and 15, in both of which a thin layer of cells is seen, both above and at each side of each radius (*f*). But the case is altogether altered in the plane above the radii corresponding with the centre of the node.

At this point the radiating vascular laminæ forming the wedges *c* in fig. 13 become detached from each other by the intrusion of cellular masses, as seen in fig. 16. Sometimes these cells are in a single row (16 *g*), when they are undistinguishable from the ordinary medullary rays; at others, as at 16 *h*, *h'*, we have unmistakable proof that they are identical with the fusiform cells of the prosenchymatous cellular tracts (fig. 6 *d*). It appears that at each node the fusiform cells and the muriform ones of the medullary rays become blended and undistinguishable from each other, a connexion being thus established between these tissues at each articulation, such as does not exist in the internodes.

It is almost impossible to describe or delineate the wonderful meanderings of the vessels as they ascend across the node. One object of their windings is their redistribution to the two nearest vascular tracts immediately above the node. Fig. 15 attempts a representation of this singular rearrangement, the same thing being partially shown in the upper part of the similar tangential section (fig. 6). The vessels of the vascular tract (15 *c*) diverge as they pass between the two medullary radii (*f*, *f*), to be redistributed to the vascular tracts (*c'*, *c'*), part going to the one and part to the other, whilst many are deflected hither and thither, as if unable to decide which course to take. Throughout all these serpent-like contortions we have abundantly displayed the arrangement represented in fig. 16. But the most remarkable feature connected with this redistribution is seen above the vascular tract (15 *c*), at the portion of the node immediately below the prosenchymatous tract (*d*) of the internode above. At this point we have appearances that vary in different examples. Sometimes, as in fig. 6 *d′*, we have a number of long sinuous vessels, converging at a central point, where we find a small cluster of similar but transversely divided ducts, with a few cells in its centre; at others, as in fig. 15, these *transversely divided* tubes occupy a much larger portion of the area, whilst their open orifices radiate in somewhat regular lines from the central point, which consists, as before, of a small cluster of transversely divided cells. Around the margin of this circular area, which closely resembles a transverse section of some coniferous branch, we again find the long vessels, bending away in the plane of the section, to contribute their respective shares to the two vascular tracts 15 *c′*, *c′*. A vertical section, made in the plane of a medullary ray, enables us to interpret these appearances. As the vessels of the vascular tracts approach a node, instead of following the outline of the pith, as in the rest of their course, they are suddenly deflected from it, bending outwards in parallel arched curves, but return to their original direction after passing the node. Mr. Binney found a similar arrangement in his *Calamodendron*. Fig. 17 is a diagram designed to explain these appearances. It represents a section somewhat like that shown in fig. 10; but in order to illustrate the relations of the several parts to the medullary radius, the diagram is made to represent an oblique section, passing from fig. 15 *f* on the left to *c′* on the right of the same figure. But the arched vessels (17 *c*) must be understood to pass downwards, *on each side* of the medullary radius (17 *f*), and not to terminate abruptly at the latter, as the diagram indicates.

But, in addition to these arching vessels, we have at the nodes numerous groups of divergent vessels (17 *e*), varying in diameter in different parts of the plant, which proceed directly outwards towards the bark. It is one of these groups, transversely divided, which produces the appearance of a transversely divided branch, seen in fig. 15. In the centre of the latter group the cells and vessels are divided at right angles to their longer axes, whilst the peripheral ones run in the plane of the section. I think there can be no doubt that this arrangement was destined to supply some external appendages of the main stem with vascular tissue. Whether these appendages were branches or leaves is doubtful; but I incline to the former opinion.

Mr. Binney has recorded somewhat analogous arrangements in his Tab. 3. fig. 4.

Before attempting to interpret the facts which I have described, a further glance at the discoveries of some other recent observers is necessary.

Nothing could be more unsatisfactory than the state of our knowledge of the internal structure of the stems of Calamites prior to the investigations of M. Unger; and valuable as those were, they only threw light upon one branch of the question. Guided by that light, M. Adolphe Brongniart suggested, in his 'Tableau des genres de Végétaux Fossiles'*, that two distinct groups of plants had hitherto been confounded under the name of Calamites. He proposed that these should henceforth be separated, retaining the old name for one section, which he regarded as allied to the *Equisetæ*, and employing the generic term *Calamodendron* for the other, which he thought ought to be ranged amongst the Gymnospermous Dicotyledons. The structure of the genus *Calamitea* of Cotta, as demonstrated by Unger, is regarded by M. Brongniart as typical of his *Calamodendron*. He describes this genus as exhibiting, amongst other features, a woody cylinder composed of large radiating vertical tracts of transversely barred vessels intermingled with true medullary rays, alternating with

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* Dictionnaire Universelle d'Histoire Naturelle.

corresponding tracts, composed of masses of a peculiar form of woody fibre, with one large medullary ray in the centre of each tract.

My friend Mr. Binney has thrown further light upon this subject in his volume 'On the Structure of *Calamites* and *Calamodendron**', which only came into my hands after the previous pages had been written. Mr. Binney does not attempt to separate *Calamites* from *Calamodendron,* but contents himself with describing his specimens, which are of great interest. The woody cylinder of every one of them essentially resembles that examined by Unger, in consisting of radiating alternate tracts of barred or scalariform vessels, and of coarse cellular tissue. This copious development of scalariform vessels demonstrates that Mr. Binney's examples present the same type of organization as the *Calamitea* described by Unger.

Dr. Dawson, of Canada, detected in *Calamodendron* some traces of what he terms "wood-cells, with one row of large pores on each side"†. And in another part of his paper he again refers to them as tissues in which "the disks or pores are large and irregularly arranged, either in one row or several rows." Of course neither of these descriptions exactly agrees with the reticulated fibres of my plant; but as Dr. Dawson expressly associates his examples with the fibres seen in *Dadoxylon,* I conclude that he may have met with a *Calamodendron* of a type approaching nearer to my plant than to those of M. Unger and Mr. Binney.

Dr. Dawson has further illustrated this subject in the 2nd edition of his 'Acadian Geology,' where he figured

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* Observations on the Structure of Fossil Plants found in the Carboniferous Strata, by E. W. Binney, F.R.S., F.G.S.—Part 1. Calamites and Calamodendron. London, printed for the Palæontographical Society, 1868.

† "On the conditions of the deposition of Coal, more especially as illustrated by the Coal-measures of Nova Scotia and New Brunswick," Quarterly Journal of the Geological Society, vol. xxii. 1866.

the tissues* referred to; but none of his figures correspond exactly with the vessels of my plant, fig. 163 E alone exhibiting an approach to a resemblance.

Be this as it may, we now have suggested to us the possible existence of three types of *Calamodendron*, in each of which the stem consists of woody wedges, disposed vertically around the pith from which their component laminæ radiate, and which are separated from one another by alternating vertical cellular masses either of parenchyma or prosenchyma, which latter connect the pith with the bark. But if the descriptions of Mr. Binney and Mr. Carruthers are correct and generally applicable, it is also obvious that, though constructed on a common plan, important differential characters separate two of the types from the third. In Mr. Binney's plants the woody wedges are exclusively composed of scalariform tissues, and appear to contain no true medullary rays. On this point Mr. Carruthers speaks strongly, and relies upon the fact as one evidence of the Cryptogamous character of these fossils†; and Mr. Binney does not appear to differ materially from Mr. Carruthers. He figures in his plate 3. fig. 6 what he terms "medullary (?) bundles;" but these are the representatives of my prosenchymatous tracts, and not of the muriform medullary rays.

The thick radiating cellular laminæ separating the woody wedges in Mr. Binney's specimens consist of ordinary parenchyma. In Unger's *Calamitea* the woody wedges consist, as in Mr. Binney's examples, of scalariform tissue (vaisseaux rayés); but these vessels are separated by "des rayons médullaires très-étroits, d'un seul rang de cellules, et peu étendus en hauteur" (Tableau des genres de Vé-

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* *Loc. cit.* figs. 162 *c*, *d*, and 163 E.

† "On the Structure and Affinities of *Lepidodendron* and *Calamites*, by W. Carruthers, Esq., F.L.S." Seemann's Botanical Magazine, Dec. 1866; see also Journal of Botany, Dec. 1867, "On the Structure of the Fruit of *Calamites*."

gétaux Fossiles, &c., extrait du Dictionnaire Universelle d'Histoire Naturelle, 1849, p. 50), which obviously correspond to my true medullary rays. The intermediate cellular laminæ consist of what Brongniart terms "woody fibres" (fibres ligneuses), but which appear to be identical with my prosenchymatous tissue, each layer having in its centre what he terms one large medullary ray composed of two or three vertical rows of cells. In my plant the woody wedges are constructed, like Unger's *Calamitea,* of elongated vessels, separated by numerous medullary rays; only the vessels are reticulated instead of scalariform. The intermediate cellular tracts also appear to resemble Unger's, consisting, as I have shown, of a peculiar prosenchymatous tissue. I find here nothing like the central medullary ray of Unger; but instead of it we have the large verticillate medullary radius in the upper extremity of each prosenchymatous tract. Thus we learn that Unger's plant has the scalariform vessels of Mr. Binney's type, with the medullary rays of mine, and constitutes, in these respects, an intermediate link between the two.

The prosenchymatous tissue is of considerable interest on several grounds. When divided vertically, whether on the plane of the medullary rays or tangentially, the cells appear to be arranged in no special order beyond what we see in many compressed forms of parenchyma. But when we turn to the transverse section, we discover the regularly linear radiating arrangement which I have described, and which is so characteristic of vascular tissues, as well as of the pleurenchymatous elements of Gymnospermous Exogens. From their occupying the same position as the cellular laminæ of other *Calamodendra,* which are unquestionably prolongations of the pith, we might almost regard them as huge medullary rays. These, however, as we have seen, exist in addition to them. They so closely resemble, in their linear arrangement, the pleurenchyma of coniferous wood, that it is difficult to regard them otherwise than as an elementary form of simple pleurenchyma, or woody fibre. But it is their elongated shape, the obliquity of their overlapping extremities, and their radiating disposition in the transverse section which give them that character, and not the existence of ligneous deposits in their interior.

The conspicuous occurrence of these prosenchymatous cells in a Calamite acquires additional interest from the circumstance that they are identical with those already described by Mr. Binney, under the name of "elongated utricles," as occurring in *Sigillaria vascularis**. In that plant the outer of the two woody zones which the stem contains is entirely composed of this peculiar tissue; but physiologically the fact deserves notice that, as Mr. Binney points out, it gradually passes into the ordinary parenchyma of the inner part of the stem. I may further observe that small masses of the same tissue, also disposed towards an arrangement in radiating lines, constitutes the external ridges of the living *Equisetum limosum* and its allies. Longitudinal strips, torn from the epiderm of that plant and viewed from within, exhibit an alternate arrangement of longitudinal bands that strikingly resembles what we find in tangential sections of *Calamites*; only the long vascular tracts of the latter are wanting in the recent plant, their place being occupied by cellular tissue. May we regard this prosenchyma as a rudimentary form of pleurenchyma? Dr. Dawson refers to the same tissue under the name of "bast-tissue;" but this term is only appropriate to true pleurenchyma, and not to the more rudimentary type under consideration.

We may now inquire what are the true affinities of these various forms of *Calamites*? Schimper, Carruthers, and

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* "A Description of some Fossil Plants, showing Structure, found in the Lower Coal-seams of Lancashire and Yorkshire, by E. W. Binney, Esq., F.R.S." Phil. Trans. 1865, p. 591.

Hooker are disposed to regard the type described by Binney and Unger as Equisetaceous. On the other hand, though M. Brongniart believes in an Equisetaceous form of Calamite, he does not regard the above examples as belonging to it. He considers that they are *Calamodendra,* which he places amongst the Gymnospermous Exogens. Are we yet in a position, in the face of these discrepant opinions, to arrive at a conclusion on the moot point? The importance now attached to the doctrine of evolution gives significance to the question, and renders an answer desirable.

The inferiority of the cellular to the vascular plants is obvious and admitted. When we ascend to the vascular Cryptogams, we find that they retain indications of their natural alliance with the cellular forms, in having the vascular elements largely intermingled with cellular ones; whilst in the Gymnospermous Exogens the purely cellular element is almost eliminated from their woody layers, being only represented there by the medullary rays. Viewed in this aspect, Mr. Binney's *Calamodendron* appears to approximate to the recent Acrogens, in whose stems cellular and prosenchymatous tissues are abundant, combined with vessels of a scalariform type, but which, so far as I know, exhibit neither reticulated vessels nor medullary rays. But when we ascend to the Gymnospermous Exogens, we find the cellular element disappearing, whilst vascular tissues present themselves, chiefly in a reticulated*, spiral, scalariform, or glandular form. In my plant, whilst we have abundance of muriform medullary rays, we have few if any transversely barred vessels; every duct of which the structure can be traced is distinctly reticulated, whilst the prosenchymatous cells, as we have seen, assume

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* I may observe that a small form of reticulated pleurenchyma, apparently almost identical with the reticulated vessels of my plants, enters largely into the woody zone of the living *Araucaria imbricata.*

a pleurenchymatous arrangement and aspect; so that in all these respects my specimen approaches nearer to the Gymnospermous Exogens than to the Acrogens. In the *Sigillaria vascularis* described by Mr. Binney (Phil. Transactions, 1865), we find abundance of the identical form of prosenchyma seen in my Calamite, associated with cellular tissue, scalariform vessels, and muriform medullary rays; whilst in *Sigillaria Brownii,* according to Dr. Dawson, we have an inner cylinder of scalariform vessels surrounded by an outer one of true glandular fibre.

It is clear that all these plants exhibit, so far as their stems are concerned, indications of mutual affinity with Acrogenous Cryptogams on the one hand, and with Gymnospermous Exogens on the other. The prevalence of cellular and scalariform tissues points in the former direction, whilst the arrangement and apparently exogenous mode of growth, both of their vascular and prosenchymatous elements, are suggestive of the latter. This is especially the case with the Calamites, which seem to me to constitute a well-defined link, connecting the Exogens with the Acrogens.

But whilst it appears reasonable to locate the Calamites, as a whole, in the position just indicated, the minor differences which the several types seem to present suggest the necessity for some change in their generic grouping. I would therefore propose that, until the correctness or otherwise of Brongniart's opinion that there exists an Equisetiform class of Calamites is determined, we retain his two genera of *Calamites* and *Calamodendron.* The genus *Calamites* to embrace all the Equisetaceous forms, if any such really exist, whilst *Calamodendron* may comprehend the *Calamitea* of Unger, and possibly Mr. Binney's specimens, the genus being characterized by the prevalence of *scalariform* vessels and medullary rays, and by the absence of verticillate medullary radii.

In addition, I would suggest the establishment of the new genus *Calamopitus* (κάλαμος-πίτυς) for those forms in which the woody elements consist of *reticulated* vessels associated with medullary rays, and having verticils of medullary radii near the nodes*.

The exact value of these medullary radii, as indicative of a generic distinction, remains to be ascertained, as well as the extent to which they are associated with reticulated vessels. If it should be found that they occur in Calamites with scalariform vessels, or if Calamites having none but reticulated vessels exist without traces of verticillate medullary radii, then it may be necessary to abandon the the term *Calamopitus*, and associate the whole series as variable examples of Brongniart's genus *Calamodendron*. At present, however, we know that neither Binney's nor Unger's plants possess the verticillate radiating prolongations of the pith. They are also absent from silicified Calamites from Autun, of which M. Brongniart showed me fine specimens many years ago, and which, he now informs me, are identical with Mr. Binney's plants. Dr. Dawson says that true Calamites "can always be distinguished" [*i. e.* from piths of *Calamodendra*] "by the scars of the leaves or branchlets which are attached to the nodes." But my plants indicate precisely the opposite conclusion to this, viz. 1st, that such scars appear to

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* I do not altogether like this arrangement, because I have the strongest doubts respecting the existence of an equisetiform type of Calamitea part from what Mr. Binney has described. I should have preferred applying the old generic name *Calamites* to Mr. Binney's plants, and assigning Brongniart's term *Calamodendron* to my own. But when an author founds a genus, he is entitled to define it, and M. Brongniart has distinctly identified *Calamodendron* with a vascular zone consisting of scalariform vessels and true medullary rays. The plan proposed presents the further difficulty that, if Mr. Binney's specimens are really deprived of medullary rays, that important characteristic feature will exclude them from *Calamodendron*, and involve the necessity for establishing an additional genus for their reception. But as I regard this as a doubtful point, I am not, at present, prepared to separate them from *Calamodendron*.

belong exclusively to medullary casts; and, 2nd, that they are *not* the scars of leaves or branchlets. I am confirmed in this conclusion by the fact that, though these scars are very distinct on those joints of the pith of the specimen represented in fig. 1, from which the verticillate medullary radii have been broken away, *they are wholly absent from the longitudinal ridges of the exterior of the specimen.* If, as I believe, mine is a correct interpretation of these scars, we must abandon the notion that they were due to internal vascular bundles, or that they were scars occasioned by the separation of external branches; and, as a consequence, the verticillate arrangement of the branches suggested in the restorations of Dr. Dawson will fail to be sustained, *so far as it rests upon the position of these scars,* though supported by other facts. Of the external appendages of my plant as yet I know nothing. Since the axes of the cones described by Mr. Binney are characterized by the presence of scalariform tissue, they must belong to his plant rather than to mine; and there seems no reason to doubt that some species of *Asterophyllites* also represent the foliage of the same. All that we know respecting the structure of my plant externally to the woody zone is suggested by fig. 1, where we have a very thick layer of sandstone surrounding the woody cylinder, and which must be regarded as representing an equally thick bark. Whether the longitudinal flutings of its exterior really indicate the outermost part of the bark, or whether this, in turn, has been invested by an epidermal layer, represented by the thin covering of coal with which such Calamites are frequently covered, has yet to be determined. Of its growth we know nothing beyond the fact that the plant has developed offshoots from the base of its central stem, as appears obvious from the common occurrence of stems the lower extremities of which exhibit a strongly marked lateral curvature.

I believe that the two specimens described are both from the upper coal-measures. Fig. 1 was found in a sandstone quarry near Oldham; and though I am doubtful respecting the precise locality whence fig. 2 was obtained, I believe that it came from near Peel. I have a similar specimen from the Peel Delf-rock, near Worsley, a sandstone belonging to the Upper Coal-measures.

Since reading the preceding memoir, I have had the advantage of studying a very beautiful and important series of sections of carboniferous plants, prepared and chiefly collected by Mr. J. Butterworth, of High Crompton, near Oldham. Amongst these are several instructive sections of Calamites. This valuable contribution to microscopic science requires me to modify one or two conclusions at which I had previously arrived, since the specimens render plain several points which were formerly obscure. Most of them correspond very closely with Mr. Binney's examples in having the vascular tracts composed of scalariform vessels. *But in one I can trace a decided transition from the scalariform to the reticulated type,* thus suggesting the possibility of a link between Mr. Binney's specimens and my own. Nor is this all; in other specimens I find the cellular tracts most variable in their structure. Sometimes the cells are elongated *transversely,* so that one long cell extends horizontally across the cellular tract, reaching from one vascular tract to another. In another example the reverse is the case; the cells are much elongated vertically, but have rectangular extremities. In *the same* tract these rectangular cells pass into the prosenchymatous type, in which the ends of the cells diagonally overlap each other; and yet in the same specimen are parts of corresponding tracts in which the cells are of the ordinary parenchymatous type seen in Mr. Binney's plants. But whilst on these points Mr. Butterworth's specimens exhibit so many features in common with Mr. Binney's, as in mine, the woody tracts are *all* provided with some medullary rays. This structure identifies these specimens with the Calamitea of Unger, and makes it more probable than before that a further study of Mr. Binney's fine examples will reveal medullary rays in them also. The only absolute and permanent distinction that still appears to separate my type of stem from the rest is the existence of the verticillate medullary radii, which are equally absent from all other described forms, but which alone I should scarcely regard as constituting a sufficient basis for a generic distinction.

But, as I shall presently show, there are other circumstances which justify the provisional retention of my genus *Calamopitus*. Mr. Butterworth's specimens accord in a remarkable degree with my description of the curious arched deflection of the barred vessels from the surface of the pith when passing the nodes, and also reveal traces of vascular bundles passing from the interior to the exterior of the woody zone at the same point, as indicated in my fig. 17; but this arrangement is much less complicated than in my examples. The vessels of each vascular tract ascend in a compact form until they reach the node; they then divide right and left to be distributed to the two contiguous vascular tracts above. Each bundle undergoes no change in doing so, beyond bulging out somewhat to admit of a large admixture of cells like those of the cellular tracts, returning immediately to the former compact arrangement as the diverging bundles enter their respective vascular tracts in the joint above. The specimens indicate that the exterior of the woody zone had a furrowed Calamite-like outline, the external ridges corresponding with the *vascular* tracts, whilst the depressions marked the external line of the *cellular* ones—an arrangement reversing that which these structures bear to the surface of the pith.

But Mr. Butterworth's collection has, in addition, furnished me with the cone of my plant. It has been of much larger dimensions than those described by Mr. Binney, as well as much more complex in its internal structure, and otherwise distinct; but that it belonged to my plant is shown by the reticulated vessels of its central axis, as well as by their curious arched arrangement when crossing the nodes—an arrangement to which I have already referred as constituting so remarkable a feature of the plant which I have described. From this axis there are given off at each node ten well-defined radiating peduncles, each of which divides into bract-like appendages, which bend first downwards, and then curve upwards and outwards, first sending upwards into the cone two sporangium-bearers. The sporangia have walls consisting of a single layer of oblong tabular cells; and their interior is filled with defined cellular tissue. Each spore seems to be a spherical body, consisting of two layers, exhibiting no trace of elaters or external appendages. I have but briefly indicated a few of the features of this beautiful cone, because its more minute and elaborate features demand that I should devote to it a separate memoir, which I hope to be able to lay before the Society at an early date. The peculiarities of this cone seem to justify the establishment of the genus *Calamopitus*.

All the additional facts which Mr. Butterworth's invaluable specimens have revealed confirm my previous conviction of the close affinity existing between the structure and growth of the woody wedges of my Calamite and corresponding wedges taken from the stems of some Dadoxylons. Of course this resemblance implies that in their growth these wedges have been exogenous, which is unquestionably true. So far I agree with the opinion always held by M. Adolphe Brongniart respecting the stems of his genus *Calamodendron.* But that distinguished botanist has further held that these plants were *Gymnospermous* Exogens, which of course involved reproduction by means of stamens and pistils; but this, as I have just shown, is not the case. The cone to which I have referred is unmistakably Cryptogamic in its type. The same remark applies to the cones described by Mr. Binney. The conclusion, therefore, at which I arrive is, that the Calamites constitute essentially *one* large group of plants, with some considerable range of variation in the details of their internal organization, but not more than exists in many well-defined family groups of living plants (as, for example, the Equisetaceæ), and that their stems were exogenous, so far as the woody cylinder was concerned, and closely related to those of the Dadoxylons. But, on the other hand, their fructification was Cryptogamic, but not necessarily Equisetiform, though not without some features of resemblance to that type of recent Cryptogams. Thus we have in the Calamite a combination that appears to have no living representative. Mr. Darwin may fairly point to these plants as indicating a generalized structure which at some later period became differentiated, through the Dadoxylons of the carboniferous rocks and the true *Equiseta* of the Oolites, into the modern types of Coniferous and Equisetiform plants. It must be remembered, however, that these Dadoxylons are not *all* true Conifers like the living types. We have not yet found in them (with one exception) the peculiar glandular disk, with its central spot, which occurs in all the modern Coniferæ, even in the Cycadeæ. Consequently these supposed Conifers themselves may ultimately be shown to constitute an additional connecting link between the ancient and modern types of gymnospermous vegetation. Every additional study of the older fossil plants, including those of the Oolitic age, makes plain the difficulty, if not the impossibility, of identifying them exactly with living types. Special organs may and do exhibit close resemblances; but the general combinations are frequently different. In this respect the recent Cycads retain something of the features of the more ancient vegetation. In their stems we find the scalariform and annular vessels of the Acrogens side by side with the gymnospermatous inflorescence and the Araucarian forms of glandular tissue linking them with Conifers, whilst in one aberrant genus (*Stangeria*) we have the stems and cones of Cycads combined with the foliage and nervures of a true Fern. The differentiation which has been so complete in other plants appears to have remained unaccomplished in the Cycadeæ.

Various attempts have been made to *restore* the Calamite, as well as the other plants of the Coal-measures; and though such attempts have been hitherto, and still are, premature, they are not altogether useless, since they mark the successive stages of progress towards truth. In the instance of the Calamites, we have especially the restorations of M. Deslongchamps (given in the 'World before the Deluge' of Louis Figuier) and those of Dr. Dawson (Acadian Geology, 2nd ed. p. 442). The former represents the plant as giving off from its central stem large bushy branches, like those of some modern Araucarias. To this idea the structure of the woody zone affords no support. If any *large* branches had sprung from the main stem, the structure of this zone would have shown unmistakable evidences of the fact in the lateral deflection of large masses of vascular tissue. But I see no evidence of such large deflections. We have deflections on a small scale; but the diameter of the largest and thickest of these divergent vascular bundles never exceeds the breadth of one of the longitudinal ribs of a Calamite; hence the vascular portions of these appendages must have been slender. At the same time I infer that they were something more than leaves, from the arrangement of the vessels of these bundles when seen in a transverse section; they exhibit, as is seen in fig. 17, the radiating arrangement of a woody twig, rather than what we should expect in vessels merely destined to supply a leaf. The only wonder is, that lateral appendages so freely supplied with vascular tissues should ever be deciduous, which those of the Calamites seem to have been, or we should frequently find them *in situ*, which has not yet been done. The difficulty is partially surmounted by the supposition that such small branches were jointed, and exceedingly slender, especially at their points of attachment to the stem; and we find such slender-jointed twigs in the Asterophyllites, to which so many observers have referred as probably constituting the branches and foliage of Calamites. With this decision I am strongly disposed to agree. For the above reasons I regard the restorations of Dr. Dawson as approaching nearer to the truth than those of M. Deslongchamps. But of course I differ from Dr. Dawson when he regards his restorations as representing an Equisetaceous type of plant distinct from *Calamodendron*.

During this investigation I have been forcibly impressed with the almost universal occurrence, within the interiors of other plants, of the cylindrical rootlets of *Stigmaria*; they appear to have penetrated every thing that was penetrable. They have forced their way most abundantly into the interior of the lax piths of Calamodendra, Lepidodendra, and Dadoxylons, often making the interpretation of sections of these plants difficult to the eye unfamiliar with the aspect of these ubiquitous rootlets. Their penetrating tendency has culminated in one of Mr. Butterworth's specimens, in which one rootlet has forced its way into the interior of another rootlet which only happened to be a size larger than itself.

EXPLANATION OF PLATES I.–V.

1. Specimen of Calamite from a Coal-measure Sandstone-Quarry near Oldham: *a*, lower end of the pith, consisting of seven or eight joints; *b*, two verticils of medullary prolongations, or "*verticillate medullary radii*," penetrating the ligneous zone *c*; *d*, single joint of a Calamite, enclosing the above; *e*, supposed bark, replaced by sandstone.
2. Exterior of specimen of Calamite in ironstone, as it existed before being cut up into microscopic sections: *a*, internodes; *b*, nodes; *c*, projecting wedges of reticulated vessels; *d*, intermediate tracts of fusiform cells, or prosenchyma; *f*, extremities of "*verticillate medullary radii*."
3. Transverse section through the centre of an internode: *a*, pith; *c*, *c*, two vascular laminæ; *c′*, *c′*, inner margins of the same laminæ, corresponding to longitudinal grooves of Calamite; *d*, a prosenchymatous lamina.
4. Transverse section of vessels of fig. 3 *c*, more highly magnified.
5. Transverse section of prosenchymatous cells of 3 *d*, more highly magnified: *a*, proper cell-wall; *b*, infiltrated mineral matter.
6. Tangential section of part of a node, and of the internode below, made transversely to the verticillate medullary radii: *c*, *c*, two vascular tracts; *d*, *d*, *d*, portions of three prosenchymatous tracts; *d′*, converging vessels near the base of a prosenchymatous lamina, supplying a lateral branch above the node; *d″*, corresponding part to *d′*, but not giving off a lateral branch; *f*, *f*, *f*, three transversely divided medullary radii.
7. Portion of fig. 6 *c*, more highly magnified: *e*, *e*, medullary rays, divided at a right angle to their course; *g*, *g*, elongated vessels.
8. Portion of prosenchymatous tract, fig. 6 *d*, with the fusiform cells more highly magnified.
9. Vertical section of a vascular tract, made in the plane of a medullary ray: *c*, reticulated vessels; *e*, cells of a medullary ray in their lateral aspect.
10. Vertical oblique section of a node, nearly in the plane of one of the medullary radii: *a*, pith; *b*, central internal projection of the node into the pith; *c*, vascular tract above the node; *d*, prosenchymatous tract below the node; *f*, medullary radius.
11. Transverse section of part of fig. 10 *c*, nearly in the plane of *b*, consisting of transversely divided vessels disposed in radiating lines: *i*, *i*, small groups of irregular prosenchymatous cells at each inner angle of the projection.
12. Two vessels of fig. 9 *c*, more highly magnified.

13. Transverse section in the plane of the verticillate medullary radii: *a*, pith; *c*, *c*, woody wedges; *f*, *f*, *f*, medullary radii.
14. Portion of fig. 13, more highly magnified: *a* pith; *c*, extremities of reticulated vessels; *d*, *d*, a few rows of prosenchymatous cells; *f*, *f*, two medullary radii.
15. Tangential section of part of a node, close to the pith: *c*, vascular tract below the node; *c'*, *c'*, two similar tracts above the node; *d*, lower extremity of a prosenchymatous tract above the node, with section of the vessels supplying a lateral branch; *d*, *d*, uppermost portions of two prosenchymatous tracts below the node; *f*, *f*, two transversely divided medullary radii.
16. Diverging vessels of fig. 15 *c*, more highly magnified: *g*, *g*, reticulated vessels; *h*, *h'*, transverse sections of intercalated prosenchymatous cells.
17. Diagram illustrating the apparent direction of the vessels in fig. 10: *a*, pith; *b*, node; *c*, vessels of vascular tract deflected into outward curves on crossing the node; *d*, prosenchymatous cells; *e*, vessels passing to the surface to supply a branch.

XI. *Some Remarks on Crystals containing Fluid.*

By J. B. Dancer, F.R.A.S.

Read before the Microscopical and Natural-History Section, Dec. 2nd, 1867.

Mineralogists and most microscopists are aware that many years ago it was noticed that certain natural crystals contained fluid pent up in cavities in their interior; and in the year 1818 Sir David Brewster had his attention accidentally directed to this subject by the explosion of a crystal of topaz, which he had exposed to a red heat for the purpose of expelling its colouring-matter. The sudden expansion of the fluid imprisoned in the cavities of this specimen caused it to split into numerous fragments; and Sir David was then induced to investigate the nature of the fluid, the form of the cavities which contained it, and the arrangement of these cavities in reference to the crystalline form of the mineral.

It appears that Sir Humphry Davy was the first to examine the chemical nature of the fluid, and the gas which was sometimes contained in these cavities.

He collected the fluid, in fine capillary tubes, from quartz crystals obtained from various localities, and found that in every case except one the fluid was water nearly pure; and in this single specimen it appeared to be naphtha, the gas nitrogen, about 65 times as rare as the atmosphere. In one cavity the gas, name not mentioned, was 10 times as compressed as atmospheric air.

In the naphtha-cavity there was almost a perfect vacuum.

Mr. Sivright had previously found fluid in crystals of calc-spar, sulphate of barytes, and sulphate of lime.

Sir David Brewster discovered fluid-cavities in the emerald, beryl, cymophane, and feldspar. He also found fluid entangled in the following crystals, which were deposited from aqueous solutions:—Sulphates of iron, zinc, copper, nickel, soda, magnesia, ammonia, magnesia and iron, soda and magnesia, alumina and ammonia, and ammonia and magnesia, nitrates of silver and strontium, oxalic acid, tartrates of potash and soda. He next examined crystals formed by heat and sublimation, but could not detect a trace of any fluid; this he considered highly favourable to the aqueous origin of crystals containing water. In the prosecution of this inquiry, Sir David Brewster employed many ingenious experiments; a detailed account of these may be found in the 'Transactions of the Royal Society of Edinburgh.'

He discovered the existence of two new fluids in the cavities of minerals, which are immiscible, and which possess remarkable physical properties; Dana thus describes them:—

"One of these fluids has been named Brewstoline, after its discoverer; it is a liquid hydrocarbon, transparent and colourless, and is nearly 32 times as expansible by heat as water, increasing one-fourth of its volume by an increment of 30° at 50° Fahr. On exposure to the air, it undergoes quick motions and changes, and finally evaporates, leaving a residue of minute solid particles, which, from the moisture of the hand alone, suddenly become fluid again; the residue volatilizes by heat, and dissolves in acids without effervescence.

"The other fluid, which is sometimes found with Brewstoline, has been named Cryptoline, from the Greek word 'kryptos,' concealed. This liquid, when exposed to the air, speedily hardens into a yellowish, transparent, resinous body, not volatilizable by heat, or soluble in alcohol or water, but dissolving rapidly, with effervescence, in sulphuric acid. Nitric and hydrochloric acids also dissolve it. The index of refraction is nearly the same as that of water."

No satisfactory explanation has been given of the presence of this organic fluid. These fluids were first discovered in quartz, topaz, chrysoberyl from Quebec, and in amethyst from Siberia. Since this discovery, Sir David has examined many hundreds of specimens of topaz by the aid of the microscope and polarized light, and discovered many cavities, filled with crystals of various primitive forms and with different physical properties. These crystals are either fixed or moveable; some of them have their axis of double refraction coincident with those of the specimen which contains them. Some of these crystals melt by the application of heat, and recrystallize on cooling; others do not recover their crystalline form again; others resist the most powerful heat. During the examination of a diamond, Sir David found a small cavity which, when seen by polarized light, exhibited four luminous sectors; this appeared to prove that the diamond, when in a soft state, had been compressed by an elastic force proceeding from the cavity. This inference countenances the opinion that the diamond was of vegetable origin, from a substance which had once been in a plastic state, like amber and other gums.

Prince Albert permitted Sir David to examine the Koh-i-noor diamond; this contained three black specks, scarcely visible to the naked eye; but under the microscope, with polarized light, they were shown to be cavities, surrounded by sectors of light. In the two smaller diamonds which accompanied the Koh-i-noor there were several cavities showing the same phenomenon. The examination of nearly fifty diamonds lent by Messrs. Hunt and Roskell, showed numerous cavities of the most singular forms, round which the substance of the stone had been compressed.

In some diamonds from the East-India Company's Museum, large cavities were found, which would have prevented them from being cut into brilliants; in fact, it appears that diamonds are more subject to flaws than any other stones used by jewellers. Some diamonds derive their black colour from the number of cavities they contain, which will not permit any light to pass through them.

About twenty-four years since, Sir David Brewster presented me with a specimen of topaz with fluid-cavities, the first I possessed. Since that time I have been in the habit of examining other crystals. Through the kindness of friends I have had many interesting specimens for inspection; amongst these, fluid-cavities have been found in quartz crystals from South America, Norway, the Alps, Ireland, Wales, and also in some quartz crystals which Mr. E. W. Binney, F.R.S., brought from the Isle of Man.

A specimen of fluor-spar, given to me by a friend, contained a fluid-cavity much larger than any I had seen in quartz and topaz. It was my intention to show this to our Members; but on trying some experiments for the purpose of ascertaining the degree of heat required for the expansion of the fluid, the cavity unfortunately burst;

and the crystal being immersed in hot water at the time, it was not possible to collect the fluid for examination. Since this paper was written, Sir David Brewster has informed me that he has experimented with the fluid-cavities in fluor-spar, and found they burst at a temperature of 150°, and that the fluid is water.

When a precious stone contains many of these cavities, which to unassisted vision look like flaws and cracks, it is laid aside as imperfect and unfit for ornamental purposes. On close examination under the microscope, many of those supposed to be perfect exhibit cavities. Some specimens of ruby I have examined show entangled crystals and fluid-cavities, which are beautifully seen by polarized light.

For the information of those Members who may not have seen specimens of quartz and topaz containing fluid, I will describe their usual appearance. The cavities most frequently are of an irregular figure, more or less angular, the fluid generally nearly filling the cavity, a little bubble being alone visible, and easily moved about as the crystal is inclined in different directions. If, during the examination under the microscope, a small degree of heat is applied to the crystal, the fluid will expand and fill the cavity, causing the bubble to disappear entirely; and on cooling, a number of small bubbles make their appearance, which, after a time, unite.

After the Meeting, we can try this experiment.

How Nature operates in forming these crystals is a matter of mere conjecture: time is doubtless a very important element; our experiments are so limited in this respect, that we cannot hope to imitate her successfully.

Many attempts have been made to produce diamonds and other precious stones by the prolonged action of intense heat.

When a boy, I recollect my father experimenting for a long time in this direction; but the results of his experiments, and also those of many others, have not been very satisfactory.

The examination of a number of precious stones by the aid of the microscope has suggested that this instrument would be a valuable assistant in detecting spurious from real gems; of course I mean in those sufficiently transparent for microscopical examination.

I imagine there are but few stones used in jewellery which are so perfect that the microscope will not reveal some flaws, although possibly very minute ones. On comparing these imperfections with those so abundant generally in artificial stones, it is easy, with a little experience, to decide, by this method alone, whether the stone is spurious or genuine. This applies to all gems mounted in open settings.

With respect to the formation of those crystals containing fluid-cavities, Sir David Brewster states, in a paper read before the Royal Society of Edinburgh, March 1862, that "In my paper of 1826 I was driven to the conclusion that cavities containing the two new fluids were formed by highly elastic substances, when the minerals themselves were either in a state of fusion, or rendered soft by heat. At this time I was acquainted only with the two new fluids, and some of their chemical and physical properties; but when I had studied their arrangement in strata, this opinion acquired additional weight. Had these cavities been arranged in planes parallel to the primitive or the secondary face of the crystal, some argument might have been urged in favour of their aqueous formation; but when it was found that the strata of cavities traversed the crystal in all possible directions, that they were bent also into curves of contrary flexure, and that even individual cavities had a curvilinear shape, it was impossible to resist the conclusion that the cavities were formed and thus capriciously distributed when the substance of the crystal was in a soft or plastic state. This conclusion derives considerable strength from the fact that the water-cavities in crystals deposited from an aqueous solution are never thus arranged.

"The discovery of pressure-cavities in topaz and diamond may be considered as completing the evidence for the igneous origin of these minerals, and of the rocks which contain them. We know that gas, in a state of compression, exists in minerals. In the pressure-cavities we have not only the seat of an elastic force, but its direct action upon the substance of the crystal.

"Though of equal density throughout, as is proved by the equality of its polarized tints, the crystal has its density increased round the pressure-cavity, the density being a maximum close to the cavity. Such a structure is impossible in crystals formed by aqueous deposition; and hence there is not a single example of a pressure-cavity in any of them. They exist, however, in amber and in glass, substances that have once been in a plastic state; and I have produced them artificially by compressing a solution of gum arabic between two plates of glass, so as to include some bubbles of air. The air in these cavities being exposed to changes of temperature, compresses the circumjacent gum, and gives it that variation of density which produces four luminous sectors in polarized light, exactly of the same character as those which are found in topaz and diamond."

Mr. Sorby discovered numerous fluid-cavities in the quartz of granite, in the quartz of volcanic rocks, and also in the feldspar ejected from the crater of Vesuvius; from this it is inferred that these researches tend to confirm the theory of the igneous origin of granite and eruption-rocks in general.

Mr. Sorby, in a paper read at the Leeds Meeting of the

British Association, entitled "On a new Method of determining the Temperature and Pressure at which various Rocks and Minerals were formed," states that "When crystals are artificially formed from solution in water, they catch up and hermetically enclose in their solid substance small quantities of that liquid, so as to produce fluid-cavities, which, from the nature of the circumstances under which they originate, are just full of the liquid at the temperature and pressure at which they are formed.

"This fluid is also affected by changes of temperature and pressure, only that, of course, the actual amount of the change of dimensions, and the laws connecting it with the temperature and pressure, are not the same as in the case of air. If, then, a crystal be formed at an elevated temperature, but under no very great pressure, when it cools down to the ordinary heat of the atmosphere, the fluid in these cavities contracts, so as to leave a vacuity, the relative size of which must of course depend upon the height of the original temperature.

"Such fluid-cavities are easily seen with a suitable magnifying-power, and the relative size of the vacuity can be measured by means of a micrometer.

"Applying these principles to the study of natural crystals, it is found that, whilst some indicate a temperature not materially higher than that of the atmosphere, many must have been formed at a heat rising upwards to that of dull redness, which is specially the case with igneous and metamorphic rocks. If, however, the crystals were formed under very great pressure, of course the above conclusions would be invalidated, and the calculated temperature would be too low; but if we could form some approximation to the actual temperature, we could deduce from the relative size of the vacuities in the fluid-cavities the pressure under which the crystals were generated.

"When the fluid-cavities in granite rocks are studied in this manner, they lead us to conclude that such rocks were formed under very great pressure, varying in different cases, but of such a magnitude as clearly points to their deep-seated plutonic origin."

Those interested in this inquiry may consult the paper by Sir David Brewster in the Transactions of the Royal Society of Edinburgh, Mr. Sorby's paper in the Report of the British Association Meeting at Leeds, also his paper in the Quarterly Journal of the Geological Society, vol. xiv., and a paper entitled "The Microscope in Geology," in the 'Popular Science Review' for October 1867, by David Forbes, F.R.S.

Note.—My thanks are due to E. W. Binney, Esq., F.R.S., Joseph Manchester, Esq., and J. W. Botsford, Esq., for the loan of crystals and precious stones containing fluid-cavities, some of which are described in this paper.

XII. *On Observations of Atmospheric Ozone.*

By Joseph Baxendell, F.R.A.S.

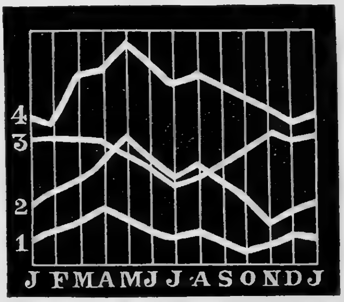
Read October 20th, 1868.

The remarkable nature of the conclusions arrived at by Mr. Mackereth, F.R.A.S., in his paper "On Ozone and its Probable Connexion with Solar Radiation," read at the meeting of the Physical and Mathematical Section held on the 21st of April last, has led me to examine and discuss the series of ozone observations made at the Radcliffe Observatory, Oxford, during the years 1856–65, as well as several other series which have fallen under my notice. It will be seen, from the results which I now proceed to give, that Mr. Mackereth's conclusions are not borne out, but that nevertheless the subject of atmospheric ozone is one of much interest, and merits more attention on the part of meteorologists and physicists than it has yet received.

Observations at the Radcliffe Observatory, Oxford.

At the end of the volume of 'Radcliffe Observations' for 1864 a table is given showing the "Mean Monthly Quantities of Ozone, as determined by Schönbein's Ozonometer in a period of Ten Years, commencing with 1856." The observations had been made twice daily, at 10h P.M. and 10h A.M.; and the mean monthly values are as follows:—

	10h P.M.	10h A.M.	10h P.M. and 10h A.M.
January	2·5	2·8	2·65
February	2·9	3·7	3·30
March	3·6	3·9	3·75
April	4·3	4·5	4·40
May	5·3	5·8	5·55
June	4·4	4·8	4·60
July	3·6	4·1	3·85
August	4·0	4·5	4·25
September	3·6	3·9	3·75
October	2·8	3·2	3·00
November	1·9	2·0	1·95
December	2·1	2·5	2·30



The curve No. 2 in the annexed diagram is a projection of the numbers in the last column, while curve No. 1 is a projection of Mr. Mackereth's results.

It will be seen that the principal maximum and principal minimum both occur one month later at Oxford than at Eccles, and that a secondary maximum occurs at both stations in the month of August.

It is stated at the foot of the table in the 'Radcliffe Observations' that the following facts are deducible from the above numbers:—

1. That the greatest quantity of ozone generally occurs in the spring, and the least quantity in October and November.

2. That the absolutely greatest quantity occurs in May.

3. That there is in every month less ozone in the evening than in the morning.

During the whole period of ten years there is only one instance in the month of May of a complete absence of ozone during 24 hours; and this instance occurred in 1859.

Taking the means of the monthly values given in the table for each year, we have the following annual values:—

	Annual means at 10h P.M.	Annual means at 10h A.M.	General annual means.
1856	4·92	3·50	4·21
1857	3·19	2·46	2·82
1858	2·58	3·61	3·09
1859	2·06	2·91	2·48
1860	1·78	2·65	2·21
1861	2·44	3·71	3·07
1862	3·77	4·98	4·37
1863	4·62	5·50	5·06
1864	4·22	4·23	4·22
1865	4·25	4·36	4·30

According to Mr. Mackereth's results the annual amounts of ozone are greatest when the intensity of solar radiation is greatest, or when solar spots are most numerous; but a glance at the last of the above columns of numbers shows that at Oxford this relation does not hold good: on the contrary, the annual amounts of ozone are least when solar spots are most frequent, or the intensity of solar radiation greatest; and this is especially the case with the amounts of ozone observed during the day. Thus the mean annual amount of ozone for both day and night during the years 1858–62, when the number of solar spots was above the average for the ten years, was 3·04, and for the remaining five years 4·12; whilst the mean annual values of the day-amounts alone for these periods were respectively 2·52 and 4·24. The corresponding mean annual values of the night-amounts were 3·57 and 4·01.

Comparing the mean day- and night-amounts for the years 1858–62 with those for the years 1856–7 and 1863–5, we have the following results:—

	Mean annual value of day-amounts of ozone.	Mean annual value of night-amounts of ozone.	Ratio of day- to night-amount.
Years 1858–62 ...	2·52	3·57	0·70
Years 1856–57 and 1863–65	4·24	4·01	1·05
Differences.........	1·72	0·44	

It appears therefore that in years of maximum solar radiation and sun-spot frequency the day-amount of ozone is only seven-tenths that of the night, while in years of minimum it is slightly in excess of that of the night—and also that while the difference between the night-amounts of the two periods is only 0·44, that between the day amounts is 1·72, or nearly four times as much. The effect therefore of an increase in the frequency of solar spots appears to be to reduce the amount of atmospheric ozone to a much greater extent during the day than during the night.

Admitting that a connexion exists between the frequency of solar spots and the amount of ozone in the earth's atmosphere, it will be seen that the amount of ozone was exceptionally low in 1857 and exceptionally high in 1863. The anomalous action which produced the unusually high rate for 1863 would no doubt extend its influence to Eccles and produce an exceptionally high amount of ozone at that station, which, as Mr. Mackereth's series commenced in 1863, has evidently misled him as to the true nature of the connexion between solar-spot frequency and the amount of atmospheric ozone.

Grouping the Eccles and Oxford monthly means according to the seasons, we have the following results:—

	Eccles.	Oxford.
Winter	1·28	2·75
Spring	2·13	4·56
Summer	1·37	4·23
Autumn	0·89	2·90

Both at Eccles and Oxford the maximum occurs in the spring; but the minimum occurs at Eccles in the autumn, and at Oxford in the early part of winter.

The Rev. Robert Main, F.R.S., Director of the Radcliffe Observatory, Oxford, has kindly favoured me with a copy of the unpublished results of his ozone observations for the years 1866 and 1867; and although he believes that the results for 1866 cannot be relied on, owing to test-papers of an unsatisfactory quality having been used during a considerable portion of that year, yet the means for the two years, which are 4·57 for 1866 and 4·12 for 1867, bear out very fairly the conclusions I have drawn from the results of the previous ten years' observations, the amounts being above the average of the entire series, and corresponding to a sun-spot frequency considerably below the average.

Observations at the Lisbon Observatory.

The first volume of the Annals of the Lisbon Observatory contains a table of the monthly and annual results of a series of ozone observations made during the years 1856–63. The mean monthly amounts of ozone were as follows:—

	Night.	Day.	Mean.
January	5·8	5·1	5·4
February	5·8	5·2	5·5
March	5·8	5·0	5·4
April	5·8	4·8	5·3
May	5·3	4·3	4·8
June	4·7	3·8	4·3
July	3·8	3·0	3·4
August	4·4	3·3	3·8
September	4·7	3·8	4·3
October	5·3	4·5	4·9
November	6·0	5·3	5·6
December	5·7	5·0	5·3
Year	5·3	4·4	4·8

A projection of the numbers in the last column is shown in curve No. 3; and it will at once be seen that this curve differs altogether in character from the curves for Eccles and Oxford, the maximum occurring in November and the minimum in July, thus showing clearly that the monthly amounts of ozone have no dependence upon the monthly amounts of solar radiation.

The mean amounts of ozone at Lisbon in the different seasons are:—

Winter	5·40
Spring	5·16
Summer	3·83
Autumn	4·93

The maximum therefore is in the winter, and the minimum in the summer quarter. I may also remark that while at Eccles and Oxford the maximum rises higher above the mean of the year than the minimum falls below it, at Lisbon the reverse of this takes place, the difference between the maximum and mean values being less than between the minimum and mean.

Taking now the annual amounts, we have:—

1856	4·4	1860	5·4
1857	5·2	1861	4·7
1858	4·7	1862	4·6
1859	5·1	1863	4·6

Dividing these numbers into two groups according to solar-spot frequency, and taking the means, we have:—

Mean annual amount of the four years 1858–61, when the number of solar spots was above the average ……………	4·97
Mean annual amount of the years 1856–57 and 1862–63, when the number of solar spots was below the average …	4·70
	0·27

The slight difference between these two numbers shows that the amount of ozone at Lisbon is but slightly affected by the frequency of solar spots. The mean value, however, for years of maximum solar-spot frequency is slightly greater than that for minimum years; but we have seen that at Oxford the value in the former case was considerably less than in the latter. This circumstance, taken in connexion with the difference in the epochs of maximum and minimum of the annual curves of the two stations, has suggested to my mind the idea of a belt of ozonized air in the middle latitudes of our hemisphere, which has a motion to the northward during the spring months of the year, and a return movement to the southward during the autumn months, and that its mean position for the year varies with the increase or decrease of solar-spot frequency, or with the increase or decrease of the disturbances in the earth's magnetic elements.

Observations at Hobart Town, Tasmania.

A valuable series of ozone observations was made at Hobart town, Tasmania, by Mr. Francis Abbott, F.R.A.S., during the years 1858–65, and part of the year 1857, from which he deduced the following mean monthly values of the amount of atmospheric ozone at that station, the numbers being the sums instead of the means of the day- and night-values:—

January	6·89
February	7·01
March	7·01
April	6·99
May	6·80
June	6·50
July	7·09
August	7·52
September	7·96
October	7·92
November	7·56
December	7·19
	7·18

Grouped according to the seasons we have—

Winter	7·02
Spring	7·80
Summer	7·02
Autumn	6·92

The maximum occurs therefore in the spring, and the minimum in the autumn quarter, thus agreeing very fairly with the Eccles and Oxford results in the northern hemisphere.

The annual amounts derived from Mr. Abbott's observations are—

1858	7·04	1862	6·96
1859	6·70	1863	7·77
1860	6·85	1864	7·67
1861	6·82	1865	8·17
	6·85		7·64

During the first four years the frequency of solar spots was above the average, and the mean amount of ozone was 6·85; and during the last four years sun-spot frequency was below the average, and the mean amount of ozone was 7·64, or 0·79 greater than in the first period, thus agreeing precisely with what we have found for Oxford, where the amount of ozone is least in years of greatest solar-spot frequency.

Observations at the Melbourne Observatory.

During the years 1858–60, and again during 1863–65, ozone observations were made at the Melbourne Observatory by Mr. Robert J. Ellery, the Government Astronomer. Grouping the monthly means according to the seasons, we have—

Winter	5·25
Spring	4·16
Summer	3·60
Autumn	4·14

Here the maximum occurs in the winter and the minimum in the summer quarter, the character of the changes thus agreeing almost exactly with that shown by the Lisbon observations.

The annual values are—

1858	3·6	1863	4·5
1859	4·4	1864	4·6
1860	4·5	1865	5·1
	4·17		4·73

The mean for the three years of maximum solar-spot frequency is 4·17, and that for the three years of minimum is 4·73, the difference being 0·56, thus showing an action precisely similar to that shown at Hobart Town and at Oxford.

Ozone observations were made at the Sydney Observatory during the four years 1860–63, but I have not yet been able to meet with the details. The annual amounts, however, were—

1860	6·15
1861	5·20
1862	4·23
1863	4·00

Here we have a gradual diminution of the amount of ozone corresponding to a gradual decrease in the frequency of solar spots, thus exhibiting a character similar to that presented by the Lisbon observations, and tending to show that there exists in the southern hemisphere a belt or zone of ozonized air similar to that which I have supposed to exist in the northern hemisphere, and subject to similar movements.

The published 'Greenwich Observations' afford a series of only three years of daily observations of ozone; and the results differ widely from those obtained at Oxford and Eccles, owing, I presume, to a less sensitive kind of test-paper having been used, and probably also to a difference in the method of exposure of the papers. The annual curve shows a maximum in September, and another at the end of April and beginning of May, and a principal minimum in November. The means for the seasons are,—

Winter	0·91
Spring	0·94
Summer	0·94
Autumn	0·88

The numbers for winter, spring, and summer are much less than those obtained at Eccles, where I believe the observations were made with Moffat's test-papers—and very strikingly less than those obtained at Oxford, where Schönbein's papers were used.

From the results of observations which I have made during the last few months with the new papers prepared by Mr. Mackereth, it seems probable that the amount of ozone near the earth's surface is dependent upon the height at which clouds are formed in the atmosphere. In order to test this view I have examined the results of Mr. Crosthwaite's valuable series of observations of the heights of the clouds at Keswick, as given at page 40 of Dr. Dalton's 'Meteorological Observations and Essays,' 2nd edition, the only reliable series of the kind of which I have at present any knowledge; and I find that out of every 100 observations the number of times that the elevation of the clouds exceeded 1000 yards was for each month as follows :—

January	34·6	July	48·5
February	30·4	August	52·0
March	51·8	September	46·6
April	52·8	October	42·6
May	63·1	November	38·3
June	58·5	December	31·1

A projection of these numbers is shown in curve No. 4.

The means for the seasons are—

Winter	32·0
Spring	55·9
Summer	53·0
Autumn	42·5

It appears therefore that the elevation of the clouds is greatest in the spring quarter, when the amount of ozone, according to the Oxford observations, is also greatest, and least in winter, when the amount of ozone is also least. I am not at present aware of the existence of any observations by which it can be ascertained whether this relation holds good in other latitudes.

It may be objected that the conclusions I have drawn from the few series of observations to which I have referred rest on an insufficient basis of facts; and knowing, from long experience in meteorological inquiries, the danger of relying too much on deductions from a limited number of observations, I regard it as very probahle that further investigations may render some modifications of my conclusions necessary. The subject is one in which the meteorologist requires the aid of the chemist. The method now employed of detecting the presence of ozone in the atmosphere, and measuring its result, is very imperfect; and the causes of its frequent sudden development and almost equally rapid disappearance are, at present, involved in mystery; they are, however, as I have shown, evidently connected with other meteorological phenomena, upon which they may throw much light; and their importance in a sanitary point of view cannot be doubted. Although, therefore, my investigation has failed to establish the validity of Mr. Mackereth's conclusions, it will, I trust, serve to show the desirability of giving increased attention to observations of atmospheric ozone, and of attempting to determine with greater certainty and exactness than has yet been done, the nature of the changes to which this important constituent of the earth's atmosphere is subject, and of their relation to other atmospherical phenomena.

XIII. *On Measurements of the Chemical Intensity of Total Daylight made during the recent Total Eclipse of the Sun, by* Lieut. J. HERSCHEL, R.E. By Prof. H. E. ROSCOE, F.R.S.

Read December 15th, 1868.

THE following communication contains the results of observations upon the varying intensity of the chemical action of total daylight during the total eclipse of the sun on August 18th last, made at Jamkhandi (75° 20′ E.; 16° 30′ N.), in India, by Lieut. John Herschel, R.E. The method employed was that described by me in the Bakerian Lecture for 1865 (Phil. Trans., 1865, p. 605), and consists of the comparison of tints effected by the total daylight acting for a given time upon uniformly sensitive chloride-of-silver paper. The weather at Jamkhandi during the eclipse was most unfavourable for such observations. Lieut. Herschel writes that "in truth nothing could have been more disturbing than the constant and rapid hurrying over of cloud of all degrees of darkness at low altitudes combined with light fleecy ones, nearly stationary, at a greater height." At no time during the eclipse did the estimated amount of cloud fall below 4, the average being about 7; and frequently the sun was obscured by heavy cloud, though at intervals it shone clear and bright.

By arranging the 42 different observations of the intensity of daylight into 9 groups, we eliminate the variations among the separate consecutive observations arising from the unfortunately variable state of the weather, and obtain the following series of regularly diminishing and increasing numbers, giving the chemical intensity (I) of total daylight occurring during the eclipse for the apparent solar times indicated :—

Time.	I.	Time.	I.
$7^h 57^m$	0·590	$9^h 0^m$	0·005
8 8	0·320	9 15	0.102
8 16	0·270	9 25	0·140
8 28	0·110	9 31	0·125
8 45	0·090		

In order to determine how far these values of chemical intensity correspond to the numbers giving the relative areas of the solar disk remaining uneclipsed, it is necessary in the first place to allow for the variation in chemical intensity caused by alteration in the sun's altitude from the time of first contact at $7^h 44^m 6^s$ until the point of last contact at $10^h 21^m 7^s$. On a former occasion (Phil. Trans. 1867, p. 559) I have shown that the relation between the sun's altitude and the chemical intensity is represented by the equation

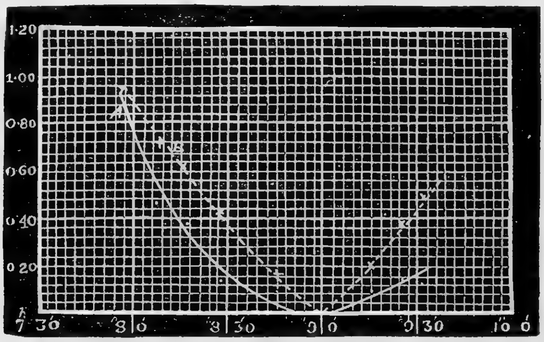
$$CI_a = CI_o + \text{Const.} \times a.$$

Where CI_a signifies the chemical intensity at any altitude (a) in circular measure, CI_o the chemical intensity at the altitude 0°, and Const. is a number to be calculated from the observations. In default of any special series of experiments made at Jamkhandi for the purpose of ascertaining the value of the constant at that place, I take the determinations made at Para (*loc. cit.*) (in lat. 1° 25′ S.) as probably giving a fair approximation to the truth;

for I have shown in the paper above referred to that even for places so differently situated as Heidelberg and Para no very great difference exists in the value of the constant.

In the following Table the relation of the chemical intensity to the sun's altitude is seen in column IV. when the intensity at the highest altitude, 53° 15′, is taken as the unit. Column V. contains the values of I corrected for variation of altitude; and column VI. contains the relative areas of the sun's disk remaining uncovered at the corresponding apparent solar time found in column I., the area of the solar disk before the commencement of the eclipse being taken as the unit.

Apparent solar time. I.	(I.) II.	Altitude. III.	IV.	V.	VI.
$7^h 57^m$	0·590	30° 30′	0·622	0·948	0.94
8 8	0·320	33 30	0·671	0·476	0·71
8 16	0·270	35 0	0·705	0·383	0·62
8 28	0·110	38 30	0·753	0·146	0·43
8 45	0·090	42 30	0·820	0·100	0·17
9 0	0·005	46 0	0·885	0·005	0·00
9 15	0·102	49 45	0·933	0·109	0·21
9 25	0·140	52 0	0·966	0·145	0·37
9 31	0·125	53 15	1·000	0·125	0·49



The curve A represents the variations of chemical intensity corrected for altitude, and the curve B shows the increase and diminution of obscuration of the sun's disk, the abscissæ representing the time, and the ordinates the corresponding chemical intensity and area of exposed disk.

The observations unfortunately could not be carried on, owing to cloud, beyond 9ʰ 23ᵐ, at which time the sun's disk was still more than half eclipsed, so that the rise of the curve beyond this point cannot be given.

If we compare the curve of chemical intensity (which is nearly symmetrical on both sides of the totality) with the curve of solar obscuration, it will be seen that the rate of diminution of the chemical intensity of total daylight during the first portion of the eclipse up to the point at which the disk is half obscured is greater than corresponds to the area of darkened solar disk, whilst from this point up to totality the rate of diminution of chemical action is much less than that of the exposed portion of the disk.

Determinations were also made from time to time with the arrangement for shading off all the direct sunlight from the sensitive paper; and these, combined with alternate observations of the intensity of total daylight, give the separate intensities of diffused and direct sunlight. Owing to the very rapid changes which occurred constantly in the condition of the solar disk from passing clouds, only a few isolated sets of these double observations, made when the disk was free from cloud, are of value.

These observations, however, are sufficient to indicate that the rate of diminution and increase of intensity of the chemically active rays in the direct sunlight is proportional to the changes of area in the exposed portion of the sun's surface.

Chemical Intensities.

Apparent solar time.	Total.	Diffuse.	Direct.	Fraction of disk visible.
$7^h 57^m$	0·948	0·788	0·160	0·94
8 29	0·183	0·146	0·037	0·43
8 45	0·113	0·090	0·023	0·17
9 0	0·000	0·000	0·000	0·00
9 16	0·152	0·124	0·028	0·21

From these numbers it appears that the differences observed between the curves of total chemical intensity and area of solar disk must be due to variations in the intensity of the diffused light; and the rapid diminution observed during the first part of the eclipse may be explained by the dark body of the moon cutting off the light from the highly luminous portion of sky lying on one side of the sun's disk.

I have to thank Mr. Baxendell for his kindness in furnishing me with the astronomical data required in the above calculation.

XIV. *On War Rockets.* By James Nasmyth, C.E., Corresponding Member of the Society.

Read December 29th, 1868.

Under the impression that the improvement suggested in the following remarks on the above-named subject may lead to important results when carried into effect, I have ventured to solicit the favour of the attention of the Members of the Manchester Literary and Philosophical Society to the subject, in the hope that it may interest them, and by their kind favour be recorded in their Transactions, and so place the suggested improvements in question at the service of the public.

The valuable properties possessed by rockets as implements of warfare are so great that, could precision of flight be added, they would rise to a position of the highest importance as destructive agents.

The comparative lightness and portability of rockets, and the fact of their combining gun and charge, shot or shell, all in one and the same projectile, together with their alarm-producing and highly destructive properties, have (notwithstanding their wildness or uncertainty of flight) caused them to be employed in warfare, in many instances with most effective and important results.

It is with the object of giving to such rockets all the advantages of rifle action, and so securing precision in their flight, that I desire to suggest means for effecting that important object by an agency that appears to me to be at once simple and effective.

Before proceeding to describe the means whereby I propose to effect the object in question, I would premise that what constitutes the true rifle principle in a projectile is not only the condition of axial rotation in the line of flight, but, above all, the condition that the projectile possesses the highest degree of axial rotation *from the first instant* of its forward course.

Unless this latter condition be present, no subsequent axial rotation, be it ever so great, can correct a bias or unprecise flight *after* the flight has commenced.

The grand desideratum to be sought for is, that at the instant the rocket commences its flight it shall possess the highest degree of axial rotation. With such conditions present, we shall confer on our rocket all the properties (as regards precision of flight) of the most perfect rifle projectile.

It is difficult by words alone to convey a perfectly clear idea of the mechanical arrangement by which I propose to effect this desirable object. I have therefore accompanied these remarks with an illustrative diagram of the mechanism by means of which I propose to confer on the rocket the requisite degree of high axial rotation, so that, like a true rifle projectile, it shall possess that indispensable condition of precision of flight from and at the very instant it sets out on its course.

Before proceeding to describe the distinctive features of my contrivance for effecting the object in question, it may be as well to allude to the means that have been employed in the endeavour to secure to war rockets rifle action or precision of flight. These consist in placing the rocket in a V-shaped trough, by which the direction and inclination of the rocket is suitably secured previously to commencing its flight, and so far holding the rocket fair in the direction of the object aimed at.

Besides this, an endeavour is made to give the rocket axial rotation during its flight by causing the propulsive gases, while issuing at the rear of the rocket, to rush through skew holes. This latter arrangement does, to a certain extent, give to the rocket axial rotation. But, as axial rotation given by such means does not come into effective operation until the rocket has proceeded a long way on its course, it comes into action *too late* to have any influence in securing precision of flight.

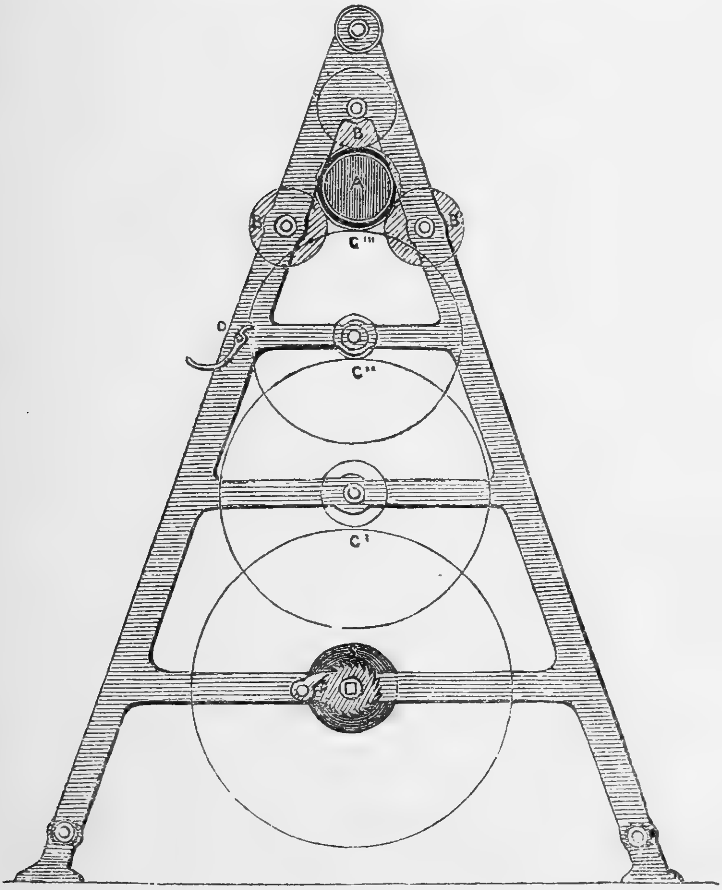
In order, then, to effect our object, I place the rocket inside a tube (into which it slides freely); to this tube, which serves to secure the aim of the rocket, I give, by mechanical means, an axial rotation of some thousands of revolutions per minute, which is transmitted to the rocket then resting within it.

The rocket, while thus revolving on its axis at the high velocity above named, is then fired, and so rushes forth from its guide-tube impressed with all the conditions of a perfect rifle projectile, and, as such, with every condition present that can secure its reaching the object aimed at.

Reference to the diagram which accompanies this paper will enable the reader to obtain, as I trust, a clear idea of the mechanical means and arrangements by which I propose to effect the object in question.

It consists of a suitable iron stand, supporting the rocket and its guide-tube A, the guide-tube resting on loose friction wheels, which, while preserving with the utmost exactness the direction of the axis of the guide-tube and rocket, permits the guide-tube and rocket to revolve on its axis with all due facility.

The requisite amount of axial rotation is conveyed to the guide-tube and thence to the rocket resting within it, by means of a powerful clock-spring S, transmitting its rotation to the rocket through the train of wheels C′, C″, C‴.



Previously to firing the rocket the wheel C″ is locked by the catch or trigger D; the spring S is then wound up and the aim and elevation of the rocket adjusted. The match of the rocket is then lighted; and in order to secure energetic combustion of the rocket ere it is allowed to rush forth on its course, the rocket is held in check by three slight springs within the guide-tube at the rear of the rocket, by means of which it is not permitted to rush forth until the proper energy of discharge of propulsive gases has been acquired. As soon as this is the case, the rocket frees itself and then rushes forth impressed with and possessing every condition of a true rifle projectile, combined with all those important properties which rockets possess as implements of warfare.

XV. *On a Diurnal Inequality in the Direction and Velocity of the Wind, apparently Connected with the Daily Changes of Magnetic Declination.* By JOSEPH BAXENDELL, F.R.A.S.

Read before the Physical and Mathematical Section, January 5th, 1869.

WHILE engaged lately in a discussion of the observations of rainfall made at the stations belonging to the Manchester Corporation Water-works, a question arose which rendered it desirable to ascertain, if possible, whether any law of periodicity existed in the daily changes of direction and force of the wind. It has long been known that the force or velocity of the wind is generally greater during the day than during the night; but I am not aware that this diurnal variation of force has been discussed in connexion with the direction of the wind, or with other meteorological phenomena. On proceeding to collect materials for such a discussion, I found that the only published results of observations of the wind that were available for this purpose were those given in the annual volumes of the Radcliffe Observatory, Oxford, which are derived from bi-hourly observations with a self-registering anemograph. This instrument was first mounted in July 1856, at a height of 22 feet from the ground, but was removed, in August 1858, to a station at the top of the tower of the Observatory, at an elevation of 110 feet, where it has since remained. The results derived from its indications are given in the annual volumes of the 'Radcliffe Observations,' in three tables, numbered XII., XIII., and XIV. Table XII. shows the "Mean Monthly Directions of the Wind every Two Hours;" Table XIII. the "Mean Bi-horary Velocities of the Wind for each Month, estimated in the Directions given in Table XII;" and Table XIV. the "Actual Mean Monthly Bi-horary Velocities." Up to the end of 1858 the mean monthly directions were determined without regarding the velocities; but in reducing the observations of 1859, the present director, the Rev. Robt. Main, F.R.S., took account of the velocities in determining the mean directions, and this method has been used through the succeeding years; and, as it is undoubtedly more correct than that employed by the late Mr. Johnson, I have omitted from my discussion the results obtained in the years 1856–58. The following Table I. contains the mean bi-horary directions of the wind, extracted from Table XII. of the annual volumes of the 'Radcliffe Observations;' and Table II. contains the corresponding mean velocities in miles, taken from Table XIII.

TABLE I.

	0 h.	2 h.	4 h.	6 h.	8 h.	10 h.	12 h.	14 h.	16 h.	18 h.	20 h.	22 h.
	°	°	°	°	°	°	°	°	°	°	°	°
1859	241	244	237	231	231	226	230	231	225	232	232	235
1860	249	254	252	253	249	250	252	247	247	250	245	257
1861	234	230	227	233	224	228	232	232	232	230	231	240
1862	234	235	234	232	227	231	228	229	229	235	229	233
1863	225	226	229	222	226	218	218	217	213	214	219	219
1864	206	211	208	200	189	187	189	196	202	198	206	201
1865	198	204	195	193	187	188	190	196	194	200	202	198

TABLE II.

	0 h.–2 h.	2 h.–4 h.	4 h.–6 h.	6 h.–8 h.	8 h.–10 h.	10 h.–12 h.	12 h.–14 h.	14 h.–16 h.	16 h.–18 h.	18 h.–20 h.	20 h.–22 h.	22 h.–0 h.	
1859	11·4	10·4	9·4	8·4	9·1	8·5	8·2	7·5	7·8	7·4	7·5	9·0	
1860	18·3	18·6	17·6	17·7	15·9	17·1	15·0	14·9	14·8	15·1	17·6	19·2	16·8
1861	16·1	17·3	14·9	11·2	11·7	9·1	11·4	11·0	11·9	12·3	12·8	16·6	13·0
1862	18·7	17·2	16·5	14·1	13·9	14·2	14·1	14·2	14·2	14·1	16·2	17·7	15·4
1863	21·2	20·2	18·5	16·8	17·5	16·9	17·6	17·0	16·3	16·3	18·5	20·9	18·1
1864	13·9	14·3	12·9	10·8	11·0	11·7	11·2	10·5	9·5	9·6	11·3	12·6	11·6
1865	14·1	13·7	11·4	10·6	10·7	11·0	10·5	9·9	10·0	10·8	12·9	13·9	11·6

Resolving the directions in each of the columns of Table I. into their rectangular co-ordinates A and B, in the direction of the meridian and at right angles to it, by using the corresponding velocities in Table II. and taking the means of the values of A and B, we obtain the mean direction θ for the hour indicated at the head of the column, from the equation

$$\tan\theta=\frac{B}{A}$$

and the corresponding mean velocity v from the equation

$$v=\frac{A}{\cos\theta}.$$

The mean directions and velocities thus obtained are shown in Table III.

TABLE III.

h.	Mean direction. ° ′	Mean bi-horary velocity.	h.	Mean direction. ° ′	Mean bi-horary. velocity.
0	227 41	15·58	12	221 43	11·84
2	229 46	15·35	14	222 29	11·58
4	227 52	13·83	16	221 57	11·55
6	226 0	12·08	18	224 10	11·64
8	221 33	12·02	20	224 5	13·32
10	220 27	11·74	22	227 43	14·78

In Table XII. of the results of the 'Radcliffe Observations' the mean directions are given for the hours 0 h., 2 h., 4 h., &c.; but in Table XIII. the velocities are given for the bi-horary intervals 0 h.–2 h., 2 h.–4h., 4 h.–6 h., &c., or for the hours 1 h. 3 h. 5 h., &c. It is evident, therefore, that the velocities in Table III. do not strictly correspond to the hours and directions opposite to which they are placed; and although the errors which would arise from this cause might not materially affect the ultimate results, I have thought it desirable to reduce the velocities to the times for which the directions are given. This I have done by simply taking for each hour the mean of the velocities for the two preceding and two following hours. The corrected results are given in Table IV.

TABLE IV.

h.	Mean direction. °	′	Mean bi-horary velocity.	h.	Mean direction. °	′	Mean bi-horary velocity.
0	227	41	15·18	12	221	43	11·79
2	229	46	15·47	14	222	29	11·71
4	227	52	14·59	16	221	57	11·56
6	226	0	12·95	18	224	10	11·60
8	221	33	12·05	20	224	5	12·48
10	220	27	11·88	22	227	43	14·05

On looking over the numbers in this Table it will be seen that from about 9 h. to 19 h., or from 9 P.M. to 7 A.M., the velocity of the wind is nearly constant; it afterwards rapidly increases, attains a maximum a little before two o'clock, and then returns rapidly to the night's rate. The variations in the direction of the wind also follow those of the velocity pretty closely. The total movement of the air between 9 P.M. and 7 A.M. is 58·52 miles in a mean direction from S. 42° 8′ W.; and between 7 A.M. and 9 P.M. the total movement is 96·67 miles from S. 46° 36′ W. It appears, therefore, that at about 7 A.M. a force which has been almost, if not quite, inoperative during the previous ten hours begins to act on the wind from a westerly direction, and, gradually but rapidly increasing in intensity, produces its maximum effect between 1 and 2 P.M.; it then gradually diminishes, and finally ceases to act about

9 p.m. The intensity of this force as measured by the changes which it produces in the direction and velocity of the wind is at its mean value during its increase at about 9 h. 32 m. A.M., and during its decrease at about 5 h. 12 m. P.M. Now these times correspond very nearly with those at which the magnetic declination is at the mean for the day as determined from the Greenwich magnetic observations; and the idea is therefore at once suggested that the daily variations in the direction and velocity of the wind are probably connected in some way with the diurnal changes of magnetic declination. It will be seen that the following comparison of the two classes of phenomena strongly supports this view.

	h. m.		h. m.
The force which acts upon the wind during the day begins to operate about	7 0 A.M.	Mag. dec. at its principal max. east about	7 48 A.M.
Is at its mean value	9 32 ,,	Mean position	9 42 ,,
,, max. ,,	1 26 P.M.	Max. west	1 0 P.M.
,, mean ,,	5 12 ,,	Mean position	5 27 ,,
Ceases to operate or becomes almost inappreciable	9 0 ,,	Secondary max. east	9 54 ,,

With one exception, the epochs of the wind force occur somewhat earlier than those of the magnetic variations; but, considering the nature of the wind observations, and that they only extend over a period of seven years, and have not been reduced with special reference to this inquiry, these differences may probably be in great measure due to uncompensated errors in the results derived from the indications of the anemograph.

We have seen that the movement of the wind from 7 A.M. to 9 P.M. was 96·67 miles in a direction from S. 46° 36′ W. to N. 46° 36′ E.; but if the direction and velocity which prevailed from 9 P.M. to 7 A.M. had continued unchanged from 7 A.M. to 9 P.M., the movement during the latter interval would have been only 81·93 miles in a direction from S. 42° 8′ W. From these distances and the difference of the angles of direction we find that the effect of the additional force which acts upon the wind from 7 A.M. to 9 P.M. was to impel the air through a distance of 16·3 miles in a direction from S. 69° 36′ W. to N. 69° 36′ E. Now this direction is almost exactly that of a perpendicular to the magnetic meridian. Referring to the Greenwich Magnetical Observations, we find that the magnetic declination in the years 1859–1865 was as follows:—

	°	′	
1859	21	23·5	W.
1860	21	14·3	„
1861	21	5·4	„
1862	20	52·0	„
1863	20	46·0	„
1864	20	38·0	„
1865	20	35·0	„

The mean for the seven years is 20° 56′·3 W., a perpendicular to which is S. 69° 3′·7 W. to N. 69° 3′·7 E., thus differing only 32′·3 from the above determination This close relation to the direction of the magnetic meridian, taken in connexion with the points of agreement between the phases of the two classes of phenomena already adverted to, seems to establish beyond doubt the fact that the force which produces the diurnal inequality in the direction and velocity of the wind, and the earth's magnetic force, are in some way intimately connected with and dependent upon each other.

It has, I believe, been generally supposed that the mean position of the magnetic needle for the 24 hours of each day indicates the direction of the true magnetic meridian, or is that in which the needle is subject to the least amount of disturbing force, and that its diurnal oscillations are due to two forces acting alternately in opposite directions—or to one force, the acting centre of which changes its position so that during part of the 24 hours it is on one side of the magnetic meridian, and during the other part on the opposite side; but the results of this discussion of the Oxford Anemograph Observations seem to show that the greatest easterly deflection of the needle should be taken as the direction of the true magnetic meridian—and that the daily oscillations are due to one disturbing force only, which, when in operation, acts always in the same direction.

Table V.

	h. h. 0—2	h. h. 2—4	h. h. 4—6.	h. h. 6—8	h. h. 8—10	h. h. 10—12	h. h. 12—14	h. h. 14—16	h. h. 16—18	h. h. 18—20	h. h. 20—22	h. h. 22—0	Means.
1859	25·18	24·52	21·93	19·88	18·83	18·23	18·23	18·19	17·81	19·13	21·68	26·19	20·82
1860	25·53	25·02	23·04	21·18	20·03	19·32	18·56	18.49	18·31	19·68	22·64	26·24	21·50
1861	23·76	23·10	21·18	18·58	18·00	17·39	17·55	17·13	17·51	18·06	20·02	23·34	19·63
1862	24·83	23·88	21·60	19·75	18·93	18·71	18·32	18·21	18·28	19·48	22·70	24·79	20·80
1863	25·01	24·75	22·63	20·04	18·99	18·54	19·06	18·33	18·45	19·26	21·77	25·02	20·99
1864	23·49	23·89	21·58	19·15	18·03	17·70	17·21	16·65	16·39	17·18	20·32	22·88	19·54
1865	22·54	21·99	20·07	17·77	16·74	16·31	16·48	16·26	16·39	17·16	19·92	22·12	18·65
Means.	24·33	23·88	21·72	19·48	18·50	18·03	17·91	17·61	17·59	18·56	21·29	24·36	20·27
Differences	0·45	2·16	2·24	0·98	0·47	0·12	0·30	0·02	—0·97	2·73	3·07	0·03	

Table V. shows the actual mean annual bi-horary velocities of the wind for the seven years 1859–65; and it will be seen from the differences between the mean values at the foot of the Table that the maximum velocity occurs at about 0 h. 45 m., the minimum at about 17 h. 0 m., and that during the increase of velocity the rate of change was greatest at about 21 h. 30 m., and during the decrease at about 5 h. 30 m. Here we have the maximum occurring somewhat earlier than when the direction of the wind is taken into account, thus bringing all the phases of the wind changes somewhat in advance, in point of time, of those of the magnetic variations, and indicating therefore that the magnetic disturbances are due to electrical currents generated or modified by changes and disturbances of the atmosphere.

Since writing the above I have been kindly favoured by Mr. Main with a copy of the unpublished results of the Anemograph observations for 1866. These I have combined with the results of the previous seven years, and have obtained the following mean bi-horary values of the direction and movement of the wind for the entire period of eight years:—

h.	Mean direction. °	′	Mean bi-horary velocity.
0	224	11	15·43
2	225	19	15·73
4	223	10	14·89
6	220	20	13·32
8	216	36	12·41
10	214	42	12·24
12	216	2	12·12
14	216	47	11·96
16	216	13	11·82
18	218	56	11·87
20	218	53	12·71
22	222	26	14·25

The mean direction was S. 40° W., and mean daily movement 159 miles. The direction of the disturbing force was from S. 69° 3′ W.; and it had the effect of giving to the air a mean daily movement in that direction of 16·86 miles. The mean magnetic declination for 1866 was 20° 28′ W., and the mean for the eight years 1859–66 was 20° 52′·8 W. A line perpendicular to this is S. 69° 7′·2 W. to N. 69° 7′·2 E. Thus the mean direction in which the disturbing force acted during the eight years, differed only 4′·2 from that of a perpendicular to the magnetic meridian. The observations for 1866, therefore, fully bear out the conclusions to which I had been led by the discussion of those of the preceding seven years.

It will be evident that a force which moves the atmosphere daily through an average distance of 16 or 17 miles, in a direction differing considerably from the mean direction of the wind, is an important element to be taken into account in framing a scientific system of forecasting the state of the weather.

XVI. *Note on the Organs of Fructification and Foliage of* Calamodendron commune (?). By E. W. Binney, F.R.S., F.G.S., &c.

Read December 29th, 1868.

In my paper on *Calamodendron,* published in vol. xxi. of the 'Transactions of the Palæontographical Society,' p. 27, is figured and described a plant, with organs of fructification attached to it, from the lower Brooksbottom seam of coal, near Ewood Bridge, in the county of Lancaster. The fossil consists of a stout stem, having traces of longitudinal ribs and furrows, and seven joints at which knots appear. From these last-named parts, on each side of the stem, are seen to proceed seven cones or spikes, all about half an inch in length, springing outwards in a nearly horizontal direction in the specimen. These cones do not show externally any trace of a central axis, but are composed of crown-shaped masses of sporangia contained in receptacles arranged around an axis. Eight or nine of these receptacles can be seen in one cone. Unfortunately, the specimen being in soft shale, no evidence can be obtained of its internal structure, so as to ascertain if the sporangia contained any spores. If this is not the same plant as Professor Goeppert's *Aphyllostachys jugleriana,* it is closely allied to it. The fruit-stalk, nodes, and knots, as well as the form and dimension of the cones of the two specimens, are so much alike that is hard to distinguish one from the other. The learned professor was not certain as to what formation the fossil came from; but, from its similarity to the Ewood-Bridge specimen, there can scarcely be any doubt as to its being of Carboniferous age.

My friend Mr. John Aitken, of Bacup, furnished me with the specimen. At the time the monograph was published, my opinion was that the only point in which the specimen differed from Goeppert's was that the cones in it only possessed eight or nine receptacles, whilst the Professor's had fifteen or sixteen. The resemblance of the specimen to Dr. Ludwig's larger one, figured and described by that author in Dunker and von Meyer's 'Palæontographica,' vol. x. 1861 to 1863, was also noticed, and its difference in the number of receptacles for containing sporangia pointed out.

Since the publication of the monograph, Captain Aitken, of Irwell Vale, and Mr. John Aitken have been so good as to conduct me to the place where the specimens were found, and I have collected myself far more perfect and complete specimens than those which had previously come under my observation.

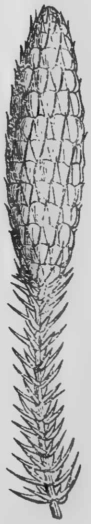
In both Goeppert and Ludwig's specimens the cones or spikes, like the leaves, were arranged in whorls at each node of the fruit-stalk. The same arrangement was observed by me in the first specimen from Ewood Bridge which came under my observation. Now, although that undoubtedly appears to have been the more common form of attachment of the cones to the fruit-stalk, there is clear evidence that some of these organs occurred at the extremity of the branches, as seen in the fructification of the common *Equisetum*; or it may more probably have been that the two forms of fructification were united in one fruit-stalk and formed a terminal panicle. Up to the present time, however, the two forms have not been found absolutely joined together.

Fig. 1. Plate VI. represents a branchlet (natural size) of the Ewood-Bridge plant, of which Mr. John Aitken has kindly allowed me the loan. From a stem, which is slightly ribbed and furrowed, are seen to proceed, at its nodes, thirteen whorls of verticillate leaves, which take a suberect direction. Each leaf was composed of from six-

teen to twenty sets of verticillate leaflets, and was about nine-tenths of an inch in length. In form and size the leaf resembles the cones of *Calamodendron commune*, whose foliage is unknown, so far as my knowledge extends.

Fig. 2. Plate VI. represents a fruit-stalk found by myself at Ewood Bridge. It consists of a stout rachis, indistinctly ribbed and furrowed, four inches in length, and exhibiting twelve nodes, at which strongly marked knots appear. From these knots the whorls of fruit-cones or spikes proceeded, covered with imbricated scales. The number of receptacles for containing spores varied from fourteen to eighteen in each cone. In the specimen not more than four cones can be seen coming from one node; but if all had been preserved and exposed, probably they were from twelve to sixteen in number. At the base of the cone, in several instances, whorls of delicate leaves (Asterophyllites) are visible. In the highest cone on the left-hand side of the stem they may be more distinctly seen than at the lower portions in the other cones.

The annexed woodcut* is a representation of one of the cones or spikes, covered with imbricated scales, at the terminal part of a branch. It is evidently of the same kind as those previously described as proceeding from the nodes, and is most probably the terminal portion of a panicle of fruit-cones.



The fruit-stalks, cones, and leaves above described are found all compressed together, with scarcely any admixture of the remains of other plants, except the long simple leaves of Asterophyllites, stems of Calamites, and a few leaflets

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* The woodcut is slightly enlarged in length from the original, and nearly double its diameter. The size of the specimen is about that of one of the smaller figured in Plate VI. fig. 2.

of *Neuropteris*. These three different parts of the plant have been a little disturbed by pressure; but any one who saw them *in situ* could scarcely have a doubt of their all three having been connected together and formed one plant before the pressure was applied. The stratum in which they occur is nearly one entire mass of cones and leaves with a few fruit-stalks. Within the space of about a square foot as many as one hundred cones have been met with. These organs of fructification are nearly always of a yellowish brown colour, and have very much the appearance of crude paraffine, similar to what is found in the upper sporangia of *Triplosporites* (*Lepidostrobus*?) *Brownii*, and now generally considered to be microspores. This hydrocarbon*, or one nearly allied to it, probably served to protect the organs of fructification by a waxy coating from too much moisture of the habitat of the aquatic plant, and has also since preserved it for our examination by its indestructible nature. Of all the parts of fossil plants none have been so well preserved as the organs of fructification.

The number of receptacles or cells containing sporangia in the first specimens I met with, as previously stated, varied from eight to nine in number; but in those described in this communication they run from fourteen to eighteen, thus showing that the Ewood-Bridge specimens have a greater resemblance to Ludwig and Goeppert's specimens than had been at first supposed.

In my monograph it was mentioned that the small cones found with the *Calamodendron commune* were the fructification of that plant, so far as the evidence of identity of structure in the stems of the two could prove it.

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* Professor John Morris, F.G.S., I believe, was the first author to notice the nature of this substance, in the capsules of his Coalbrook-Dale fossil, described by him in vol. v. part 3 (new series) of the 'Transactions of the Geological Society of London.'

When we look at the shape and size of the cones of the Ewood-Bridge specimens, it is evident that they resemble the cones showing structure more than any other figured and described with which I am acquainted. The form and size of the leaf represented in Plate VI. fig. 1 also very much resembles the cones of my *Calamodendron commune.*

The cones, whether proceeding from the nodes of the stem in whorls, or at the end of the branches, have at their bases delicate leaves (Asterophyllites). The stem of the branch to which the cone was attached in the last-named specimen is remarkably slight for the size of the cone (as seen in the woodcut), a character which appears common with regard to the organs of fructification of coal-plants at the terminal parts of branches. Of course the axis of the cone is only a prolongation of the stem of the plant. In the Ewood-Bridge specimens, as yet, no evidence has been obtained as to the spores contained in their sporangia to identify them with the cones of *Calamodendron commune*; but, as previously stated, in their external characters the one very much resembles the other, and, although found in different localities, the fossils occupy about the same geological position in the Lancashire coal-field. One thing appears pretty certain, namely that both these small cones are the organs of fructification of Calamites of some kind; and at present my observations have led me to the conclusion that they are most probably the organs of my *Calamodendron commune,* or of a plant nearly allied to it, and having a similar structure. They do not afford us any information as to the anatomy of the plant, as do my specimens possessing structure; but they are useful as showing the nature of the foliage and the connexion of the organs of fructification with the stem of the plant.

Addendum.

Since the above communication was read, and when it was in the press, I have had the pleasure of perusing the first volume of Professor Schimper's magnificent work, 'Traité de Paléontologie végétale ou la flore du monde primitif dans ses rapports avec les formations géologiques et la flore du monde actuel.' The learned author has done me the honour to figure the fructification of my *Calamodendron commune* as *Calamostachys Binneyana,* and alludes to the *Aphyllostachys jugleriana* of Professor Goeppert as resembling my cone in form and size. Dr. Ludwig's specimen he describes as *Calamostachys typica,* and figures a beautiful specimen showing that the fructification was a terminal panicle.

My Ardwick specimens he considers to belong to *Annularia longifolia* rather than to *Asterophyllites longifolius.* This probably is more correct, as these two genera of fossil plants have not been so clearly distinguished in England as they have been on the Continent. My specimens had been examined and pronounced Asterophyllites by two of our most eminent fossil-botanists. The Ardwick and Holywell specimens exhibited no internal structure, and were figured and described for the purpose of showing the leaves, branches, and fruit-stalks of plants allied to *Calamodendron commune,* of which my specimens merely gave the external form and internal structure.

EXPLANATION OF PLATE VI.

(*Calamodendron commune.*)

Fig. 1. Specimen of a branchlet of *Calamodendron commune*? in a bed of coal-shale found above the Lower Brooksbottom seam of coal at Ewood Bridge, Lancashire, from the cabinet of Mr. John Aitken, of Bacup, showing the foliage of the plant. Natural size.

Fig. 2. Specimen of a fruit-stalk, with imbricated cones or spikes, of *Calamodendron commune*? from the same locality as the last-named specimen, found by the author, and now in his cabinet. Natural size.

I am indebted to Mr. J. N. Fitch for the beautiful and truthful delineation of the specimens.

XVII. *On the Permian Strata of East Cheshire.*

By E. W. Binney, F.R.S., F.G.S., &c.

Read November 16th, 1869.

In communications to this Society and printed in its Memoirs, most of the sections of Permian strata in the counties of Lancaster and Cheshire have been given*. These had been found by amateur geologists, who rambled over the country at their leisure; but when the Geological Surveyors came into the district and went over every parish, it was to be expected they would make some discoveries. Accordingly we find, in the Memoir explaining the map of the district lying between Macclesfield and Stockport, Mr. E. Hull, F.R.S., describes what he terms a patch of Permian strata at Torkington in the following words:—"Torkington. A curious little patch of Permian beds occurs at Torkington, near Hazel Grove. The beds are only to be seen in the two brook-courses; and so far as it is possible to make out their relationship to the Coal-measures, they appear to lie in a trough formed in the lowest beds of the middle series—in fact, over the Redacre mine.

"The patch appears to be about one-fourth of a mile in breadth from east to west, and is bounded on both sides by carboniferous grits and shales. If we follow the brook

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* Vols. xii. and xiv., and vols. ii. and iii. Third Series, of the Society's Memoirs.

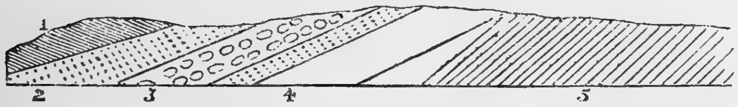
course east from Torkington, we find the following series, in the descending order, all the beds dipping westward :—

" Permian beds ..	1. Red Marls. 2. Soft red breccia, a sandy matrix containing angular fragments of coal-measure grits, shales, and pieces of earthy hæmatite.
" Coal-measures ...	3. Soft purple grits, red shales, with bands of earthy hæmatite.

"Owing to the amount of drift over this part of the country, it is impossible to examine the exact northern and southern limits of this little outlier of Permian beds."

As it is desirable to collect all the sections showing the relation of the Permian strata to the underlying Coal-measures, probably we may be excused for bringing this portion of Mr. Hull's labours before the Society. The section is exposed in a small brook-course called Ockley Brook, flowing between the Green Clough and Blackwood farms on the estate of Mr. Legh, of Lyme, about a mile a little south of east from the Hazel-Grove railway-station.

Ockley-Brook Section.



1. Drift. 2. Red clays (Permian). 3. Breccia. 4. Soft red sandstones and red and variegated shales. 5. Middle Coal-measures.

When I lately examined the section, only about one hundred yards of the strata were exposed. They occurred in the following descending order :—

1.	Brown Till.	
2.	Permian beds...	Red marls.
3.	....	Red breccia, chiefly composed of Coal-measure sandstones, some of them two inches in diameter, for the most part quite angular, with small pieces of quartz, quartzite, and hæmatite, in a paste of sandy clay about ten yards in thickness.

4. Permian beds?.. { Soft and hard sandstones of a bright-red colour, and red and variegated shales about thirty yards in thickness.
5. Middle Coal-measures.

The dip of the strata was to the north-west—the Permian beds at about an angle of 10°, and the Coal-measures about 20°. No. 4 appeared more like Permian than Carboniferous strata, especially the upper portion, some five yards in thickness, which could not be distinguished from the Collyhurst sandstone. Under this bed occurred a band of fine-grained hard sandstone of a bright-red colour; and then came red and variegated shales, which passed into the Middle Coal-measures.

There was no decisive evidence to separate the red sandstones from the underlying shales; so they are classed together as doubtful Permian; but probably we shall not be far wrong in placing the former as Permian and the latter as Carboniferous. In the Geological Survey Map, as well as in the Memoir previously quoted, Mr. Hull has placed these Permian strata as an isolated patch entirely surrounded by Coal-measures. He does not give the evidence on which he comes to this conclusion; but so far as the Ockley-Brook section shows, it does not appear to me probable that Coal-measures succeed the Permian strata on the dip, but, like the Norbury-Brook section to the south, it is more likely they are succeeded by Triassic strata. In this last-named section, described by me many years ago, at Norbury Mill, the breccia had more of a conglomerate character, and only 5 feet 3 inches thickness, being separated from the Middle Coal-measures by 12 feet of red clays. Certainly no such a bed of breccia as that seen in Ockley Brook, to my knowledge, has ever been previously noticed in Cheshire. The Permian sandstone only 5 yards in thickness is but a poor representative of that rock seen so near as at Heaton Mersey, where it was 200 yards thick. In the Ockley-Brook, as well as the Norbury section, there appeared to be no evidence of a fault, but only the covering up of the inferior by the superior strata.

When we take the strike of the Coal-measures seen near Norbury Mill and follow it northwards through Torkington, Offerton, and Brinnington to Beat-Bank Bridge, the strata exposed in Ockley Brook may be a little to the east of the line, but in my opinion not so much as to allow the Coal-measures to come in again on the dip and so make the Ockley-Brook Permian strata an isolated patch. The uppermost bed of red clays is only seen for a few yards before it disappears under the drift. So far as my knowledge extends, no actual borings have been made on the dip to ascertain the nature of the strata; but, from the reddish colour of the drift northwards, it appears probable that Trias beds succeed the Permian clays.

The Permian sandstone of Ockley Brook, of five yards thickness, was not seen at all in the Norbury section; but if it be the same sandstone as that found at Fogbrook and Stockport, estimated by Mr. Hull to be 500 yards thick, it has diminished greatly in the distance of twc miles.

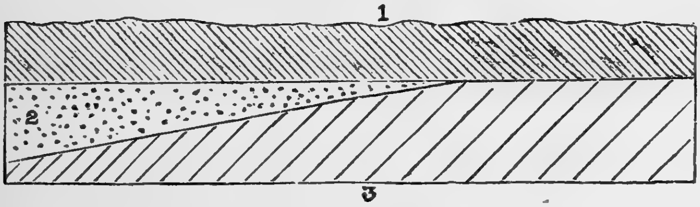
As a great portion of the future supply of coal must, most probably, be looked for under the Permian and Triassic formations in Great Britain, it is very essential that all the circumstances under which the Carboniferous strata disappear beneath those deposits should be carefully ascertained and correctly described. When Permian or Triassic beds are found on the rise of the strata they indicate a fault where the Coal-measures have been thrown down; but when they are met with on the dip of the strata they may indicate a down-throw fault similar to the one last mentioned, or else an overlap of the Permian or Triassic strata simply resting unconformably on the Carboniferous strata. Some authors have described both these as faults. In the beginning of this century certain geologists and practical miners often supposed that when the Coal-measures disappeared under the Permian and Triassic strata, generally then known as "red ground," they were cut off by a fault, and it was useless trying to follow them. "Red-rock faults" were then used in the same sense, whether found on the rise or dip of the strata. Now it is of the utmost importance that these two classes of phenomena should be carefully distinguished; and accordingly most geologists have done so, and termed the former a fault, because the strata are there displaced, and the latter an overlap, because the underlying strata are not displaced, but simply covered up by the superior strata.

Of course when Coal-measures disappear on their dip under superior beds, they can generally be followed, provided there are no faults; and if there are faults, the beds can be found at some depth or other. Owing to these circumstances, Permian and Triassic strata have often been supposed to indicate the presence of coal under them. No doubt they do where profitable seams of coal disappear under them; but when millstone-grit or mountain-limestone in Lancashire and Cheshire disappear on their dip under Permian or Triassic strata, such strata do not give any evidence of the existence of profitable coal seams under them, but only of beds of mountain-limestone or millstone-grit seen near them. This holds good only for the southern or midland districts of England, so far as profitable coal is concerned; for it is well known that in Scotland both these deposits contain valuable seams of coal.

Mr. Hull, in his map of the district, lays down the country from Macclesfield to Stockport, so far as it relates to the Coal-measures, by supposing the latter strata on their dip to be bounded by what he terms the "Red-rock fault," whereas I have in all my papers described the Coal-measures in that district as only overlain by Permian or Triassic strata, there being, in my opinion, no evidence of "a fissure along which relative displacement of the adjoining rock-masses has taken place:" such not having been given, it can only be put forward as hypothetical, and without any evidence of facts to sanction it.

When my papers were published, there was abundant evidence in support of my views in other districts, but not so much near Stockport. However, this has lately been supplied on the line of the so-called "Red-rock fault," in a shaft and bore at Pollitt's Farm, Brinnington, where, under a soft red sandstone without pebbles, most probably Trias (Lower Mottled Sandstone), the Coal-measures containing seams of coal were met with, thus clearly showing that such strata there were not dislocated by a fault, but simply overlain by Trias.

Pollitt's-Farm Section.



1. Drift deposits. 2. Lower Mottled Sandstone. 3. Middle Coal-measures.

At this place, a little to the west of the so-called "Red-rock fault," a shaft was sunk through the drift deposits to the depth of 99 feet; and then a soft red sandstone without pebbles was bored through, 97 feet; and regular Coal-measures were reached at that depth. The bore was continued to a depth of 250 yards from the surface, and nine small beds of coal were met with. These strata most probably belonged to the Middle Coal-measures, and had a westerly dip; but on such points there is no direct evidence. When, however, we take the Beat-Bank-Bridge section on the north and that of Fogbrook to the south, from the information that the shaft and bore give us it is only fair to suppose that the Pollitt's-Farm section, midway between the two, is but a repetition of them. The rivers Tame and Goyt have swept the drift away at Beat-Bank Bridge and Fogbrook, and exposed to view the Coal-measures and overlying red sandstones, whilst at Pollitt's Farm those strata are covered up by the original deposit of drift. All these three sections are on the line of the so-called "Red-rock fault."

On the Fogbrook and Stockport Red Sandstone.

Mr. Hull, in showing the thickness of the Triassic strata in the county of Chester, gives the following Table* :—

Formation.	Subformation.	Thickness.
		feet.
KEUPER	Red Marl	3000
	Lower Keuper Sandstone	450
BUNTER	Upper Mottled Sandstone	700
	Pebble-beds	800
	Lower Motted Sandstone	800
		5750

In the east of Cheshire and Lancashire this author considers that he has found no evidence of his third and lowest division of the Bunter, namely the Lower Mottled Sandstone. Now, as a general rule, where he found his Lower Mottled Sandstone he has seen no Permian rock like the Collyhurst Sandstone or its overlying marls with magnesian-limestone fossils. It was once suggested to him by me that these marls and limestones might in some parts of the west of Cheshire and Lancashire have thinned out and thus allowed the two soft red sandstones to unite and form one bed. This, however, Mr. Hull would not

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* Transactions of the Geological Society of Manchester, vol. ii. p. 31.

on any account allow*. Now, when he comes into the neighbourhood of Manchester and Stockport, he not only quietly gets rid of his Lower Mottled Sandstone, some 800 feet in thickness, by stating that it is not to be found, although there are sandstones exactly resembling it at Ardwick and Clayton, near Manchester, and at Stockport, seen dipping under the Pebble-beds, but he also summarily disposes of the red marls and limestones and makes as much Permian sandstone as he can at the expense of the upper portions of the Permian and the lowest parts of the Trias.

Of course it is well known that soft red sandstones are difficult strata to identify where there are no red marls containing magnesian-limestone fossils above or under them. At Heaton Mersey no doubt these red marls were met with, about 130 feet in thickness, resting upon a thick bed of Collyhurst Sandstone, some five or six hundred feet; and probably the red marls seen at Tradis Brook, near Heaton Norris, may be a thinner representative of them lying on the Permian sandstone; but it must be borne in mind that as yet no fossil organic remains have been found in these marls, although diligent search has been made for them. The Pebble-beds are brought in by the fault at Heaton Norris; and under them dips the soft red sandstone seen on the Denton Road about a mile out of Stockport, at Brinnington, and Fogbrook. This rock Mr. Hull regards as the Collyhurst Sandstone, the Lower Mottled Sandstone and the Permian marls having thinned out or or at least disappeared†.

The passage of the Pebble-beds at Heaton Norris and Stockport into the underlying soft red sandstone is so gradual and regular that no one has been able to find a

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* Transactions of the Manchester Geological Society, vol. ii. p. 33.

† Geology of the Country round Stockport and Macclesfield, p. 35 (Mem. Geological Survey).

break between the two; and any geologist who disposes of two beds of 130 and 800 feet thickness should give some evidence of their disappearance. In their characters no doubt the two red sandstones are much alike; but in the Collyhurst sandstone, after thirty years' search, a pebble of the size of a common pea has never been found to my knowledge; however, in the lowest part of the Fogbrook sandstone are some beds of breccia quite different from any thing ever seen in that part of the rock at Collyhurst. If these beds had been found on the top of the Collyhurst sandstone they would have been in their place; but at the bottom they are rather against than in favour of the rock being Permian. Of course the occurrence of this bed anywhere is by no means conclusive as to the age of the rock; but when taken with the disappearance of two such thick deposits as the Lower Mottled Sandstone and the Permian marls, which have to be got rid of by violent means, it no doubt has its value. Whether the sandstone at Fogbrook is taken to be Trias or Permian (and by mineralogical characters it is no doubt difficult to decide), there is little doubt of its being the same rock as that seen in the Tame at Beat-Bank Bridge and in Capt. Fox's shaft at Pollitt's Farm, Brinnington.

Mr. Hull has divided his red sandstones of the Bunter by the pebble-beds in the middle, which he considers is sufficient to distinguish them. This may be the case or not. In the soft Permian sandstone at Collyhurst the breccia or conglomerate beds are on the top, at other places further north they are in the middle of the sandstone, and at Canobie, in Scotland, they are at the bottom. I think it very probable that on further observation the pebble-beds of the Bunter will be found to shift in a similar manner.

What is contended for is, that Mr. Hull should not abandon his own classification when it suits his purpose to do so in the neighbourhood of Manchester and Stockport without adducing sufficient reasons for so doing. No evidence has yet been given of the disappearance of the Lower Mottled Sandstone and the Permian marls; and until such is given, Mr. Hull ought to be held fast to his own classification, and allow the soft red sandstone which dips under the pebble-beds at Stockport to be the lowest member of his triple division of the Bunter, which is the lowest division of his triple division of the Trias. This plan of dividing into three parts, as at present used, is probably more fashionable than useful.

The pebble-beds under the town of Stockport have had bores made in them to the depth of nearly 600 feet without going through them; so an estimated thickness of 800 feet is not too great to put them down at. Between these beds and the underlying soft sandstone seen in the valley to Fogbrook, no one has found any other boundary-line than that the pebbles cease to occur in the soft sandstone. What is required to prove the Fogbrook sandstone Permian, if red marls with magnesian-limestone fossils cannot be found lying above them, is to show the pebble-beds resting on eroded surfaces of Permian sandstone; and until such is done, the Fogbrook sandstone ought to be regarded as Lower Mottled Sandstone in its proper position dipping under the pebble-beds. When that evidence is produced (to show that the pebble-beds repose on eroded surfaces of the sandstone), I shall be the first to admit that I have been in error in describing the Fogbrook sandstone as the lowest member of the Trias and not Permian.

In the Norbury-Brook section, as previously stated, no trace of the Permian sandstone was seen, and the overlying breccia or conglomerate was only 5 feet 3 inches in thickness. At Ockley Brook, about a mile further north, the breccia was 30 feet in thickness, and the Permian sandstone 15 feet, thus showing an increase in that direction;

but this is scarcely sufficient to prove the last-named rock to be the same as that seen two miles further north at Fogbrook and Stockport, estimated by Mr. Hull as 1500 feet in thickness.

In works of such authority as the Geological Maps and Memoirs of the Survey every care should be taken to ascertain the boundaries of the workable coal-fields in a manufacturing district, where a supply of coal is of such vital importance. A mistake under an official survey can hardly be rectified by an amateur geologist like myself; but it is desirable that the exact nature of this so-called "Red-rock fault" and the true age of the Fogbrook and Stockport sandstone should be more carefully investigated, and, where necessary, rectified in the Government maps. So far as my knowledge extends, there is no more evidence of a fault between Macclesfield and Stockport, where the Trias and Permian beds cover the Coal-measures, than is to be found on the eastern side of the *Pennine* chain between Sandyacre and Sunderland, where Carboniferous strata disappear under Permian.

XVIII. *On the Organic Matter of Human Breath in Health and Disease.* By ARTHUR RANSOME, M.D., M.A., M.R.C.S.

Read February 22nd, 1870.

THE following analyses of the amount of organic matter contained in human breath were made by the method of water-analysis invented by Messrs. Wanklyn and Chapman.

The aqueous vapour of the breath was condensed in a large glass flask, surrounded by ice, or snow and salt, by which a temperature of several degrees below zero was obtained.

In the first essays, the number of breaths was counted, and the flask washed out with distilled water; but this was soon found to be unsatisfactory, as the extent of the expirations varied so greatly.

The aqueous vapour was then collected, and measured and tested as follows:—

If enough fluid had been obtained, a certain quantity (say ♏xx) was mixed with 50 cub. centims. of distilled water and tested for free ammonia by means of the Nessler test.

An equal portion of the fluid was then mixed with 30 ♏ of a saturated solution of carbonate of soda, and about 10 oz. of pure distilled water, ascertained by distillation to be free from ammonia.

The mixture was then distilled, and the distillate was tested for ammonia until it ceased to give any indication of its presence.

This testing would give all the free ammonia, together with any of this gas arising from the action of the carbonate of soda—for instance, from the decomposition of urea (see Water Analysis, p. 55).

50 cub. centims. of a strong solution of permanganate of potash and caustic potash was then added to the retort, and distillation again continued; the quantity of ammonia now given off would arise from the destruction of organic matter.

The results of these examinations are given in the Tables at the end (pp. 245–247), Table I. giving the records of healthy breath, Table II. of breath from persons affected by various disorders.

In both Tables are given, in successive columns, (1) the number of the observation, (2) the nature of the case,

(3) the age of the individual, (4) the period of the day, and (5) the extent of breathing; then follows, in millegrams, the amount of free ammonia, or ammoniacal salt, determined (6) directly (by the Nessler test) and (7) by distillation with carbonate of soda. A column (8) is then provided for any difference between these two readings, giving the ammonia from urea or other matters decomposable by the weak alkali. The ammonia obtained by oxidation of the organic matter comes next (9), then the total amount of ammonia obtained (10), and finally a calculation of the quantity of ammonia to be obtained from 100 minims of the fluid collected.

The number of examples I have collected is still small; but I have brought them forward now in the hope that others might be induced to undertake the same inquiry.

It is one which requires many observers; and I think that the results, so far as they have been obtained, justify the attempt to enlist others in the service.

1. *Healthy Breath.*—The breath of 11 healthy persons was examined, and the quantity of aqueous vapour was ascertained in 7 instances. The persons examined were of different sexes and ages; and the time of the day at which the breath was condensed varied.

It may be observed that the amount of free ammonia varies considerably, and I have not, so far, been able to connect the variation with the time of the day, the fasting or full condition.

It has been stated by more than one observer, that urea is sometimes present in the breath; it was therefore sought for in 15 cases, 3 healthy persons and 12 cases of disease; but it was only found in 2 cases of kidney disease, in 1 case of diphtheria, and a faint indication of its presence occurred in the breath of No. 8, Table III., a pregnant female suffering from catarrh*.

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* No albumenuria was present in these two cases.

The quantity of ammonia arising from the destruction of organic matter also varies somewhat, possibly from the oxidation of albuminous particles by the process of respiration; but it may be noticed that, in healthy persons, there is a remarkable uniformity in the total quantity of ammonia obtained by the process. Amongst adults the maximum quantity per 100 minims of the fluid is 0·45, and the minimum is 0·35.

It is not easy to estimate the total quantity of organic matter thus got rid of by the lungs of even healthy persons.

We are told by Messrs. Wanklyn and Chapman that every part of organic ammonia discovered corresponds to about 10 parts of albuminous matter; but, on the other hand, the quantity of aqueous vapour carried off by the breath varies with age and season. If, however, we take the ordinary quantity of this fluid for an adult as about 10 oz. in the 24 hours, and the average amount of ammonia given off as 0·4 of a millegram in every 100 minims of fluid, then we obtain the rough approximation that in ordinary respiration about 0·2 of a gramme, or 3 grains, of organic matter is given off from a man's lungs in 24 hours.

At first sight this seems to be a very minute quantity to be thus disposed of; but when it is remembered that the most impure water examined by the authors of the process only contained 0·03 of a gramme of organic matter per *litre*, it will be allowed that there is ample quantity to permit of putrefaction, and to foster the growth of the germs of disease.

We cannot doubt that much of the disease which arises as a consequence of overcrowding finds some of its sustenance in the impure vapours arising from the lungs and the general surface of the body.

2. *In Disease.*—In diseased states of the system we find a much greater variation in the amount and kind of organic matter given off. The breath of 17 cases of disease was examined.

In 3 cases of catarrh, in 1 case of measles, and in 1 of diphtheria the total ammonia obtained was much less than in health, in no case rising higher than 0·2 of a milligram—a result which is probably due to the abundance of mucus in these complaints, by which the fine solid particles of the breath are entangled.

The cases of whooping cough were children, and therefore we cannot be sure that the deficiency of organic matter in these instances was not due to age; and this is the more probable since the only healthy child's breath contained 0·275 milligram of ammonia in 100 minims, a quantity less than that contained in the breath of any healthy adult.

In 2 cases of phthisis, with abundant expectoration, the total ammonia was also less than in health; but in 1 case of this disease, with abundant purulent sputa, but associated with Bright's disease, a large amount of organic matter was given off.

We cannot doubt, however, that the albuminuria which was present in this case had an influence upon the result. A portion of the ammonia was, in fact, due to urea, or to some kindred substance; and we may, perhaps, ascribe the general excess of organic matter to some peculiarity in the breath due to the kidney-disease.

It is in fact in kidney-diseases that the largest amount of organic matter of all kinds is to be found in the breath. The ammonia in one case of Bright's disease rises as high as 0·9 millegram in ℳ 100 of fluid, in another to 0·825, and in a third (a slight case) the quantity is 0·5.

Both the free ammonia and that due to urea are very largely present in one case, No. 16, and they were probably equally abundant in case No. 15.

Probably if the sputa in these cases had been examined, a much larger proportion of matters decomposable by carbonate of soda would have been found. I would suggest that the presence of these substances, either in the bronchial mucus or in the aqueous vapour of the breath, would be a fair indication that their elimination by the kidneys and skin is deficient, and that it might point to the need of measures directed to increase the activity of those organs.

In one case of Ozæna, the total quantity of ammonia obtained was greater than in any of the healthy subjects; but the free ammonia did not seem to be in excess. The case of typhus fever was obtained in the fever wards of the Manchester Royal Infirmary; but it was scarcely a fair example of this disease, since it was already convalescent; there was, however, apparently a deficiency in the total amount of organic matter got rid of from the lungs. I might have attributed this fact to the feebleness of respiratory power, the blast of air being insufficient to carry with it much foreign matter, had not the cases of kidney-disease (Nos. 14, 15, 16, and 17) been equally, if not more feeble.

As a matter of curiosity the air of a railway carriage containing 8 persons was examined, after 15 minutes occupation, with the windows shut and the ventilators open. In this instance the breath was inspired through the apparatus, about 80 inspirations being taken. Perhaps two cubic feet of air would thus pass through the freezing-mixture. Very little mixture was condensed; but what was obtained was strongly charged both with free ammonia and organic matter (see Table III. No. 18).

Organic Matter in the Atmosphere.

Before considering the nature of the organic matter to be found in human breath, it may be well to advert briefly to the prior question of the amount and kind of organic matter in the air breathed.

There has lately been much discussion as to the priority of the discovery of organic matter in both fresh and respired air; and yet it is certain that, from very early times, men have recognized the fact that the air is the vehicle of many substances both organic and inorganic. The old writings are full of disquisitions upon the teeming air.

Boerhaave calls it the "instrumentum catholicum," and speaks of the "corpuscula quæ in aëre perpetuo obvolitant," and he shows how "terra tota ex aëre cadentia recipit omnia, ita rursum aër de terra universa accipit. Fitque inter bina hæc perpetua quasi omnium revolutio, distillatio assidua."—*Elementa Chemiæ* (Leyden, ed. 1732), p. 484.

Medical men also, from the time of Hippocrates, have been only too prone to ascribe all kinds of diseases to the constituents of the atmosphere. Sydenham says, "Since it is the will of God, the Supreme Arbiter and Regulator of all things, that the human frame be by nature adapted to the reception of impressions from without, it follows that it must also be liable to a variety of maladies. These arise partly from the particles of the atmosphere, partly from the different fermentations and putrefactions of the humours. The first insinuate themselves amongst the juices of the body, disagree with them, mix themselves up with the blood, and finally taint the whole frame with the contagion of disease."—*Med. Obs.* Sect. I. chap. 1.

In recent times also, the fact of the presence of organisms in air has been fully recognized.

The great controversy which has now been going on for many years, chiefly on the Continent, on the subject of spontaneous generation, and on the causes of fermentation, turns entirely upon the difficulty of keeping out all taint of organic matter from the atmosphere.

I do not know who first proposed the use of cotton wool as a filter for the air; but several investigators have certainly used it. Schwann, Schroeder, and Dusch (Annales Ch. Pharm. lxxxix. 332), Helmholtz (Journal de Chemie, xxxi. 434), Van den Broek (Ann. Ch. Pharm. cxv. 75).

Pasteur, by using gun-cotton as his filter, and then dissolving this substance in ether, was able to demonstrate in many ways the presence of numerous germs and spores in the atmosphere.

It is to Dr. Angus Smith that we owe the discovery of the extreme readiness with which living organisms are formed in the condensed breath of crowded meetings.

Amongst his other elaborate researches upon the air of towns, Dr. Smith has all along included this now prominent subject, and has frequently called attention to the varying amounts of organic matter in the air of the country and that of towns, especially in the crowded courts and alleys of this city.

The following Table gives the quantity washed down by rain in different places, determined by the Wanklyn and Chapman method :—

TABLE I.

Ammonia in Rain-waters.

Place.	Date.	Ammonia, parts in 1,000,000, or grammes in a cubic metre.	Ammonia of albumen.
Ross, near Helensburgh ...	Jan. 16, 1869.	0·00	0
Clydeford, Glasgow	Jan. 1869.	1·25	0
London Hospital	Feb. 1869.	2	0·3
” ”	” ”	2·2	0·3
” ”	” ”	3	0·4
Glasgow, St. Rollax	Dec. 1868.	5·75	0
” Netherfield	Jan. 1869.	5·5	0
Manchester	Dec. 1868.	6	1
Newcastle-on-Tyne	” ”	5· & 0·6	0

In an Appendix to Dr. Angus Smith's last report to the Privy Council upon the working of the alkali Acts, Mr. Dancer has remarked upon the nature of the organic matter contained in the washings of 2495 litres of the air of Manchester. He discovered in them many forms of life, fungoid matter, sporidia and zoospores, and much lifeless organic substance, vegetable tissue, partially charred objects, fragments of weather-worn vegetation, hairs of leaves, fibres, cotton filaments, granules of starch, and hairs of animals.

Mr. Dancer makes the calculation that about "37½ millions" of spores or germs of organic matter would be contained in the quantity of air examined, an amount "which would be respired in about 10 hours by a man of ordinary size when actively employed."

I would submit, however, with deference to Mr. Dancer, that in this calculation there is a serious possibility of error. There seems to have been a considerable interval of time (how long, is not stated in Dr. Smith's Report) between the commencement of the collection of the fluid and the examination of it by the microscope. It is well known how rapidly organisms increase in numbers in suitable fluids; and it seems reasonable to believe that many of the spores discovered by Mr. Dancer may have been developed in the fluid itself.

I have myself made a few observations upon the organic contents of respired air, which may be interesting at the present time.

Several years ago a letter, by a person who signed himself "Investigator," appeared in the 'Times' newspaper, urging the microscopic examination of the air during the prevalence of epidemics, blights, and murrains. Shortly after this, in the year 1857, I adopted his method, and exposed glass plates, covered with glycerine, in different places—amongst others, in the dome of the Borough Gaol in Manchester. All the respired air from the cells in this building is conducted into the dome by the system of ventilation in use in the establishment. The plates were afterwards carefully searched with the microscope; but at that time I could recognize little except fibres of cotton and wool, and shrivelled epithelial scales: there were also some singular-looking bodies; but these, I found afterwards, were contained in the glycerine used to cover the slips of glass.

Upon another occasion, during a crowded lecture at the Free-Trade Hall, about 3000 persons being present, I drew the air from one of the private boxes (raised about 40 feet above the audience) by means of exhausting bellows, through a system of narrow tubes filled with distilled water, the operation being conducted for a space of about 2 hours. The water was emptied from the tubes, allowed to settle for 36 hours, and the sediment was examined microscopically. The following objects were noted at the time, and sketched under the microscope, the $\frac{1}{4}$-inch power being used:—fibres, separate little cellules, nucleated cells surrounded by granular matter (about 6 in 1 drop of water), numerous scales like degenerated epithelial scales.

The dust from the top of one of the pillars in the private boxes, which had not been disturbed for 3 weeks, was also examined shortly afterwards, and the following objects were noted as being present:—

1. A few fibres of cotton and wool.
2. Black masses of various shapes and sizes, which were taken to be specks of coal-dust.
3. Semitransparent little lumps, refracting light strongly.
4. Crystalline substances, having a laminated texture (query, fragments of glass?).
5. Shrivelled pieces of membrane, epithelial scales.
6. Collections of granules.
7. Variously coloured fragments, blue, pink, and yellow, probably portions of dress.

I have also searched with the microscope several of the specimens of aqueous vapour from the lungs.

In all of them epithelium, in different stages of deterioration, was abundantly present; but very few spores were found in any fresh specimen. On the other hand, after the fluid had been kept for 24 or 36 hours, even in a cold room during the recent cold weather, myriads of vibriones and many spores were found.

In a case of diphtheria, straight-celled confervoid filaments were noticed in addition; and in 2 other cases, 1 of measles and 1 of hooping cough, abundant specimens of a small round-celled conferva, like the *Penicillium glaucum*, were found, and these were seen to increase in size and in numbers for 2 days, after which they ceased to develop.

These differences in the nature of the bodies met with are interesting, as showing some occult differences in the nature of the fluid given off in the several cases; but many additional observations would be needed before we could draw any inferences from them.

They certainly do not as yet afford any proof of the germ theory of disease, nor do they justify the alarming doctrines which some persons would draw from them; they simply point to the readiness with which the aqueous vapour of the breath ferments or putrefies; and they show the danger of overcrowding, and the paramount importance of ventilation.

TABLE II.

Amount of Ammonia obtained from Healthy Human Breath, by Messrs. WANKLYN and CHAPMAN'S method of Water-analysis (in milligrammes).

No.	Case.	Age.	Time.	Extent of breathing.	Free ammonia.	By distillation with carbonate of soda.	Difference due to urea, &c.	Organic ammonia from oxidation.	Total.	Amount in 100 drops of the fluid collected.
1.	Male. Healthy, strong, large.	33	4 hours after late dinner.	10 prolonged breaths, about 2 in a minute.	not	separated.	....	....	0·06	....
2.	Male. Healthy, medium size.	35	4 hours after late dinner.	15 breaths, about 4 in 1 minute.	0·03	....	....	0·045	0·075	....
3.	Ditto	...	5 hours after late dinner.	20 breaths, 4 in 1 minute.	0	....	....	0·10	0·10	....
4.	Ditto	...	1 hour after late dinner.	25 breaths, ♏xx. collected.	♏x. 0·03	♏x. 0·03	0	0·05	0·08	0.400
5.	Ditto	35	1 hour after late dinner.	20 prolonged breaths.	0·035	....	....	0·05	0·085	....
6.	Male. Healthy, strong.	31	1 hour after breakfast.	20 breaths, ♏xxv. collected.	0·035	....	....	0·06	0·095	0·370
7.	Male. Healthy, undersized.	40	1 hour after late dinner.	7 minutes, ♏xx. collected.	0·0	....	....	0·085	0·085	0·425
8.	Male. Healthy, middle height, strong.	18	4 hours after midday dinner.	15 minutes, ♏xl. collected.	♏xx. 0·035	♏xx. 0·035	0	0·055	0·09	0·45
9.	Male. Healthy.	9	1 hour after dinner.	20 minutes, ♏xlv. collected.	♏xx. 0·02	♏xx. 0.02	0	0·035	0·055	0·275
10.	Female. Healthy, medium size.	29	1 hour after luncheon.	20 breaths, ♏x. collected.	0·01	....	....	0·025	0·035	0·35
11.	Ditto	24	2 hours after 6 o'clock tea.	20 breaths, ♏x. collected.	0·015	....	....	0·025	0·04	0·4

Table III.—Showing the amount of Ammonia obtained from the Breath in different Diseases (in milligrammes).

No.	Case.	Age.	Time.	Extent of breathing.	Free ammonia.	By distillation with carbonate of soda.	Difference due to urea &c.	Organic ammonia from oxidation.	Total.	Amount in 100 drops of the fluid collected.
1.	Female. Measles, 10th day.	19	After breakfast.	15 minutes, ♏xlv. collected.	♏xx. 0	♏xx. 0	0	0·03	0·03	0·15
2.	Female. Phthisis, advanced, much expectoration,	28	Afternoon.	15 minutes, ♏l. collected.	♏xx. 0·01.	♏xx. 0·01	0	0·03	0·04	0·2
3.	Female. Phthisis, advanced, much expectoration.	33	1½ hour after breakfast.	20 minutes, ♏lx. collected.	♏xxx. 0·01.	♏xxx. 0·01	0	0·04	0·05	0·165
4.	Female. Catarrh, pregnant, 6mos.	29	1 hour after luncheon.	15 minutes, ♏xlv. collected.	♏xx. 0·005.	♏xx. 0·01	0·005	0·03	0·04	0·2
5.	Male. Slight catarrh.	7	Shortly after dinner.	10 minutes, ♏xx. collected.	………	♏xx. 0·01	………	0·03	0·04	0·2
6.	Male. Slight catarrh.	32	½ hour after late tea.	40 breaths, ♏xxx.	♏xv. 0	♏xv. 0	0	0·45	0·45	0·3
7.	Female. Diphtheria, improving, no albumen.	28	10 A.M.	♏lxx.	♏xx. 0	♏xx. 0·01	0·01	0·03	0·04	0·2
8.	Female. Whooping cough, 4 weeks.	11	1 hour after dinner.	25 minutes, ♏xc.	♏xx. 0·01.	♏xx. 0·01	0	0·05	0·06	0·3
9.	Female. Whooping cough. 4 weeks.	8	Ditto.	15 minutes, ♏xlv.	♏xx. 0·01	♏xx. 0·01	0	0·045	0·055	0·275
10.	Male. Typhoid fever, 9th day.	20	3.30 P.M.	10 minutes, ♏xx.	♏xx. 0·02	………	………	0·04	0·06	0·3

11.	Female. Phthisis, tubercle, both sides.	28	2 hours after dinner.	20 short breaths.	0.	....	....	0·05	0·05	....
12.	Female. Phthisis, advanced.	18	11 A.M.	♏vii.	....	....	....	0·02	0·02	0·29
13.	Female. Slight ozæna.	34	2 hours after dinner.	♏l.	♏xx. 0·02	♏xx. 0·02	0	0·08	0·10	0·5
14.	Female. Advanced phthisis, incipient albuminuria, abundant expectoration.	17	4 P.M.	♏xxiv.	♏xii. 0·005	♏xij. 0·02	0·015	0·06	0·08	0·666
15.	Male. Slight albuminuria, under water-treatment.	68	2 hours after breakfast.	15 minutes, ♏xl.	♏xx. 0·02	♏xx. 0·02	0	0·08	0·1	0·5
16.	Male. Albuminuria, *uræmia* impending. P.M. week after: large white kidney.	13	4 P.M.	♏xl.	....	0·12	....	0·21	0·33	0·825
17.	Male. Albuminuria. Dropsy. P.M. 2 weeks after: large white kidney.	45	4 P.M.	♏xl.	♏xx. 0·045	♏xx. 0·08	0·035	0·10	0·18	0·900
18.	Air of railway-carriage. 8 passengers, after 15 minutes, windows shut, ventilation open, 80 inspirations.	...	....	Halitus only condensed.	0·03	... .	....	0·03	0·06	....

XIX. *On a new Form of Calamitean Strobilus from the Lancashire Coal-measures.* By W. C. Williamson, F.R.S., Professor of Natural History in Owens College, Manchester.

Read October 19th, 1869.

In the last communication which I had the privilege of laying before this Society, I mentioned that I had found, in the collection of Mr. Butterworth, of High Crompton, portions of a fossil strobilus from the Lancashire Coal-measures, which exhibited some remarkable features, and which I believed had not improbably belonged to the *Calamopitus* that I was then describing. Since that comunication was made, Mr. Butterworth has kindly prepared for me some additional sections of the specimen; and having subjected the whole of them to a careful examination, I now beg to communicate the results to the Society. When the specimen came into the possession of Mr. Butterworth, it consisted of but three oblate joints or segments of what had once been a larger structure. In its general aspect it appears to have closely resembled, if it was not identical with, one which Mr. Binney figured* and referred to as resembling the *Aphyllostachys jugleriana* of Goeppert. Mr. Binney describes his strobilus as being about half an inch in length, and consisting of eight or nine crown-shaped masses or joints, each of which, calculating the proportions indicated by Mr. Binney's figures, must have been about the $\frac{1}{16}$ of an inch in length, and from $\frac{3}{16}$ to $\frac{1}{4}$ in transverse diameter. Mr. Butterworth's strobilus has considerably exceeded these dimensions. The specimen is somewhat compressed laterally; hence its transverse section presents an oval figure. The length of each joint, or internode, supporting one verticil of sporangia is about $\frac{1}{4}$ inch, its greater dia-

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* "Observations on the Structure of Fossil Plants found in the Carboniferous strata. By E. W. Binney, Esq., F.R.S., F.G.S. Part I. Calamites and Calamodendron" (Palæontographical Society, 1868), Plate vi. fig. 1.

meter being $\frac{7}{16}$, and its lesser one $\frac{5}{16}$ of an inch. Two of the three internodes had been sliced into sections before I saw the specimen; but the third, which fortunately happened to be the lowest one of the three, was preserved intact, and is represented in figures 1 and 2, the former being its lateral aspect, and the latter that of its inferior surface. Externally, each internode of the strobilus has exhibited a series of strongly marked, rounded, longitudinal ridges and furrows, the former being apparently about 20 in number, though, owing to the fragment being somewhat injured on one side, I could not count them with exactness. These are invested by numerous closely lapping, thin, membranous, vertical bracts. Each of these bracts appears to have occupied one of the furrows between the ridges, its margins having overlapped, or been in contact with, those of its nearest neighbours along the line of each ridge; but this point also was not very clear, owing to the exceeding thinness of the bracts and the closeness with which they were in mutual contact. The upper part of the fragment having been cut off in making a transverse section, I could not decide whether these bracts terminated with the upper edge of their own internode, or whether their tips were prolonged over the joint above.

Fig. 2 represents the concave base of the specimen. I do not believe that this has been the actual base of the strobilus. In all probability there has intervened between it and the common peduncle one small joint. That if not the actual basal segment it must have been nearly so, is shown by the rapid contraction of its outline, inferiorly, when seen in its lateral aspect (fig. 1). In the centre is the medullary cavity (fig. 2 *a*), a cylinder with an internal diameter of about $\frac{1}{14}$ of an inch. This exhibits no structure, its interior being filled with infiltrated crystalline carbonate of lime. That it was hollow when entombed, and has not lost its tissues through fossilization, is shown by the presence in its interior of some of the ubiquitous rootlets of *Stigmaria* in an uncompressed condition. Extending from this medullary cavity to the periphery of the base of the strobilus is an almost unbroken disk of cellular membrane, the surface of which, under a low magnifier, exhibited myriads of very fine radiating striæ, running from the centre to the circumference. This horizontal disk was not extended in one plane, but variously inflected, presenting, when seen from below, two inner circles, each of which was slightly convex, surrounded by a larger concave one. Immediately surrounding the medulla is an unbroken ring about $\frac{1}{100}$ of an inch in breadth, perfectly flat, save at its inner border, where it is convex; but I doubt if this convexity existed in the living strobilus. From the outer margin of this ring the disk is extended in the same plane for rather more than the $\frac{1}{20}$ of an inch. In this latter portion we have a circle of symmetrically arranged pyriform figures, also filled with crystalline carbonate of lime. These figures indicate open spaces, dividing the membranous disk into ten principal inner peduncles and twenty secondary outer ones. A little beyond the outermost boundary of these spaces, the disk has bent downwards, forming on the inferior surface of the segment a convex ring, enclosing the parts just described, after which the disk reverses its direction, forming a concave space that extends to the acute inferior peripheral margin (fig. 2 *b*) of the internode. At this point the disk subdivides into a multitude of minute ovato-lanceolate bracts, which bend abruptly upwards, and which constitute, as already described, the outermost investment of the strobilus.

Fig. 3 represents a transverse section of a portion of the Strobilus, made in the plane of the flat disk surrounding the pith of fig. 2, being, in fact, a section of part of the central axis of the strobilus, where it passes through a nodal bractigerous verticil: *c c* represent two of the ten primary peduncles given off from the central axis. Each of these has an average breadth of about $\frac{1}{40}$ of an inch. It consists chiefly of cellular tissue, the cells being of various sizes. In the ring immediately investing the pith the cells are the largest, being from about $\frac{1}{400}$ to $\frac{1}{600}$ of an inch in diameter. In the centre of each peduncle they are about $\frac{1}{800}$ of an inch in diameter. At *d d* are two pores, which are sections of two canals, running the entire length of the axis of the strobilus, and which serve in an important manner to identify homologous parts in different sections. These canals have a diameter of $\frac{1}{400}$ of an inch, and the cells immediately surrounding them range from about $\frac{1}{1400}$ to $\frac{1}{1600}$ of an inch in width, these being much smaller than the rest. The dark and dense patches, *e e*, I believe mark the position of some important bundles of reticulated vessels to which I shall again have to refer. At *f f f* are three of the pyriform spaces separating the primary peduncles. The transverse section of each of these is broadly ovate at its inner extremity, and acuminate in the opposite direction. On each side of this acuminate portion, and separated from it only by a very thin film of oblong cells, is the ovate basis of a smaller, but otherwise similar, space (*g*), at the peripheral end of which is a patch of tissue (*h*) somewhat denser than the rest, and which marks the starting-point of a spore-bearing peduncle or sporangiophore, which ascends almost vertically into the substance of the strobilus. These sporangiophores are twenty in number, or double that of the primary peduncles. The remainder of this section consists chiefly of coarse cellular tissue, with the exception of the darkly shaded portions, *i i*, which are masses of spores. Owing to the peculiar inflections of the bractigerous disk given off from each node, no one continuous horizontal section can be made through its entire plane, as will readily be understood on reference to the restored vertical section of the strobilus, fig. 13, where the dotted line, *x x*, represents the direction of the horizontal section, fig. 3. From this it will be seen that, after giving off the ascending sporangiophores (*h*), the bractigerous disk continues its outward course for a very short distance, and then bends suddenly downwards, resuming its upward direction as it approaches the exterior of the strobilus. As the sporangia accommodate themselves to these curvatures, it follows that a section made in the line (fig. 13) *x x* will intersect the sporangia at its peripheral margin, as is done in the instance of fig. 3, *i i*.

Fig. 4 represents a transverse section of the entire strobilus, made at an angle somewhat inclined to the central axis *; hence, whilst in the part opposite to *x* it intersects the strobilus nearly in the same plane as fig. 3, throughout the remainder of the section (as at *y*) it has crossed the segment somewhat higher up, revealing the structure of the central axis and of the sporangiophores, *h h'*, after their detachment from the bractigerous disk and their consequent separation from the axis. In this section we see the medullary cavity at *a* surrounded by the woody axis, the innermost part of which consists of the coalesced bases of the ten primary peduncles. Each of the latter exhibits the two small pores seen in fig. 3 *d*, and which obviously indicate continuous canals, running vertically through the woody axis. The relations of the woody axis to the bractigerous disk and surrounding mass of sporangia have been somewhat disturbed by a rupture apparently due to some shrinking of the cone prior to fossilization; but, notwithstanding this, we have no difficulty in identifying the various parts of the section. Thus at *c* we have one of the primary peduncles flanked on either side by one of the large pyriform spaces, *f f*. At the peripheral extremity of this peduncle we see the rounded inner boundaries of the two smaller pyriform spaces, which, though somewhat disturbed, can

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* As indicated by the line *w w* in fig. 13.

be traced up to the two well-marked sporangiophores h'' h''. If we proceed right and left from this starting-point, owing to the obliquity of the section, we can trace the gradual divergence of the sporangiophores from the central axis, and their isolation amidst the masses of sporangia which they have supported—also the contraction of what I have termed the primary peduncles of the bractigerous disk into mere prominent longitudinal ridges on the exterior of the central axis, like those on the stem of a Calamite, whilst the large pyriform cavities, in like manner, become deep grooves separating these ridges. In this figure little attempt has been made to delineate the complicated and interrupted outlines of the sporangia, *i i*, except at the peripheral margin, *l*, where their distinct continuity shows that we possess the outermost portion of the strobilus. In the interior of the structure, the arrangement of these sporangia has been much disturbed, apparently by inequalities in the mutual pressure to which they have been subjected, resulting from the growth of the spores.

Fig. 5 represents the central portion of a transverse section made at a higher point than the last, corresponding to the line *y y* in fig. 13. The cylindrical axis surrounding the medulla (*a*) is now more sharply defined, owing to the absence from this internodal portion of the bractigerous disk. The homologues of the bases of the ten primary peduncles of fig. 4 are identified by the two small canals in each (*c*, *c*), which here approach the outer surface of the axis: we thus see, as I have already suggested, that these primary peduncles are merely nodal prolongations of ten somewhat rounded, projecting ribs running longitudinally along the exterior of the central axis; these ribs, becoming increasingly prominent as they approach the node, both from above and from below, gradually converge, and at length coalesce, so as to enclose the intermediate grooves, which are thus converted into pyriform openings (*f*) perforating the bractigerous disk. The vertical section, of which one side is represented in fig. 7, seems to indicate that the disk attains its greatest development a little below the actual node of the axis which is indicated by fig. 7 *e*, since at *c* we have cellular tissue extended peripherally towards the bractigerous disk below, whilst at *c′* we have a similar expansion of the axis proceeding towards the next disk above, the next superior node (corresponding to *e*) not being contained within the section. The axis at the internodes (fig. 5) is closely invested by the mass of sporangia, the outlines of which are often more distinctly traceable here than in fig. 4; but in other respects the unfigured peripheral portion of the former section corresponds closely with the lower part of fig. 4. At this part of the internode the sporangia have obviously no connexion with the central axis, beyond that of mutual contact; but the lines of their intersected walls can be traced, in several instances, radiating from the isolated sporangiophores. The entire thickness of the wall of the axial cylinder at this point is about $\frac{1}{50}$ of an inch at the ridges (fig. 5 *c*), and $\frac{1}{80}$ of an inch at the intervening grooves.

Fig. 6 represents a vertical section made through the centre of the segment figs. 1 and 2. *a* is the medullary cavity, bounded on each side by the woody axis. At *c* we have a contribution from part of the internode immediately above the node, towards the formation of the bractigerous disk, *k*; and from the latter there ascends obliquely upwards and outwards the sporangiophore, *h*. At *k* we find the bractigerous disk continuing its outward course for a short space in the horizontal plane, then bending suddenly downwards in a sweeping curve, and resuming its upward course to support the bracts (*k′*) investing the exterior of the strobilus. All the darkly shaded portions of this figure represent sporangial masses—the rounded portion, *i*, being especially invested with the cellular wall of this sporangium, showing that the latter structure originally filled the contiguous depression in the peripheral portion of the bractigerous disk. It may be observed that, in all the vertical sections, the disk is seen to receive vascular and cellular contributions, both from above and below the node to which it belongs, though chiefly the latter—a condition not unlike what occurs in the reproductive spikes of many living Equisetaceæ, where a similar thickening of the sporangiophores takes place.

Fig. 7 exhibits the right half of a vertical section like the last; but, from its importance, it is represented as more highly magnified. *a* is part of the medullary cavity, in immediate contact with which is the prosenchymatous tissue, everywhere forming the innermost part of the solid woody axis. The cells are oblong, and of various lengths. Sometimes they have rectangular septa, but more frequently they present obliquely overlapping extremities. The structure of the outer part of the woody cylinder varies according to the line in which the vertical section has been made. We here find that the axis begins to enlarge at the centre of the internode, *e′*, and continues to do so gradually as we ascend to the node above. The enlargement is the result of additional prosenchymatous cells (*c′*) added to the exterior of the longitudinal ridges. At *f* the section has laid open a narrow segment of one of the larger pyriform canals, fig. 3 *f*, throughout a great part of its length; whilst at *c″* we have the thin film of prosenchyma which has separated that canal from one of the smaller ones, fig. 3 *g*. At the lower part of the section we have some important features exhibited. At *e* is the node, marked by a constriction of the medulla, arched over by a group of reticulated vessels. These are identical, both in their structure and arrangement at this point, with what I have described in *Calamopitus*. They spring from the medulla below the node, at an oblique angle, and arch over the node, returning to the medullary tissue at nearly the same angle as that with which they arose; but instead of terminating abruptly, they now proceed upwards (fig. 7, *e'*) parallel with the pith, forming the outermost portion of the external longitudinal ridge of the axis. Unfortunately I possess no tangential section of this part of the structure; consequently I am unable to speak with certainty respecting the superficial arrangement of these vessels; but a careful study of the various sections has led me to the conclusion that there are two of these woody bundles in each external rib of the axis. I believe that their position in the transverse section is indicated by fig. 3 *e*, or immediately external to each one of the longitudinal canals, fig. 3 *d*—which accounts for their position at the exterior of the ribs in the section fig. 5, *c*. If this explanation be correct (and I have little doubt about it), some important inferences are suggested by the fact. It indicates that these vascular bundles are the homologues of the woody wedges of Calamites, and that the small canals in like manner represent those of which, as Mr. Binney and others have pointed out, one forms the innermost angle or starting-point of each woody wedge.

From each node we find the cellular tissue *c* descending a short distance, but proceeding rapidly outwards to form the upper part of the next inferior bractigerous disk. At *h* we have the sporangiophore of the same disk, but forced inwards, away from its normal direction, to pass upwards between the two sporangia *i* and *l*. It will be observed that this upper surface of the bractigerous disk is very different from the lower one. In the latter, the gradually enlarged ribs ascend from the centres of the internodes, like ten buttresses, sustaining the disk with its sporangiophore and mass of sporangia. On the other hand, at the upper surface, the ribs, descending from the internode above, are but slightly enlarged; hence a slight concavity in the disk is adapted to receive the inferior surface of the inner sporangium *l*, which rests upon it.

Fig. 8 represents portions of two of the vessels from fig. 7, *e*, more highly magnified, and exhibiting the reticulated character which has hitherto proved so distinctive of *Calamopitus*. They have a variable diameter of from $\frac{1}{800}$ to $\frac{1}{1600}$ of an inch.

The outlines of some of the sporangia are very distinct in fig. 7, the sporangium-walls (*l*, *l'*) being more continuous and regular than usual, whilst the spores (*i*), indicated in the drawing by the dark mottled surfaces, are packed very closely round the main axis.

Fig. 9 is a transverse section of one of the sporangiophores from the unfigured part of the section of which fig. 5 is the central portion. Its dorsal surface, *h*, is rounded; but its opposite or inner margin projects as a strongly defined keel (*h'*), owing to two deep lateral excavations, which gives this part of the organism a compressed form. It chiefly consists of densely aggregated elongated cells, or prosenchyma, with vague traces of vascular tissue; but in all the longitudinal sections, its structure is so dense and black that the details are not easily made out. The greatest diameter of this sporangiophore is about $\frac{1}{100}$ of an inch, and its lesser or transverse diameter about $\frac{1}{130}$ of an inch.

The numerous spores are enclosed in sporangia, the structure of the investing membranes of which appear to be almost identical with those of the Calamitean strobili described by Mr. Binney and Mr. Carruthers. These sporangium-walls are cellular, the oblong cells being arranged vertically to the two surfaces of the membrane, which is about $\frac{1}{800}$ of an inch in thickness, whilst the individual cells have a diameter of from $\frac{1}{1200}$ to $\frac{1}{2000}$ of an inch, the latter being the more usual dimensions. Externally, the ends of the cells are generally plane (fig. 10, *l'*), whilst their inner extremities (*l*) are somewhat convex and turgid; but these differences are not constant. In some instances these membranes can be traced continuously for considerable distances round the several sporangia; but in other cases they seem to have been disturbed and broken up by the swelling of the spores. I apprehend that this derangement, combined with the regular symmetry and exquisite preservation of the vasculo-cellular portions of the cone, may be accepted as an indication that the spores had reached maturity, rather than been in a half-developed state. In all cases the undulating outlines of the sporangia indicate the same thing, the membranes having been apparently corrugated and shrivelled, their protective functions having been nearly fulfilled. The exact number of the sporangia clustered round each sporangiophore is not certain. I have not been able to trace more than three in many instances; but occasionally I find indications of a fourth. We may safely conclude from three to four to have been the normal number associated with each sporangiophore. I am unable also to make out accurately the position and extent of the surfaces attaching the sporangia to the sporangiophores. I have already pointed out the distinctness of the sporangial membranes immediately beneath the external investing bracts on the left side of fig. 4. The *spores* (figs. 11, 12) exist as a dense mass of separate cells, packed closely together within the sporangia; but occasionally detached ones are imbedded in the translucent carbonate of lime with which parts of the fossil are infiltrated, so that their structure is not difficult to determine. They consist of an outer (*i*) and an inner cell-wall (*i'*), the latter obviously representing the primordial utricle of authors and enclosing some peculiar cell-contents. In some instances, as shown in fig. 12, these cell-contents are aggregated into a dark well-defined central mass; but in others, as in fig. 11, this mass has no defined outline, being gradually merged in the inner cell-membrane (*i'*) which encloses it, whilst occasionally it is absent. I was at first inclined to believe that the dark central portions seen in fig. 12 were the spores, enclosed within a true cell; but after a very careful conjoint examination made by Mr. Carruthers and myself, we satisfied ourselves that each cell in its entirety constituted a separate spore. They have an average diameter of from $\frac{1}{250}$ to $\frac{1}{320}$ of an inch, whilst the dark central mass is from $\frac{1}{320}$ to $\frac{1}{350}$ of an inch.

As a rule, I distrust most detailed restorations of fossil plants; but in this instance the specimen is in such exquisite preservation that there can be no doubt as to the general plan of its construction. Fig. 13 may be regarded as a vertical section of its five lower segments, of which the sporangia are supplied to two of the lower ones, whilst the upper two show the relations of the axis to the bractigerous disk and its appendages. That the pith has been fistular in the fully developed cone I infer from the beautiful preservation and sharply defined outline of the cellular tissue lining the interior of the woody axis, combined with the presence, within the cavity, of perfect rootlets of *Stigmaria,* the latter especially proving that the axis was hollow when the strobilus fell into the mud which the *Stigmaria*-roots were permeating with their ubiquitous fibres.

We have next to consider the probable relations of this strobilus to other Coal-measure plants, and especially to the somewhat similar structures described by Mr. Binney* and Mr. Carruthers†, the specimens studied by the latter gentleman having been also derived from Mr. Binney's collection, and being identical in nature with those figured by him. In all essential features my plant corresponds with these, as well as with that described by M. Ludwig and referred to by Mr. Carruthers in the above memoir. Mr.

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* *Loc. cit.*

† "On the Structure of the Fruit of Calamites," by Wm. Carruthers, Esq., F.L.S., Journal of Botany, Dec. 1867.

Carruthers's careful description makes a comparison of the points of agreement and difference easy. Describing Mr. Binney's specimen he says, "At regular intervals the axis gives off whorls of appendages which are *alternately* foliar and fruit-bearing." We here note the first difference. In my example *each* node gives off both these elements. "The foliar whorl consists of twelve leaves, which proceed horizontally from the axis until they reach the circumference of the strobilus, where they take an ascending direction. The leaves are united together by their margins until they reach the outside of the fruit, and form a continuous septum, dividing the strobilus into a series of chambers." This description, allowing for the inflections which I have described, identifies the structure spoken of with my bractigerous disk. Thus far we find the essential conformation of the two fruits exhibiting a close correspondence. Mr. Carruthers says, "Between each foliar whorl there is a verticil of leaves specially developed for the support of sporangia." On this point the two types differ. Instead of this alternation, in my specimen the fruit-bearing organs, or sporangiophores, are developed at each node instead of at alternate nodes; and in the place of shooting out at right angles directly from the central axis, they spring obliquely, or almost vertically, from the upper surface of the foliar verticil or bractigerous disk. In Mr. Carruthers's description, the end of each of these sporangiophores is described as being peltate. In only one instance have I been able to trace the upper part of the sporangiophore in my example; and, like the specimen described by M. Ludwig, it appears to have been a thorn-like process, unprovided with any peltate extremity. The structure of the sporangium-wall is identical in the two cases; and the arrangement of the sporangia around the sporangiophore is also similar, making allowance for the vertical position of the latter in my instance, and its horizontal one in

Mr. Binney's. "The spores are simply globular bodies, frequently exhibiting an outer and an inner wall." This description also tallies with my own; but Mr. Carruthers continues, "Sometimes, however, they appear to be composed of a single wall; and then the outer wall is represented by lines more or less separated from the spores. These I believe to be elaters, similar in structure to those of *Equisetum.*" I have found spores exhibiting something of this appearance when the outer cell-membrane had been broken up, either by contraction or, more especially, during mineralization; but I am satisfied that my specimen contains no elaters. In the structure of the central axis, again, we have a difference. In Mr. Binney's specimens the centre of that axis is occupied by a bundle of scalariform tissue. Nothing of the kind exists in my plant. Its central portion presents every appearance of having been fistular, or only occupied, in its young state, by cellular tissue. The only vessels to be seen are in the woody axis, where they are reticulated and unaccompanied by any scalariform ones. At the same time, there can be no doubt that the two types are constructed upon the same general plan, the differences which they present being but generic and not ordinal ones. They resemble each other too closely in their common features to leave a doubt that if the one is Calamitean so also is the other; and since no one appears to doubt that such is the character of Mr. Binney's strobilus, I may fairly claim the same rank for my own. What, then, is the signification of the points in which they differ? It will be remembered that the Calamitean stem which I described under the name of *Calamopitus* was characterized by the possession of reticulated vessels instead of the scalariform ones common in other types of Calamite, and by a peculiar arched arrangement of those scalariform vessels wherever they crossed the node, which latter arrangement is common to *all* the types of Calamite of which I have hitherto seen the internal structure; and, in addition, *Calamopitus* has its projecting ridges composed of longitudinal wedges of vascular tissue, separated by intervening ones of prosenchyma.

Fig. 8 shows that my strobilus exhibits the first of these characteristics; figure 7 *e* displays the second; whilst, if I am correct in my interpretation, we find the third feature echoed in the longitudinal arrangement of the vascular bands (fig. 7, *e e'*), and in the relationship of those bands to the small longitudinal canals (fig. 3, *d*), as well as to the masses of cellular prosenchyma (fig. 7, *c c'*) which separate them. Bearing in remembrance the apparently obvious fact that my strobilus is an indisputable Calamitean fruit, and that *Calamopitus* is the only Calamitean stem hitherto described possessing the true reticulated vessels which it exhibits, it becomes more than probable that some close relationship exists between the two plants. If they are not actually the stem and fruit of the same species, at least the fruit must have belonged to some hitherto undiscovered stem with reticulated vessels closely allied to *Calamopitus**. It is true that, in the specimen of the latter which I described, I could not trace the small canal seen at the inner angle of the woody wedges of other Calamites; but I have already found specimens which indicate the possibility that this apparent absence may have been due to mineralization masking, by dark carbonaceous deposits, what may have existed in its minimum rather than its maximum degree.

At first there seems to be a difficulty in admitting the association of a cryptogamic fruit with an exogenous stem; but we do not escape this difficulty by refusing to accept my suggestion. No one can doubt that *Calamopitus* is merely

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* Since this memoir was read I have obtained stems with reticulated vessels, but otherwise like Calamodendra, to which the strobilus may have belonged.

a highly developed Calamitean plant; neither does any one now doubt that the fruit of Calamites was cryptogamic. Besides which, we must remember that a cambium-layer and an exogenous mode of growth are still found associated with Cryptogamic inflorescence in all the living Marsileaceæ; so that the possibility of the combination which I have suggested is in strict accordance with conditions known to exist, instead of being, as some have supposed, abnormal, and contradicted by all modern experience.

The only other known Coal-measure plants in which reticulated structures abound are those for which I have proposed the name of *Dictyoxylon**. But whatever objections may suggest themselves to identifying the strobilus with *Calamopitus* militate in a tenfold degree against a similar identification with *Dictyoxylon*. The latter is not only exogenous in growth, but probably a true Exogen in the technical sense of the word, if not even an actual Conifer; hence there is the greatest improbability that it bore a Cryptogamic strobilus. But, on the other hand, admitting that *Calamopitus* and this strobilus are equally Calamitean in type, that they exhibit essential features which they possess in common, and that in both cases these features point to a higher organization than is usual amongst the more ordinary Calamites, I am justified in concluding that the subsistence of a close relationship between the two fossil plants is more than probable. Assuming this probability to be established, what light does the fact throw upon the affinities of the above fossils with recent plants? I find in my strobilus, as already stated, nothing like Equisetiform elaters. The spores are simple cells, which is also the case with recent Equisetiform spores in their young state. But, for reasons already given, I believe the fruit described to have been fully developed. Consequently, so far as it goes, it gives no support to the

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* Monthly Microscopical Journal, No. viii. August, 1869.

idea that *Calamopitus* was Equisetaceous; on the other hand, whilst the stem was more complex than that of living Equisetaceæ, the fossil spores are more simple in their organization than in the recent genus. The fruit sustains the conclusion at which I arrived from a study of the stem, that *Calamopitus* possessed a higher organization than the known forms of *Calamodendron.*

The exogenous growth of the stem by the regular addition of new vascular bundles to the exterior of the woody axis, indicates the possession by these plants of a cambium-layer such as exists in the living Marsileaceæ. Unfortunately we know too little of the cortical layer of Calamites to affirm any thing respecting it; but it becomes of great importance to ascertain whether it was persistent, receiving *internal* additions from the cambium-layer (in which case we should expect to find it in some degree ruptured externally), or whether it was thrown off annually and annually reproduced as in the living *Isoëtes*. As yet we possess no facts throwing light upon either of these problems*; but when we obtain their solution, we shall doubtless find in it the explanation of the great differences observable, both in the aspect of the cortical layer of fossil Calamites and in the opinions of authors respecting its thickness and aspect.

Mr. Butterworth informs me that he found the specimen here described in a nodule from the upper foot-coal at Roe Buck, in Strinesdale, Saddleworth.

INDEX TO PLATES VII., VIII., & IX.

Fig. 1. Lateral aspect of the lowest segment of the specimen, enlarged four diameters.

Fig. 2. Inferior surface of the same segment, showing the pyriform canals around the medulla, *a*; *b*, margin where the bractigerous disk divides into separate bracts.

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* This bark has now been obtained, and will shortly be described.—April 21, 1870.

Fig. 3. Transverse section of part of central portion of fig. 2, made in the line *x x*, fig. 13: *c*, *c*, primary peduncles; *d*, *d*, longitudinal canals of axis; *e*, *e*, bundles of reticulated pleurenchyma; *f*, *f*, larger pyriform apertures in the bractigerous disk; *g*, *g*, smaller apertures; *h*, *h*, bases of the sporangiophores; *i*, sporangia.

Fig. 4. Slightly oblique transverse section of one of the upper segments of the strobilus, made in the line *w w* of fig. 13, passing through the bractigerous disk opposite to *x* (fig. 4), and a little above it in the rest of the section: *a*, medulla; *c*, primary peduncles; *h*, *h′*, *h″*, sporangiophores; *i*, sporangia; *l*, cellular sporanginm-walls, seen in section.

Fig. 5. Central part of a transverse section of half of the segment figs. 1 and 2, made in the plane *y y* of fig. 13: *a*, medulla; *c*, *c*, external ridges of the central axis, identical with the primary peduncles of fig. 4; *i*, sporangia.

Fig. 6. Vertical section of segment figs. 1 and 2: *a*, medulla; *c*. central axis; *h*, sporangiophore; *i*, *i′*, sporangia; *k*, bractigerous disk; *k′*, bract.

Fig. 7. Lateral half of a vertical section of one of the upper segments: *a*, medulla; *c*, cellular tissue descending from the node *e* towards the bractigerous disk below; *c′*, prosenchyma ascending towards the next bractigerous disk above; *c″*, thin layer of prosenchyma separating the larger pyriform canal *f* from one of the smaller ones not seen in the section; *e*, arches of reticulated pleurenchyma forming the node; *e′*, the same pleurenchyma prolonged upwards, external to the prosenchyma of the axis; *h*, sporangiophore belonging to the bractigerous disk *c*; *i*, *i*, spores; *l*, inferior wall of a sporangium resting upon the bractigerous disk *c*; *l′*, external wall of the sporangium *i*.

Fig. 8. Two reticulated fibres from 7 *e*, more highly magnified.

Fig. 9. Transverse section of upper part of a sporangiophore: *h*, peripheral margin; *h′*, inner margin.

Fig. 10. Transverse section of part of a sporangium-wall: *l*, inner surface; *l′*, outer surface.

Figs. 11, 12. Detached spores: *i*, outer cell-membrane; *i′*, inner-cell membrane.

Fig. 13. Restored vertical section of five inferior segments of the strobilus: *a*, medulla; *b*, outer margin of bractigerous disk; *c*, central axis; *h*, sporangiophores; *i*, *i*, sporangia; *k*, external bracts; *l*, sporangium-walls; *w w*, plane of section, fig. 4; *x x*, plane of section, fig. 3; *y y*, plane of section, fig. 5.

XX. *A Search for Solid Bodies in the Atmosphere.*
By R. Angus Smith, Ph.D., F.R.S., &c.

Read March 31st, 1868.

I have so frequently for many years attempted to find, and have found, organic substances which have passed from the air into the liquids in which they were collected, that perhaps the Society will scarcely attend to another attempt, although it indicates, I think, some progress. It was in the year 1847 that I first collected what I believe was matter from the respiration and perspiration, and found that as it was kept it grew into distinct confirmed forms.

Whilst examining some matters relating to the cattle-plague, I found one or two remarkable points. I had before that time used aspirators to pass the air through liquids, except in the oxidation experiments. At that time I used simply a bottle which contained a little water. The bottle was filled with the air of the place, and the water shaken in it. The difference of air was remarkable. A very few repetitions would cause the liquid to be muddy, and the particles found in many places were distinctly organic.

Before speaking of my last experiment, it may interest the Society first to hear of a few of these previous attempts, the latest made till recently. I shall therefore read from a report to be found in the appendix to that on the cattle-plague.

"Mr. Crookes also brought me some cotton through which air from an infected place had passed. It was examined at the same time. Taking the cotton in the mass, nothing decided was seen; but when it was washed, some of the separate films were coated over with small nearly round bodies presenting no structure, or at least only feeble traces of it, and perhaps to be called cells. I had not sent gun-cotton, as I intended, to Mr. Crookes, fearing the rules of the post; otherwise there would have been more certainty that the bodies spoken of did not exist previously on the cotton. However, Mr. Dancer, who has examined cotton with the microscope oftener than most persons, even of those experienced in the subject, had never observed a similar appearance.

"The liquid had also a number of similar bodies floating in it.

"It was then that Mr. Crookes sent a liquid which he had condensed from the air of an infected cowshed at a space a little above the head of a diseased cow. This was also examined, and it presented similar indications of very numerous small bodies. Not being a professed microscopist, I shall not attempt a description, but add that they clearly belonged to the organic world, and were not in all cases mere débris. We found also one body a good deal larger than the rest; it resembled somewhat a *Paramecium,* although clearly not one.

"We found no motion whatever; and only this latter substance could be adduced as an absolute proof of any living organized being present. Next day I examined the same liquid; and, whether from the fact of time being given for development or from other causes, there was a very abundant motion. There were at least six specimens in the field at a time, of a body resembling the *Euglena,* although smaller than I have seen it. When these minute bodies occur, it is clear that more may exist; and germs in this early stage are too indefinite to be described. The existence of the vital spark in the organic substances in the air alluded to is all I wish to assert, confirming by a different method the observations of others. It might, of course, be said that since the bottle was opened at Mr.

Dancer's the air at that place may have communicated them. I answer that, before it was opened, a good glass could detect floating matter; some of it, however, as in the microscope, proved indefinite enough.

"Finding this, and fearing that the long time needful to collect liquid from the atmosphere might expose it also to much dust, I used a bottle of about 100 cubic inches dimensions, and putting into it a very little water, not above five cubic centimetres, I pumped out the air of the bottle, allowing the air of the place to enter. This was done six times for each sample, the water shaken each time, and the result examined. This was done with the same bottle that was used in my early experiments with permanganate, and by the same method, except that water instead of that salt was used. At first considerable numbers of moving particles were found; but it was needful to examine the water used, and here occurred a difficulty. It was not until we had carefully treated with chemicals and then distilled the water again and again that we could trust it. Particles seemed to rise with the vapour; and if so, why not with the evaporating water of impure places.

"Having kept an assistant at the work for a week, and having myself examined the air of three cow-houses, I came to the conclusion that the air of cow-houses and stables is to be recognized as containing more particles than the air of the street in which my laboratory is, and of the room in which I sit, and that it contains minute bodies, which sometimes move, if not at first, yet after a time, even if the bottle has not been opened in the interval. There is found in reality a considerable mass of débris, with hairs or fine fibres, which even the eye, or at least a good pocket-lens, can detect. After making about two dozen trials, we have not been able to obtain it otherwise. Even in the quiet office at the laboratory there seemed some indications.

"I found similar indications in a cow-house with healthy cows; so I do not pretend to have distinguished the poison of cattle-plague in these forms; but it is clear that where these exist there may be room for any ferment or fomites of disease; and I do not doubt that one class is the poison itself in its earliest stage. It would be interesting to develope it further.

"I have recorded elsewhere that I condensed the liquid from the air of a flower-garden, and found in it, or imagined I found, the smell of flowers. I do not remember that I looked much to the solid or floating particles, thinking them to be blown from the ground; but it does not affect the result, whether they be found constantly in the air or are raised by the action of currents.

Lately I tried the same plan on a larger scale. A bottle was filled with air and shaken with water. The bottle was again filled and shaken with the same water; and this was repeated 500 times, nearly equal to 2½ million cb. c., or 2495 litres*. As this could not be done in a short time, there was considerable variety of weather—but chiefly dry, with a westerly wind. The operation was conducted behind my laboratory, in the neighbourhood of places not very clean, it is true, but from which the wind was blowing to other parts of the town. I did not observe any dust blowing; but if there were dust, it was such as we may be called on to breathe. The liquid was clouded, and the unaided eye could perceive that particles, very light, were floating. When examined by a microscope, the scene was varied in a very high degree; there was evidently organic life. I thought it better to carry the whole to Mr. Dancer and to leave him to do the rest, as my knowledge of microscopic forms is so trifling compared with his."

ADDITION, MARCH 1870.

I certainly considered that I saw motion caused by

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* I think the total quantity is not correct; but it is unimportant.

life; Mr. Dancer says, "few living organisms were noticed." However, I defer to him in all matters connected with the microscope. There were many forms evidently organic, although not in motion. My belief was that they might be developed by care, as I had treated others similarly many years ago. In a memoir "On the Air and Water of Towns" (Report of the British Association, 1848), speaking of matter from the breath, I said, "If it be allowed to stand for a few days (about a week is enough), it will then show itself more decidedly by becoming the abode of small animals," &c.; and when speaking of the condensed matter on glass and walls, it was said, "If allowed to stand some time, it forms a thick apparently glutinous mass; but when this is examined by a microscope, it is seen to be a closely matted confervoid growth, or, in other words, the organic matter is converted into confervæ, as it probably would have been into any kind of vegetation that happened to take root." I was quite familiar, therefore, with the idea of developing these germs; the matter is found in the exhalations, and on that they may feed. It seems as if a choleric germ or a plague germ might grow there indifferently; and I do not see any thing more mysterious than natural action, which, however, is wonderful enough.

XXI. *Microscopical Examination of the Solid Particles collected by Dr. Angus Smith from the Air of Manchester.*

By J. B. Dancer, F.R.A.S.

Read March 31st, 1868.

The air had been washed in distilled water, and the solid matter which subsided was collected in a small stoppered bottle, and on the 13th of this month Dr. Smith requested me to examine the matter contained in this water. An illness prevented me from giving it so much attention as I could have wished.

The water containing this air-washing was first examined with a power of 50 diameters only, for the purpose of getting a general knowledge of its contents; afterwards magnifying-powers varying from 120 to 1600 diameters were employed.

During the first observations, few living organisms were noticed; but, as it afterwards proved, the germs of plant and animal life in a dormant condition were present.

I will now endeavour to describe the objects found in this matter, and begin in the order in which they appeared most abundant.

1st. *Fungoid Matter.*—Spores or sporidia appeared in numbers; and, to ascertain as nearly as possible the numerical proportion of these minute bodies in a single drop of the fluid, the contents of the bottle were well shaken, and then one drop was taken up with a pipette; this was spread out by compression to a circle ½ an inch in diameter. A magnifying-power was then employed which gave a field of view of an area exactly 100th of an inch in diameter, and it was found that more than 100 spores were contained in this space; consequently the average number of spores in a single drop would be 250,000. These spores varied from the 10,000th to the 50,000th of an inch in diameter. The peculiar molecular motion in the spores was observable for a short time, until they settled on to the bottom of the glass plate; they then became motionless.

The mycelia of these minute fungi were similar to that of rust or mildew (as it is commonly named), such as is found on straw or decaying vegetation.

When the bottle had remained for 36 hours in a room at a temperature of 60°, the quantity of fungi had visibly increased, and the delicate mycelial thread-like roots had completely entangled the fibrous objects contained in the bottle and formed them into a mass.

On the third day a number of ciliated zoospores were observed moving freely amongst the sporidia. I could not detect any great variety of fungi in the contents of the bottle; but I cannot presume to say that all the visible spores belonged to one species; and as there are more than 2000 different kinds of fungi, it is possible that spores of other species might be present, but not under conditions favourable for their development. Some very pretty chain-like threads of conidia were visible in some of the examinations.

The next in quantity is vegetable tissue. Some of this formed a very interesting object, with a high power, and the greater portion exhibited what is called pitted structure. The larger particles of this had evidently been partially burnt and quite brown in colour, and were from coniferous plants, showing with great distinctness the broad marginal bands surrounding the pits; others had reticulations small in diameter. They reminded me of perforated particles so abundant in some kinds of coal.

The brown or charred objects were probably particles of partially burnt wood used in lighting fires.

Along with these reticulated objects were fragments of vegetation, resembling in structure hay and straw and hay seeds, and some extremely thin and transparent tissue showing no structure. These were doubtless some portions of weather-worn vegetation. A few hairs of leaves of plants, and fibres similar in appearance to flax, were seen; and, as might have been expected in this city, cotton filaments, some white, others coloured, were numerous—red and blue being the predominant colours. A few granules of starch were seen by the aid of the polariscope; and several long elliptical bodies, similar to the pollen of the lily, were noticed. After this dust from the atmosphere had been kept quiet for three or four days, animalcula made their appearance in considerable numbers, the monads being the most numerous. Amongst these were noticed some comparatively large specimens of *Paramecium aurelia*, in company with some very active Rotifera; but after a few days the animal life rapidly decreased, and in twelve days no animalcula could be detected.

Hairs of Animals.—Very few of these were noticed, with the exception of wool; of this both white and coloured specimens were mixed up along with the filaments of cotton.

After each examination as much of the drop of water as could be collected by the pipette was returned to the bottle, in order to ascertain if any new development of animal or vegetable life would take place, and the stopper of the bottle was replaced as quickly as possible to prevent the admission of the particles from the air in the room; and I am tolerably certain that the objects named in this paper are those which the bottle contained when Dr. Smith brought it to me.

The particles floating in the atmosphere will differ in character according to the season of the year, the direction of the wind, and the locality in which they are collected, and, as might be expected, are much less in quantity after rain.

The small amount of fluid now remaining in the bottle emits the peculiar odour of mildew; and at present the fungoid matter appears inactive.

For the purpose of obtaining a rough approximation of the number of spores, or germs of organic matter contained in the fluid received from Dr. Smith, I measured a quantity by the pipette and found it contained 150 drops of the size used in each examination. Now, I have previously stated that in each drop there were about 250,000 of these spores;

and as there were 150 drops, the sum total reaches the startling number of 37½ millions; and these, exclusive of other substances, were collected from 2495 litres of the air of this city*—a quantity which would be respired in about 10 hours by a man of ordinary size when actively employed. I have to add that there was a marked absence of particles of carbon amongst the collected matter.

XXII. *On the Mean Monthly Temperature at Old Trafford, Manchester,* 1861 *to* 1868, *and also the Mean for the Twenty Years* 1849 *to* 1868. By G. V. VERNON, F.R.A.S., F.M.S.

Read before the Physical and Mathematical Section, December 7th, 1869.

IN Vol. I., 3rd Series, of the Memoirs of the Society, in a paper "On the Irregular Oscillations of the Barometer at Manchester," I gave reductions of the mean monthly temperatures observed by myself for 1849 to 1860, and in the present communication I have given the values for the succeeding years down to the end of 1868, completing a period of 20 years. It is scarcely necessary to remark that these values have all been carefully reduced to the Greenwich standard, and corrected by means of the Tables of Diurnal Range, computed and published by Mr. Glaisher.

As will be seen by the notes appended to Table I., I have been indebted for a few months' observations to Mr. Mackereth's observations, made at Eccles, and which closely represent those made at Old Trafford.

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* Behind Dr. Angus Smith's laboratory.

Table II. contains the difference of the mean temperature of each month from that of the whole period, 1849 to 1868; but unfortunately the month of August is almost entirely deficient in the earlier series, and this is a gap I see no chance of filling with observations made at any station which would be at all fairly comparable with the remainder of the series.

The mean temperatures of the months of the various periods have been as follows:—

	1849 to 1860.	1861 to 1868.	1849 to 1868.
	°	°	°
January	38·3	37·2	37·8
February	37·8	38·9	38·2
March	41·3	40·1	40·8
April	46·6	46·8	46·6
May	51·8	51·7	51·7
June	57·5	56·4	57·0
July	60·5	58·3	59·6
August	...	57·6	58·7 *
September	55·2	55·1	55·1
October	48·5	49·0	48·7
November	41·2	41·1	41·2
December	38·9	40·4	40·4
Mean	...	47·3	47·9

The mean of the period 1861 to 1868 appears to have been on the whole rather colder than the average of the last twenty years, and apparently chiefly owing to the lower mean temperature of the month of July—July only exceeding 60°·0 in two years, 1865 and 1868, whilst in the earlier period July in six years exceeded 60°·0, viz. in 1850, 1852 (67°·9), 1854, 1855, 1857, 1859.

On examination of the mean variations of temperature for each month (Table II.), we find that the greatest amount of variation of the mean monthly temperature may be expected in February, and the least in October;

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* From 9 years only.

or, arranging the months in their order of amount, we have:—

	°		°
October	1·50	March	2·17
April	1·67	July	2·35
September	1·89	November	2·37
May	1·91	August	2·61
January	2·02	December	3·01
June	2·06	February	3·26

One point in this seems somewhat curious; and that is, that the wettest and driest months should be liable to about the same changes of temperature; the distribution seems very irregular, months widely apart coming next one another as regards this element of temperature.

At the bottom of Table II. I have given the values of the probable variation of mean temperature for each month, computed from my own observations for the twenty years; and as the late Manuel J. Johnson gave similar values, computed from Dalton's observations, 1794 to 1818 (Radcliffe Observations, vol. xv. 1854), I annex them for comparison.

	Dalton. 25 years, 1794 to 1818.	Old Trafford. 20 years, 1849 to 1868.
	°	°
January	±2·7	±1·80
February	1·8	3·50
March	1·6	1·93
April	2·1	1·49
May	1·9	1·70
June	1·6	1·84
July	1·8	2·09
August	1·2	2·48
September	1·6	1·68
October	1·7	1·33
November	2·0	2·11
December	±1·8	±2·68

There are evidently great discrepancies between the two series of values, which it is quite out of my power to explain; but reference to the monthly means for Old Trafford show that the probable variations during the period 1849–1868 must have been much greater for some of the months than those given from Dalton's earlier period, especially in the month of February. In 15 years of the Old-Trafford observations, the variation of the mean temperature of February from the mean of the period exceeded 1°·8 (the amount from Dalton's observations) very considerably. The same may be said of other months, but not to the same extent.

Table I.—Mean Monthly Temperature at Old Trafford, Manchester, 1861–1868.

Year.	Jan.	Feb.	Mar.	Apr.	May	June	July	Aug.	Sep.	Oct.	Nov.	Dec.
	°	°	°	°	°	°	°	°	°	°	°	°
1861	35·0	40·0	43·0	44·4	49·3	56·6	59·3	……	55·2	52·8	38·0	39·5
1862	38·1	41·0	42·0	41·2	47·8	48·0	56·7	58·0	54·7	49·1	36·8	43·0
1863	42·4	41·8	43·8	47·6	50·9	57·0	51·5	52·4	47·7	48·9	45·0	42·9
1864	35·6	35·2	38·8	48·3	54·2	56·0	58·3	55·6	54·7	49·0	43·0	48·5
1865	35·0	35·5	36·5	50·5	53·9	59·8	61·6	58·1	60·9	49·0	43·1	41·9
1866	39·7	34·7	35·5	47·3	48·9	59·6	57·0	57·5	53·8	49·0	43·7	41·6
1867	33·2	39·9	37·2	48·7	53·3	56·6	58·4	60·5	56·2	47·7	38·7	38·4
1868	38·9	42·8	43·8	46·2	55·3	57·5	63·3	61·3	57·6	46·6	40·8	44·1
Means, 1861 to 1868	37·2	38·9	40·1	46·8	51·7	56·4	58·3	57·6	55·1	49·0	41·1	40·4

August, 1862.—Determined by reduction from the observations made at Eccles by Thomas Mackereth, Esq., F.R.A.S. I may remark here that our observations are nearly identical, or, where different, seem to be so almost by a constant difference.

January, 1863.—From Mr. Mackereth's observations.

April, May, June, July, August, and September, 1868.—Taken from Mr. Mackereth's observations.

TABLE II.—Difference between the Mean Temperature of each Month and the Average of the same Month for Twenty Years.

Year.	Jan.	Feb.	March.	April.	May.	June.	July.	August.	Sept.	Oct.	Nov.	Dec.
	°	°	°	°	°	°	°	°	°	°	°	°
1849	……	……	+2·0	−1·9	+1·6	−0·7	+0·2	……	+1·0	−0·6	+2·7	−1·8
1850	−3·4	+5·9	0·0	+2·1	−1·2	+1·6	+0·8	……	−1·2	−3·1	+3·7	−0·9
1851	+3·6	+1·2	+1·7	……	−1·0	……	−1·4	……	−1·5	+2·8	−5·6	+0·3
1852	+2·2	+1·8	+0·3	+1·4	+1·0	+1·6	+8·3	……	−0·8	−4·1	+3·8	+4·8
1853	+2·8	−5·4	−2·6	−1·0	−0·1	+1·6	−1·0	……	−1·0	……	+0·5	−5·2
1854	−0·1	+0·8	+3·2	+2·8	+0·6	+0·1	+0·7	……	+2·1	−1·7	−0·9	−0·6
1855	−1·1	−9·6	−2·6	−0·5	−3·2	+0·1	+2·2	……	−0·2	−0·2	+0·3	−4·8
1856	−0·1	+2·9	+0·3	+0·6	−1·5	−1·3	−0·9	……	−0·9	+3·1	−2·0	−1·9
1857	−1·3	+1·1	+1·0	−0·2	+1·0	+4·2	+1·0	+4·2	+2·6	+3·3	+2·6	+4·8
1858	+0·1	−2·9	−0·2	−0·2	−1·8	+6·3	−1·7	+3·0	+4·1	−0·5	−2·0	+0·4
1859	+3·3	+2·9	+4·0	−1·4	+2·6	+3·2	+4·7	……	+0·1	−0·2	−1·6	−6·3
1860	−0·1	−3·7	−1·0	−2·2	+2·8	−1·8	−2·0	……	−3·4	−0·1	−1·2	−5·9
1861	−2·8	+1·8	+2·2	−2·2	−2·4	−0·4	−0·3	……	+0·1	+4·1	−3·2	−0·9
1862	+0·3	+2·8	+1·2	−5·4	−3·9	−9·0	−2·9	−0·7	−0·4	+0·4	−4·4	+2·6
1863	+4·6	+3·6	+3·0	+1·0	−0·8	0·0	−8·1	−6·3	−7·4	+0·2	+3·8	+2·5
1864	−2·2	−3·0	−2·0	+1·7	+2·5	−1·0	−1·3	−3·1	−0·4	+0·3	+1·8	+8·1
1865	−2·8	−2·7	−4·3	+3·9	+2·2	+2·8	+2·0	−0·6	+5·8	+0·3	+1·9	+1·5
1866	+1·9	−3·5	−5·3	+0·7	−2·8	+2·6	−2·6	−1·2	−1·3	+0·3	+2·5	+1·2
1867	−4·6	+1·7	−3·6	+2·1	+1·1	−0·4	−1·2	+1·8	+1·1	−1·0	−2·5	−2·0
1868	+1·1	+4·6	+3·0	−0·4	+3·6	+0·5	+3·7	+2·6	+2·5	−2·1	−0·4	+3·7
Means ……	2·02	3·26	2·17	1·67	1·91	2·06	2·35	2·61	1·89	1·50	2·37	3·01
Probable variations.	1·80	3·05	1·93	1·49	1·70	1·84	2·09	2·48	1·68	1·33	2·11	2·68

XXIII. *On the Suspension of a Ball by a Jet of Water.*

By Osborne Reynolds, M.A., Professor of Engineering, Owens College, and Fellow of Queens' College, Cambridge.

Read March 8th, 1870.

When a ball made of cork, or any very light material, is placed in a concave basin, from the middle of which a jet of water rises to the height of four or five feet, the jet maintains the ball in suspension; that is to say, it takes and keeps it out of the basin. The ball is not kept in one position, it oscillates up and down the jet; nor is its centre kept exactly in a line with the jet, it often remains for a long time on one side of it. In fact, the ball appears to be in equilibrium when it is struck by the jet in a point about 45° below the horizontal circle. In this way, for some seconds at a time, the ball appears as though it were hanging to the jet, and then oscillates in an irregular manner about this position. If its oscillations become so great that it leaves the jet, it instantly drops, but in descending it generally comes back into the jet before it reaches the basin. The friction of the water causes the ball to spin rapidly; and as it moves about the jet, it spins sometimes in one direction, sometimes in another, always about a horizontal axis. Of the water which strikes the ball, part is immediately splashed off in all directions, part is deflected off at the tangent, and part adheres to the ball, and is carried round with it, until it is thrown off by centrifugal force.

The only explanations of this that appear to have been offered are based on one or the other of the following assumptions, viz. that the centre of gravity of the ball remains directly over the jet, or that the jet is accompanied by a current of air which tends to carry the ball into it. With respect to these assumptions, the fact that the ball will come back again into the jet when driven entirely away from it must upset the truth of the first, and at the same time it appears to establish the truth of the second. However, some experiments, which will be subsequently described, made with a view to ascertain if this current exists, show that it does not. Besides which, Mr. Routledge and Mr. Wild have made some experiments. The former found that when the jet, directed horizontally to avoid the influence of the falling drops, was brought very near to a light ball suspended by a thread, the ball showed no tendency to move towards the jet; and Mr. Wild settled the point by showing that the action of the ball is the same in a vacuum as it is in air. It appears, then, that neither of these assumptions is satisfactory.

Now, of the forces which act on the ball, its weight acts at its centre in a vertical line, and is the only force which is not due to the action of the water. When the jet strikes the ball directly underneath, it will produce a force acting upwards in a vertical line, the magnitude of which depends on the height, and may therefore balance the weight of the ball. In this position the ball is, by the action of these two forces, in equilibrium, in the same manner as if it were balanced on a point. The slightest deviation in the jet will upset it; and then the jet will strike it on one side of the vertical line through its centre: when so struck, the forces at the point of contact may be resolved into two, of which one acts along the normal to the surface, or through the centre of the ball, and is due to the impulsive action of the water (this is called P), and another in the tangent plane at the point of contact (p) (this is due to the friction of the water, and is called R). If W be the weight of the ball, then P, R, and W are the only forces which at first sight appear to exist; and the question is, can the ball be in equilibrium under the action of P, R, and W? This question is easily answered; for these forces are necessarily in the same plane; but they do not all pass through the same point, and therefore they are not in equilibrium. To balance these forces, then, there must be some other force acting on the ball in the same plane with them, and which does not pass through *p*, or the centre of the ball. Now, besides the water which leaves the ball at *p*, there is the water which adheres to the ball until thrown off by centrifugal force; and to this we must look for the required force. The effect of a drop adhering to the ball will be very complex, being due to its weight, centrifugal force, and friction. However, if we neglect the weight as being very small, and therefore only able to increase slightly the weight of the ball, and to shift the point at which it acts a little way from the centre, the forces which the drop will produce may be stated accurately. For whenever a drop whose weight is w (lbs.) comes on to the ball with a velocity v (feet per second), and leaves with a velocity u, its whole effect, minus that of its weight while it is on the ball, is equivalent to a force $\frac{wv}{g}$ (lbs.) acting for one second in the direction in which the drop was moving and at the point at which it comes on to the ball, and a force $\frac{wu}{g}$ at the point at which it leaves and in a direction opposite to that in which it flies off. The first of these forces will form part of P and R; and therefore, besides the forces at the point P, the effect of any adhering drop will be equivalent to a reaction such as would be produced if the force necessary to throw the drop from the ball were concentrated at the point at which it leaves. If several drops be leaving the ball at the same time, the several reactions will have a single resultant, which will not pass through *p*, or the centre, unless they should be distributed equally all round the ball, in which case the reactions would simply produce a couple on the ball, and would not have a single resultant. If the drops are not leaving equally all round, the resultant will act in a direction opposite that to in which the greatest number fly off. If, then, more water is thrown off in one direction than in another, and this direction is the same as that of the resultant of the three forces P, R, and W, this water will produce a force such as it has been shown must exist. First, then, is there any reason why more water should be thrown off in one direction than in another? and, second, in what direction will that be? The water comes on to the ball at *p*, and that which adheres is at first spread out in the form of a thin film, on which centrifugal force immediately acts to collect it at the equator. As it collects at the equator, the adhesion becomes less, compared with the mass of water, and the drops separate themselves and fly off; in this way the water would begin to leave at *p*, and go on until it was all thrown off, so that much more water would leave above *p* than below. But, besides this, the weight of the water will tend to keep it on or to throw it off, and its action to keep it on will be greatest up to the top, after which the conditions for its leaving become more favourable; so that the water may begin to leave at *p*, or not till it has passed over the top of the ball; but in either case most of the water will be thrown off before it gets below the horizontal circle on the opposite side to *p*. On examining the ball, it appears that the water begins to leave at the top. But in either case by far the greater part of the water flies away from the jet.

It was the discovery of this fact which has enabled me to explain the phenomenon; for this water causes a resultant reaction, which is the additional force necessary to maintain the equilibrium of the ball.

Let this resultant reaction be called Q: it will act towards the jet, and its effect will be, first, to force the ball into the jet, and so will help to counteract the obliquity of P; secondly, it will assist in supporting the ball; and, thirdly, since it opposes the rotation, it will balance the tangential force R, caused by the friction at p; and, provided it have the proper magnitude, together with the forces P, R, and W, it is all that is requisite to explain the equilibrium.

It remains to explain the fact, that the ball will fall back again into the jet after it has been driven out of it. This may be done; for the force P which forces the ball out, ceases as soon as contact ceases; but not so with Q, which drives the ball back again towards the jet; for there will still be some water to be thrown off, so that perhaps for half a revolution Q will continue undiminished, and so bring the ball back again into the jet.

Position of Equilibrium.

With respect to the position of the ball when in equilibrium, nothing very definite can be established, as there are no known laws of adhesion; but it may be shown by general reasoning, that there are limits between which the point p must be, so that there may be equilibrium.

Let the point p be at a fixed height, and let P equal the full force of the jet at this height when acting on the bottom of the ball or on a perpendicular plane. Then, if a be the angle which the normal at p makes with the vertical,

$$P = P' \cos a,$$

and the horizontal component

$$P \sin a = \frac{P'}{2} 2 \sin a \cos a = \frac{P'}{2} \sin 2a;$$

therefore

$$P \sin a = \frac{P'}{2} \text{ and is a maximum when } a = 45^\circ,$$

and

$$P \sin a = 0 \text{ when } a = 0 \text{ or } a = 90^\circ;$$

so that the tendency of the jet to force the ball to one side increases from nothing to $\frac{P'}{2}$ as p moves from the bottom to a point at which the normal makes an angle of 45° with the vertical, and then decreases to nothing as p moves to the middle of the ball.

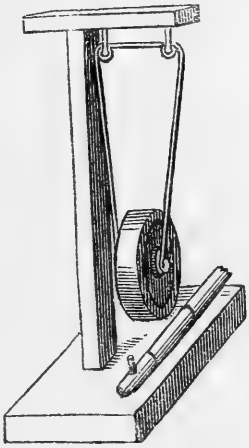
The force Q may be fairly assumed to increase as the speed of rotation increases; and this will be as the point of contact moves from the bottom to the middle of the ball. In the same way the force R, which will necessarily increase as Q increases, will increase as p moves from the bottom to the middle of the ball; and its horizontal component will follow nearly the same law as that of P.

Considering, then, the horizontal forces only, there must be some position for p in which the horizontal component of Q and R will be equal to that of P; and if a horizontal circle be drawn through this point, it will limit the part of the ball in which equilibrium is possible.

For any deviation without this circle the equilibrium will be stable; *i.e.* if the centre of the ball gets so far from the jet that the ball is struck in some point without this circle, it will come back again. As to the nature of the equilibrium for any deviation within this circle, I cannot speak positively; but it is probably nearly neutral all over the enclosed area. This seems to agree very well with the fact that the ball is in equilibrium when struck 45° below its horizontal circle and oscillates about this position.

The following is a description of some experiments. The object was to ascertain :—first, whether or not air is the medium by which the water acts on the ball; secondly, how far the horizontal equilibrium of the ball depends on its rotation; and, thirdly, what is the exact position of the point in which the ball must be struck so as to be in equilibrium, and, moreover, what is the nature of the equilibrium.

The apparatus employed in these experiments consisted of a wheel three inches in diameter and half an inch broad at the rim, made of painted wood, capable of turning very freely about its axis, and suspended by two wires, with its axis horizontal, so that it could swing like a pendulum. A vertical jet of water was so arranged that it could be made to strike the reel at any point from below, or to miss it altogether. This was done by bringing the jet out of a horizontal pipe which would slide backwards and forwards in the same direction as the wheel could swing. This pipe was furnished with a cock, so that the force of the jet might be altered.



In experiment No. 1, the pipe from which the jet issued was pushed so forward that the jet missed the reel by about an inch, and the jet was turned on to rise about six feet above the reel; the pipe was then brought back until the water passed as near as possible to the reel without touching it—but there was no apparent effect produced on the reel. The tap was turned so as to increase and then diminish the height to which the jet rose—still, without any effect.

Experiment No. 2 was made with the same apparatus as No. 1. The reel was then changed for one six inches in diameter, and the same experiment repeated.

The jet was placed so that it missed the reel (when hanging freely) by about two inches, and the water was turned on to rise about six feet. The reel was then pushed forward until it touched the jet and then let go; it immediately began to turn about its axis, but left the jet, swinging backwards and forwards, touching the jet each time, and each time gaining in speed of rotation. This went on for several oscillations; but as it got to turn faster, it appeared to stick to the jet for an instant before letting go; and having done this once or twice, it stuck to the jet altogether, and remained in contact with it, spinning rapidly. The experiment was then repeated with the jet at different distances, and with the larger wheel; the result was the same in all cases. I found it possible, however, either to increase or to diminish the force of the jet enough to prevent the reel from remaining in contact with it. The limits were about 2 and 8 feet.

In experiment No. 3, the position of the reel when free was carefully marked, so that the least alteration could be noticed, and the jet was placed directly under its centre. In this position the jet did not cause the reel to move to either side in particular, but to oscillate backwards and forwards. The jet was then pushed slowly forwards, and the motion of the ball watched. At first it moved away from the jet slightly, and remained away until it was struck about 60° from its lowest point, after which it gradually came back to its initial position, which it reached when struck about 65° from its lowest point.

The forward motion of the jet being continued, the ball began to follow the jet, the point in which it was struck moving upwards very slowly. When the reel finally fell from the jet and came back into its initial position, the jet missed it by about 2½ inches.

During the experiment the force of the jet was altered; but within moderate limits this did not affect the position of equilibrium.

This clearly shows that the position of equilibrium is about 25° from the horizontal circle, and for any deviation below this the equilibrium is much more nearly neutral than for any deviation above it.

XXIV. *On the Composition of the Water of the Irish Sea.*

By T. E. Thorpe, Ph.D., and E. H. Morton.

Read March 22nd, 1870.

Thanks to the investigations of Forchhammer, Von Bibra, Bischof, and others, our knowledge concerning the nature and distribution of the saline constituents of sea-water, and of the causes of the variations in its composition as observed in various parts of the world, is tolerably extensive and precise. English chemists, however, have contributed next to nothing to the general stock of our information on this subject. This is not a little remarkable, especially when we consider the peculiarly favourable condition in which this country is placed for researches of this kind, by reason of its insular position. A few observations by John Davy, made in the course of his long voyages, two memoirs by Marcet in the 'Philosophical Transactions' for 1819 and 1822, on the temperature and saltness of various seas, and an elaborate analysis by Schweitzer of the water of the English Channel, made in 1838*, constitute by far the chief portion of the work done in this direction by English chemists. The chemical history of the sea is mainly to be derived from the researches and observations of chemists, principally French and German, the majority of whom were located at considerable distances from the sea-board, and who laboured, therefore, under all the disadvantages which this circumstance necessarily entails.

So far as we can learn, the water of the Irish Channel has never been analyzed. We have been induced, there-

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* Phil. Mag. xv. 1839.

fore, to undertake its analysis, in the hope of supplying information respecting the nature and extent of the modifications effected in the composition of sea-water by its proximity to our coasts. Accordingly Captain Temple, of the 'Bahama Bank,' light-ship, kindly collected for us a quantity of the water in the immediate neighbourhood of his vessel. The vessel is situated in lat. 54° 21′ N., and long. 4° 11′ W., seven miles W.N.W. of Ramsey, Isle of Man, and is placed nearly equidistant from the shores of England, Scotland, and Ireland. During the greater part of the day a strong current setting in from the south, probably from the Atlantic, flows past the ship into the North Channel, and thence again into the ocean. The water, therefore, taken for analysis, was originally that of the deep ocean, which had traversed almost the entire length of the Irish Channel, and had consequently been exposed to all the influences due to the neighbouring seaboard, and to the influx of the numerous rivers along the coasts.

The water was obtained in the early part of January 1870. The meteorological conditions at the time of collection, and for some time previously, were in no wise remarkable. The analysis was commenced immediately on receipt of the water. Its specific gravity, compared with distilled water, free from air, and possessing the same temperature, was found to be

At 0° C.	1·02721
15° C.	1·02484

These numbers differ but slightly from that usually accepted as representing the mean specific gravity of the water of the ocean. The water of the Atlantic, according to Von Horner, possesses the specific gravity 1·02875 at 0°; that of the English Channel at 15°·5 was found by

Schweitzer to be 1·0271; on nearing the land the specific gravity fell to 1·0268.

The following substances are stated by Forchhammer to exist in sea-water*:—

Acids, or elements replacing them: Sulphuric, carbonic, phosphoric, silicic, boracic, nitric, arsenic, bromine, chlorine, iodine, fluorine.

Bases: Sodium, potassium, magnesium, lithium, cæsium, rubidium, ammonia, strontium, barium, iron, manganese, aluminium, zinc, cobalt, nickel, lead, copper, and silver.

In addition to these are variable quantities of organic matter, concerning the true nature of which we know as yet absolutely nothing. Dr. Angus Smith and Mr. Hunter, however, have already applied the permanganate reaction to determine the amount of this organic matter. None of the peculiar organic substances, such as crenic and apocrenic acids, butyric, acetic, propionic, and formic acids, discovered in several mineral waters on the Continent, have been detected in sea-water. Of the thirty-one elements occurring in sea-water, the quantitative determination of nine or ten is alone possible. These are sodium, magnesium, potassium, calcium, iron, chlorine, bromine, and sulphuric and carbonic acids. We have further attempted to estimate the amount of nitric acid and ammonia. The remaining constituents have either been detected in the ashes of sea-plants and animals, whence their existence in sea-water has been inferred, or, as in the case of phosphoric acid and fluorine, they have been found in the boiler-deposits of sea-going steamers.

I. *Estimation of the Total Solid Contents.*

The weighed quantity of water was evaporated to dryness in a platinum crucible, and the dried mass exposed to

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* Proc. Royal Soc. xii. p. 130, 1862.

a temperature of about 180° C., until its weight appeared to be constant. Sea-water contains a notable quantity of magnesium chloride, which decomposes during the process of drying, evolving hydrochloric acid. This source of error was easily obviated by adding, as recommended by Mohr, a small known quantity of recently ignited sodium carbonate to the water, when the magnesium chloride is converted into the more stable carbonate.

	Water taken. grams.	Residue obtained. grams.	Amount in 1000 grams of water. grams.
I. ...	51·1564	1·7309	33·8360
II. ...	50·7608	1·7178	33·8411
		Mean............	33·83855

According to Forchhammer, the amount of the total saline constituents of the water of the northern portion of the Atlantic, between the parallels 30° N. lat. and a line from the north of Scotland to the north of Newfoundland, is subject to very slight variation: the mean quantity amounts to 35·976 grams per 1000. It is therefore evident from the above result, that the relative amount of solid matter contained in sea-water is perceptibly diminished in the neighbourhood of coasts; and this in the case of the Irish Channel can only arise from the admixture of fresh water flowing down in the form of rivers from the land. This conclusion is fully borne out by the older analyses of Clemm, Figuier and Mialhe, and Bischof, of the water of the German Ocean, collected in the neighbourhood of the coasts. In no case did the amount of saline constituents exceed 33 grams per 1000. (Min. 30·5, max. 32·8.)

II. *Estimation of the Sulphuric Acid, as Barium Sulphate.*

	Water taken. grams.	$BaSO_4$ obtained.	SO_4 in 1000 grams.
I. ...	102·272	0·6447	2·5972
II. ...	100·206	0·6295	2·5884
		Mean	2·5928

In the older analyses, the sulphuric acid was calculated as SO_3: the above number reduced to the amount corresponding to this formula is 2·16075. Probably no constituent of sea-water is subject to greater variations in amount than the sulphuric acid. This may be due to several causes, among which may be enumerated (1) the varying amounts of sulphates brought down by rivers, and (2) the fact that the sulphuric acid contained in sea-water is frequently reduced in the presence of organic matter to sulphuretted hydrogen*. The variation in the Atlantic, according to Forchhammer, may amount to 0·0145 per cent. (max. 0·2436 per cent., min. 0·2289 per cent.). The minimum quantity, it will be observed, somewhat exceeds that found in the water of the Irish Channel, again showing the influence of the influx of fresh water from the rivers. The water of the English Channel, according to Bischof, contains a similar amount of SO_3, viz. 0·2141 per cent., with which our result agrees perfectly.

III. *Determination of the Total Amount of Lime.*

We found some little difficulty at the outset in exactly determining the proportion of this constituent by the ordinary method of separation, owing to the facility with which varying quantities of magnesia coprecipitate with the calcium oxalate. By repeatedly dissolving the mixed oxalates in hydrochloric acid, and reprecipitating the calcium salt by the addition of ammonia and a few drops of ammonium oxalate, we at length obtained it perfectly free from ad-

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* See Hayes, Sillim. Americ. Journ. March 1851, p. 241.

mixed magnesium oxalate. The precipitate was collected, dried, ignited, and weighed as caustic lime.

	Water taken.	CaO obtained.	Amount in 1000 grms.
I.	153·138	0·0878	0·57466
II.	153·328	0·0883	0·57589
III.	153·266	0·0881	0·57482
		Mean	0·57512

The amount of lime contained in the water of the Irish Channel is somewhat less than that usually found in the Atlantic Ocean,—according to Forchhammer 0·0597 per cent.

IV. *Estimation of the Magnesia, determined in the filtrates obtained in the foregoing estimations.*

	Water taken.	P.	MgO in 1000 grms.
I.	153·328	0·8649	2·03275
II.	153·266	0·8642	2·03192
		Mean	2·03233

According to the authority already quoted, the amount of magnesia contained in the water of the ocean far distant from land varies about 0·0093 per cent.; max. 0·2209 per cent., min. 0·2116 per cent.

V. *Estimation of the Calcium Carbonate.*

The lime contained in sea-water exists as sulphate and as carbonate, the latter salt being dissolved in an excess of free carbonic acid. On boiling the water the gas escapes, when the calcium carbonate separates out. Its amount was determined by boiling a weighed quantity of water for about an hour, taking care to add distilled water from time to time in order to prevent the precipitation of the calcium sulphate. The water on cooling was again weighed, divided into two portions, and filtered through dry filters;

by again weighing the filtrate, the amount of the original water employed in the several determinations was easily calculated. The lime in solution was then precipitated as oxalate, with the precaution indicated in the preceding paragraph.

	Water taken.	CaO obtained.	CaO in 1000 grms.
I.	... 205·860	0·1130	0·54892
II.	... 214·020	0·1173	0·54808
		Mean	0·54850

The total quantity of lime contained in the water of the Irish Channel amounted, according to the determinations contained in Section III., to 0·57512 gram per 1000. On subtracting from this the amount contained in 1000 grams of the boiled water existing in combination with sulphuric acid, the quantity of lime remaining amounts to ·02662 grm., equivalent to ·04754 calcium carbonate per 1000 grms. of water. The washed calcium carbonate contained a mere trace of magnesium carbonate. This amount of calcium carbonate, although agreeing with the older determinations of Bischof and Schweitzer, made on the water of the English Channel, is in all probability too low, on account of the solubility of the carbonate in solution of the alkaline chlorides. No sufficient data exist for supplying the proper correction to this result. According to John Davy, only in the vicinity of coasts does sea-water contain calcium carbonate. In the water of the ocean, far away from land, he failed to detect even a trace; and in the numerous analyses made by Von Bibra on specimens collected in various parts of the world, no mention is made of this ingredient.

VI. *Estimation of the Total Amount of Alkaline Chlorides.*

To the weighed portion of water was added a small quantity of barium chloride, in order to separate the sulphuric acid; the magnesia, together with the excess of the barium salt, was removed by boiling the solution with milk of lime in a silver dish. The water was then filtered, and the lime in solution precipitated by ammonium carbonate, and oxalate. On again filtering, adding a small quantity of hydrochloric acid, and evaporating to complete dryness, a minute amount of silica was rendered insoluble, together with the traces of magnesia which had escaped previous separation. This process was repeated until the chloride dissolved to a *perfectly* clear solution. The dried mass was then heated to dull redness, until it ceased to lose weight.

Water taken.	Mixed chlorides obtained.	Amount in 1000 grams.
57·1062	1·3875	27·15000
57·1440	1·3920	27·21725
	Mean	27·18363

In the foregoing process no account has been taken of the minute quantity of lithia present in sea-water, the whole of which would probably be contained in the mixed chlorides—partly in the state of chloride, partly in that of oxide. Although the presence of this substance in sea-water may easily be demonstrated by means of the spectroscope, a few decigrams of the solid residue amply sufficing for its detection, its quantitative determination has hitherto not been attempted, more probably on account of the imperfect nature of the methods employed in its separation than of the relatively minute quantity contained in the water. Indeed it is almost certain that the proportion of this element exceeds that of the ammonia or nitric acid, silica, or oxide of iron, the amounts of which substances in sea-water may be ascertained with some degree of precision.

VII. *Determination of the Potash and Soda contained in the mixed Chlorides.*

Probably none of the processes employed in the analysis of sea-water is so unsatisfactory as that generally used in the determination of the amount of sodium and potassium. The quantity of the latter element contained in sea-water is relatively very small; and the loss of potash in the ordinary method of separation, as the platinum salt, amounts, even under the most favourable circumstances, to upwards of one per cent. The difficulty in applying this method for this purpose has already been pointed out by Usiglio* in his "Memoir on the Composition of the Water of the Mediterranean." We have preferred, therefore, to determine the proportion of potash and soda contained in the mixed chlorides by the indirect method of estimating the amount of chlorine present in the mixture, and calculating from this the proportion of the two alkalies. This manner of proceeding is certainly more expeditious, and, we believe, if conducted with due care, quite as accurate as the other method.

	Mixed chlorides.	AgCl obtained.	NaCl.	KCl.	In 1000 grms. Na.	K.
I. ...	1·3875	3·3848	1·3492	0·0383	10·3890	·39300
II. ...	1·3920	3·3957	1·3540	0·0380	10·4150	·38963
				Means	10·4020	0·39131

VIII. *Determination of the Bromine.*

When silver nitrate is added to a cold solution of the alkaline bromides and chlorides, in quantity insufficient to precipitate the whole of the halogens, and the mixture of the silver salts allowed to remain in contact with the liquid for a few days, the whole of the bromine is removed from the solution, and is contained in the precipitate as silver bromide. Upon this principle is based the method

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* Ann. de Chimie, ser. 3. xxvii. 104.

which we have employed in the determination of the amount of bromine contained in the water of the Irish Sea. This method has been carefully studied by Fehling, and we have therefore followed the directions given by that chemist in his memoir*. To the measured quantity of sea-water, in each case a litre, weighing 1027 grms., a small quantity of solution of silver nitrate was added (about $\frac{1}{25}$ of that required to effect complete precipitation), and the liquid repeatedly shaken for eight days. The precipitates were then collected, thoroughly washed, dried, and weighed.

	Water taken. grms.	Amount of mixed silver salts.
I.	1027	2·85068
II.	1027	2·31020

The mixed silver salts were then transferred to porcelain boats, and heated in a stream of pure hydrogen until they ceased to lose weight. All the weighings were made by the method of vibrations, as described by Bunsen†. By this means a difference of one-hundredth of a milligram is easily detected.

Amount of AgCl and AgBr taken. grms.	Ag obtained. grm.	Equivalent to AgCl. grms.	On the original amount. grms.
3·39584	1·78195	2·36790	2·81736
2·11510	1·56640	2·08148	2·27347

Hence the amount of AgBr is

		Bromine in 1000 grms.
I.	0·14029	0·05827
II.	0·15501	0·06438
	Mean	0·06133

The presence of iodine in sea-water has not been conclusively demonstrated. Its existence in the water has simply been inferred from the fact of its being contained in

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* Journ. f. prakt. Chem. xlv. 269.

† Phil. Mag. 1867, xxxiv.

various fucoid plants. About 100 grms. of the dried residue of the water of the Irish Channel, evaporated with sodium carbonate, were digested with absolute alcohol for some days, the solution evaporated to dryness, and the solid matter again treated with absolute alcohol, filtered, and a second time evaporated to dryness. The dried salt was then dissolved in a few drops of water, a small quantity of clear starch paste added, together with two drops of a solution of nitrogen teroxide in sulphuric acid. Not the slightest coloration was perceptible. Assuming, with Stromeyer, that $\frac{1}{450000}$ part of iodine may be thus detected, and assuming, further, that the delicacy of the reaction is not interfered with by the presence of bromides or chlorides, it follows that the amount of iodine contained in sea-water cannot exceed 1 part in 100,000,000 of water, and is probably much less.

IX. *Determination of the joint amount of Chlorine and Bromine, by precipitation as Silver Salts.*

	Water taken. grms.	Silver salt obtained.	Containing AgCl.	Cl per 1000 grms.
I. ...	25·6110	1·9319	1·9283	18·6182
II. ...	25·6306	1·9352	1·9315	18·6348
			Mean	18·6265

X. *Estimation of the Ammonia.*

5237·7 grms. of the sea-water were boiled with a quantity of pure caustic soda (made from metal) in a distilling-apparatus until about two litres had passed over; this was a second time distilled through a good condensing-arrangement. The second distillate, containing all the ammonia, weighed 365·78. The greatest care was taken in the operation to prevent the solution absorbing ammonia from the atmosphere of the laboratory; and the vessels employed were perfectly clean and new. The ammonia in solution was then estimated by Nessler's method. One cubic centimetre of the solution of ammonium sulphate employed in the comparison was equivalent to 0·00010 of nitrogen. The amount of this solution required to give a tint equal to that afforded by 50 cubic centimetres of the distillate was (1) 6·2, (2) 6·9, (3) 6·2; mean, 6·4. Hence the ammonia in 1000 grms of water =0·000108.

XI. *Estimation of the Nitric Acid.*

The liquid remaining in the retort was carefully decanted, when clear, from the precipitate formed on adding the caustic soda solution; this precipitate was repeatedly washed, and the washings added to the main bulk of the solution. The entire quantity of the solution was then concentrated until about 20 cub. cent. only of liquid remained; this was filtered into a small flask; a large excess of pure caustic soda was then added, and the solution was boiled for about an hour, in order to remove completely any traces of ammonia which might have been absorbed from the air of the laboratory. When cold, two compound helices of zinc and iron were thrown into the solution, which was further treated according to the method of Vernon Harcourt. The ammonia contained in the distillate, derived from the action of the nascent hydrogen on the nitrate in solution, was estimated by Nessler's method. The weight of the distillate was 96·3 grms. One cubic centimetre of the ammoniacal solution used for comparison was equivalent to 0·000112 NO_3H. The amount of this solution required to afford a tint equal to that caused by 25 cub. cents. of the distillate was (1) 19·0, (2) 17·8, (3) 20·2; mean, 19·0 cub. cent. Hence the amount of nitric acid contained in 1000 grms. of sea-water is 0·001563 grm.

The ammonia and nitric acid contained in sea-water are doubtless produced by the decomposition of nitrogenous organic matter. Now, since this process of decomposition is continually going on to an enormous extent on the surface of the earth, we should naturally expect to find far more ammonia and nitric acid in sea-water than analysis shows to be actually present. The amount of the substances thus formed on the earth must be very large; but the quantity carried down to the sea by the rivers is exceedingly minute: we are acquainted with at least fifty analyses of rivers flowing directly into the sea; but in none does the proportion of nitrate exceed 1 part in 100,000 of water, even when the river receives the sewage matter of large towns. This remarkable disappearance of nitrate and ammoniacal salts is undoubtedly to be traced to the peculiar absorptive power of soils for salts containing nitrogen in a form available for the nourishment of plants. Way has shown that nitrates and ammoniacal salts are completely removed from solution on filtration through a layer of soil.

XII. *Estimation of the Iron.*

The precipitate formed in the retort on adding the caustic soda to the sea-water was assumed to contain the whole of the iron as ferric oxide. This precipitate was dissolved in dilute sulphuric acid, the iron reduced by means of zinc, and its amount estimated by a permanganate solution, of which one cubic centimetre was equivalent to 0·00106 of oxygen. Amount of Fe_2O_3 found in 1000 grms. =0·00465.

The following synoptical Table shows the mean results of the foregoing determination: the numbers express the amount of the various ingredients in 1000 grms. of the sea-water:—

1.	Chlorine	18·62650
2.	Bromine	·06133
3.	Sulphuric acid (SO_4)	2·59280
4.	Lime (total)	·57512
5.	Calcium carbonate	·04754
6.	Magnesia	2·03233
7.	Mixed alkaline chlorides	27·18363
8.	Potassium	·39131
9.	Sodium	10·40200
10.	Ferric oxide	·00465
11.	Ammonia	·00011
12.	Nitric acid	·00156
13.	Fixed constituents	33·83855

Comparison of the total amount of fixed constituents found directly, with the sum of the several constituents associated on the assumption that the strongest acid is combined with the strongest base, &c.

Sodium chloride	26·43918
Potassium chloride	·74619
Magnesium chloride	3·15083
Magnesium bromide	·07052
Magnesium sulphate	2·06608
Magnesium carbonate	traces.
Calcium sulphate	1·33158
Calcium carbonate	·04754
Lithium chloride	traces.
Ammonium chloride	·00044
Magnesium nitrate	·00207
Silicic acid	traces.
Ferrous carbonate	·00503
	33·85946
Amount directly determined	33·83855

The water employed in the foregoing analysis was collected in mid-winter; it becomes interesting to know if its composition is uniform during the various seasons of the year. Fortunately we can offer some evidence on this point. In August 1865, after a continuance of exceptionably fine weather, one of us collected some sea-water in the neighbourhood of the Bahama-Bank Light-ship, and determined the total quantity of its saline constituents, together with the amount of chlorine and sulphuric acid.

I. Total solid contents.

	Water taken. grms.	Residue obtained.	Amount per 1000 grms.
I. ...	46·9516	1·6026	34·1330
II. ...	34·3920	1·1704	34·0312
		Mean	34·0821

II. Determination of Sulphuric Acid.

	Water taken. grms.	$BaSO_4$ obtained.	SO_4 in 1000 grms.
I. ...	51·213	0·3275	2·6347
II. ...	57·120	0·3242	2·6130
		Mean	2·6239

Equivalent to 2·1870 SO_3.

III. Determination of Chlorine.

	Water taken.	Mixed salts obtained.	AgCl.	Cl in 1000 grms.
I. ...	51·1760	3·8862	3·8788	18·7344
II. ...	51.1567	3·8853	3·8780	18·7376
III. ...	51·0848	3·8790	3·8718	18·7340
			Mean	18·7353

Hence we see that the proportion of solid matter contained in the water of the Irish Sea is somewhat greater in summer than in winter, the variation amounting to 0·0144 per cent. It is also conclusively proved that the relative amount of saline constituents present in the water of the Irish Channel is invariably less than that contained in the water of the Atlantic Ocean lying between the same parallels.

According to Forchhammer, the mean proportion of the leading constituents of the water of the Atlantic, far away from the shores, is as follows:—

	Cl.	SO_3.	CaO.	MgO.	Total salts.
Absolute amount per 1000 grms.	19·865	2·362	0·588	2·199	35·976
Relative amount ...	100	11·89	2·96	11·07	181·10

Arranged in this manner, our determinations on the water of the Irish Sea give the following proportions :—

		Cl.	SO_3	CaO.	MgO.	Total salts.
Absolute amount per 1000 grms. …	Summer	18·735	2·187	……	……	34·082
	Winter..	18·627	2·161	0·575	2·032	33·838
Relative amount.	Summer	100	11·67	……	……	181·91
	Winter..	100	11·63	3·09	10·93	182·09

We have not attempted to determine the amount and nature of the gases dissolved in the water of the Irish Sea. When compared with the amount dissolved in river-water, the quantity of the gases contained in sea-water is exceedingly small. It would be interesting to determine the proportion contained in mixtures of fresh and sea-water at the mouths of rivers. Taking Mr. Hunter's researches on the composition of the gases dissolved in normal sea-water as the basis of the comparison*, we should thus be in a position to judge of the influence of the river-water in this particular direction.

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* Journ. Chem. Soc.

XXV. *On the Influence of Changes in the Character of the Seasons upon the Rate of Mortality.*

By Joseph Baxendell, F.R.A.S.

Read April 5th, 1870.

In the summer of last year, I undertook a discussion of the Rainfall observations made at the stations of the Manchester Corporation Water-works during the 14 years 1855–1868; and among the results obtained I found that although the total amount of rainfall in different years appeared to be governed by no regular law, yet the proportional amounts in the different seasons, during the eight years 1855–62, exhibited a marked contrast to those in the six years 1863–68, the amounts in the spring and summer months exceeding those in the autumn and winter months during the former period, while in the latter the autumn and winter amounts exceeded those of spring and summer. The results for the central station, Arnfield, are as follows:—

	Total fall of rain during the spring and summer months.	Total fall of rain during the autumn and winter months.	Difference.
	inches.	inches.	
1855	17·36	13·27	+ 4·09
1856	21·08	21·53	− 0·45
1857	22·37	14·22	+ 8·15
1858	19.66	16·48	+ 3·18
1859	20·46	19·74	+ 0·72
1860	24·02	15·56	+ 8·46
1861	17·71	14·72	+ 2·99
1862	21·31	17·68	+ 3·63
1863	15·37	22·82	− 7·45
1864	14·54	16·80	− 2·26
1865	13·48	15·89	− 2·41
1866	20·71	26·79	− 6·08
1867	18·71	20·75	− 2·04
1868	12·61	22·86	−10·25

It will at once be seen that during the first eight years the differences, with one unimportant exception, have all the sign *plus*; while during the remaining six years they have all the sign *minus*. The average value of the differences in the first eight years was +3·84, and in the last six years −5·08 inches. The returns from the other stations of the Manchester Corporation Water-works exhibit similar results. It is evident, therefore, that at the end of 1862 a marked change took place in the character of the climate of this locality, the spring and summer seasons becoming much drier, and the autumn and winter months wetter, than they had been during the previous eight years. I may add that this altered character of the seasons was continued through the last year, 1869, the total rainfall at Arnfield during the spring and summer months having been only 12·48 inches against 27·57 inches in the autumn and winter months, thus showing a difference of −15·09 inches.

In considering the differences in the temperature, humidity, and pressure of the atmosphere, and in the direction and force of the wind, in the two periods, as indicated by this marked difference in the distribution of rainfall, it seemed to me highly probable that corresponding differences would exist in the state of the public health, and that the mean rate of mortality during one period would be sensibly different from that during the other. I therefore extracted from the annual Reports of the Registrar General the rates of mortality in Lancashire, Cheshire, and the West Riding of Yorkshire during the years included in the two periods, omitting the last year of the series, 1868, as the Report for that year has not yet been published. These rates are as follows :—

	Annual rate of mortality per cent. in		
	Lancashire.	Cheshire.	West Riding.
1855	2·680	2·197	2·223
1856	2·464	2·048	2·212
1857	2·628	2·269	2·368
1858	2·719	2·267	2·491
1859	2·454	2·169	2·396
1860	2·371	2·173	2·360
1861	2·592	2·164	2·321
1862	2·560	2·246	2·364
1863	2·629	2·396	2·573
1864	2·718	2·300	2·656
1865	2·832	2·328	2·667
1866	3·016	2·538	2·684
1867	2·683	2·252	2·443

Taking the means for the eight years 1855–62, and the five years 1863–67, we have

	Average annual rate of mortality per cent. in		
	Lancashire.	Cheshire.	West Riding.
1855–62	2·558	2·191	2·342
1863–67	2·775	2·363	2·605
Differences	0·217	0·172	0·263

These numbers show that the average rate of mortality in Lancashire, Cheshire, and the West Riding was decidedly greater during the five years of dry springs and summers with wet autumns and winters, than during the eight years when the seasons were of an opposite character. The differences are equivalent to an increase of 8·4 per cent. in the number of deaths in Lancashire, 7·8 per cent. in Cheshire, and 11·2 in the West Riding,—the mean amount of increase being 9·1 per cent., which, in Lancashire alone, represents an increase of more than seven thousand in the total number of deaths in one year.

Observations of rainfall were commenced at the Gorton station of the Manchester Water-works in 1847; and Mr. Wilson having kindly furnished me with copies of the monthly amounts for the eight years 1847–54, I have grouped them in six-monthly periods, as I had done the returns for 1855–68, and have obtained the following results:—

	Total fall of rain during the spring and summer months. inches.	Total fall of rain during the autumn and winter months. inches.	Difference.
1847	17·22	24·72	— 7·50
1848	19·25	21·71	— 2·46
1849	13.59	19·82	— 6·23
1850	14·59	15·18	— 0·59
1851	17·14	13·20	+ 3·94
1852	13·38	23·96	—10·58
1853	15·72	14·10	+ 1·62
1854	13·94	20·03	— 6·09

In six years out of the eight the fall of rain during spring and summer was less than during autumn and winter, while in two only was it in excess. The mean difference for the entire period was —3·48. It is evident therefore that the general character of the climate during this period was similar to that of the period 1863–68; and therefore I inferred that the mean rate of mortality would be found to be correspondingly high. The following figures for Lancashire will show that this inference was correct:—

	Annual rate of mortality per cent. in Lancashire.
1847	3·582
1848	2·765
1849	3·037
1850	2.464
1851	2·647
1852	2·889
1853	2·818
1854	2·766
Mean rate ...... =	2·871

The mean rate 2·871 is slightly above that of the five years 1863–67, which was 2·775, and is 0·313 above that of the favourable years 1855–62. This difference of 0·313 is equivalent to an excess of 12·2 per cent. per annum in the number of deaths. It thus appears that during a period of eight years, 1847–54, in which the spring and summer rainfall was considerably below that of autumn and winter, the average rate of mortality in Lancashire was 2·871, and that in the next following eight years, 1855–62, in which the rainfall of spring and summer was considerably above that of autumn and winter, the rate of mortality fell to 2·558, but that during the five following years, 1863–67, when the spring and summer rainfall again fell decidedly below that of autumn and winter, the mean rate of mortality rose to 2·775 in spite of all the sanitary improvements that had been made in almost every town and district in the county. Looking to the enormous sums of money that have been expended of late years, by boards of health and town councils, in making sanitary improvements, these results show clearly that the effects of meteorological changes upon the public health far exceed those arising from defective drainage, ill-contrived privies and water-closets, crowded dwellings, and an insufficient supply of good water, and that no material and permanent reduction in the rate of mortality can reasonably be hoped for until a close and careful study of the influence of changes in the state of the atmosphere upon the production and development of disease has led to the discovery of the best means of guarding against and counteracting the effects of unfavourable changes of the weather, and the people have been induced to avail themselves of such means to ward off or mitigate attacks of disease.

In the Reports of the Registrar General the causes of death are divided into five classes :—

I. Zymotic.
II. Constitutional.
III. Local.
IV. Developmental.
V. Violent deaths.

The following Table shows the number of deaths of males in Lancashire, in each class, during the years 1855–67 :—

Year.	Class.					Total deaths from ascertained causes.
	I.	II.	III.	IV.	V.	
1855	6909	5548	12194	3968	1589	30208
1856	6814	5444	11049	3598	1495	28400
1857	7628	5476	11901	3859	1648	30512
1858	9559	5296	11603	3899	1513	31870
1859	6964	4862	11976	3769	1574	29145
1860	5500	5049	12876	3988	1609	29013
1861	7166	5394	13553	4477	1600	32190
1862	7958	5304	13460	4157	1487	32366
1863	9606	5319	13045	4071	1642	33683
1864	9738	5506	14119	4284	1792	35439
1865	11068	5958	13849	4700	1926	37501
1866	11899	6207	15720	4868	1987	40681
1867	9039	6162	15050	4968	1809	37028

The mean annual numbers for the eight years 1855–62, and the five years 1863–67 are :—

1855–62	7312	5297	12325	3964	1564	30463
1863–67	10270	5830	14356	4578	1831	36866

The ratios of the numbers in each class to the total number of deaths are as follows :—

1855–62	·240	·173	·404	·130	·051	......
1863–67	·278	·158	·389	·124	·049	......

A glance at these numbers shows that the greater mortality in unfavourable years arises from an undue increase in the number of deaths from zymotic diseases, or those which are commonly regarded as preventible.

An impression prevails very generally that when the rate of mortality is above the average the excess is due to a disproportionate increase of deaths among infants and young children; but if the increased mortality is due to meteorological causes we should expect that the increase would be relatively less among the very young and the very old, who are least exposed to the vicissitudes of the weather, than among the more actively employed and exposed classes of the community. To test this point, however, I have collected in the two following Tables the number of deaths at different ages which occurred in Lancashire during the years 1855–67. The first Table shows the number of deaths of males, and the second that of females:—

Deaths at different ages. Males. Lancashire.

Year.	Total deaths.	Under 5 years.	5 to 15.	15 to 25.	25 to 35.	35 to 45.	45 to 55.	55 to 65.	65 to 75.	75 to 85.	85 to 95.	95 and upwards.
1855	30525	15049	2072	1892	1836	1999	2089	2103	1012	2212	247	14
1856	28693	14496	1935	1857	1727	1887	1971	1868	1690	1064	189	9
1857	30770	15684	2015	1895	1815	1942	2091	2035	1827	1223	233	10
1858	32447	16437	2628	1949	1833	2039	2169	2115	1871	1171	222	13
1859	29686	14522	1799	1724	1755	2005	2241	2285	1959	1177	210	9
1860	29639	13773	1544	1753	1875	2207	2334	2401	2167	1357	218	10
1861	32789	16345	1761	1923	1929	2239	2342	2441	2174	1387	239	9
1862	32933	16364	2159	1940	1895	2168	2371	2407	2106	1292	221	10
1863	34295	17161	2497	1852	1931	2355	2415	2479	2132	1219	241	13
1864	36099	16614	2540	2083	2241	2782	2839	3002	2371	1359	258	10
1865	38275	18032	2451	2189	2506	2782	3072	3114	2467	1383	271	8
1866	41530	19216	2809	2553	2785	3114	3336	3326	2651	1435	289	16
1867	37786	18067	2390	2091	2363	2642	2808	3012	2697	1449	258	9

Deaths at different ages. Females. Lancashire.

Year.	Total deaths.	Under 5 years.	5 to 15.	15 to 25.	25 to 35.	35 to 45.	45 to 55.	55 to 65.	65 to 75.	75 to 85.	85 to 95.	95 and upwards.
1855	29363	13222	1925	2048	2206	2058	1916	2086	2083	1453	346	20
1856	27356	12638	1764	2017	2047	1915	1781	1803	1880	1222	268	21
1857	30041	13818	1830	2109	2225	2100	1941	2093	2062	1490	346	27
1858	31560	14744	2528	2162	2116	2186	1953	2026	2099	1382	342	22
1859	29084	12750	1787	2024	2179	2229	2054	2151	2138	1432	323	17
1860	28093	11704	1542	1898	2224	2221	2134	2297	2237	1481	337	18
1861	31375	14461	1705	2170	2279	2273	2119	2273	2291	1511	259	34
1862	31482	14139	1889	2019	2246	2295	2286	2496	2330	1465	300	17
1863	32907	15247	2282	2033	2306	2341	2197	2405	2258	1490	323	25
1864	34485	14693	2435	2186	2580	2568	2598	2765	2667	1619	348	26
1865	36418	15879	2288	2409	2745	2816	2783	2879	2616	1605	379	19
1866	39254	17148	2503	2469	3160	3023	2967	3065	2819	1703	378	19
1867	35157	15529	2177	2169	2544	2644	2499	2846	2641	1702	383	23

The mean annual values for the two periods, 1855–62 and 1863–67, and their ratios to the mean annual number of deaths at all ages, are as follows:—

Males.												
Year.	Mean annual number of deaths at all ages.	Under 5 years.	5 to 15.	15 to 25.	25 to 35.	35 to 45.	45 to 55.	55 to 65.	65 to 75.	75 to 85.	85 to 95.	95 and upwards.
1855 to 1862	30935	15333	1989	1867	1833	2061	2201	2207	1976	1235	222	10·5
1863 to 1867	37597	17818	2538	2153	2365	2735	2894	2986	2463	1369	263	11·2
Ratios 1855–62		·495	·064	·060	·059	·066	·071	·071	·063	·039	·007	·0003
Ratios 1863–67		·473	·067	·057	·062	·072	·077	·079	·065	·036	·006	·0003
Females.												
1855 to 1862	29794	13434	1871	2056	2190	2159	2023	2153	2140	1429	315	22·0
1863 to 1867	35644	15699	2336	2253	2667	2678	2609	2792	2600	1624	362	22·4
Ratios 1855–62		·451	·062	·069	·073	·072	·067	·072	·072	·048	·010	·0007
Ratios 1863–67		·440	·065	·063	·074	·075	·073	·078	·073	·045	·010	·0006

From these numbers it is evident that in unhealthy years the increase of mortality is relatively greater between the ages of 25 and 75 than at any other age, and that very young and very old lives are relatively much less affected than in years when the general rate of mortality is below the average, thus confirming the view I have taken that the excess of deaths in unfavourable years is principally due to meteorological causes.

It has long been admitted that the state of the public health is affected by changes in the state of the weather; and for many years the Registrar General has regularly included in his Reports statements of the weekly, quarterly, and annual mean and extreme values of the various meteorological elements as observed at Greenwich; but these statements as usually given are of little value to sanitary science. To be of use, the results of meteorological observations ought to be regularly grouped and discussed with reference to the various questions which arise in connexion with the origin and development of diseases; but this obvious and very important course of proceeding appears to be entirely neglected by boards of health, health committees, and officers of health; and, as a natural consequence, their misdirected efforts, made without a due regard to the true principles of sanitary science, have hitherto failed entirely to effect any improvement in the state of the public health, or any reduction in the general rate of mortality, as is clearly shown in the mortality returns. Thus, during the 15 years 1838–52, the average annual rate of mortality for the whole of England was 22·35 to 1000 persons living; and during the succeeding 15 years, 1853–67, it was 22·47. It is therefore evident that, notwithstanding the so-called sanitary improvements, made, at great cost, in almost every town and district in the kingdom during the latter period, the public health remained in the same unsatisfactory state in which it had been during the previous 15 years; and the death-rate still showed a tendency to increase.

From a Table at page xlii of the 30th Annual Report of the Registrar General, it appears that, during the six years 1857–62, the average annual rate of mortality in 142 districts and 56 subdistricts, comprising the chief towns in England, was 23·70 per 1000 living, while in the following five years, 1863–67, it was 25·35; and in the remaining districts and subdistricts of England and Wales, comprising small towns and country parishes, it was 19·74 in the former period, and 20·41 in the latter. Now, as the character of the weather is rarely, if ever, the same over the whole of England and Wales as that which may happen to prevail in the Manchester Water-works district, but will often be widely different in some localities, it is evident that the whole of the differences between these numbers cannot fairly be attributed to meteorological causes alone, and therefore that during this period of 11 years the general rate of mortality was slowly increasing both in town and country districts, but in a much higher ratio in the former than in the latter; and yet it is in the large towns that what are supposed to be sanitary improvements have been carried out to the greatest extent, and where, therefore, had the schemes adopted been based on sound principles, their effect in checking the increase in the rate of mortality would have been most apparent.

It will, no doubt, excite surprise in the public mind to find that, after so many years' trial, and the expenditure of so much public money, the schemes carried out by our sanitary authorities have produced absolutely no improvement whatever in the general sanitary condition of the people, nor even prevented an increase taking place in the average rate of mortality; but, as I have indicated above, our sanitary authorities seem never to have made any serious and systematic attempt to discover the true causes of the fluctuations which take place in the rate of mortality, and trace out the modes of operation by which their effects are produced. Almost all that has been done in this direction has been accomplished by private individuals; and I may refer, as a noteworthy instance, to a valuable paper in Vol. I. Series III. of the Society's Memoirs, by Dr. A. Ransome, and Mr. G. V. Vernon, F.R.A.S., "On the Influence of Atmospheric Changes upon Disease." Sanitary officials, however, seem, for the most part, to act as if they were strangely ignorant of the value and importance of applying modern methods of scientific research to questions relating to health and disease, and were content to aim at no higher object than that of discharging the duties which properly belong to nuisance-inspectors and scavengers. Until these gentlemen form a much higher estimate than they have hitherto done of the dignity and importance of their profession, and of the difficulties they have to overcome, we cannot reasonably hope to see any real and permanent improvement in the general state of the public health*.

The general results of the above investigation may be briefly recapitulated as follows:—

1. That the influence of meteorological causes in producing fluctuations in the rate of mortality is much greater than that of any other recognized influence.

2. That the class of diseases which is most affected by meteorological changes is Class I., Zymotic diseases.

3. That the relative increase in the number of fatal cases of disease at different ages, in unfavourable seasons, is greatest between the ages of 25 and 75 years, or amongst those classes of the community who are most exposed to vicissitudes of weather.

4. That the sanitary measures which have been carried

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* In justice to the corporation of Salford, I must state that at the suggestion of the Mayor, Thomas Davies, Esq., a meteorological station was established within the borough, at the front of the Town Hall, at the beginning of the year 1868, and that observations have since been regularly made under the superintendence of Thomas Mackereth, Esq., F.R.A.S., for systematic comparison with the weekly returns relating to the sanitary condition of the borough. Mr. Mackereth informs me that these comparisons have already yielded decisive evidence of a close connexion between meteorological changes and the development of certain diseases which are unfortunately too prevalent in Salford—thus confirming the results arrived at by Dr. Ransome and Mr. Vernon, and proving the soundness of the view taken by the Mayor when he urged the desirability of establishing a meteorological station in or near the centre of the borough. I believe this is the only instance of the establishment, by a public body, of a meteorological station in the centre of a large town.

out during the last 15 to 20 years, by boards of health, health committees, and officers of health, have produced no perceptible improvement in the state of the public health, nor checked the growing increase in the rate of mortality, notwithstanding the enormous outlay they have involved, and therefore that a thorough reform of our existing sanitary system is urgently required.

XXVI. *Notes of the Rarer Mosses of Perthshire and Braemar.* By George E. Hunt, Esq.

Read before the Microscopical and Natural History Section, Feb. 1st, 1869.

In the present day there are two widely separated classes of botanists:—one consisting of those who endeavour to simplify the determination of species, by uniting all that have a general resemblance in their more conspicuous characters; the other of those who note the most minute details of structure, and separate as a species every form which can be distinguished by a definite character, however insignificant. In Britain the latter class prevails, at least so far as Phænogamic botany is concerned—and doubtless because the field is so narrow that this is almost the only way of finding new material, so long as the attention is confined to the examination and discrimination of British specimens; and every record of variation *is* of importance, as tending ultimately to show how far a species may vary. This is not, to the same extent, the case with Cryptogamic botany, which in some of its departments had, until comparatively recently, been much neglected: thus, among the fungi, not only do we see frequent additions of microscopic, but occasionally also the reader is astonished by the announcement of hitherto overlooked species of large size; in the lichens, the additions of the last seven or eight years must much exceed a hundred species; and even in the mosses, the most fully investigated group of the three, they may be counted by dozens.

The mosses, though possessing little economic value, yet serve both for a quiet and pleasing adornment of rocks, walls, &c., and also in the most barren situations prepare a slight soil for the development of higher organisms. Every different situation is more or less distinguished by the presence of certain genera. On newly turned soil *Phascum, Weisia* and *Pottia* prevail; on walls, *Trichostomum, Tortula, Bryum, Rhacomitrium*; on trees, *Orthotrichum, Zygodon*; in bogs, *Sphagnum, Hypnum,* and numerous other genera; and on rocks a multitude of genera and species, which, again, increase greatly in number with a higher elevation.

Three alpine regions in Scotland stand preeminent for the variety of their mosses:—1st, Ben Lawers, in Perthshire, with the adjoining peaks; 2nd, the Clova District, in Forfarshire; 3rd, Braemar. All these were long since searched by able botanists, as Hooker, Gardiner, Drummond, Wilson, Arnott, Greville, and others; but such is their richness, that a year hardly ever passes without some discovery. There are several causes for this richness, *e.g.* elevation, moisture of climate, and nature of soil.

Ben Lawers is situated on the north of Loch Tay, and attains an elevation of 3984 feet above the level of the sea: it is the highest point in Perthshire. Its lower slopes consist of extensive moors, interspersed with peat-bogs, and here and there crossed by rocky streams, which have cut deep channels for themselves through the moors. On the upper parts of these slopes, in bogs, occur the following mosses (at an elevation of from 2500 to 3000 feet):—

Sphagnum Mülleri.
—— teres.
—— Girgensohnii.
Campylopus compactus.
Splachnum vasculosum.
—— sphæricum.
Tayloria serrata.
Dissodon splachnoides.
Webera albicans, var. glaciale.
Bryum Duvalii.
—— latifolium.
Mnium cinclidioides.
Hypnum trifarium.
—— sarmentosum.

And on rocks in streams,

Hypnum arcticum.

At the same elevation, on the dry turfy edges of rocks, are found :—

Webera polymorpha.
Oligotrichum hercynicum.
Hypnum Muhlenbeckii.

On dry rocks :—

Grimmia patens.
Dicranum virens.

On damp rocks :—

Hypnum callichroum.

And on débris :—

Conostomum boreale.
Webera Schimperi (*Wils.* not of *Bry. Eur.*)

The upper portion of the mountain is composed of micaceous schist, here forming precipices of considerable extent, there broken up into enormous masses, tossed about in an infinite variety of forms—and the surface of the whole readily decomposing, and forming one of the finest soils for alpine plants. No streams, and only one or two minute runlets from bogs of very diminutive size, are to be met with at this elevation.

On ledges of the precipices, on grassy turf, and in deep crevices of the rocks here, we meet with the following :—

Weisia crispula.
Arctoa fulvella.
Dicranum virens.
—— falcatum.
—— Blyttii.
—— Starkii.
—— longifolium.
Stylostegium cæspititium.
Tortula fragilis.
Grimmia ovata.
—— spiralis.
—— torta.
Zygodon lapponicus.
Encalypta rhabdocarpa.
—— commutata.
Distichium inclinatum.

Distichium capillaceum.
Webera Schimperi, *Wilson* (not of *Bry. Eur.*).
Bryum pallescens.
—— demissum.
Mnium spinosum.
Timmia norvegica, *Zett.*
Conostomum boreale.
Polytrichum sexangulare.
Cylindrothecium concinnum.
Leskia nervosa.
Myurella julacea.
—— apiculata.
Hypnum plicatum.
—— reflexum, forms (or allied species known in British herbaria as H. reflexum, H. Starkii, H. micropus, H. glaciale, var.).
Hypnum cirrhosum.
—— atrovirens.
—— Halleri.
—— dimorphum.
—— umbratum.
—— Oakesii.
—— callichroum.
—— sulcatum.
—— Bambergeri.
—— pulchellum.

And in boggy ground,

Dissodon splachnoides.

The species indicated in the previous lists are the less frequent ones only of the mountain; eleven of them have been added to the British flora within the last four years, viz. :—

*Sphagnum Mülleri.
—— teres.
—— Girgensohnii.
Campylopus compactus.
Bryum latifolium.
*Tortula fragilis.
*Mnium spinosum.
*Timmia norvegica.
*Leskia nervosa.
*Hypnum sulcatum, *Sch.*
*—— Bambergeri.

And of these, seven (marked *), together with the following (whose discovery dates further back), viz.

Myurella apiculata,
Hypnum plicatum,
Hypnum cirrhosum,
—— Oakesii,

have, at the present date, no other known station in Britain than Ben Lawers.

The only notable Perthshire alpine species which appears to be absent is *Hypnum hamulosum,* Bry. Eur. (*H. hamulosum β. micranthum,* Wils. Bry. Brit.), which exists in great profusion towards the summit of Craig Challeach, near Killin.

Ben Lawers produces certainly 180 species (and probably many more), *i. e.* one-third of the whole known in Britain. If the damp woods, rocks, and walls near the bank of Loch Tay are taken into account, there is a great accession to the number, and they will then amount to about 300. These latter, however, are mostly common and universally distributed; and only the following rarities call for notice, viz.:—

Tortula papillosa.
Grimmia tricophylla.
—— Hartmannii.
Ulota Hutchinsiæ.
—— Ludwigii.
Habrodon Notarisii.

On entering Braemar, the character of the country greatly changes, and with it the vegetation. The valleys (which themselves have an altitude of about 1000 feet) and many of the lower ridges of the mountains are composed of slaty rocks. The higher mountains, viz. those of the Cairngorm range, are granitic, and, owing to their massiveness, and to their usually rounded summits, present at first sight a somewhat monotonous aspect, and deceive the eye by giving the appearance of much less altitude than is really the case, many of their peaks being upwards of 4000 feet in height, and Ben-mac-dhui, the loftiest of the range, reaching to 4295 feet, being second in Britain only to Ben Nevis, which exceeds it by 111 feet.

The slaty rock, from the ready decomposition of its surface, its numerous crevices, and its generally damp character, forms a favourite ground for mosses. At the lowest level the following species occur:—

On trees	Orthotrichum speciosum.
,,	Ulota Drummondii.
,,	Antitrichia curtipendula.
On rocks	Hypnum callichroum.

Dr. Dickie also finds on the decayed wood of dead fir trees the very rare *Buxbaumia indusiata*. The same gentleman finds on débris, at a slightly higher altitude (1500 feet), *Buxbaumia aphylla* and *Anacalypta latifolia*; and in like situations *Encalypta ciliata* is frequent.

The moors, rocks, and streams of Glen Callater and Loch Kandor produce:—

Sphagnum curvifolium.
Audreæa falcata.
Grimmia atrata.
Dicranum Starkii.
—— falcatum.
Webera Ludwigii.
Tetraplodon angustatus.
Splachnum vasculosum.
Mielichhoferia nitida.
Hypnum dilatatum.
—— arcticum.

Of these, *Mielichhoferia nitida* has a special interest attached to it from the fact of its having been rediscovered in 1868 by Messrs. Ferguson and Roy, in the same station where, in 1830, a single tuft had been found by Dr. Greville. The only other British locality known is above Ingleby, Greenhow, in Yorkshire, where Mr. Mudd, now of the Botanic Gardens, Cambridge, collected it in 1862.

On the granite of Ben-mac-dhui (and doubtless on other mountains also of the Cairngorm range) a few species are developed, which are well worthy of notice, on account both of their rarity and their extraordinary luxuriance. They are all clustered together in great profusion, in and round the streamlets proceeding from the snow-beds which perpetually exist a few hundred feet below the summit of the mountain, and they grow in a soil consisting of the pure débris of granite. These are:—

Andreæa nivalis.
Dicranum arcticum, *Sch.*
(D. Starkii β. molle, *Wils.*).
Webera Ludwigii.
Hypnum molle, *Dicks.*
Polytrichum sexangulare (on dry ground).
Arctoa fulvella (on rocks).

Andreæa grimsulana has also been gathered, and doubtless is to be found not far removed from the species above indicated; but I was unfortunate enough to overlook it.

Hypnum molle, Dicks. (*fide* Wilson), is one of the least understood of British mosses, and owes its disentanglement and identification as a separate species to Mr. Wilson. The plant usually distributed under this name, both in Britain and on the Continent, is *Hypnum dilatatum,* Wils. MSS.

The following are the characters of the two species:—*H. dilatatum,* Wils. (*H. molle,* Bry. Eur.). Plant of a somewhat firm growth; leaves rotundo-ovate, rather concave, suddenly apiculate, texture very close, areolæ long and very narrow; nerve double, short, slender, but well defined. Growing at a low elevation. Localities in Britain—North Wales, Yorkshire, Perthshire, Clova, Braemar; widely distributed on the Continent.

H. molle, Dicks. (*H. alpestre* (?), Bry. Eur., not of Swartz). Plant very weak and flaccid, the tufts falling to pieces on being removed from the water; leaves varying from ovate to rotundo-ovate, flat, or sometimes very slightly reflexed towards the apex, gradually tapering upwards, or very rarely suddenly apiculate; texture somewhat loose, areolæ larger and wider than in *H. dilatatum*; nerve rather long and thick, ill defined, single or double. Growing at a great elevation. Localities in Britain—Ben-mac-dhui, Ben Nevis; on the Continent—Norway and Switzerland. This plant is very liable to be passed over as *H. ochraceum*, as it much resembles, in general aspect, some forms of that species.

H. alpestre, Sow., is to be expected in Scotland; it is to be distinguished by its elliptical, very concave leaves, with the margin conspicuously reflexed towards the apex, which is usually itself recurved; nerve double, rather thick, distinct. Found in Norway.

Dicranum arcticum, Sch. (*D. Starkii β. molle*, Wils.). Although described under the latter name in Wilson's 'Bryologia Britannica,' it has for some time past been nearly lost sight of in Britain, and is so little known in Europe that in 1866 it was figured and described in Part 3, Suppl. to Bry. Eur., by Dr. Schimper as a new species, allied to *Starkii*, the only European station there mentioned as known being the Dovrefjeld, in Norway; also found in Greenland and Labrador. Its general appearance better distinguishes it from *D. Starkii* than its microscopic characters. It is quite erect, growing in large loose patches; stems three or four inches in height, elastic, very robust; foliage of a fine purplish-brown colour. *D. Starkii* has the stem almost always decumbent at the base, slender, usually about an inch in height, sometimes taller; foliage yellowish green. The fruiting-time also appears to differ, as in the beginning of July, when *D. Starkii* was in perfection, only very old capsules of *D. arcticum* were to be found. *D. arcticum* has the leaf wider in the lower part, and rather more suddenly contracted upwards than *D. Starkii,* also a thinner nerve, and the alar cells usually, but not always, more deeply coloured. The areolation is the same in both species; but *D. arcticum* has the leaves a little more pellucid. The male flower is gemmiform, and situated close to the base of the perichætium in both species. *D. arcticum,* however, has usually *several* perigonia. In *D. Starkii* they are usually *solitary.* Abundant on Ben-mac-dhui and Ben Nevis.

The preceding sketch of Braemar is necessarily incomplete, as I have explored but few of its many rich localities; but from the species I have indicated it will be seen how entire is the change of the moss-flora from that of Perthshire, and that it will richly repay the naturalist who may devote his time to its exploration, whilst the scenery around him must excite his interest and admiration, and of itself would amply repay him for a visit.

XXVII. *Brief Notes on the Laws of Physical Force.*

By J. C. Dyer, Esq., V.P.

Read March 3rd, 1868.

The wide acceptance of the new doctrine of the conservation of physical energy, or force, has led me to submit a few remarks thereon.

In the first place, let us keep in mind that there are five kinds of natural forces continually exerted by material bodies. Three of these are mechanical forces, and, as such, are measurable by the known laws of physics (viz. the force of gravity, of inertia, and of elasticity), each being coextensive with and inherent properties of all tangible bodies. The actual exertion of these, by their equal action and reaction, serves to sustain the positions and forms (whether in motion or at rest) of the entire material universe; and it is only by the observed action of such balanced forces that we are assured of the existence of material bodies.

Many theories have been propounded to account for the presence of these several forces in matter; yet no real light has been shed upon the questions involved in such inquiries, beyond the fact that all these forces are found to be essential forces of the bodies exerting them. The first two are directly as the quantity of matter, and the third, or elastic force, varying in degrees of action according to the condition of bodies as solids, liquids, gases, or vapours. I have elsewhere aimed to show that a pervading "calorific element" constitutes the essence, or is the source, of the elastic forces in all ponderable bodies—a question here passed by.

Besides the above mechanical forces, we have to consider the two other forces—namely, the chemical and vital forces.

The affinities and repulsions of the component parts of matter, by which its mutations are effected, constitute the chemical forces. These forces are of varying and complex intensities, and are still but vaguely understood even by our most able practical chemists; yet the reality of such forces is extensively evidenced to our senses, though they are seldom such as can be distinctly measured like the mechanical forces.

With respect to the vital forces, they are of a still more complex and recondite nature, and we must be content in our present state of knowledge to place these forces among the many other sublime and mysterious laws impressed upon organic matter by an all-wise Providence, and which are not yet placed within the range of man's mental vision. We simply know that the vital forces are, in part, controlled by the will, and a greater portion of them are called into action by organic stimulants; and since both kinds of action cease with death, they are properly treated as vital forces. Whilst we know not how the vital forces control the mechanical and chemical forces exerted in and by living beings, there is no lack of proof that they do in fact command the other forces exerted through organic nature.

I shall here cite an eloquent passage from an essay (given in a popular journal, 'Once a Week,' for 13th October, 1865) by one of the most able exponents of what is called the "New Philosophy," as follows, namely:—"The great philosophical doctrine of the present era of science, as the conservation of energy has been worthily styled, teaches us that the activity which we see manifested in all the natural forces is a constant quantity, or, in other words, that there is a definite amount of force distributed through nature, which is invariable in amount, and which we can neither add to nor take from, but whilst the force in one form disappears it reappears in another form. But what do we mean by the term force? The simplest definition of the term is that which describes it as something which produces or resists motion. When we think of force we think of something moving or moved."

This definition of the term force would be unexceptionable, if what is called force were held simply as indicating the properties of the bodies in which it acts, and not as a something, or energy, existing by itself as an entity apart from matter, but as a part of it. Were it merely intended to declare that the sum of the mechanical forces, as gravity, inertia, and elasticity, is a constant quantity, just as is that of the bodies in which these forces are inherent properties, then there would be no grounds for asserting this fact as disclosing a "great new doctrine;" for the indestructible nature of matter, and of its properties, were physical truths known and taught in our childhood.

The relations of matter and force—at least as regards the mechanical forces—have been set forth in such ample detail, comprising all known facts, in the elements of physical science, that it would seem needless to repeat the fact of the unchangeable nature of the material universe, both as to the amount and the kind of forces pertaining to matter, but for the enunciation of theories implying such changes; wherefore to treat of the conservation of the natural forces, and of the disappearance of "one kind of force and reappearance of another kind," must be held as employing unmeaning or misleading terms. It is not that any of the forces themselves can disappear, or be changed from one to another kind of force, but that the motions generated by them may cease, or be rendered quite uniform when the moving forces are exactly balanced, by their action in opposite directions.

In short, the term force, used in an abstract sense, and without defining the kind of force intended, is worse than idle or unmeaning, because when so used by eminent physicists it cannot fail to puzzle and mislead the younger students in the sciences. The author quoted frankly states that the dogmas he expounds "seem not quite so clear to the unscientific mind."

Considering that this city is so widely distinguished for its practical application of the mechanical and chemical sciences, it would reflect much discredit on the community for us to be unable to comprehend the simple elements of the natural forces, so largely guided by our hands. Hence a few words seem called for, to distinguish the several forces acting conjointly or separately, and to protest against their being confounded together as an abstract entity to be conservated.

There seems no ground for asserting any new discoveries relating to the natural forces; yet a boundless field lies before us for new applications of those forces to the ever expanding and varying uses of man.

In addition to the before-mentioned fallacies, of including all physical forces under one head, and treating them as an abstract entity, it is assumed by the new philosophy that vast mechanical forces are continually exerted by special natural agencies, which hitherto have not been known to exert or to possess this kind of force in any degree. To sustain this assumption, its authors have not been able to adduce any facts, or even analogies, in known phenomena, to prove or render probable the exertion of such forces, as, for instance, the alleged mechanical forces of the solar rays when they are intercepted by aqueous vapour in the air, and the like forces assumed to be in continuous action by and among the agitated atoms and molecules or the ultimate particles of tangible bodies, and thus, by their internal motions, tending to change or to restore their sizes and forms when the bodies are altered in either by external forces. Now, considering that such ultimate particles, called "atoms" and "molecules," are not discoverable by any analysis or known mutations of matter, to assert the exertion of such invisible moving forces, as apart from the tangible matter exerting those forces, is clearly a gratuitous assumption, unwarranted by any known properties of material bodies. With respect to the solar rays, we have many striking evidences of their chemical action in terrestrial phenomena; but no proof whatever has yet been adduced to show their mechanical force or action upon tangible bodies, unless their impinging on the optic nerves, giving the sensation of light, may be held to be a mechanical action; but even if this be so, the force exerted can be but slight. Wherefore these newly discovered forces must "vanish into air, into thin air."

The same high authority (before named)—after giving a brilliant exposition of "the source of the solar rays," or, as it is termed, "the origin and sustentation of the heat of the solar furnace"—and showing how the growth of vegetables depends on the solar influence, and that our stores of coal come from plants, also that by the combustion of coal steam-power is obtained and machinery driven, &c., then adduces the following case, namely:—

"From machine power we turn to muscular power. Between the steam-engine and living bodies there is the closest analogy. The nutritive materials upon which life depends are no more nor less than combustible substances, which actually undergo a slow combustion. The conversion of food into work done is effected by the same process as that which turns coal or wood into motive force."

That we must eat to live, and the steam-engine must have steam to work it, are simple facts that require no comment; but the forces exerted to sustain the movements of a metallic engine, and the living organisms, are so widely different in the two cases, that one could hardly expect to see their close analogy asserted by any writer on the laws of physics, except under strange illusions concerning the kinds of forces employed in the economy of nature.

It is an obvious fallacy to assert such close analogy to exist between the functional energies of living beings, and the force of steam in an engine. The distinction in the two cases is plainly seen by reference to the two kinds of force called into action by each of them—the first being a series of organic energies, that connect and sustain the successive motions through the complex structure of the said beings, the other being simply the elastic force that drives the engine. To support this alleged analogy, its authors should be enabled to bring some sort of proofs that the vital forces do result solely from the chemical forces evolved in the living system; but this is not sustained by any known facts, or deductions from facts. No "new philosophy" is required to teach us that all organic beings depend for their existence on solar heat, as also on the due supply of proper food. We also know that "the nutritive materials for sustaining life" contain a large portion of carbon, in the forms of gelatine, fibrine, saccharine, starch and other analogous bodies in common food; but these facts afford no explanation as to the origin, development, and final decay of "nature's living progeny."

The vital forces, from the first germ to the close of life, exhibit, in a clear plan, a train of connected and harmonious actions, in guiding and controlling the chemical and mechanical forces that are exerted in carrying on the functional powers of life—showing that the vital force was adapted to and designed to control the others.

The marvellous energy of the heart far exceeds the force of any hydraulic engine of the like size; the forces exerted by the peristaltic and other internal muscles, as also the chemical forces acting through the lungs, stomach, and general circulations, are alike inexplicable upon any theory, however ingenious, of slow combustion, or of machine power.

Although the term vital force may be somewhat vague, it seems to point out the special energies that originate organic forms, and multiply and expand them by a series of spontaneous actions, from the seeds and ova, to build up the living structures of plants and animals; and upon the attainment of their maturity, the subsidence of those energies commences; nor can any intrinsic aid or stimuli prevent their extinction at the appointed time.

If the term force be made to include those exerted by the vital organs, and if the conservation of such forces be a practical reality, then the living energies need never to "fade away and expire," except through the wilful neglect of the conservative doctors.

I will here add, in the words of a profound thinker, that, in organic life, "every train of development exhibits in its course an adherence to plan, which can only have its ground in an internal vital destination. It exhibits at the same time an independence of all external influences, which testifies to the internally given force of vitality"*.

It must be observed that these comments on vital forces relate solely to those functional energies that produce physical action, and which are quite apart from the "moral and intellectual forces" exerted externally by sentient beings upon each other—a subject of wider and far higher nature than any of mere physical action.

I venture to recommend a careful perusal of the learned and eloquent essay (in 'Once a Week') before quoted, in order to a clear and full comprehension of what is called "the great philosophical doctrine of the present era of science."

All know that, in the act of breathing, the carbon in the blood is converted into its acid form, whereby the heat of

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* Ray Society's Bot. and Phys. Memoirs, 1863.

warm-blooded animals is mostly kept up. This process is properly enough termed combustion. It is alike known that, in the act of digestion, all sorts of food taken into the stomach are decomposed and converted into one uniform fluid (chyle), which process bears no resemblance whatever to that of combustion; yet it is equally certain that animal life depends as strictly upon the digestion of food as upon healthful respiration. Indeed those chemical powers of the stomach are even more hidden and wonderful than the mechanical energy of the heart, before noticed.

By the experiments of Dr. Beaumont, in the well-known case of the Canadian soldier's (St. André's) stomach, it was clearly proved that the many kinds of animal and vegetable food taken were dissolved and converted into a semifluid of one uniform substance in the course of about two to four hours, by the juice or fluid secreted by the epigastrium, the secretions following in succession and laying hold of the morsels of food as they were swallowed.

The secreted liquor (gastric juice) has never been formed in any other than "Nature's laboratory," nor has the chemist ever been able to discover the nature or constituents of this general solvent. How puerile, then, to talk of the analogy of these vital forces to those of slow combustion.

In conclusion let me add that I make no pretence to any novelty in what is above said relating to the laws of force, but merely to have noticed some of the new principles and views that appear to be wholly untenable under every known law. To treat fully the many physical questions connected with the natural forces, and especially those of the vital forces, would demand (in place of a few pages) a work of ample size and labour, without exhausting the inquiries concerning obscure phenomena.

XXVIII. *On a General System of Numerically Definite Reasoning.* By Professor W. Stanley Jevons, M.A.

Read January 25th, 1870.

The system of numerical reasoning described in this paper arises from the combination of arithmetical or algebraical calculation with logical reasoning. The purpose is to determine, as far as possible, the numbers of individual objects which may compose classes or groups of objects under any given logical conditions—the data consisting of those logical conditions, and the numbers of individuals in certain other related classes explained.

Only two or three previous writers have bestowed attention on this subject. Professor De Morgan is probably the first logician who pointed out that syllogistic arguments may exist in which the numbers of objects forming the several terms of the syllogism may be exactly defined, and that inference is often possible with such premises when it would not otherwise be valid. Logicians have for ages introduced notions of quantity into the syllogism; but they restricted themselves to the vague quantities *all, a part,* or *none.* Professor De Morgan enjoys the high honour of showing that definite numbers may also be the subject of syllogistic argument; and his system is fully stated in the 8th chapter of his 'Formal Logic,' "On the Numerically Definite Syllogism"*.

2. The late Professor Boole has also treated this subject, but under a different name, and in a very different form. His chapter on the subject† is entitled "On Statistical Condi-

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* See also an abstract in his 'Syllabus of a Proposed System of Logic,' p. 27.

† 'Laws of Thought,' chapter xix. p. 295. The use of the word statistical as equivalent to numerical is erroneous, although sanctioned by so high an authority as Sir J. Herschel, who applied it to the numbering of the stars. Statistical means what refers to the State or People.

tions," by which he evidently means, "Numerical Conditions." It contains a most remarkable and powerful attempt to erect a general method for ascertaining the higher and lower limits of logical classes, to be employed as a subsidiary portion of his general calculus of probabilities. A paper on the same subject had been previously written by him, and entitled "Of Propositions Numerically Definite," but was only published after his death, by Professor De Morgan in the 'Transactions of the Cambridge Philosophical Society' (vol. xi. part ii. 1868). Of these writings of Professor Boole I must say, what I have elsewhere said of other portions of his writings, that they appear in themselves perfect and almost inimitable. At the same time I must add that Boole's extraordinary power of analysis, and his perfect command of symbolic methods, usually led him to over-estimate the part they should play in reasoning, and to under-estimate the value of a simple and intuitive comprehension of the subject. The very principle which he fearlessly adopts, that unintelligible symbols may give intelligible and even demonstrative results, will probably be rejected by future mathematicians, as it has been lately rejected in the strongest terms by Mr. Sandeman*.

3. As Mr. Boole's logical views were the basis from which I started in forming the simple but general system of logical forms explained in my 'Pure Logic' in the year 1864, and, more simply still, in my 'Substitution of Similars,' published in 1869, so the numerically definite system of reasoning which is here described arises from a simplification of the previous methods of De Morgan and Boole.

4. In the qualitative system of logic, which I have given in the works referred to, a term is taken to mean the quality or group of qualities which belong to and mark out a class of objects. Thus, the general term A stands for any group of qualities belonging to a class of objects.

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* 'Pelicotetics,' 1868, Preface, pp. ix and x.

Let the term, when enclosed in brackets, acquire a quantitative meaning, so as to denote the number of individuals or objects which possess those qualities. Then

$(A)=$ number of objects possessing qualities of A, or say, for the sake of brevity, the number of A's. If, for instance,

$A=$ character and quality of being a Member of Parliament,

$(A)=$ number of existing Members of Parliament $=658$.

5. Every logical proposition or equation now gives rise to a corresponding numerical equation. Sameness of qualities occasions sameness of numbers. Hence if

$$A=B$$

denotes the identity of the qualities of A and B, we may conclude that

$$(A)=(B).$$

It is evident that exactly those objects, and those objects only, which are comprehended under A must be comprehended under B. It follows that wherever we can draw an equation of qualities, we can draw a similar equation of numbers. Thus, from

$$A=B=C,$$

we infer

$$A=C;$$

and similarly from

$$(A)=(B)=(C),$$

meaning the number of A's and C's are equal to the number of B's, we can infer

$$(A)=(C)$$

But, curiously enough, this does not apply to negative propositions and inequalities. For if

$$A=B\sim D$$

means that A is identical with B, which differs from D, it does not follow that

$$(A) = (B) \sim (D).$$

Two classes of objects may differ in qualities, and yet they may agree in number. This is a point which strongly confirms me in the opinion I have already expressed, that all inference really depends upon equations, not differences*; and I shall therefore employ throughout this paper only equations which may be almost indifferently used in the qualitative or quantitative meaning.

6. I shall employ, as in logic, a joint term, such as A B (or A B C), to mean the class possessing all the qualities of A and B (or of A and B and C). To every positive term there corresponds a negative term, denoted by the corresponding small italic letter. Thus the negative of A is *a*, of B *b*, and so on. If, then, A means *man*, *a* means simply *not man*. Hence *a b* will mean the combination of qualities of *not being* A and *not being* B.

7. The sign ·|· is used to stand for the disjunctive conjunction *or*, but in an unexclusive sense. Thus

$$A = B \cdot\!|\!\cdot C$$

means that whatever has the qualities of A, must have either the qualities of B or of C; but it *may have the qualities of both*. This unexclusive character of the terms and signs of logic, which creates a profound difference between my system and that of Prof. Boole, prevents me from converting alternatives into numbers as they stand. It does not follow from the statement that A is either B or C, that the number of A's is equal to the number of B's added to the number of C's; for some objects, or possibly all, may have been counted twice in this addition. Thus, if we say *An elector is either an elector for a borough, or for a county, or for a university*, it does not follow that the

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* Substitution of Similars, pp. 16, 17.

total number of electors is equal to the number of borough, county, and university electors added together; for some men may be found in two or three of the classes.

8. This difficulty, however, is avoided with great ease; for we need only develop each alternative into all its possible subclasses and strike out any subclass which appears more than once, and then convert into numbers. Thus, from

$$A = B \cdot|\cdot C$$

we get

$$A = BC \cdot|\cdot Bc \cdot|\cdot BC \cdot|\cdot bC\,;$$

but striking out one of the terms BC as being superfluous, we have

$$A = BC \cdot|\cdot Bc \cdot|\cdot bC.$$

The alternatives are now strictly exclusive, or devoid of any common part, so that we may draw the numerical equation

$$(A) = (BC) + (Bc) + (bC).$$

Thus, if $A =$ elector,
$B =$ borough elector,
$C =$ county elector,
$D =$ university elector,

we may from the proposition,

$$A = B \cdot|\cdot C \cdot|\cdot D$$

draw the numerical equation

$$(A) = (BCD) + (BCd) + (BcD) + (Bcd) + (bCD) + (bCd) + (bcD).$$

9. The process of development employed above is the great peculiarity of Prof. Boole's system of logic, and that which I have adopted. It depends upon the primary law of thought, usually called the Law of Excluded Middle, but which I prefer to call the *Law of Duality.* Whatever

the terms A and B may consist of, it is necessarily true, according to this law, that

A is B or not B;

in symbols

$$A = AB \cdot\!|\!\cdot Ab.$$

If any third term C enters into a problem, it is equally certain that

$$A = AC \cdot\!|\!\cdot Ac;$$

and combining these two developments, we have

$$A = ABC \cdot\!|\!\cdot ABc \cdot\!|\!\cdot AbC \cdot\!|\!\cdot Abc.$$

The same process of subdivision can be carried on *ad infinitum* with respect to any terms that occur; and this Indirect Method of Inference, which I have described in the books mentioned, consists in determining the possible existence of the various alternatives thus produced. The nature and procedure of this method will, as far as possible, be rendered apparent in the mode of treating numerical questions. It has also been partially explained to the Society, in connexion with the logical abacus, in which the working of the method is mechanically represented (Proceedings of the Manch. Lit. and Phil. Soc. April 3rd 1866, p. 161; see also Philosophical Transactions, 1870, p. 497).

10. The data of any problem in numerically definite logic will be of two kinds:—

1. The logical conditions governing the combinations of certain qualities or classes of things, expressed in propositions.
2. The numbers of individuals in certain logical classes existing under those conditions.

The *quæsitum* of the problem will be to determine the numbers of individuals in certain other logical classes existing under the same logical conditions, so far as such numbers are rendered determinable by the data. The usefulness of the method will, indeed, often consist in showing whether or not the magnitude of a class is determined or not, or in indicating what further hypotheses or data are required. It will appear, too, that where an exact result is not determinable we may yet assign limits within which an unknown quantity must lie.

11. Let us suppose, as an instance, that in a certain statistical investigation, among 100 A's there are found 45 B's and 53 C's; that is to say, in 45 out of one hundred cases where A occurs B also occurs, and in 53 cases C occurs. Suppose it to be also known that wherever B is, C also necessarily exists. The data then are as follows:—

$$\text{Numerical equations} \begin{cases} (A) = 100 & \dots\dots \quad (1) \\ (B) = 45 & \dots\dots \quad (2) \\ (C) = 53 & \dots\dots \quad (3) \end{cases}$$

$$\text{Logical equation} \quad . \quad . \quad B = BC.$$

Let it be required to determine

(1) The number of cases where C exists without B.

(2) The number of cases where neither B nor C exists.

The logical equation asserts that the class B is identical with the class B C, which is the true mode of asserting that all B's are C's. Two distinct results follow from this, namely:—1st, that the number of the class BC is identical with the number of the class B; and, 2nd, that there are no such things as B's which are not C's.

The logical equation is thus exactly equivalent to two additional numerical equations, namely,

$$(B) = (BC) \quad \dots\dots \quad (4)$$
$$(Bc) = 0 \quad \dots\dots \quad (5)$$

We have now full means of solving the problem; for, by the law of duality,

$$(C) = (BC) + (bC)$$

By (4)

$$= (B) + (bC),$$

Thus

$$53 = 45 + (bC),$$

whence

$$(bC) = 8,$$

which is the first quæsitum.

To obtain the second, the number of A*bc*'s, we have

$$(A) = (ABC) + (ABc) + (AbC) + (Abc)$$
$$100 = 45 + 0 + 8 + (Abc)$$

Hence

$$(Abc) = 47.$$

I now proceed to exemplify the use of the method by applying it to examples drawn chiefly from previous writers.

12. Professor De Morgan suggests the following as an argument which cannot be put into any ordinary form of the syllogism*.

"For every man in the house there is a person who is aged; some of the men are not aged. It follows, that some persons in the house are not men."

This argument proceeds, as I conceive, not by any form of syllogism, but by a pair of simple equations. Taking

A = man,
B = aged person,

and putting w, w' for unknown and indefinite numbers, the first premise gives the equation

$$(A) = (B) - w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

meaning that the number of aged persons equals or exceeds the number of men. The second statement may be put in this form,

$$(Ab) = w' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

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* Syllabus of a proposed System of Logic, 1860, p. 29.

which implies that there is a certain indefinite number of men who are not aged.

Develope A and B in (1) by the law of duality, and we have

$$(\mathrm{AB}) + (\mathrm{A}b) = (\mathrm{AB}) + (a\mathrm{B}) - w.$$

Subtract (AB) from both sides, and insert for (Ab) its value in (2), and we have

$$(a\mathrm{B}) = w + w' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (3)$$

which proves that there are some aged persons in the house who are not men, and assigns their quantity, so far as it can be assigned. The number of such persons we learn is at least equal to the number of men who are not aged, and exceeds it by w—that is, the excess of the number of aged persons over the men, if such excess exists, which the premises do not determine.

Adding (ab) to both sides of (3) we get

$$(a) = w + w' + (ab)\,;$$

but this expression contains two unknown quantities, namely, w and (ab). As no quantity can be intrinsically negative, w' is the lowest limit of the number of persons who are not men; and the number is to be increased by w, if it have value, and also by the number of persons, if such there be, who are neither men nor aged.

13. The most celebrated instance to which this method can be applied is one also proposed by Professor De Morgan*, and discussed by Boole†. It is as follows:—

Most B's are A's . . . . (1)
Most B's are C's . . . . (2)
Therefore some C's are A's . . . . (3)

Here, of course, *most* means more than half, and is one of

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* Formal Logic, p. 163.

† Trans. of the Cambridge Philosophical Society, vol. xi. part ii. p. 1.

the few quantitative expressions used in ordinary language. We can easily represent the two premises in the form

$$(AB) = \frac{(B)}{2} + w \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

$$(BC) = \frac{(B)}{2} + w' \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

To deduce the conclusion, we must add these equations together, thus,

$$(AB) + (BC) = (B) + w + w'.$$

Developing the logical terms on each side, we have

$$(ABC) + (ABc) + (ABC) + (aBC) = (ABC) + (ABc) + (aBC) + (aBc) + w + w',$$

Subtracting the common terms, there remains

$$(ABC) = w + w' + (aBc).$$

The meaning of this conclusion is, that there must be some C's which are A's, amounting to at least the sum of the quantities w and w', the unknown excesses beyond half the B's which are A's and C's. The number (aBc) is wholly undetermined by the premises, but it cannot be negative; in proportion as its amount is greater, so is the number of the ABC's. The conclusion, in short, is that $w + w'$ is the lower limit of (ABC).

14. The above problem is only one case of a more general problem, which may be stated as follows:—Given the numbers of three classes of objects, A, B, and C, to determine what circumstances or conditions will necessitate the existence of a class ABC.

This may be solved very simply

$$\begin{aligned}(B) + (C) - (A) &= (ABC) + (ABc) + (ABC) + (AbC) - (A)\\ &= (ABC) - (Abc),\end{aligned}$$

$$(ABC) = (B) + (C) - (A) + (Abc).$$

It is evident that the number of ABC's is indeterminate, because there is no condition to determine $(\mathrm{A}bc)$. But reducing this to its minimum, zero, we learn that the lower limit of (ABC) is the excess of the sum of B's and C's over A's. As no result can be negative, we also learn that if $(\mathrm{A}bc) = 0$, then (A) cannot exceed $(\mathrm{B}) + (\mathrm{C})$.

15. This method gives us a clear view of the conditions of any logical argument. Take a syllogism in Barbara, thus :—

Every A is B : in symbols A = AB.
Every B is C „ „ B = BC.
∴ Every A is C „ „ A = AC.

What additional information do we require in order to determine the number of all the classes of objects concerned ?

There are altogether eight conceivable combinations of A, B, C, and their negatives, *a*, *b*, *c*, according to the laws of thought; but of these, four combinations are rendered impossible by the premises, so that we have four quantities assigned—

$$(\mathrm{AB}c) = 0, \qquad (\mathrm{A}bc) = 0,$$
$$(\mathrm{A}b\mathrm{C}) = 0, \qquad (a\mathrm{B}c) = 0.$$

There remain, then, four unknown quantities ; and unless we have these assigned directly or indirectly, we do not really know the relative numbers of the classes. But the numbers of any four existing classes may, by a proper arrangement of equations, be made to yield the number of any other existing class. Thus, if

$$(\mathrm{A}) = 93 \qquad (\mathrm{C}) = 190$$
$$(a\mathrm{BC}) = \ 5 \qquad (abc) = \ 4,$$

we may draw the following conclusions :—

$$(ab\mathrm{C}) = (\mathrm{C}) - (\mathrm{A}) - (a\mathrm{BC}) = 190 - 93 - 5 = 92\,;$$
$$(\mathrm{B}) = (\mathrm{ABC}) + (a\mathrm{BC}) = 93 + 5 = 98.$$

16. It is interesting to compare my mode of treating numerically definite propositions with the earlier mode of Professor De Morgan. Taking X, Y, and Z to be the three terms of the syllogism, he adopts* the following notation :—

u = whole number of individuals in the universe of the problem.
x = number of X's.
y = number of Y's.
z = number of Z's.

Making m denote any positive number, mXY means that *m or more* X's are Y's. Similarly uYZ means that *u or more* Y's are Z's. Smaller letters denote the negatives of the larger ones, somewhat as in my system. Thus $m\mathrm{X}y$ means that m or more X's are not Y's, and so on.

From the two premises

$$m\mathrm{XY} \text{ and } n\mathrm{YZ},$$

Mr. De Morgan draws the two distinct conclusions

$$(m+n-y)\mathrm{XZ} \text{ and } (m+n+u-x-y-z)xy.$$

Let us consider what results are given by my own notation. The premises may be represented by the equations

$$(\mathrm{XY})=m+m' \qquad (\mathrm{YZ})=n+n',$$

where m and n are the same quantities as in Mr. De Morgan's system, and m' and n' two unknown but positive quantities, indicating that the number of XY's is *m or more*, and the number of YZ's is *n or more*.

The possible combinations of the three terms X, Y, Z, and their negatives are eight in number, namely :—

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* Syllabus, p. 27. Mr. De Morgan denotes negative terms by small Roman letters, for which I have substituted italic letters.

$$\begin{array}{ll} \mathrm{XYZ}, & x\mathrm{YZ}, \\ \mathrm{XY}z, & x\mathrm{Y}z, \\ \mathrm{X}y\mathrm{Z}, & xy\mathrm{Z}, \\ \mathrm{X}yz, & xyz, \end{array}$$

and these altogether constitute the universe, of which the number is n. The problem is at once seen to be indeterminate in reality; for there are eight classes, of which the number would have to be determined, and there are only six known quantities, namely, u, x, y, z, m, and n, by which to determine them. Accordingly we find that Mr. De Morgan's conclusons, though not absolutely erroneous, have little or no meaning. From the premises he infers that $(m+n-y)$ *or more* X's are Z's. Now

$$\begin{aligned} m+n-y &= (\mathrm{XY})+(\mathrm{YZ})-\mathrm{Y} \\ &= (\mathrm{XYZ})-(x\mathrm{Y}z). \end{aligned}$$

Thus Mr. De Morgan represents the number of the whole class, XZ, by a quantity indefinitely less than its own part, XYZ. It is quite true that if the second side $(\mathrm{XYZ})-(x\mathrm{Y}z)$ of this equation has value, there must be at least this number of X's which are Z's; but as $(x\mathrm{Y}z)$ may exceed (XYZ) in any degree, this may give zero or a negative result, while there is really a large number of XZ's. The true and complete expression for the number of XZ's is found as follows :—

$$\begin{aligned} &(\mathrm{XZ}) = (\mathrm{XYZ}) + (\mathrm{X}y\mathrm{Z}) \\ &= (\mathrm{XYZ}) + (\mathrm{XY}z) + (\mathrm{XYZ}) + (x\mathrm{YZ}) - (\mathrm{Y}) + (\mathrm{X}y\mathrm{Z}) + (x\mathrm{Y}z) \\ &= m + m' + n + n' - y + (\mathrm{X}y\mathrm{Z}) + (x\mathrm{Y}z). \end{aligned}$$

Among these seven quantities, only m, n, and y are definitely given. The two m' and n' are two indefinite quantities, expressing the uncertainty in the number of XY's and YZ's, while there are two other unknown quantities, the numbers of XyZ's and xYz's arising in the course of the problem.

17. Mr. De Morgan's second conclusion, that the number of not-X's which are not Y's is

$$(m+n+u-x-y-z)$$

or more, may be examined in like manner. By developing the classes numbered in each of these quantities, and striking out the redundant terms, we obtain $(xyz)-(\mathrm{X}y\mathrm{Z})$, in which the term $(\mathrm{X}y\mathrm{Z})$ is wholly undetermined. Here, again, we have as the lower limit of the class xz a quantity indeterminately less than its own part xyz. The number (xz) may accordingly be of any magnitude, while the limit here assigned to it is zero, or even negative.

Exactly similar remarks may be made concerning the other conclusions which Mr. De Morgan draws. Thus, from $m\mathrm{X}y$ and $n\mathrm{Y}z$ (mX's or more are not Y's, and nY's or more are Z's) he infers

$$(m+n-x)x\mathrm{Z} \text{ and } (m+n-z)\mathrm{X}z.$$

But it will be found by analysis that the first of these results has the following meaning:—

$$(x\mathrm{Z}) \overset{=}{>} (x\mathrm{YZ})-(\mathrm{XY}z);$$

that is to say, the lower limit of the class xZ is a part of itself, xYZ, diminished by the number of another class XYz.

While believing, however, that Mr. De Morgan's mode of treating the subject admits of improvement, it is impossible that I should undervalue the extraordinary acuteness and originality of his writings on this and many other parts of formal logic. Time is required to reveal the wealth of thought which he has embodied in his 'Formal Logic,' and in his Logical Memoirs published by the Cambridge Philosophical Society.

18. In Mr. De Morgan's third paper on the syllogism *

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* Cambridge Phil. Trans. vol. x. part i. p. 8.

he puts the syllogism in the following form:—"If the fractions α and β of the Y's be severally A's and B's, and if $\alpha+\beta$ be greater than unity, it follows that some A's are B's. . . . The logician demands $\alpha=1$ or $\beta=1$, or both; he can then infer." These arguments are readily represented in my notation as follows:—

The premises are $\alpha\,.\,(Y)=(AY),$

$$\beta\,.\,(Y)=(BY).$$

Hence

$$(\alpha+\beta)\;(Y)=(AY)+(BY)$$

$$=(ABY)+(AbY)+(ABY)+(aBY),$$

$$(\alpha+\beta)\,(Y)-(Y)=(ABY)-(abY),$$

or

$$(ABY)=(\alpha+\beta-1)\;(Y)+(abY).$$

From this we learn that the number of A's which are B's, *because they are Y's*, is the fraction $(\alpha+\beta-1)$ of the Y's together with the undetermined number (abY), which cannot be negative. Hence if $\alpha+\beta>1$, the second side has a positive value, and there must be some A's which are B's. If $\alpha=1$, then this number is $\beta\,.\,(Y)$, or if $\beta=1$, it is $\alpha\,.\,(Y)$, since (abY) then $=0$. If $\alpha=1$ and $\beta=1$, then obviously $(ABY)=(Y)$.

19. In Mr. Mill's chapter "On Chance and its Eliminations"*, occurs a problem concerning the coexistence of two phenomena, in which he asserts the general proposition "that, if A occurs in a larger proportion of the cases where B is than of the cases where B is not, then will B also occur in a larger proportion of the cases where A is than of the cases where A is not."

This proposition is not proved by Mr. Mill, nor do I remember seeing any proof of it; and it is not, to my mind, self-evident. The following, however, is a proof of its truth, and is the shortest proof I have been able to find.

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* System of Logic, 5th ed. vol. ii. p. 54.

The condition of the problem may be expressed in the inequality

$$\frac{(\mathrm{AB})}{(\mathrm{B})} > \frac{(\mathrm{A}b)}{(b)},$$

or reciprocally in the inequality

$$\frac{(\mathrm{B})}{(\mathrm{AB})} < \frac{(b)}{(\mathrm{A}b)}.$$

Subtracting unity from each side, and simplifying, we have

$$\frac{(a\mathrm{B})}{(\mathrm{AB})} < \frac{(ab)}{(\mathrm{A}b)}.$$

Multiplying each side of this inequality by $\frac{(\mathrm{A}b)}{(a\mathrm{B})}$ we obtain

$$\frac{(\mathrm{A}b)}{(\mathrm{AB})} < \frac{(ab)}{(a\mathrm{B})}.$$

Restoring unity to each side, and simplifying

$$\frac{(\mathrm{A})}{(\mathrm{AB})} < \frac{(a)}{(a\mathrm{B})},$$

or reciprocally

$$\frac{(\mathrm{AB})}{(\mathrm{A})} > \frac{(a\mathrm{B})}{(a)},$$

which expresses the result to be proved, namely, that B occurs in a larger proportion of the cases where A is than of the cases where A is not.

20. The examples hitherto considered have been mostly free from logical conditions; that is to say, the classes of objects have been supposed capable of combination or coincidence in all conceivable ways. We will briefly examine the effects of certain simple logical conditions.

If there be two terms A and B, and one condition, *all A's are B's*, symbolically expressed in the equation

$$\mathrm{A} = \mathrm{AB},$$

then there will be three possible classes to be determined, namely,

AB,
*a*B,
ab,

and we shall require three assigned quantities. If we have (U) = whole number of objects, with (A) and (B), then

$$(AB) = (A)$$
$$(aB) = (B) - (A)$$
$$(ab) = (U) - (B).$$

21. If with two terms, A and B, the logical condition be A = B, there will remain two classes only, AB and *ab*, and two assigned quantities only will be required. The same would happen with any of the conditions A = *b*, *a* = B, or *a* = *b*.

22. In any problem involving three terms or classes of things, say A, B, and C, there arise eight conceivable classes, the numbers of which may have to be determined. Various logical conditions, however, greatly reduce the numbers. Thus the two conditions

$$A = B = C$$

leave only two possible classes, ABC, and *abc*.

The two conditions

$$A = AB \text{ and } B = BC$$

leave four classes,

ABC, *a*BC, *ab*C, and *abc*.

23. The two conditions A is B or C, but B cannot be C, symbolically expressed

$$A = AB \cdot\!|\!\cdot AC, \qquad B = Bc,$$

leave five classes,

AB*c*, A*b*C, *a*B*c*, *ab*C, *abc*.

24. These few examples illustrate the way in which the indirect method of inference, described in my 'Pure Logic,' determines the number of possible classes which may exist under certain logical conditions, and thus enables us to ascertain at once whether there are data sufficient to determine their magnitude. Various examples of the process may be found in the work referred to.

25. My formulæ will also, I believe, be found to yield all the aid to the calculation of probabilities which can be expected from the science of logic. When the combinations of events are not governed by any special logical conditions, the application of the logical formulæ to probabilities is exceedingly simple. It is only necessary in the logical formula to substitute for each term its probability of occurrence, and to multiply or add as the logical signs indicate.

Thus, if p is the probability of the event A happening, and q of B, then pq is the probability of the conjunction of events AB happening; similarly the probability of A not happening, that is, of a happening, is $1-p$; of b, $1-q$. According we have the following:—

Probability of $AB = pq$.
,, ,, $Ab = p(1-q)$.
,, ,, $aB = (1-p)q$.
,, ,, $ab = (1-p)(1-q)$.

26. In chapter xviii. of his 'Laws of Thought,' Boole has given several examples of the application of his very complicated General Method of Probabilities. Of these examples my notation will give a vastly simpler solution, as I proceed to show.

Boole's third example is as follows (p. 279):—

"The probability that a witness, A, speaks the truth is p, the probability that another witness, B, speaks the truth is q, and the probability that they disagree in a statement is r. What is the probability that if they agree in a statement, their statement is true?"

This is solved in the simplest possible manner. Let

$\alpha =$ prob. of A and B both speaking truth.
$\beta =$ prob. of A but not B " "
$\gamma =$ prob. of not A but B " "
$\delta =$ prob. of neither A nor B " "

Then we have the following data:—

Prob. of A speaking truth $=\alpha+\beta=p$.
" " B " " $=\alpha+\gamma=q$.
Prob. that they disagree $=\beta+\gamma=r$.

As it is certain that one or other of the alternatives must happen, we have the condition

$$\alpha+\beta+\gamma+\delta=1.$$

These four equations are sufficient to determine all the four unknown quantities by ordinary algebra. Thus

$$\alpha=\frac{p+q-r}{2},$$

$$\delta=1-(\alpha+\beta+\gamma)=1-\frac{p+q-r}{2}-r,$$

$$=1-\frac{p+q+r}{2}.$$

Now the probability required is, that if A and B agree in a statement their statement is true. By the principles of probability this is $\frac{\alpha}{\alpha+\delta}$; and inserting the above values of α and δ we have

$$\frac{\alpha}{\alpha+\delta}=\frac{p+q-r}{2(1-r)}.$$

which is the same as the result which Boole obtained in a much more complicated manner. This verifies the anticipations both of Boole himself (p. 281) and of Mr.

Wilbraham*, in his criticism on Boole's 'Method of Probabilities,' "that the really determinate problems solved in the book, as 2 and 3 of chapter xviii., might be more shortly solved." Boole remarks, indeed, that they do not fall directly within the scope of known methods; but I conceive that my logical symbols and method furnish all that is required.

27. In a similar manner we may solve the second of Boole's examples referred to by Mr. Wilbraham; this is as follows:—

"The probability that one or both of two events happen is p, that one or both of them fail is q. What is the probability that only one of these happens?"

Using α, β, γ, δ to denote the probabilities of the four obvious conjunctions of events, as before, we have the data,

$$\alpha+\beta+\gamma=p,$$
$$\beta+\gamma+\delta=q,$$
$$\alpha+\beta+\gamma+\delta=1.$$

The probability required is $\beta+\gamma$, and

$$\begin{aligned}\beta+\gamma=q-\delta&=q-1+\alpha+\beta+\gamma\\&=q-1+p.\end{aligned}$$

This is Mr. Boole's result, obtained by him in a much more complicated manner.

28. This simple substitution of the probability of an event for its logical symbol cannot be valid, however, if there be any connexion between the events which renders one more or less likely to happen when the other happens. The probabilities of A and B being p and q, the probability of AB is pq, under the supposition that B is just as likely to happen when A happens as when A does not happen, and similarly that A is just as likely to happen

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* Philosophical Magazine, 4th Series, vol. vii. p. 465; vol. viii. p. 91.

when B does as when B does not; in short, that they are *independent events.* As a case where we are not to assume logical independence, we may take the following example from Boole's work (p. 276) :—

Example 1. "The probability that it thunders upon a given day is p, the probability that it both thunders and hails is q; but of the connexion of the two phenomena of thunder and hail nothing further is supposed to be known. Required the probability that it hails on the proposed day."

Let A mean that it thunders
B " " hails;

Then there are four possible events, AB, Ab, aB, ab.

The probabilities given are—

$$\text{Prob. of A} = p,$$
$$\text{,, ,, AB} = q.$$

The probability required is that of B, which is evidently

$$\text{prob. of AB} + \text{prob. of } a\text{B}.$$

Now the probability of AB is given, but the probability of aB is not given, and we cannot assume it to be $(1-p) \times$ (prob. of B), because we are told that nothing is known of the connexion of the phenomena, which implies that they may have some connexion by causation, so that the non-occurrence of A will alter the probability of the occurrence of B. The prob. of aB is therefore unknown, except that it is the prob. of a multiplied by the unknown prob. that, if a occurs, B occurs with it, as Boole points out. Hence the only possible answer is the same as Boole's

$$\text{prob. of B} = q + (1-p)c,$$

c being an unknown quantity, of course not exceeding unity. Making c successively $= 1$ and 0, the major and minor limits of the probability are evidently $q+1-p$ and q.

Were the events A and B independent, we should have

$$\text{Prob. of B} = q + (1-p)\,(\text{prob. of B}),$$

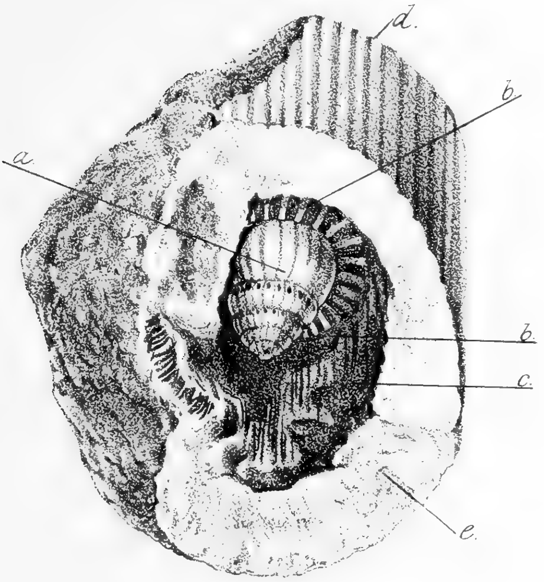
$$= \frac{q}{1-(1-p)} = \frac{q}{p}.$$

29. It may be truly remarked of what is given in this paper, that all the results can be reached by the exertion of common sense, or by ordinary mathematical calculation; and I do not doubt that problems combining logical and mathematical conditions of a more complicated character have been solved, especially in the 'Theory of Probabilities,' by those who were unconscious of using any peculiar logical method; but what I claim for my logical method and notation is, that it is in no sense or way peculiar, but represents truthfully and completely the natural course of intelligent thought. The indirect method, first explained in 1864 in my 'Pure Logic,' embodied in the mechanical device called the Logical Abacus, explained to the Society in April 1866, and further exemplified in the Logical Machine lately brought before the Royal Society, represents the exhaustive and necessary classification of objects which the mind must make under any logical conditions. Of previous systems, Boole's mathematical method could alone be said to do this; and his method was deformed by needless obscurity, and by at least one deep-seated error. It has been my purpose in this paper to exemplify the way in which a true and simple logical method lends its aid to all such mathematical problems as involve logical considerations. The number of such problems requiring solution is not great, unless, perhaps, in the theory of probabilities; but I believe that in the progress of science the number will probably increase. And whether this be

so or not, we must not estimate the value of a theory by its immediate practical results.

30. Logical method must undoubtedly be the root of all scientific demonstration, and of all sound thought in the common affairs of life; yet we find the most opposite and contradictory opinions held by different logicians as to the nature of the reasoning process. Metaphysical speculation will never remedy the present deplorable condition of the science; for it is metaphysical speculation which has mystified the subject, and rendered it the laughing-stock of scientific men. Antiquarian research into the errors of earlier logicians, in which some logicians still exclusively employ themselves, will only add to the perplexity and obscurity. I hold that logic can only be regenerated by those who will render themselves acquainted with the exact methods of research which lead to undoubted truths in the mathematical and physical sciences. Logic, in short, must be dissociated from metaphysics, with which it has no necessary connexion, and must become an exact science. We must therefore seek in every way to connect it with the other exact sciences. In this paper I have attempted to show that questions do exist in which logical and numerical methods coalesce and lend mutual aid.

Fig. 1.



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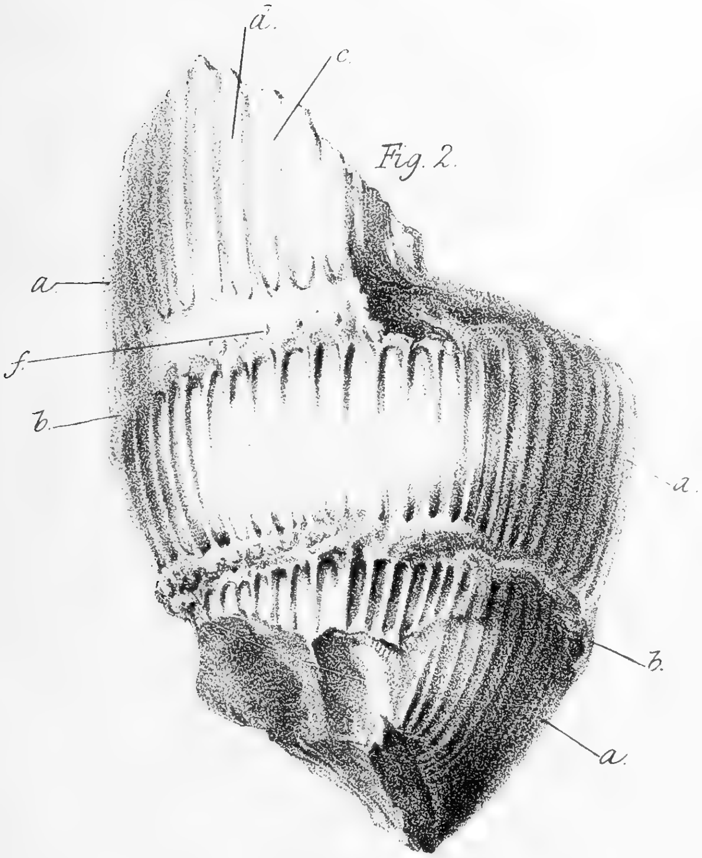

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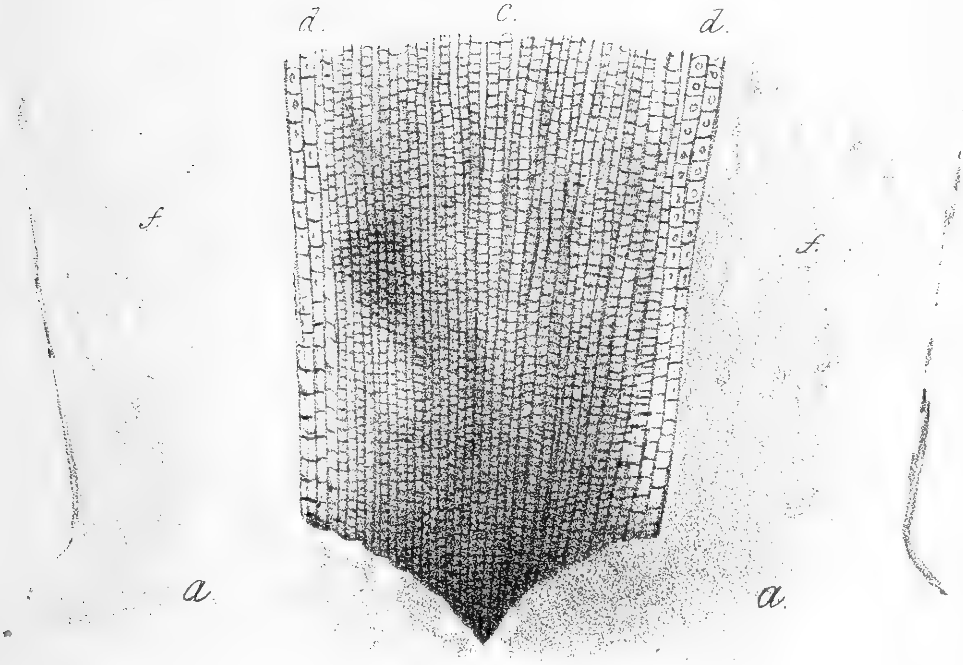

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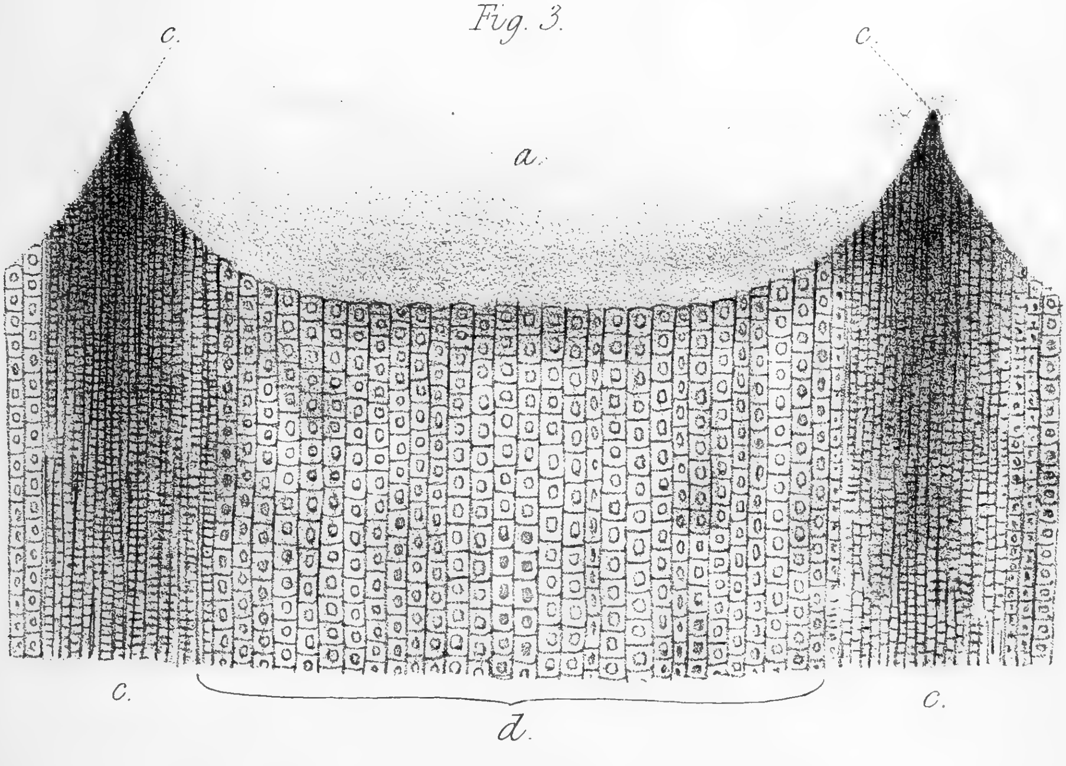

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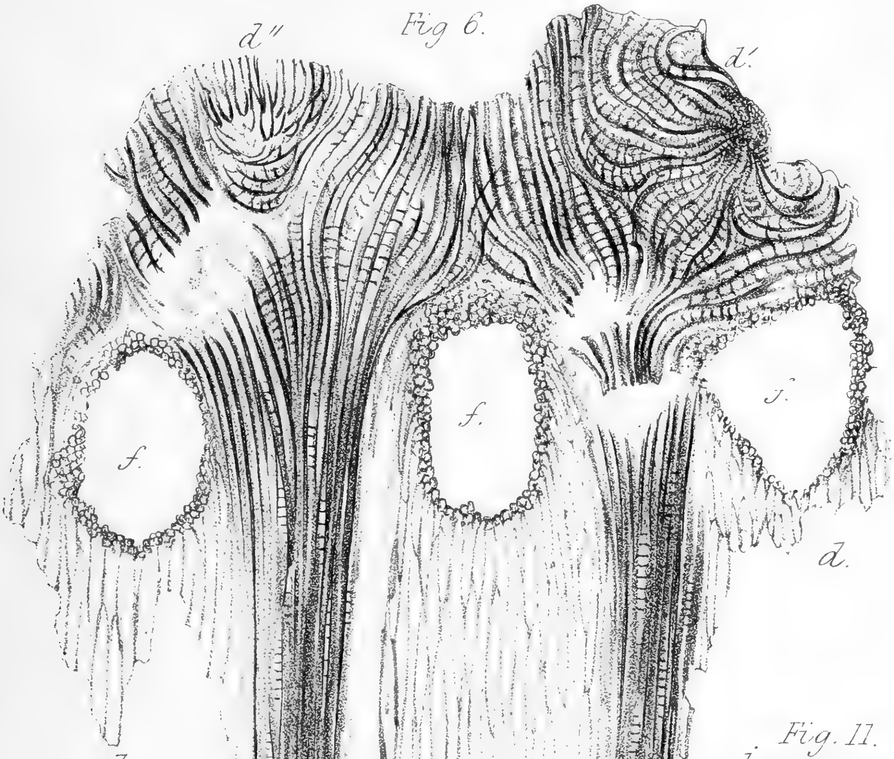

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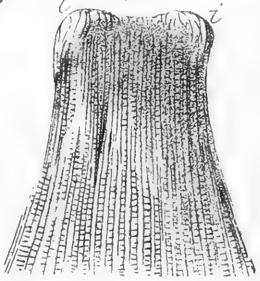

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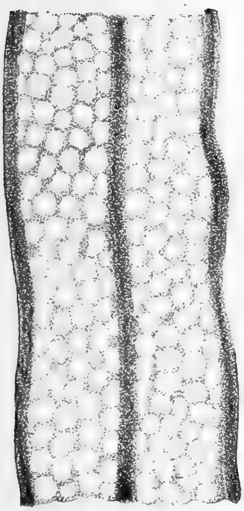

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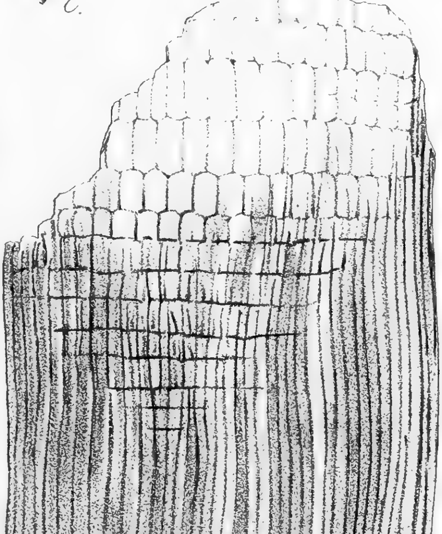

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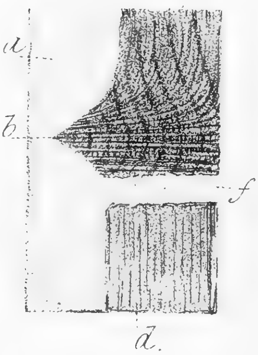

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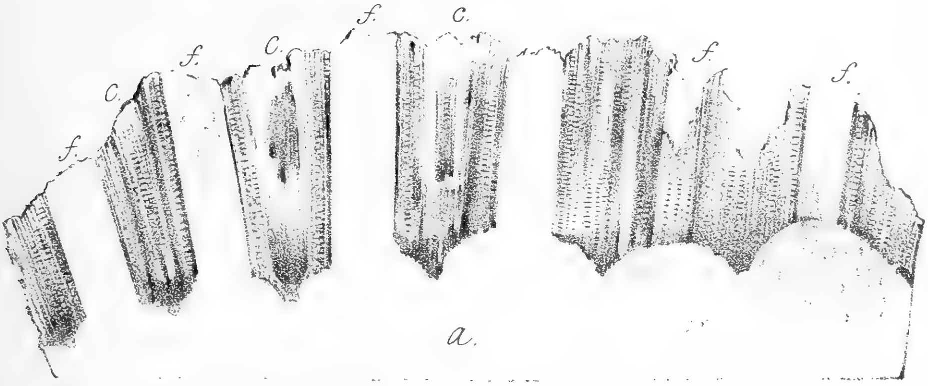

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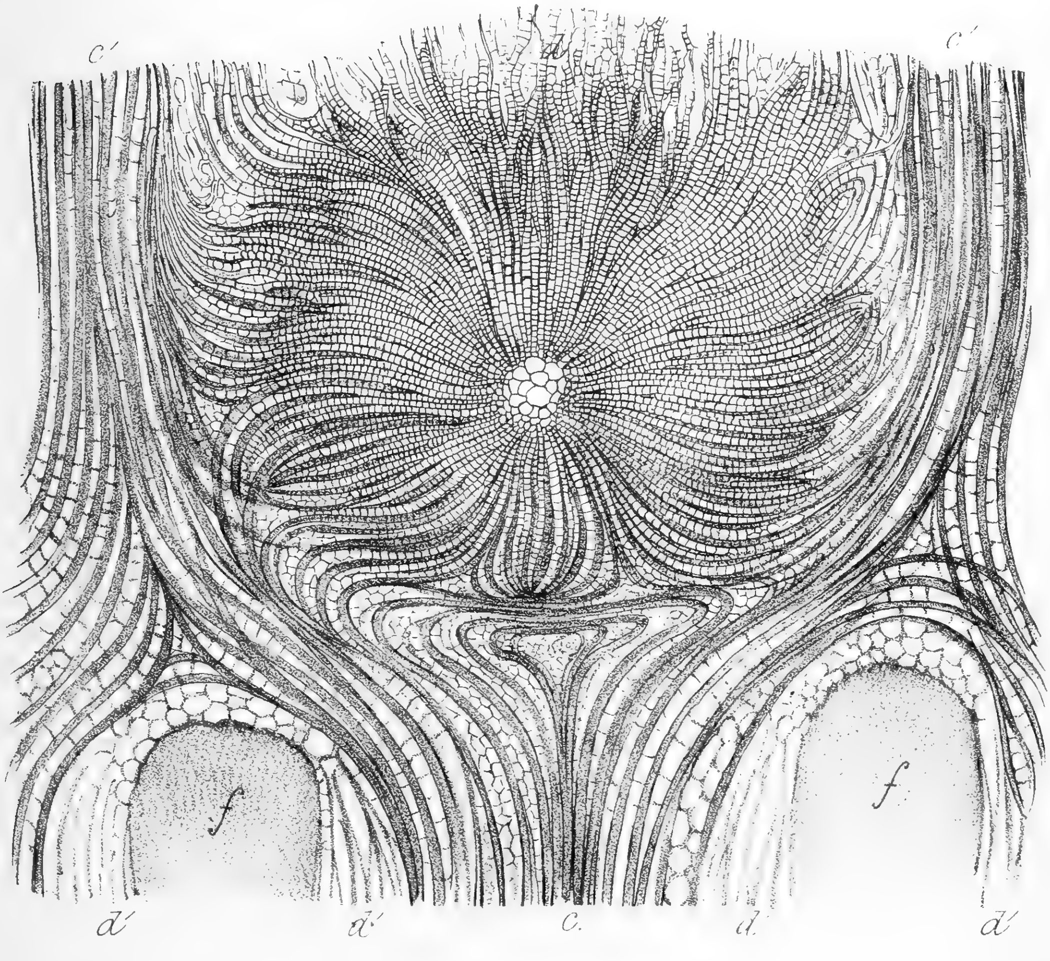

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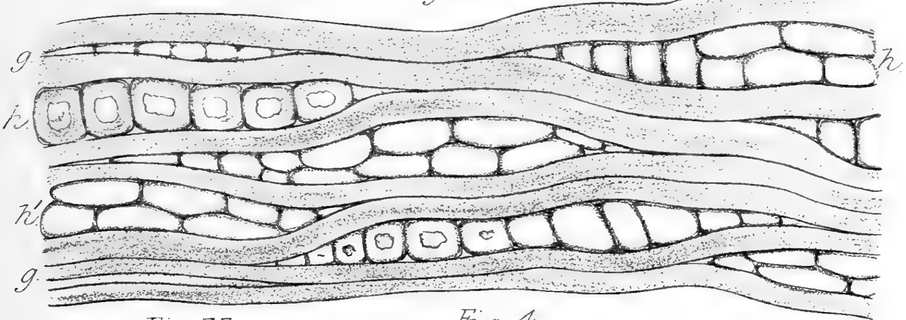

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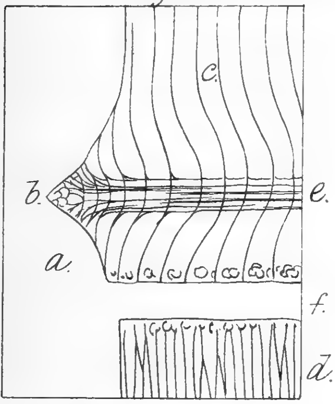
Fig. 17.



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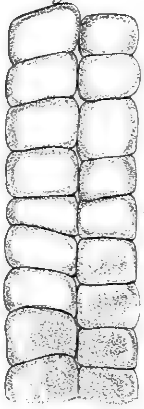

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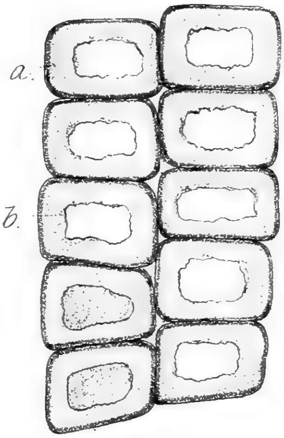

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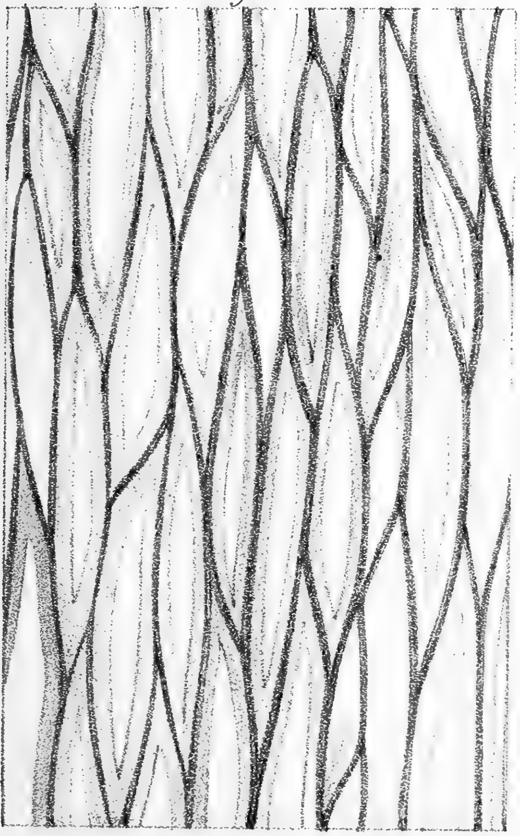

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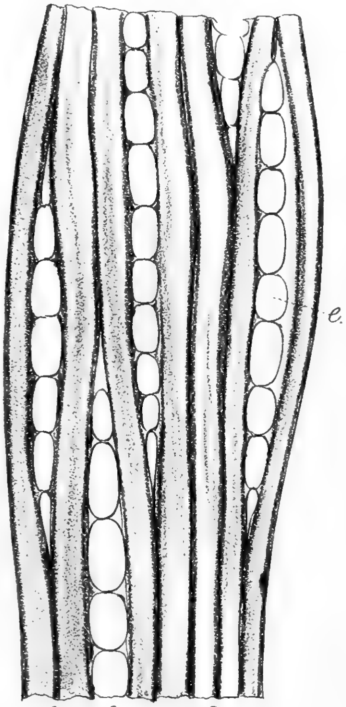

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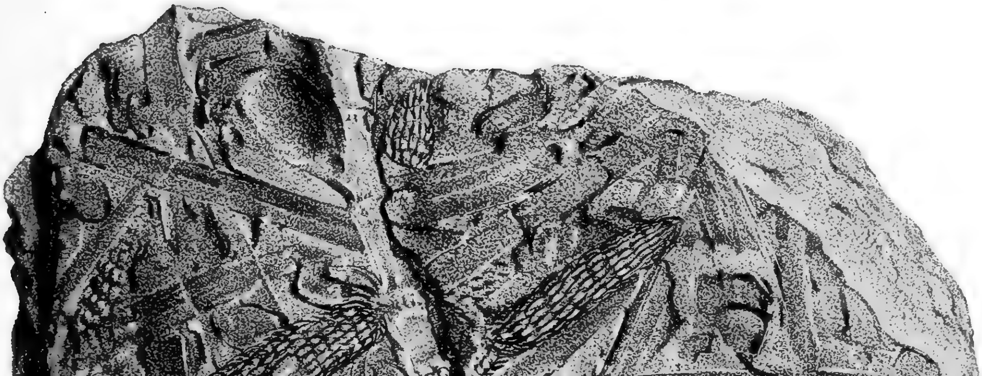

Fig. 2.

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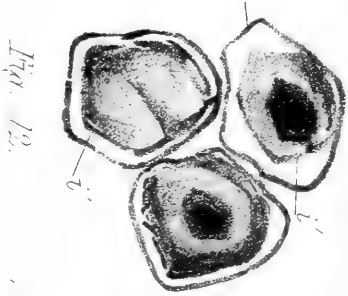
Fig. 6.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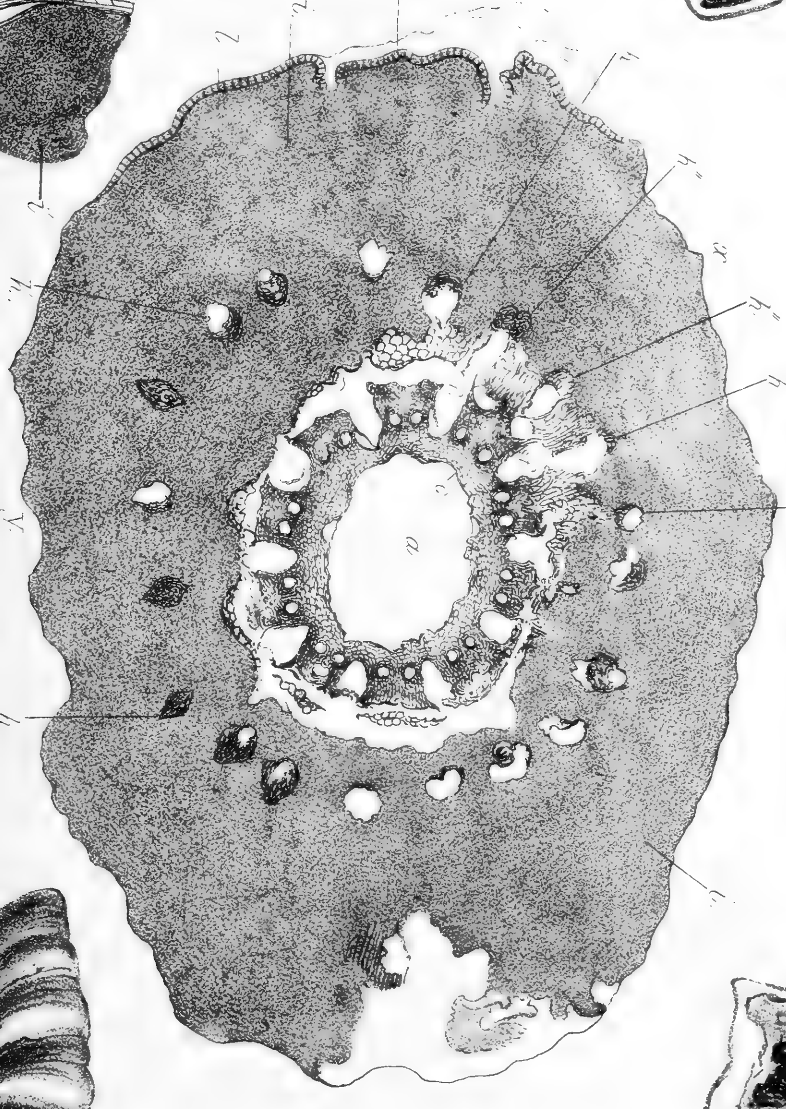
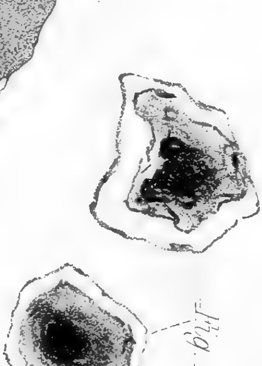


Fig. 4.


Fig. 1.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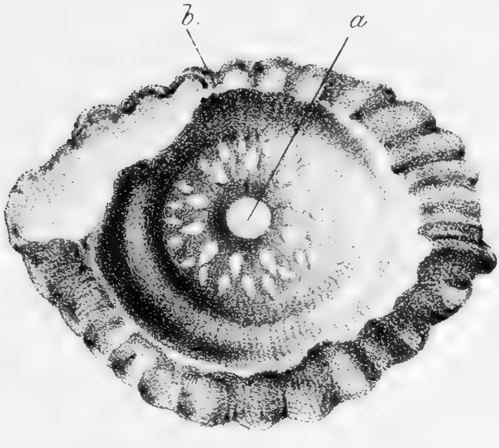

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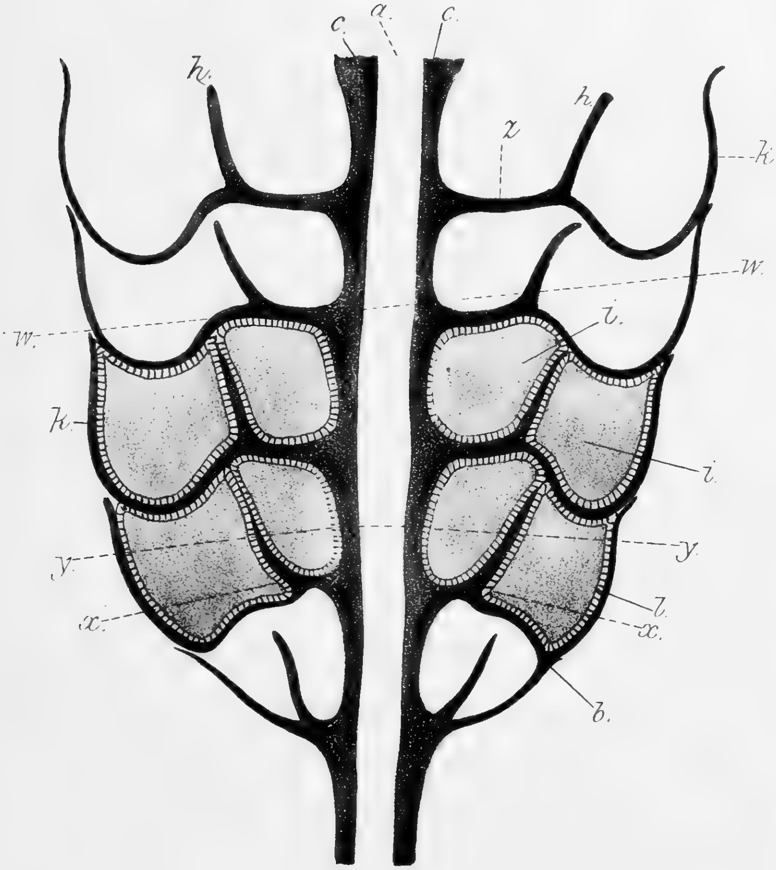

Fig. 13.

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R. Fitch, imp.

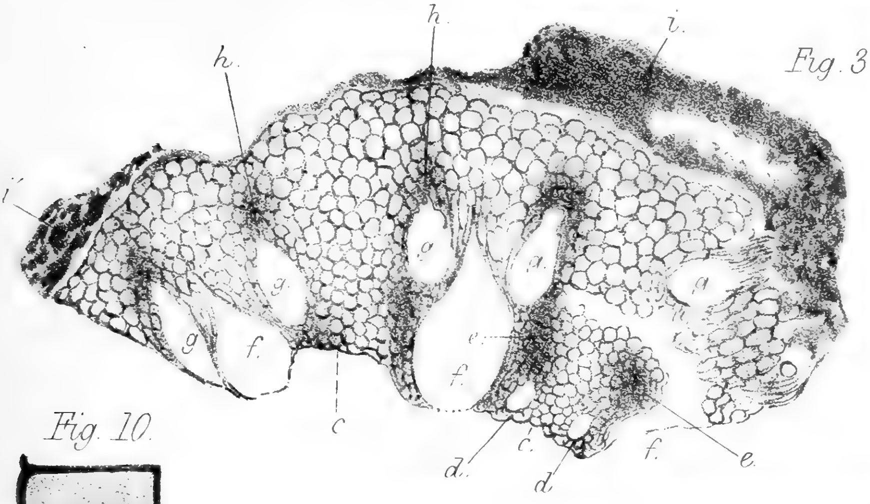

Fig. 3.

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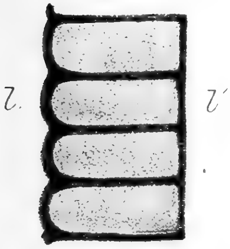

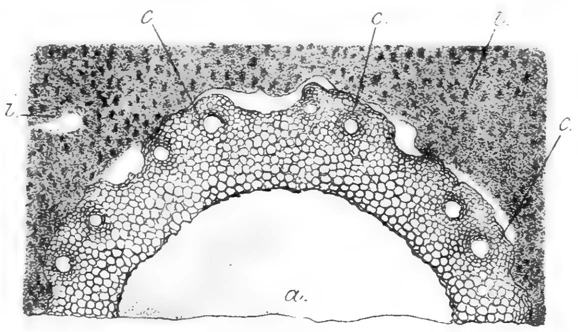

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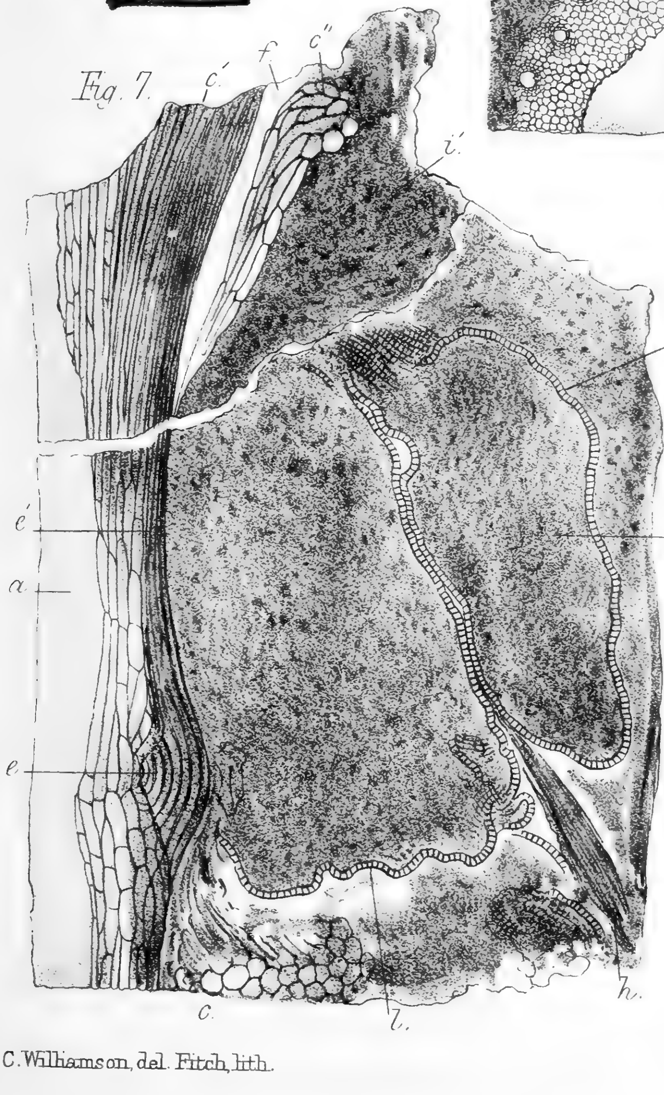


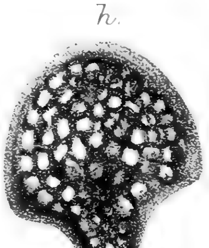
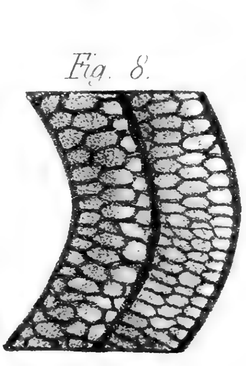


Fig. 9.

Fig. 8.



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1843, Feb. 7.	Sedgwick, Rev. Adam, M.A., F.R.S., Hon. M.R.I.A., F.G.S., F.R.A.S., Woodwardian Lecturer Univ. Cambridge. *Trinity College, Cambridge.*
1869, Dec. 14.	Sorby, Henry Clifton, F.R.S., F.G.S., Soc. Min. Petrop. Socius, Acad. Sci. Nat. Philad. et Lyc. Hist. Nat. Nov. Ebor. Corr. Mem. *Broomfield, Sheffield.*
1851, Apr. 29.	Stokes, George Gabriel, M.A., D.C.L., Sec. R.S., Lucasian Professor of Mathem. Univ. Cambridge, F.C.P.S., Mem. Batav. Soc. Rotterdam, Corr. Mem. Roy. Acad. Sc. Berlin. *Pembroke College, Cambridge.*
1861, Jan. 22.	Sylvester, James Joseph, M.A., F.R.S., Professor of Mathematics. *Royal Military Academy, Woolwich, London,* S.E.
1868, Apr. 28.	Tait, Peter Guthrie, M.A., F.R.S.E., Professor of Natural Philosophy, Edinburgh.
1851, Apr. 22.	Thomson, Sir William, M.A., D.C.L., LL.D., F.R.SS. L. and E., Prof. of Nat. Philos. Univ. Glasgow. 2 *College, Glasgow.*
1868, Apr. 28.	Tyndall, John, LL.D., F.R.S., F.C.S., Professor of Natural Philosophy in the Royal Institution and Royal School of Mines, Acadd. et Socc. Philomath. Par., Reg. Sci. Götting., Holland., Harl. Phys. et Nat. Hist. Genev., Nat. Ges. Tigur., Halæ, Marburg,

DATE OF ELECTION.	
	Vratisl., Upsal., Nat. Sc. Berol. Socius. *Royal Institution, London,* W.
1850, Apr. 30.	Woodcroft, Bennet, F.R.S., Professor, Superint. of Regist. of Patents. *Southampton Buildings, London,* W.C.

CORRESPONDING MEMBERS.

1860, Apr. 17.	Ainsworth, Thomas. *Cleator Mills, near Egremont, Whitehaven.*
1861, Jan. 22.	Buckland, George, Professor, University College, Toronto. *Toronto.*
1867, Feb. 5.	Cialdi, Alessandro, Commander, &c. *Rome.*
1870, Mar. 8.	Cockle, The Hon. Sir James, M.A., F.R.S., F.R.A.S., F.C.P.S., Chief-Justice of Queensland. *Brisbane, Queensland, Australia.*
1866, Jan. 23.	De Caligny, Anatole, Marquis, Corresp. Mem. Acadd., Sc. Turin and Caen, Socc. Agr. Lyons, Sci. Cherbourg, Liége, &c.
1861, Apr. 2.	Durand-Fardel, Max, M.D., Chev. of the Legion of Honour, &c. 36 *Rue de Lille, Paris.*
1849, Apr. 17.	Girardin, J., Off. Legion of Honour, Corr. Mem. Imper. Instit. France, &c. *Lille.*
1862, Jan. 7.	Gistel, Johannes Franz Xavier, Ph.D., late Prof. of Nat. Hist. and Geogr., Libr. Secr. and Conserv. at the Museum of Nat. Hist. Regensburg, Corr. Mem. Imper. Roy. Geol. Inst. Vienna, Acadd. and Socc. Sc. Cherbourg, Caen, Dijon, Aix, Orleans, Angers, Brussels, Rheims, Nantes, Antwerp, Linnean Socc. Caen, Angers, Marseilles, La Rochelle and Paris. 19 *Steinweg, Regensburg, Bavaria.*
1812, Jan. 24.	Granville, Augustus Bozzi, M.D., F.R.S., V.P.O.S., M.R.C.P. Lond., M.R.C.S. Engl., Knt. of the Order of St. Michael of Bavaria, of the Crown of Würtemberg, of the Lion of Züringen of Baden, and of

DATE OF ELECTION.	
	St. Maurice and St. Lazarus of Sardinia, For. Mem. Imper. Acad. Sc. Petersburg, Roy. Acadd. Sc. Turin and Naples, Nat. Hist. Soc. Dresden, Philom. Soc., Soc. Méd. d'Emulat. and Cercle Méd. Paris, Socc. Georg. and Curéo, Florence, Med.-Chir. Socc. Petersburg and Berlin, Corr. Mem. Roy. Acad. Sc. Brussels, &c. 95 *St. George's-road, London.*
1850, Apr. 30.	Harley, Rev. Robert, F.R.S., F.R.A.S. *Leicester.*
1816, Apr. 26.	Kenrick, Rev. John, M.A. *York.*
1838, Apr. 17.	Koechlin-Schouch, Daniel. *Mulhouse.*
1862, Jan. 7.	Lancia di Brolo, Federico, Duc. Inspector of Studies, &c. *Palermo.*
1859, Jan. 25.	Le Jolis, Auguste-François, Ph.D., Archiviste perpétuel and late President of the Imper. Soc. Nat. Sc. Cherbourg, Mem. Imp. Leop.-Car. Acad. Nat. Sc., Imp. Soc. Naturalists Moscow, Acad. Nat. Sc. Philadelphia, Roy. Botan. Socc. Regensburg, Leiden, Edinburgh, Botan. Soc. Canada, Linnean Socc. Lyon, Bordeaux, and Caen, Physiogr. Soc. Lund, Imp. Roy. Geol. Instit. Vienna, Imp. Roy. Zool. and Botan. Soc. Vienna, Roy. Acad. Sc. Lucca and Prague, Imp. Acad. Sc. and Lit. Chambery, Toulouse, Rouen, Caen, Lille, &c., Acad. Socc. Cherbourg and Angers, Hortic. Soc. Cherbourg, Roy. Acad. Archeol. Brussels, Socc. Nat. Sc. Catania, Athens, Boston, Dorpat, Riga, &c. *Cherbourg.*
1857, Jan. 27.	Lowe, Edward Joseph, F.R.S., F.R.A.S., F.G.S., Mem. Brit. Met. Soc., Hon. Mem. Dublin Nat. Hist. Soc., Mem. Geol. Soc. Edinburgh, &c. *Nottingham.*
1861, Oct. 29.	Maury, Captain Mathew Fontaine, LL.D., &c.
1864, Apr. 19.	Mitchell, Jesse, Captain, Superintendent of the Government Museum, Madras.
1862, Jan. 7.	Nasmyth, James, C.E., F.R.A.S., &c. *Penshurst, Tunbridge.*
1851, Apr. 29.	Pincoffs, Peter, M.D., Knt. of the Turkish Order of the "*Medjidié*" 4th Cl., Mem. Coll. Phys. London, Brussels, and Dresden, Hon. and Corr. Mem. Med. and Phil. Socc. Antwerp, Athens, Brussels, Con-

DATE OF ELECTION.	
	stantinople, Dresden, Rotterdam, Vienna, &c. *Naples.*
1869, Jan. 12.	Saint Venant, Barré de, Ingénieur en chef des Ponts et Chaussées, Corr. Soc. Sci. de Lille et de l'Acad. Romaine de *Nuovi Lincei,* Mem. Soc. Philomatique.
1867, Feb. 5.	Schönfeld, Edward, Ph.D., Director of the Mannheim Observatory.
1834, Jan. 24.	Watson, Henry Hough. *Bolton, Lancashire.*
1853, Apr. 19.	Wilkinson, Thomas Turner, F.R.A.S. *Burnley.*

ORDINARY MEMBERS.

1861, Jan. 22.	Alcock, Thomas, M.D., Extr. L.R.C.P. Lond., M.R.C.S. Engl., L.S.A. *Oakfield, Ashton-on-Mersey.*
1870, Nov. 15.	Aldis, Thomas Steadman, M.A. *Manchester Grammar School.*
1870, Dec. 13.	Angell, John. *Manchester Grammar School.*
1861, Jan. 22.	Anson, Ven. Archd. George Henry Greville, M.A. *Birch Rectory, Rusholme.*
1837, Aug. 11.	Ashton, Thomas. 42 *Portland-street.*
1865, Nov. 15.	Bailey, Charles. *Whalley View, Whalley Range.*
1824, Jan. 23.	Barbour, Robert. 18 *Aytoun-street.*
1865, Nov. 15.	Barker, Thomas, M.A., Prof. Math. Owens College. *Owens College.*
1867, Nov. 12.	Barrow, John. 3 *Egerton-terrace, Birch-lane, Longsight.*
1858, Jan. 26.	Baxendell, Joseph, F.R.A.S., Corr. Mem. Roy. Phys.-Econ. Soc. Königsberg, and Acad. Sc. and Lit. Palermo. *Crescent-road, Cheetham Hill.*
1847, Jan. 26.	Bazley, Sir Thomas, Bart., M.P. *Eynsham Hall, Oxford.*
1870, Nov. 15.	Bell, Joseph Carter, F.C.S. *Pendleton Alum Works, Newton Heath.*
1847, Jan. 26.	Bell, William. 51 *King-street.*
1870, Nov. 15.	Bennion, John A. 88 *Pollard-street.*

DATE OF ELECTION.	
1858, Jan. 26.	Benson, Davis. 4 *Chester-street.*
1868, Dec. 15.	Bickham, Spencer H., jun. *West Bank, Bowdon.*
1842, Jan. 25.	Binney, Edward William, F.R.S., F.G.S., Hon. Mem. Geol. Socs. Edinburgh and Liverpool, and Geol. and Polytech. Soc. West Riding of Yorkshire. 40 *Cross-street.*
1870, Nov. 15.	Bird, John Durham, M.D. *Heaton Chapel.*
1821, Jan. 26.	Blackwall, John, F.L.S. *Hendre, Llanrwst.*
1861, Jan. 22.	Bottomley, James. 4 *Irwell-terrace, Lower Broughton.*
1839, Oct. 29.	Bowman, Henry. 13 *Campden-grove, Campden-hill, Kensington, London.*
1855, Apr. 17.	Brockbank, William, F.G.S. 5 *Clarence-street.*
1861, Apr. 2.	Brogden, Henry. *Hale Lodge, Altrincham.*
1844, Jan. 23.	Brooks, William Cunliffe, M.A., M.P. *Bank,* 92 *King-street.*
1860, Jan. 24.	Brothers, Alfred, F.R.A.S. 14 *St. Ann's-square.*
1867, Dec. 10.	Broughton, Samuel. *Heaton-on-Mersey.*
1846, Jan. 27.	Browne, Henry, M.D., M.A., M.R.C.S., Engl. 244 *Oxford-street.*
1847, Jan. 26.	Calvert, Frederick Crace, Ph.D., F.R.S., F.C.S., Corr. Mem. Roy. Acad. Sc. Turin, Acad. Sc. Rouen, Pharmac. Soc. Paris, and Industr. Soc. Mulhouse. *Royal Institution, Bond-street.*
1859, Jan. 25.	Carrick, Thomas. 5 *Clarence-street.*
1858, Jan. 26.	Casartelli, Joseph. 43 *Market-street.*
1852, Apr. 20.	Chadwick, David, F.S.S., Assoc. Inst. C.E., M.P. 64 *Cross-street Chambers.*
1842, Jan. 25.	Charlewood, Henry. 5 *Clarence-street.*
1854, Apr. 18.	Christie, Richard Copley, M.A., Prof. Hist. Owens College. 2 *St. James's-square.*
1841, Apr. 20.	Clay, Charles, M.D., Extr. L.R.C.P. Lond., L.R.C.S. Edin. 101 *Piccadilly.*
1853, Jan. 25.	Cottam, Samuel, F.R.A.S. 2 *Essex-street.*
1859, Jan. 25.	Coward, Edward. *Heaton Mersey, near Manchester.*
1861, Nov. 12.	Coward, Thomas. *Bowdon.*
1851, Apr. 29.	Crompton, Samuel, M.R.C.S. Engl., L.S.A., F.R. Med. Chir. Soc. 24 *St. Ann's-square.*
1848, Jan. 25.	Crowther, Joseph Stretch. 28 *Brazennose-street.*
1861, Apr. 2.	Cunningham, William Alexander. *Bank, Spring Gardens.*
1854, Feb. 7.	Dale, John, F.C.S. *Cornbrook Chemical Works, Chester-road.*
1842, Apr. 19.	Dancer, John Benjamin, F.R.A.S. 43 *Cross-street.*

DATE OF ELECTION.	
1863, Feb. 10.	Darbishire, George Stanley. 14 *John Dalton-street.*
1853, Apr. 19.	Darbishire, Robert Dukinfield, B.A., F.G.S. 26 *George-street.*
1869, Nov. 2.	Dawkins, William Boyd, M.A., F.R.S. *Museum, Peter-street.*
1870, Nov. 15.	Deacon, Henry, F.C.S. *Alkali Works, Widnes.*
1861, Dec. 10.	Deane, William King. 25 *George-street.*
1855, Jan. 23.	Dickinson, William Leeson. 1 *St. James's-street.*
1824, Oct. 29.	Fairbairn, Sir William, Bart., C.E., LL.D., F.R.S., F.G.S., Corr. Mem. Imp. Inst. France, and Roy. Acad. Sc. Turin, Hon. Mem. Inst. Eng. Scot. and Yorsh. Phil. Soc. *Polygon, Ardwick.*
1861, Jan. 22.	Fisher, William Henry. 16 *Tib-lane.*
1857, Apr. 21.	Foster, Thomas Barham. 23 *John Dalton-street.*
1860, Apr. 17.	Francis, John. *Town Hall.*
1840, Jan. 21.	Gaskell, Rev. William, M.A. 46 *Plymouth-grove.*
1861, Apr. 30.	Gladstone, Murray, F.R.A.S. 24 *Cross-street.*
1817, Jan. 24.	Greg, Robert Hyde, F.G.S. 2 *Chancery-place, Booth-street.*
1849, Oct. 30.	Greg, Robert Philips, F.G.S. 2 *Chancery-place, Booth-street.*
1862, Nov. 4.	Hart, Peter. 49 *Faulkner-street.*
1839, Jan. 22.	Hawkshaw, John, F.R.S., F.G.S., M. Inst. C.E. 33 *Great George-street, Westminster, London,* S.W.
1828, Oct. 31.	Henry, William Charles, M.D., F.R.S. 11 *East-street, Lower Mosley-street.*
1868, Nov. 17.	Herford, Rev. Brooke. 6 *Arthur-terrace, Higher Broughton.*
1861, Apr. 30.	Heys, William Henry. *Hazel Grove, near Stockport.*
1833, Apr. 26.	Heywood, James, F.R.S., F.G.S., F.S.A. 26 *Kensington Palace Gardens, London,* W.
1864, Mar. 22.	Heywood, Oliver. *Bank, St. Ann's-street.*
1851, Apr. 29.	Higgin, James. *Little Peter-street, Gaythorn.*
1845, Apr. 29.	Higgins, James. *King-street, Salford.*
1848, Oct. 31.	Higson, Peter, F.G.S. *Swinton.*
1839, Jan. 22.	Hobson, John. *Rockville, Ballyshannon, Donegal.*
1861, Apr. 2.	Hobson, John Thomas, Ph.D. *Laurel House, Tyldesley.*
1854, Jan. 24.	Holcroft, George. *St. Mary's-gate.*
1855, Jan. 23.	Holden, Isaac. 64 *Cross-street.*

DATE OF ELECTION.	
1846, Jan. 27.	Holden, James Platt. *St. James's Chambers,* 3 *South King-street.*
1871, Mar. 21.	Hopkinson, John, D.Sc. 12 *York-place, Oxford-street.*
1857, Jan. 27.	Hunt, Edward, B.A., F.C.S. 42 *Quay-street, Salford.*
1859, Jan. 25.	Hurst, Henry Alexander. 11 *Peel-street.*
1866, Nov. 13.	Jevons, William Stanley, M.A., Professor of Logic &c. Owens College. *Owens College.*
1850, Apr. 30.	Johnson, Richard, F.C.S. *Oak Bank, Fallowfield.*
1865, Jan. 24.	Johnson, William B. *Altrincham.*
1870, Nov. 1.	Johnson, William H., B.Sc. 27 *Dale-street.*
1821, Oct. 19.	Jordan, Joseph, F.R.C.S. Engl. 70 *Bridge-street.*
1848, Apr. 18.	Joule, Benjamin St. John Baptist. *Cliff Point, Higher Broughton.*
1842, Jan. 25.	Joule, James Prescott, D.C.L., LL.D., F.R.S., F.C.S., Hon. Mem. C.P.S., and Inst. Eng. Scot., Corr. Mem. Roy. Acad. Sc. Turin. *Cliff Point, Higher Broughton.*
1843, Jan. 24.	Kay, Samuel. 4 *Bond-street.*
1852, Jan. 27.	Kennedy, John Lawson. 47 *Mosley-street.*
1867, Nov. 26.	Kipping, James Stanley. *Branch Bank of England.*
1862, Apr. 29.	Knowles, Andrew. *High-bank, Pendlebury.*
1830, Apr. 30.	Langton, William. *Manchester and Salford Bank, Mosley-street.*
1860, Jan. 24.	Latham, Arthur George. 24 *Cross-street.*
1863, Dec. 15.	Leake, Robert. 3 *Bond-street.*
1850, Apr. 30.	Leese, Joseph. *Altrincham.*
1857, Jan. 27.	Longridge, Robert Bentink. 67 *King-street.*
1870, Apr. 19.	Lowe, Charles. 61 *Piccadilly.*
1854, Jan. 24.	Lowe, George Cliffe. *Mill-street, Ancoats.*
1850, Apr. 30.	Lund, Edward, F.R.C.S. Engl., L.S.A. 22 *St. John's-street.*
1859, Jan. 25.	Lynde, James Gascoigne, M. Inst. C.E., F.G.S. *Town Hall.*
1855, Oct. 30.	Mabley, William Tudor. 20 *Carlton Chambers, St. Ann's-street.*
1829, Oct. 30.	McConnel, James. *Esher, Surrey.*
1838, Apr. 17.	McConnell, William. 90 *Henry-street, Oldham-road.*
1866, Nov. 13.	McDougal, Arthur. 68 *Port-street.*
1859, Jan. 25.	Maclure, John William, F.R.G.S. 2 *Bond-street.*

DATE OF ELECTION.	
1858, Apr. 20.	Mather, Colin. *Iron Works, Deal-street, Brown-street, Salford.*
1864, Nov. 1.	Mather, William. *Iron Works, Deal-street, Brown-street, Salford.*
1868, Nov. 17.	Mawson, John Isaac, C.E. 4 *Essex-street.*
1837, Jan. 27.	Mellor, William. *Lime Works, Ardwick.*
1864, Mar. 8.	Micholls, Horatio. *Southgate House, Southgate, London,* N.
1864, Mar. 22.	Montefiore, Leslie J. *The Firs, Bowdon.*
1861, Oct. 29.	Morgan, John Edward, M.B., M.A., M.R.C.P. Lond., F.R. Med. and Chir. S. 1 *St. Peter's-square.*
1870, Nov. 1.	Morris, Walter. 68 *Fountain-street.*
1852, Jan. 27.	Nelson, James Emanuel. 17 *Bridgewater-street, High-street.*
1854, Feb. 7.	Nevill, Thomas Henry. 19 *George-street.*
1850, Jan. 24.	Newall, Henry. *Hare-hill, Littleborough.*
1862, Dec. 30.	Ogden, Samuel. 10 *Back Mosley-street.*
1861, Jan. 22.	O'Neill, Charles, F.C.S., Corr. Mem. Industr. Soc. Mulhouse. 17 *North Bank, Regent's-park, London.*
1844, Apr. 30.	Ormerod, Henry Mere. 5 *Clarence-street.*
1861, Apr. 30.	Parlane, James. 10 *Dickinson-street.*
1866, Mar. 20.	Patterson, John. *Oak Mount, Fallowfield.*
1870, Nov. 29.	Piers, Sir Eustace Fitzmaurice, Bart.
1857, Apr. 21.	Platt, William Wilkinson. *Iron Works, Deal-street, Brown-street, Salford.*
1854, Jan. 24.	Pochin, Henry Davis. 42 *Quay-street, Salford.*
1860, Apr. 17.	Pocklington, Rev. Joseph Nelsey, B.A. *Rectory, St. Michael's, Hulme.*
1861, Jan. 22.	Radford, William. 41 *John Dalton-street.*
1854, Feb. 7.	Ramsbottom, John. *Railway-station, Crewe.*
1859, Apr. 19.	Ransome, Arthur, B.A., M.D. Cantab., M.R.C.S. 1 *St. Peter's-square.*
1869, Nov. 16.	Reynolds, Osborne, B.A., Professor of Engineering, Owens College. *Owens College.*
1859, Jan. 25.	Rideout, William Jackson. 38 *Church-street.*
1860, Jan. 24.	Roberts, William, M.D., B.A., M.R.C.P. Lond. 89 *Mosley-street.*
1864, Dec. 27.	Robinson, John. *Atlas Works, Great Bridgewater-street.*
1822, Jan. 25.	Robinson, Samuel. *Blackbrook Cottage, Wilmslow.*
1864, Jan. 12.	Rogerson, John. *Gaythorn.*

DATE OF ELECTION.	
1858, Jan. 26.	Roscoe, Henry Enfield, B.A., Ph.D., F.R.S., F.C.S., Professor of Chemistry, Owens College. *Owens College.*
1869, Dec. 14.	Routledge, Robert, B.Sc. *Bowdon.*
1851, Apr. 29.	Sandeman, Archibald, M.A. *Tulloch, near Perth.*
1870, Dec. 13.	Schorlemmer, Carl, F.C.S. *Owens College.*
1842, Jan. 25.	Schunck, Edward, Ph.D., F.R.S., F.C.S. *Oaklands, Kersal.*
1863, Apr. 7.	Schwabe, Edmund Salis, B.A., F. Anthrop. Soc. 41 *George-street.*
1855, Jan. 23.	Sharp, Edmund Hamilton. *Seymour Grove, Old Trafford.*
1852, Apr. 20.	Sidebotham, Joseph, F.R.A.S. 19 *George-street.*
1865, Dec. 26.	Simpson, Henry, M.D. 335 *Oxford-street.*
1869, Feb. 23.	Smart, Robert Bath, M.R.C.S. 176 *Oxford-street.*
1838, Jan. 26.	Smith, George Samuel Fereday, M.A., F.G.S. *Bridgewater Offices, Manchester.*
1845, Apr. 29.	Smith, Robert Angus, Ph.D., F.R.S., F.C.S., Corr. Mem. I.R. Geol. Inst. Vienna. 20 *Devonshire-street, All Saints.*
1859, Jan. 25.	Sowler, Thomas. *Red Lion-street, St. Ann's-square.*
1851, Apr. 29.	Spence, Peter, F.C.S., M.S.A. *Alum Works, Newton Heath.*
1864, Dec. 27.	Spencer, Joseph. 105 *Portland-street.*
1852, Jan. 27.	Standring, Thomas. 1 *Piccadilly.*
1847, Apr. 20.	Stephens, James, F.R.C.S., L.S.A. 68 *Bridge-street.*
1870, Nov. 1.	Stewart, Balfour, LL.D., F.R.S., Professor of Natural Philosophy. *Owens College.*
1858, Jan. 26.	Stewart, Charles Patrick. *Atlas Works,* 88 *Great Bridgewater-street, and Oaklands, Victoria Park.*
1863, Oct. 6.	Stretton, Bartholomew. *Bridgewater-place, High-street.*
1814, Jan. 21.	Stuart, Robert. *Ardwick Hall.*
1870, Nov. 29.	Syson, Edward John, M.D. 6 *Broughton-terrace, Great Clowes-street.*
1856, Jan. 22.	Taylor, John Edward. 3 *Cross-street.*
1870, Mar. 22.	Teale, James. *Springfield, Sale.*
1869, Nov. 2.	Thorpe, Thomas Edward, Ph.D., F.C.S. 46 *Windsor-terrace, Glasgow.*
1860, Apr. 17.	Trapp, Samuel Clement. 88 *Mosley-street.*
1821, Apr. 19.	Turner, Thomas, F.R.C.S. Engl., F.L.S., F.R. Med. Chir. S., Hon. F. Harv. Soc. 77 *Mosley-street.*

DATE OF ELECTION.	
1861, Apr. 30.	Vernon, George Venables, F.R.A.S., F.M.S., F. Anthrop. Soc., Mem. Met. Soc. Scotl. and Met. Soc. France. *Auburn-street, Piccadilly.*
1857, Jan. 27.	Webb, Thomas George. *Glass Works, Kirby-street, Ancoats.*
1858, Jan. 26.	Whitehead, James, M.D., M.R.C.P. Lond., F.R.C.S. Engl., L.S.A., M.R.I.A., Corr. Mem. Soc. Nat. Phil. Dresden, Med. Chir. Soc. Zurich, and Obst. Soc. Edin., Mem. Obst. Soc. Lond. 87 *Mosley-street.*
1869, Feb. 9.	Whitehead, Walter, M.R.C.S. 234 *Oxford-street.*
1839, Jan. 22.	Whitworth, Sir Joseph, Bart., F.R.S. *Chorlton-street, Portland-street.*
1859, Jan. 25.	Wilde, Henry. *Mill-street, Ancoats.*
1859, Apr. 19.	Wilkinson, Thomas Read. *Manchester and Salford Bank, Mosley-street.*
1851, Apr. 29.	Williamson, William Crawford, F.R.S., Professor of Natural History, Anat., and Physiol., Owens College, M.R.C.S. Engl., L.S.A. *Egerton-road, Fallowfield.*
1836, Jan. 22.	Wood, William Rayner. *Singleton Lodge, near Manchester.*
1855, Oct. 30.	Woodcock, Alonzo Buonaparte. *Orchard Bank, Altrincham.*
1860, Apr. 17.	Woolley, George Stephen. 69 *Market-street.*
1863, Nov. 17.	Worthington, Samuel Barton, C.E. *Crescent-road, Cheetham-hill.*
1865, Feb. 21.	Worthington, Thomas. *Bridgewater-Club Chambers, King-street.*
1864, Nov. 1.	Wright, William Cort, F.C.S. *The Springs, Bowdon.*

N.B.—Of the above list the following have compounded for their subscriptions, and are therefore Life Members :—

Brogden, Henry.
Rideout, William Jackson.
Sandeman, Archibald, M.A.
Smith, Robert Angus, Ph.D., F.R.S.