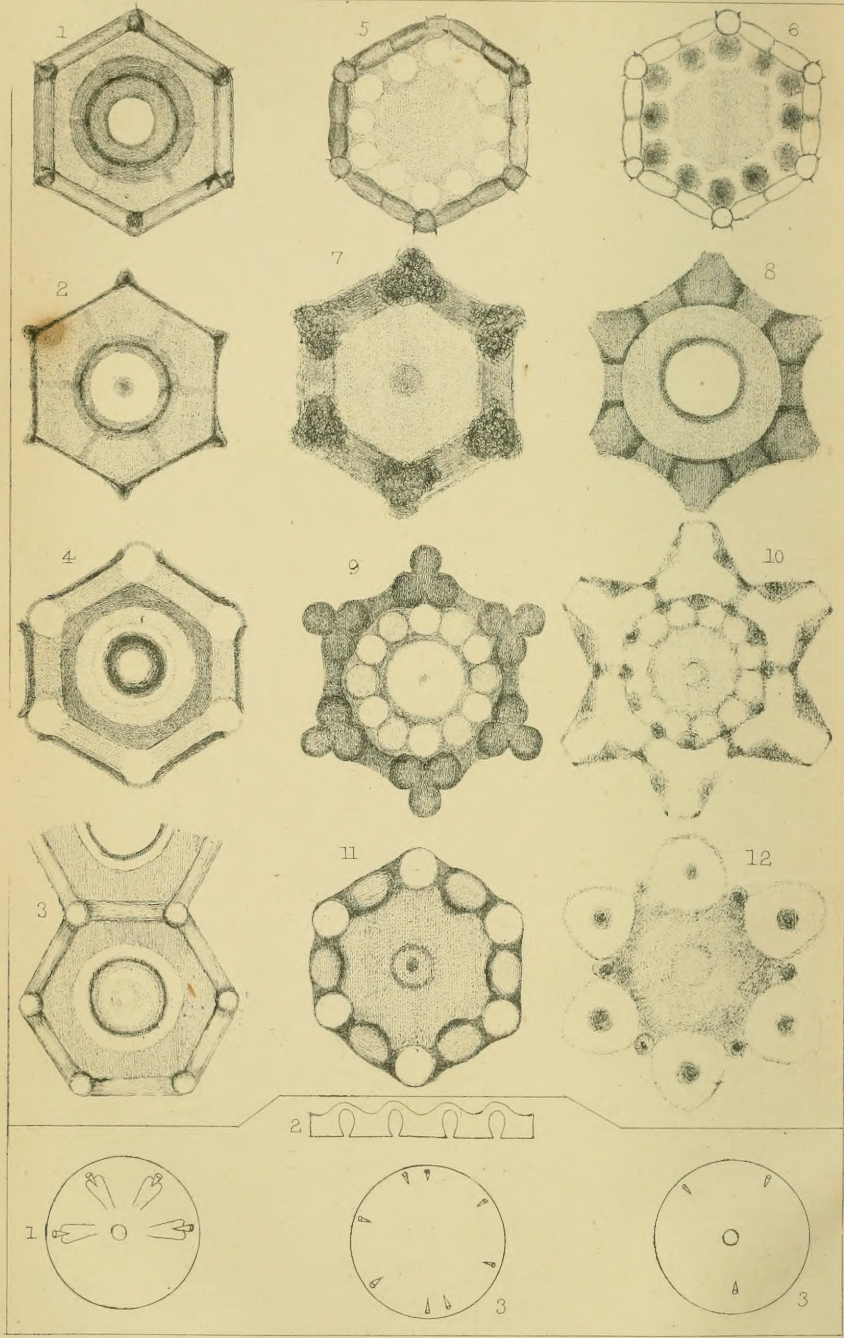


W. West & Co. sc.

Cosanodiscus in bisulphide of Carbon — Aulacodiscus formosus & A. margaritaceus.

# THE MONTHLY MICROSCOPICAL JOURNAL:

## TRANSACTIONS

OF THE

## ROYAL MICROSCOPICAL SOCIETY,

AND

## RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

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THE

# MONTHLY MICROSCOPICAL JOURNAL.

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## I.—*Observations on the Optical Appearances presented by the Inner and Outer Layers of Coscinodiscus when examined in Bisulphide of Carbon and in Air.* By J. W. Stephenson, F.R.A.S., Treasurer R.M.S., and Actuary to the Equitable Assurance Society.

(*Read before the* Royal Microscopical Society, *June* 4, 1873.)

Plate XXI. (Upper portion).

The possibility of determining the structure of minute organisms by means of the refractive indices of the media in which they are examined has been alluded to on more than one occasion, and notably in the discussion which followed Mr. Slack's paper on *Eupodiscus Argus*, when Mr. Charles Stewart stated that the silicious deposits in both plants and animals are of less refractive index than Canada balsam, and that consequently, when mounted in that medium, they appear, if convex, to act as concave lenses do in air, and *vice versâ.*

In fact, that gentleman has been in the habit, in his investigations of the Echinoderms, of determining the nature of the spicules entangled in the tissues of those animals by the appearances presented when mounted in Canada balsam. The refractive index of Canada balsam is higher than that of silicious, and lower than that of calcareous, spicules; consequently, there is no possibility of confusing the calcareous spicules of the Echinoderm with those of the silicious sponges on which the creature may have been feeding.

It was long since pointed out by Welcker as a means of

EXPLANATION OF PLATE XXI. (Upper portion).

*Coscinodiscus occulus iridis*, in bisulphide of carbon.

Fig. 1.—Hexagonal areola of inner or "eye-spot" layer, as seen when beyond the focus.
,, 2.—Ditto beyond, but nearer focus than Fig. 1.
,, 3.—Ditto, supposed true focus, showing fracture through "eye-spot."
,, 4.—Ditto, within the focus.
,, 5.—Areola of cellular or outer layer, slightly beyond the focus.
,, 6.—Ditto, ditto, slightly within focus.
,, 7.—Both layers, beyond focus.
,, 8.—Ditto, beyond, but nearer focus than Fig. 7.
,, 9.—Ditto, supposed true focus.
,, 10.—Ditto, within focus.
,, 11.—Ditto within, nearer true focus than Fig. 10.
,, 12.—Ditto within, nearer true focus than Fig. 11.

distinguishing, in transparent objects, superficial elevations from depressions, that elevations appear brighter when the body of the microscope is raised, whilst depressions, on the contrary, are brightest when it is depressed.

Although Mr. Stewart stated, when calling our attention to this mode of determining structure, that the idea was not new, he probably was the means of directing the attention of many Fellows to this much-neglected method of examination. Be this, however, as it may, it at once occurred to me that some medium of higher refractive power than Canada balsam might be selected, which, whilst reversing the optical effects arising from the transmission of light through an object viewed in air, would not render such reversed action weak by the approximately refractive equality of the object observed and the medium in which it is mounted.

To render myself more intelligible allow me to dwell for a moment on the refractive indices of diatomaceous silex and some of the various materials in which its condition may be analyzed.

If diatoms are examined in air, *i. e.* dry, they are, in some instances, too opaque for transmitted light, but on immersing them in water, of which the mean index is 1·336, they become more translucent; with media of higher refractive power the translucency increases until the mean index of strong sulphuric acid (1·434) is attained, in which they become practically invisible. As every object which is transparent and colourless becomes absolutely invisible when immersed in a colourless medium identical in refractive power with itself, we know approximately that the refractive index of diatomaceous silex is 1·434 (much below that of quartz), and this is accordingly, for diatoms, our neutral point. Although I have said colourless objects mounted in a colourless medium become invisible, it is of course equally true if both are of the same colour and of the same index.

By progressively increasing the refractive power of the mounting medium the diatoms gradually again become more and more visible until, as we all know, when mounted in Canada balsam (1·540) the coarser species are sufficiently defined for all ordinary purposes; but if we require a still greater departure from the neutral point or invisible condition, we must select some other substance of still higher refractive power. This we find in bisulphide of carbon, the index of which is 1·678, being, I believe, the highest of any known fluid.

But, if not content with this, we may carry the bisulphide higher by dissolving in it phosphorus, whose refractive index is 2·254, and thus obtain any power (with varying strength of the solution) between 1·678 and 2·254; but when such diatoms as the Heliopelta are mounted in a strong solution of phosphorus, they again become nearly, if not quite, as opaque as they were in air.

These facts being clearly established, it is evident that on the examination of any given species of diatom, or other object, in air, and in bisulphide of carbon (or phosphorus), they are seen under conditions in which the respective optical effects arising from their form, are reversed—under the first condition (in air), each part having a convex surface, gives a positive image when the body of the microscope is raised, and each concave surface a negative image; on the other hand, in bisulphide of carbon the reverse of this takes place; the concave gives a positive, and the convex a negative image, because in the former case the concavity in the silex being occupied by the bisulphide, a convex lens of that medium is produced, and similarly in the latter case the convex silex forms a concave lens of the same highly refractive fluid.

There is, however, another condition under which a positive image may be produced—a beam of light passing through a small aperture will form a picture of an object placed beneath, as may be seen in Polycistinæ and other microscopic objects, and it is therefore possible that an image may be depicted above a diatom, whether it be mounted in the dry state or in Canada balsam, and it is on this account that the necessity of two media exists.

With a view to giving some practical effect to these considerations I determined on examining some coarsely-marked diatom, and selected for the purpose *Coscinodiscus occulus iridis*, but before describing the appearances presented I may mention that the bisulphide of carbon reduces the available aperture of *dry* objectives to 73° 9′, a solution of phosphorus in bisulphide (giving a refractive index of 2) reduces it to 60°, and pure phosphorus to 52° 40′.

The first point which attracted my attention on examining the bisulphide slide was a valve, the outside uppermost, and broken through, displaying with an $\frac{1}{8}$ objective under my form of binocular, with perfect stereoscopic effect, the second or inner layer with great distinctness, demonstrating, if that were necessary, that as each diatom consists of two valves, so each valve consists of two layers, making, in the complete frustule, four layers *at least*.

Mr. George Shadbolt * proved in 1849 that the Arachnoidiscus consists of four discoid portions, and of "two annular valves"; and in a paper by Mr. Charles Stodder, read before the Boston Society of Natural History, in the year 1862,† that writer says he has found a specimen of Coscinodiscus beyond the broken edges of which "was another part of the disc, which was simply granular, with a milky aspect"; and further on he speaks of this as the "inner plate," but adds that it "is composed of spherical granules of silex, joined or cemented together by a thin plate of silex."

My sole object being to demonstrate, as far as possible, the

* See 'Transactions of the Microscopical Society,' vol. iii., 1852.

† See 'Quarterly Journal of Microscopical Science,' vol. xi., p. 214.

structure of these two layers, so essentially different, I will at once describe the results of my investigation.

The inner layer, or rather each inner layer, as there are two, is divided into well-marked hexagonal areolæ, each hexagon having a central circular spot, which gives, *both* in air and bisulphide of carbon, a positive image when beyond the focus, proving that it has neither a spherical nor concave form.

If it were convex it would, as previously shown, give a negative image in bisulphide; if it were concave it would give a negative image in air; but giving, as it does, a positive image in both, it follows that it must be either a perforation, or, if not a perforation, it is occupied by a plate of silex which gives no lens-like action in any medium.

That these spots are, however, absolutely openings, there can, I think, be little doubt; as in no case can I detect, with any power up to a Powell and Lealand's $\frac{1}{25}$, any trace of a broken film, although I have broken the layer in all directions, and the line of fracture almost invariably passes through some of the circular markings. Fig. 3.

As the fact of their being open or closed may involve a question of considerable physiological interest, it is well to compare them with the foramina of the silicious skeletons of the Polycistinæ—known to be foramina because they give egress to the characteristic pseudopodia of these animals—by so adapting the power employed that the openings may appear of about the same magnitude in each case.

In making the comparison it is well to select the smaller, broken polycistins, when it will be seen that the optical appearances presented are strikingly similar in the two cases, and strongly support the view which I have ventured to enunciate.

These circular openings, as I will venture to call them, in the inner plate, are bounded by a thicker ring of silex, and the several hexagonal areolæ are also divided from each other by similar bands, as indicated by their becoming black when beyond the focus, and bright within, when mounted in bisulphide. Figs. 2 and 4.

The outer layer is more complex in its structure, and many times thicker than that just described. It is formed of deep hexagonal cells, the depth of each cell being, as nearly as I could determine from a side view of a small fragment, about one and a half times the diameter; but this probably varies in different parts of the disk; these also give, when beyond the focus, positive images in each medium—proving, as in the case of the inner plate, that these cells are either open at each end, or, if closed, that they are so by a film or plate which is not of a single lens-like form. Figs. 5 and 6.

Inside and around each cell is a beautiful ring of bright spots, about sixteen in number, which, if seen in air, would at once be pronounced spherules; but, as they brighten in bisulphide when beyond the focus and disappear within, they must be attributed to concavities, openings, or more probably notches in the marginal structure; but, with respect to the latter, I speak with much reserve.

If anything more were necessary to prove that this diatom is not made up of bosses it would be found in the fact that the line of fracture in this layer is invariably through the cells.

The appearance of the cellular or outer layer, here described, is that presented when the inner layer is removed, and when therefore the well-known "eye spots" are wanting.

When the inner or "eye spot" layer is *in situ* an "eye" is seen through each hexagonal cell of the outer layer, the hexagonal areolæ of the former corresponding in size and position with the hexagonal cells above them.

In a few cells around the central point of the disk there is a departure from the ordinary form, and the "eye spot" is excentric, sometimes so much so as to appear wanting.

Although in these imperfect observations I have carefully avoided generalizing, I may mention that a broken Aulacodiscus, and two or three other discoid forms on the same slide, show an inner plate of similar structure, but it is not thence to be inferred that even in the discoid forms this is universal.

It seems to me quite possible, if not probable, that some animal tissues, deficient in selective power of staining, may be made to disclose their secrets to the student of minute anatomy, if examined in such media as bisulphide of carbon or oil of cassia.

I have felt considerable hesitation in bringing this matter before the Society, partly because my knowledge of the Diatomaceæ is very limited, and partly because I was unable to illustrate the appearances presented under the microscope; but my friend Mr. Stewart, to whom my best thanks are due, having kindly volunteered to make the necessary drawings, which he has done with great care and skill under an excellent immersion $\frac{1}{16}$th by Gundlach, the latter difficulty was overcome. As the chemist is able, by his reagents, to determine the composition of the various substances submitted for his investigation, by the effects they produce, rather than by mere taste, colour, or smell, so, in my opinion, ought the microscopist to be able to determine, not only the form but also the substance of many organisms which he examines, by means of the optical effects produced on transmitted light in different media, rather than by the too often illusory appearances presented without such internal and external aids.

## II.—*Remarks on Aulacodiscus formosus, Omphalopelta versicolor, &c., with Description of a New Species of Navicula.*

By F. Kitton, Norwich.

(*Read before the* Royal Microscopical Society, *June* 4, 1873.)

Plate XXI. (Lower portion).

In the April part of the Journal I noticed the fact of the discovery of certain forms of Diatomaceæ living in the harbours of Peru and Bolivia, which had previously only been found in guano or fossil deposits. Through the kindness of my friend Captain Perry, of Liverpool, who sent me portions of his gatherings, I have been enabled to study some of the species in a more perfect state than when obtained from guano or fossil deposits.

The gatherings were procured from the following localities: Iquique, Pisagua, Islay, and Callao in Peru, and Antofagasta in Bolivia, and from depths varying from 20 to 32 fathoms. The Iquique gathering was principally remarkable for the number and beauty of the specimens of *Aulacodiscus formosus.* I have frequently seen as many as a dozen valves and frustules in a single slip of the cleaned material, and I have no doubt I am within the mark when I state I have examined over a thousand specimens of it. This form, unlike most other species of the genus, seems to be subject to little variation (excepting size my largest specimen is the $\frac{1}{50}$, and my smallest $\frac{1}{130}$ of an inch each in diameter); in no case have I detected a valve with more or less than four processes; even in an abnormal valve, in which the processes occupy only half the disk, there are still four of them Plate XXI. (lower portion), Fig. 1. Mixed with this species I occasionally noticed a disk resembling Greville's figure of *A. inflatus*, but on comparing it with authentic specimens of that species I found they were not identical: further examination showed them to be recently-formed valves of *A. formosus;* many of the frustules when allowed to dry, and then placed in a drop of distilled water, split, the valves becoming detached from the cingulum, and I noticed in several instances that the frustule separated into three valves, the outer valves being of the usual dark blue colour, whilst the internal one was hyaline, the granules were also smaller and more distant. The detection of the separation of the newly-formed valves is of great interest, and forces upon us the conclusion that many so-called new species are only valves of known species in various stages of development; it at

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EXPLANATION OF PLATE XXI. (Lower portion).

Fig. 1.—Abnormal form of *Aulacodiscus formosus.*
" 2.—Ideal section of valve of *A. formosus.*
" 3.—Abnormal forms of *A. margaritaceus.*

least proves that colour and situation (in so far as their number in a certain length is concerned) is valueless. I had previously observed in the guano specimens faint traces of surface markings on the valves between the granules; in the recent specimens they are much more apparent, and if a valve is examined by means of oblique light these spaces will be found to be delicately punctate. I have also been able to satisfy myself of the nature of the large granules, by mounting a broken valve on the edge of a strip of thin glass and examining (with an $\frac{1}{8}$) the fracture in profile, the hemispherical elevations were very apparent. The elevations are, however, not solid hemispheres, but hollow, with a minute pore opening into the interior of the frustule; the following diagram represents an ideal section of a valve; Plate XXI. (lower portion), Fig. 2.

In one of the slides sent are three species of Aulacodiscus in which some of the cells are not filled with balsam; this effect may be obtained by transferring the valve into a drop of very stiff balsam, but I have no doubt that sometimes the pore is impervious.

In the Iquique material I observe many fine frustules and valves of what is supposed to be *Omphalopelta versicolor* of Ehrenberg. The form described by Ehrenberg under that name was found in the "Bermuda earth," and which is now known to be identical with the new Nottingham deposit.

I have examined many hundreds of slides of the latter, but have never found the form now known as *O. versicolor.* I have no doubt but that the true form is only the secondary plate of Heliopelta, and which is of frequent occurrence in this deposit.

The species found in the "Monteray earth," "Mexillones guano," and the Iquique gathering, have the alternate elevations and depressions very conspicuous, and on each side of the elevation a smooth space may be seen, the remainder of the valve (the centre excepted) is marked with minute but distinct granules quincuncially arranged, on the margin of the elevations, three to seven spines may usually be seen. It frequently happens on transferring a frustule to a drop of water the valves separate, and the secondary plate becomes detached; this being formed within the cingulum is smaller than the valve, the rays are less elevated, and their margins without spines, the smooth spaces are also wanting, and from a peculiar arrangement of the markings the surface of the disk resembles a piece of watered silk, or *moire antique;* were it not for these structural differences it might naturally be supposed that these plates were simply newly-formed valves like those of *Aulacodiscus formosus.*

The genera Omphalopelta and Heliopelta ought to be merged in Actinoptychus. The following are Kützing's specific characters of *O. versicolor* (species Algarum, p. 133). *O. versicolor* Ehr. Monatsberichte, 1844, p. 270. O. subtiles sepimentis radiisque senis areis radiantibus omnibus in subtilissimis lineis decussatis granulatis, hinc versicoloribus e fusco rutilis radiis validus et umbilico hexagono crystallino bene conspicuis, margine tenui radiato, spinulis areanum medio summo margine singulis; diam. $\frac{1}{21}$ of an inch, sæpius minor; Bermuda.

Obs. Omnes Omphalopelta, species *Actinoptychum senarium* æmulantur.

*Auliscus sculptus* or *cælatus;* one of these names should be cancelled, as the specific distinctions relied upon by Dr. Greville are of no value. The Iquique gathering yielding forms resembling both species; the curious mastoid processes so conspicuous in this genus in some specimens have their upper surfaces distinctly granulate.

The dredging from Pisagua contained some interesting forms, the most abundant being *Aulacodiscus margaritaceus;* some of the valves are very fine (I have seen several nearly $\frac{1}{40}$ of an inch in diameter), the number of processes varying from 3 to 12. I have occasionally found frustules the opposite valves of which differed in the number of processes; in one instance one valve had four and the other seven processes; although as a rule the spaces between the processes are equidistant, this is not invariably the case. I have one valve with eight processes; four of them occurring in pairs, each pair being on opposite sides of the valve; in another instance the valve has three processes, two of which are close to each other, and the third imperfectly developed on the opposite side of the valve; Plate XXI. (lower portion), Fig. 3.

*Auliscus racemosus* rare, some five or six specimens have only been found by myself, two about the size figured by Greville, and the others nearly twice that size; the marginal granules project distinctly from the surface of the valve, the whole of which under an $\frac{1}{8}$ objective is distinctly but delicately striate.

*A. Moronensis.*—This form is rather more plentiful, but varies much in size; my largest specimen measures ·0069, and the smallest about ·0030 in the larger diameter; both of these had previously only been found in fossil deposits, *viz.* that from Cambridge estate, Barbadoes, and the Moron deposit, both of which I believe belong to the Miocene epoch: during this period the Diatomaceæ seem to have made their first appearance.

In a gathering from Callao, also made by Capt. Perry, *A. margaritaceus* occurs sparingly, but a very fine *Coscinodiscus*, some valves being as much as $\frac{1}{40}$ of an inch in diameter, is common; this form is probably a robust state of *C. perforatus.*

Several species of Navicula, some perhaps new, occur in the gathering; one with the outline of *N. strangulata*, but with very different markings, is, I believe, undescribed. The following are its specific characters:—*N. Perryana*, n. sp. mihi. Valve with deep central constriction, dividing the valve into two ovato-cuneate portions, margin composed of small monoliform granules, markings composed of parallel costæ not reaching median line or margin; Callao.

A curious variety of *Stauroneis pulchella* is also of frequent occurrence; it has the outline and characteristic markings of that species, but the so-called stauros is wanting. The striæ are nearly of equal length throughout the valve, leaving a lanceolate smooth space, the margins of which coincide with the outline of the valve.

In a gathering from Islay, Peru, a few valves of *Aulacodiscus Kittoni*, with seven and eight processes, have been found.

## III.—*Measurement of Immersed Apertures.*

By F. H. Wenham, Vice-President R.M.S.

The measurement of the apertures of Mr. Tolles' glass was thrust upon me. I had no appeal, nor was there any condition that the result was to be private, for the arrival was proclaimed months before it reached me. Had I been forewarned thus: "If the trial favours our statements, say so; if the reverse, speak not, or we shall denounce you," then I might have found an excuse for refusal. This allegory has literally come to pass. The irritation and rancour that the trial has occasioned amongst a small clique is curious. I am coarsely accused of acting unfairly toward Mr. Tolles—of having performed a trick, and finally it is insinuated that from some mercenary motive of my own that I have disparaged the glass. Mr. Stodder (the owner) makes some remarks, that I need not dwell upon, for his eulogium being headed "Advertisement" carries its own satire with it. Having now no further occasion to take heed of the idiosyncrasies of these worthy people, to them I make my farewell bow, in satisfaction at concluding the question with Col. Woodward. Respecting his ingenious method of measuring the fluid apertures of ordinary objectives I have not the slightest objection to make. The plan is an admirable one, and quite accurate. To those who still doubt the loss of aperture on objects immersed in transparent media I recommend a trial with a piece of ground glass having a square polished edge. Let the cone of light glance over the *outer* surface, and you have a picture of the *air* angle. Now tilt up the plate a little, so that the cone of rays *enters* the glass, and the *diminished* angle is at once seen and demonstrated on the same spot.

Col. Woodward's measurements of immersed apertures, falling, as he states, within the limits laid down by me, removes a question of difference, and might end the discussion in that respect, but there are one or two sentences that call for brief remark. He says that when the combination was closed as far as possible he failed to get definite results, either in air, water, or balsam. Oftentimes I have found this to be the case, and my custom is (and will be) to set the objective in adjustment on a standard test of known average thickness of cover. I was not, therefore, conscious of "escaping any difficulty" by a stretch of liberty, as the first operation was to find the *proper* point of adjustment of the glass, which was tried on a variety of known tests requiring aperture for their development. The glass was found to be "sluggish," that is, the effect of adjustment was not at once apparent. A known Podura, having a medium cover (·005) by which a great number of object-glasses had been set to the mark "covered," was used as the point from which the air, water, and balsam apertures were measured, and the relative loss due to their refractive powers ascertained. Had I played with extra thick covers instead of those used for the slides of diatomaceous objects usually sold, *then* I might have laid myself open to a suspicion of trickery on either side. I trust that this now concludes the main point of the controversy, which rests, not upon little questions of manipulation, but upon one of optical law and theory with which Col. Woodward's measurements agree.

The next consideration is that in which an object-glass does apparently give a greater angle than I had assigned as the limit. Here, again, I consider that I am fortunate in Col. Woodward's hands; had the measurements merely been certified by credible witnesses, without any knowledge or description of construction, I should perhaps have doubted them as much as they could have misunderstood myself—a most unsatisfactory end to any discussion. I have therefore to thank Col. Woodward for the following precautionary explanation. "Now in the first place I must remark that the objective was certainly an exceptional one, and apparently put together with a view to this controversy. Instead of three combinations I found it to be constructed with four; the posterior two resembled those of other fifths of Mr. Tolles, and were together moved by the screw collar, the anterior two remaining stationary." Near twenty years ago I explained the loss of aperture consequent upon fluid mounting; till recently this has not been controverted. I showed the reduced aperture the following way:—A thick piece of polished plate glass had one surface smeared with beeswax. Various object-glasses, set to proper air-apertures, were focussed on to the clean surface. A light was then set behind, and the diameter of the well-defined circular disk on the wax marked with a needle-point. The cone, from front to back, taken in the proportion of the known thickness of the plate by a protractor, gave the loss or aperture from air to glass, and by inference, on balsam-mounted objects.

Now arose the question of a means of obtaining the full aperture on objects in balsam or fluid. It at once appeared that if the object was set in the centre of a sphere (or hemisphere) that all rays from the central point must continue their course without deviation, and that in such a case neither the length of radius of the glass hemisphere, or the refractive power of the material, would influence the results. I therefore made a number of minute plano-convex lenses of various radii; some less than the $\frac{1}{100}$th part of an inch. Such of these as turned out to be hemispheres were set exactly over a single selected diatom and balsam let in. *Before* the balsam was admitted for a well-known optical law, the object could not be seen. When a $\frac{1}{5}$th or other object-glass was brought over this lens, the arrangement might be termed a four-system one, though the optical effect of the hemisphere as a lens was *nil*, simply because there was no refraction. The balsam object was not magnified. It occupied a like focal distance to the *dry* ones outside, and the same adjustment served for either.

Here I had directly solved the problem of securing the full dry, or the same aperture on a balsam-mounted object. This was done eighteen years ago, and the experiments are described in the 'Quart. Journ. of Mic. Science,' No. XII., July, 1855. Other lenses were used upon balsam-mounted objects, under covers, but the radii being less by the thickness of covering glass, the object still occupied the diametrical plane.* As it may be doubted whether I should, for a mere demonstration, undertake the excessively difficult task of making a number of almost invisible lenses, I can reply, that though an amateur, I disliked needless trouble, and therefore made this an easy matter; and as from the practical nature of microscope work, scraps of such information are considered refreshing by some of our members, I append a separate description. It is clear that this adaptation is similar in principle to the four-combination lens sent to Dr. Woodward. I hope that I may be acquitted of attempting to claim everything, and therefore leave to Mr. Tolles the honour of proving whether such a lens will be of practical use to microscopists, in viewing such tests as are mounted in balsam. I trust that Col. Woodward, having affirmed that "the position taken by me is certainly true for objectives as ordinarily constructed," will allow that this additional lens embodies a deviation from the original question, which was to the effect that there would be no loss of angle aperture of ordinary objectives by the immersion of the front surface in fluids, and I conclude by thanking him for his impartial aid, in bringing facts so near at last, thus ending this interminable question, which I fear must have become very wearisome to the readers of the 'M. M. J.'

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### *How to Make the Atomic Lenses.*

The plano-convex and hemispherical lenses referred to were made as follows:—Strips of thin clear window-glass were drawn out into threads with the blow-pipe flame, a portion was then held in the point of the flame and fused into a spherule of the desired size. A number of these may be formed in a short time. The spherical figure is pretty accurate up to one-twentieth of an inch in diameter.

One precaution must be observed. The strips of glass from which the threads are drawn must be broken *and not cut off with a diamond*, if so, the spherules will not retain a clear polished surface, as the rippled cut of the diamond leaves its mark to the last. The blow-pipe may be an ordinary portable one, and the flame of a

* If the fourth lens is used with a *water* continuity, the object will not occupy the central or diametrical position, but a small distance within it, in accordance with the law of displacement.

common stearine candle gives heat enough. The glass used should be quite clean and always be held as near the point of the flame as possible, in order to avoid the deposit of smoke. *Large* spherules so made take an elliptical figure. Should they be required above one-twentieth they are best formed thus:—Select a clean fragment of window-glass, *broken off* (not cut), of such a bulk as will form the desired sphere. Attach this by one corner, with heat, to the point of a platinum or iron wire. Now rotate the mass while in a state of semi-fusion by twirling the wire back and forwards between the finger and thumb, holding it sometimes up or down, horizontally or inclined, according to the way that the glass seems inclined to sink. With very little dexterity spheres up to one-fifth of an inch in diameter may be so obtained, the rotation of the wire enabling the figure to be appreciated with some accuracy. When cool the spheres are pulled off the wire, which enters but a little way. These spheres are useless things enough alone.

The next step is to convert them into plano lenses. First, make a drill from a piece of round steel wire, about one-tenth diameter, of the following form (Fig. 1). The end is filed down as a square pyramid and hardened, using the blow-pipe flame for the heat. The four sides are then touched on the hone. This drill is best used with the Archimedean stock, as it makes a more truly circular hole. Now take a piece of polished plate-glass, about one inch square, and with the drill well moistened with turpentine or coal oil, make several rows of conical cavities in the face of the glass plate of gradually increasing depth to the number of fifty or more. The drill must not be used with too much pressure, as the endeavour must be to get the edges of the pits as sharp as possible. Having cleaned the block, select from the stock of spherules such as appear to match with the size of the holes in succession, lay them in proper order on a card, then heat your drilled block on a hot plate and lightly smear a piece of the best orange shellac over it, so as nearly to fill the cavities. Now pick up your spherules one at a time and drop each into its appropriate hollow, pressing it hard down with a small wire tubular socket placed over the neck. When the block is filled it resembles a small plantation of onions (Fig. 2).

Fig. 1.

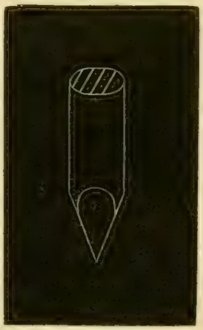
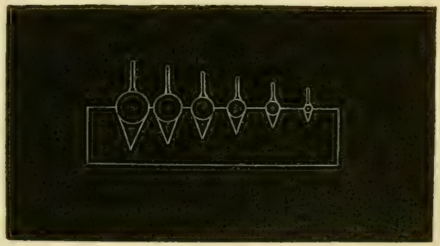


Fig. 2.



The spherules are now ground down on a flat cast-iron plate, with fine emery, to the level of the block, and the whole surface together is smoothed with the finest emery, taking care not to go much below the surface. This is then polished with fine crocus, on a bed of beeswax hardened with resin, and made to revolve as in the lathe in order to expedite the process.

All the lenses (which if the manipulation has been good are perfectly flat to the edges) are now picked out while hot with a pointed brass wire and dropped into alcohol to dissolve off the adhering lac.

It is more easy to make one hundred lenses this way than to grind and polish a single one of one-fiftieth radius by the usual method. Of course their dimensions and accuracy are a matter of chance, but for the purpose specified I found that they acted perfectly if properly selected.

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## IV.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Plate XXII.

Group C. *Rigida.*—Plants densely ramulose, forming compact cushion-like tufts. Branch leaves erecto-patent, oblong, concave, very narrowly bordered, the apex obtuse, truncate, and toothed, the margin involute for great part of its extent.

### 8. Sphagnum Ångströmii C. Hartm.

Skand. Fl. 7th ed. p. 399 (1858).

Plate.

Syn.—Lindb. Torf. No. 10 (1862). Russow Torf. p. 79 (1865). *Sph. cymbifolium β cordifolium.* Hartm. Skand. Fl. 3rd to 6th ed. (p.p.). *Sph. insulosum* Ångström M. S.—Schimper Synop. p. 683 (1860). Milde Bryol. Siles. p. 390 (1869).

*Dioicous*, in large soft dense pale green tufts, light ferruginous below. Stem simple, or sometimes dichotomous, whitish, *with three layers of thin-walled cortical cells, free from fibres or pores.*

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### EXPLANATION OF PLATE XXII.

Sphagnum Ångströmii.

*a.*—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers.
3.—Fruit and perichætium. 4.—Bract from same.
4 *p.*—Point of an inner bract.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Leaf from middle of a divergent branch.
6 *p.*—Point. 6 *a a.*—Areolation of same.
6 *c.*—Single cell from same × 200.
6 *x.*—Transverse section of same. 7.—Basal intermediate leaves 8.—Leaf from a pendent branch. 9 *x.*—Transverse section of stem. 10.—Part of a branch denuded of leaves.


R.Braithwaite del. ad nat.

*Branches crowded, usually* 5 *but sometimes* 3 *in a fascicle,* 1–2 *patulous, arcuato-decurved, the rest slender, greatly elongated, appressed to stem;* the retort cells perforated at the scarcely projecting apex. *Cauline leaves broadly obovate-lingulate, minutely auricled, the apex truncate and slightly fringed;* areolæ from middle to apex rhomboid, at middle base flexuose-rhomboid, thence to margins very narrow, flexuoso-linear, quite free from pores and fibres, or with a few weak fibres in the upper part. *Ramuline leaves densely crowded,* indistinctly 5-ranked, when moist turgidly imbricate, when dry erecto-patent, opaque, concave, *widely ovate acuminate, the apex broadly truncate, with* 6–10 *unequal obtuse teeth,* the margin incurved in the upper two-thirds, and with a faint border of two rows of extremely narrow cells; *the hyaline cells annulate-fibrose, confluent above and below,* minutely and sparingly porose, *chlorophyll cells central, much compressed;* the leaves of the pendent branches with the point rounded and indistinctly toothed.

Fruit in the coma, on a thickish white peduncle; the perichætium inflated oblong, whitish, lower *bracts ovate acuminate* muticous, middle broadly ovate-oblong, innermost broadly oblong, deeply concave, and sometimes cucullate at apex, *all with very narrow linear areolation, quite free from fibres or pores.* Spores ferruginous.

Male plants growing in the same tufts with the female, more slender; the amentula short, ovate closely imbricated, pale green, crowded in the coma; bracts roundish-ovate, in the lower part of lax wide curved non-porose cells, often free from fibres, becoming denser, porose and fibrose toward the broadly truncate toothed apex.

Hab., deep marshes in numerous places throughout Lapland, forming great tufts in the water, resembling islands (Lindberg, Ångström). Also in Finland and at Drivstuen in the Dovrefjeld mountains of Norway. Fr. July.

This fine Sphagnum resembles in habit the slender forms of *S. cymbifolium*, and may thus have been frequently overlooked; apart from the non-fibrose cortical cells, the form of the point of the branch leaves will serve to distinguish them at a glance.

According to Lindberg this species was first detected in 1825 at Karesuando in Tornean Lapland by Læstadius, and distributed along with *S. fimbriatum* and *subsecundum* under the name of *S. latifolium* var. *cordifolium.*

The specimens figured were collected by Ångström at Lycksele in Umean Lapland, and I am indebted to the kindness of Prof. Lindberg for others which are much more compact, and with straight stems.

## V.—*On the High-Power Definition of Minute Organic Particles.*

By Dr. Royston-Pigott, M.A., F.R.S., F.C.P.S., F.R.A.S., M.R.I.

Plate XXIII.

Whenever we shall be able accurately to show a fine definition of minute organic particles, great advance will have been made towards the accurate discrimination of various diseased cells: and perhaps of fluids hitherto undistinguishable under the microscope, though organically different.

But unfortunately such brilliant particles are halo'd with spurious appearances in endless combinations. It is not my intention here to enter upon the wide field of physiological research; but I imagine much that we have learned will have to be unlearned. I here record my belief that the very best work that can now be done towards perfecting our glasses is the development of the high-power definition of organic particles.

Nothing in the microscopic world is so difficult and nothing is so much the subject of dispute, yet there are certain laws of the rays of light which should be admitted and studied on all hands by those who wish honestly to pursue this research.

The first is the nature of the least circle of confusion and its effects.

The second is the nature of vision when affected by extreme angular aperture.

The third, the nature of a confusion of images when many

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EXPLANATION OF PLATE XXIII.

By the courteous permission of Dr. Colonel Woodward, I have employed Mr. Hollick to copy with the camera lucida the splendid photographs taken with the one-sixteenth immersion lens at the Army Museum, Washington. The minute tracery of the lithographs, visible with a lens, reflects great credit on the young artist.

*Degeeria domestica.*

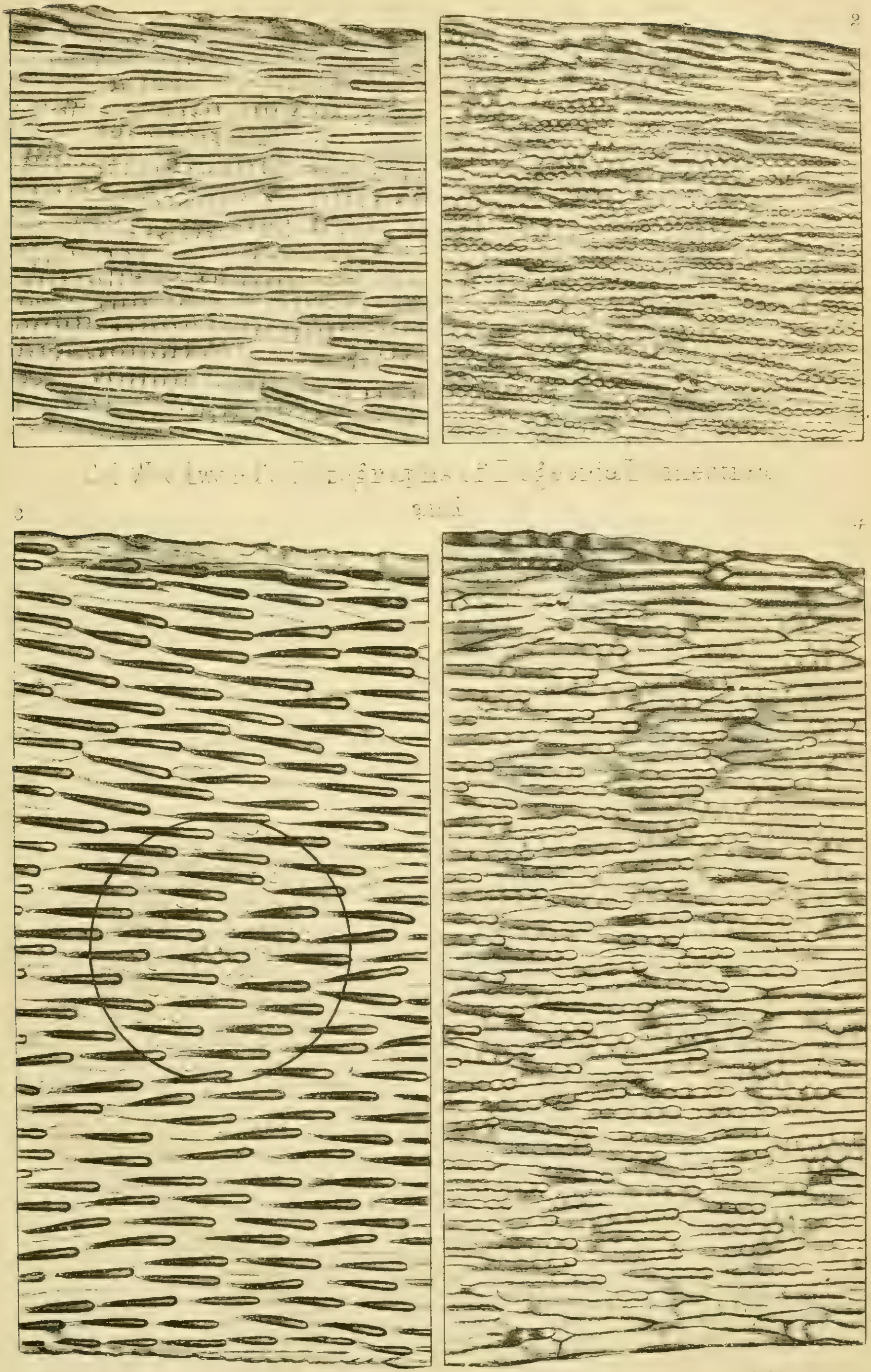
Fig. 1 shows very beautifully the two black shadows sharply edging the illusory markings, between which are shown rudimentary beading, somewhat bar-like. Just in the middle, two conterminous young tadpoles—as a lady calls the curvicollis "nails,"—are plainly visible.

Fig. 2 shows exactly the same part of the scale transformed into rouleaus of continuous beading. With a pocket lens the admirable drawing by the camera lucida can be better appreciated. Two distinct sets can then be seen, light and dark. "The tadpoles" in Fig. 1 are readily converted into long spotted forms by a sufficiently good glass. The spherules of Fig. 2 take also a variety of shadows, circular, crescentic or dotted, according to the direction, obliquity, and aperture of the illuminating cone of rays.

*Podura curvicollis.*

Fig. 3 shows a very fine definition of the "tadpoles," according to the light that is in them—an over-correction, which is generally found when a glass is adjusted to show their heads and tails, unspotted and unbeaded. But when the glass is of first-rate powers, these appearances give way first of all to markings

1 2



3 4

Podura Curvicollis.

particles of brilliant reflecting and refracting properties are huddled together; either under known or unknown arrangements in different strata.

The fourth, the nature of mixed shadows.

Fifth, the nature of perfect definition.

As the present paper is intended to be rather suggestive than demonstrative, I propose to treat very briefly these several points: not for the consideration of those whose optical education is complete, and their views unalterably matured, but for that more numerous class who have not the leisure to make these subjects a special study.

I. Circle of least confusion.

(1.) No lens, or combination of lenses yet made, can form a perfectly correct image of a brilliant point. If you place a small plano-convex lens on the stage and view the image of a miniature sun formed by it, the first glance will convince you such a thing is impossible.

*Example.*—I formed the image of a miniature sun, by optical means $\frac{1}{40}$th of an inch in diameter, upon a distant table. Again by means of a $\frac{1}{4}$-inch plano-convex lens, another image was obtained on the stage, and viewed with a very excellent objective. I then measured the solar disk, and found it enormously larger than it ought to be, even at the best focus. On diminishing the primary image almost to a point, the solar disk still retained nearly the same

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similar to Fig. 1, *viz.* elongations with jet-black shaded edges, without heads and tails, which are spurious. They become more and more continuous. In the actual photographs of the beaded forms, Fig. 2 and Fig. 4, *it is almost impossible to decide, except by the designation written upon them, which is the Degeeria and which the Curvicollis.* Fig. 3 ought, therefore, to more nearly approximate to Fig. 1 in the manner just described.

I have ventured to insert some faint white beading within the circle drawn at Fig. 3, as seen with the one-fiftieth immersion, as described at page 25; but in this case the "tadpoles" had vanished heads and tails, and were transformed into apparently continuous ribs. But it did not seem advisable to alter further Mr. Hollick's very exact and faithful representation of these splendid sun-pictures from monochromatic light. Figs. 1 and 2 are 2300 diameters, Figs. 3 and 4 3200 diameters. All of them were photographed with Powell and Lealand's immersion sixteenth of their new construction of the winter of 1869, and sent to America in December of that year.

The tadpoles are merely the result of the crossing obliquely of the two sets of bearded rouleaus which, intersecting at somewhat irregular angles, cause them to occupy an irregular natant position in the picture.

I have obtained some beautiful results with my Aplanatic Searcher and a Gundlach "one-sixteenth" of excellent quality, which would have been finer still had the lenses of their objective been more perfectly centred.

This glass is deeper than Powell and Lealand's one-eighth in the proportion of 800 to 1300. It may therefore be called a thirteenth, magnifying 1300 diameters with the C eye-piece and the front lenses screwed home.

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*Note.*—Since writing this notice a month ago, I have (June 12, 1873) fortunately succeeded in forming A NEW CORRECTION, which has surpassed all former excellence of High-power definition as observed by me.

size. This disk, which might be called spurious, is merely the diameter of the least circle of confusion. Its laws for a single lens are well known.*

I now substituted for the plano-convex stage lens one of the finest glasses ever made for excellence of definition, and obtained a disk which theoretically ought to have been the *sixty-one thousandth of an inch* in diameter; but upon accurately measuring it I found it rather less than the sixteenth thousandth of an inch. Take the $\frac{1}{60000}$th from the $\frac{1}{16000}$th, and the remainder, $\frac{1}{24000}$th, is the diameter nearly of the resultant least circle of confusion. The effect of this is not in general seen, except with very brilliant light. I say nothing here at present of the brilliant and gorgeous phenomena displayed by "THE CIRCULAR SOLAR SPECTRUM" thus obtained (already communicated to the Royal Society), nor of its revelations (severely cruel) as to the errors of construction, achromatism, and spherical aberration; but this one thing I will venture to say, that the circle of least confusion, in the unsuspected size of its diameter is sufficient to account for the great obstacles in the way, even at present, of perfect definition; and that a minute brilliant disk is swelled out by it *to nearly three times its proper size.* There is an example of this in the bright dots with which the Podura may be bespattered by drawing out the tube a long way, which produces for a corrected glass considerable *over-correction.*

Again, if a brilliant refracting *organic particle* be examined by transmitted light, its natural point of greatest condensation of light swells out, and it cannot be defined properly, on account of the bright ring or halo which surrounds it. Sometimes this result can be got rid of by using such tricks of side illumination as destroy this effect. Also, if exceedingly high powers be used, such as the *one-fiftieth* immersion, the focal depth is so shallow that the plane of vision may pass through the centre of an organic particle (*a monad* suppose) and be below the point of greatest light condensation.† I had a fine view of a monad the other day, which appeared to rotate and twist about, but a bright halo surrounded it, probably indicating the place of the cilia, which I could not see. The monad appeared *blue,* and there was a heap of them looking rather quiet but extraordinarily well defined.

The result of this effect of the circle of least confusion for bright

* Dr. Parkinson, F.R.S., 'Optics,' p. 57, has this explanation:—"Since the *circle of least confusion* is the nearest approach to a *point.* . . . . When an object of finite size is viewed by reflexion or refraction, we may consider the visible image to be the locus of the circles of least confusion. . . . . These circles will overlap each other, and the image consequently will be more or less confused."

"We may regard the comparative size of the circles of least confusion in different cases as a measure of the comparative indistinctness of the visible image."

† In refracting sphericular particles in a less dense medium, this point or focus lies above it, and *vice versâ* in a denser medium below it.

points teaches us to beware of creating them, and to manage effects with diffused light or clean shadows.

(2.) Each particular ray of the *spectrum* has its own least circle of confusion.

The red rays which most obstinately refuse extinction form a circle of least confusion farthest from the plano-convex lens of the experiment. The violet rays form one nearest and the yellow give a circle of least confusion midway. When achromatism is made as perfect as possible, there is a residuary chromatic aberration, chiefly consisting of greenish yellow on one side of the focus and pale lavender on the other. But in this case the spherical aberration is more visible in the best glasses than when the achromatism is deranged in favour of a rose red or orange.*

II. The nature of high-angled vision.

All advanced physiological observers will agree with me as to the comparative value of high angle and low angle in their microscopical researches. Dr. Carpenter, F.R.S., so justly celebrated for his attainments in this department of science, has incessantly advocated the advantage of low angle.

But there is a fashion in these things; and so large a number of people with microscopes make a toy of a noble instrument in viewing diatoms and lines (in which it is well known, high angle in a bad glass will often succeed when a low angle in a good glass will fail), that a sort of rage for high angle among the buying public has hardly yet subsided.

When an irregular solid is viewed on all sides at once, a confused image is necessarily presented. If it is a ridge we see both sides at once; the image of the left side is commingled with that of the right: if one were blue and the other yellow, the resultant image would be green; and if the two sides of the minute ridge were different, one spotted and the other plain, both sides would appear green and both sides spotted. If a minute dice could be imagined as small, say as an organic particle of the Podura, every side would appear to have the same number of dots if all the sides could be seen at once, or the dice might appear round and dotted all over. So an object-glass of high angle would view the four sides all round at once from a thousand different points of view.

It is well known that for high angles it is the outer and not the central parts of the glasses that do all the work; and that it is infinitely more easy to make an inferior high-angled objective for diatom lines to be brought out by illumination tricks, than a low-

* I defer the proof of this to another opportunity. I beg to state here, that I continually find more and more value in "the colour test" I described in this Journal for April, 1870, though at that time I confessed my ignorance of the cause of the phenomenon. "*Something recondite here beyond our ken.*"

angled one of fine qualities; yet be it ever remembered all telescopes are of low angle, however fine their performance.

A low-angled objective gives more precise details of structure; it has a deeper focus, and presents a more natural view. The human eye possesses an angular aperture of about *one degree.* Who can tell what our idea of objects would be if it imitated the modern glory of the diatomist and viewed particles with the tremendous aperture of 140 degrees! Behold the ravishing beauty of innumerable objects viewed with a four-inch objective, necessarily of small aperture, and choose between the two kinds.

Doubtless the extreme difficulty of obtaining sufficient light will always compel microscopists to require a sufficient aperture. But there is another most important aspect, *viz.* the effect of low aperture on sharpness of definition.

Take threads of spun glass, cylinders of glass, and glass globules fused at their ends. I have shown ('Quart. Jour. Mic. Sc.,' Jan. 1870) the images produced by the crossing of cylinders of glass; and the dependence of the black edge or shadow as regards its breadth upon two conditions.

(*a*) The angular aperture of the observing objective.

(*b*) The refractive index of the substance itself.

Now as the aperture is reduced by the aberrameter,* the black ring of a glass bead of $\frac{1}{2000}$th of an inch in diameter gradually broadens itself until the centre is almost occluded: whilst the black borders of the fine cylinder of glass become broader and broader also.

Now, when organic particles which appear from their action and reaction, as regards shadows and spurious disks, to be spherical, are gradually treated similarly by a contracting aperture, they also become in many cases more pronounced in a darker outline. It is curious to see the effect upon the spherules of many scales by this very thing. Relief, depth of focus, darkness of outline, in a good glass, all reward the operation. In many cases, structure unsuspected before, has started into view.

Moderate angular aperture is therefore greatly to be preferred in all researches on organic structure involving the definition of the minute organic particles of which they are composed, where darkness of outline is a desideratum as well as depth of focal perspective. In fact, some objects are much more difficult of observation with a very high than a much lower angular aperture.

The structures of the *Podura curvicollis* were first seen by me in 1862 with a fine Ross 1851 $\frac{1}{4}$ objective and 70° aperture, 16 inches draw-tube, a C eye-piece, and an oblique narrow pencil from a moderator lamp placed on a stand on a level with the table. I

* Made for me by Messrs. Beck, and consisting of an iris-diaphragm placed behind the objective.

have to regret that my residence in the North at that time prevented me superintending the preparation of the Plate. The drawing then taken by a lady of talent, unaccustomed to the microscope, was everything I could desire. Mr. Reade, our former President, however substituted, by some mistake I presume, a rough sketch of mine for the exquisite drawing sent to him as made by the lady. I endeavour to rectify that mistake by the accompanying Plate.

III. The confusion of images of a mass of refracting particles huddled together, in known or unknown forms, renders their proper definition severely difficult.

This appears at present to be "the pons asinorum" of optics, to which "the pons" of Euclid is a trifle.* It is of no use for people to try and prove a negative. A says he cannot see a certain structure. A therefore says it cannot exist. The absurdity of such reasoning would equal that of a blind man who swears there cannot be such a thing as colour, because he has never seen it.

I have shown ('Phil. Tr.,' 1870) that if the secondary image of a given structure be examined above or below the best focus, it often appears quite as real, though totally different, as the thing itself. Suppose there are two transparent structures, and the false images of the upper happen to coincide with the real image of the true; one of two things happens; either both are obliterated or a *mirage* is presented. More than this, if the object-glass happen to be over-corrected spherically, by the observer, then the false images of the lower mingle with the true images of the upper.

Conversely, if the glass happen to be under-corrected spherically, the false images of the upper structure alone mingle with the true images of the lower.†

I have no doubt this is the reason of Dr. Goring stating that of the two he "*preferred an under-corrected glass*," because this throws the false images in a lower plane more out of focus. Accordingly it will be found that in complex structures a glass under-corrected spherically will be better able to define the upper surface than one over-corrected.

IV. The nature of mixed shadows.

It is not long since objectives (at that time thought to be perfection) could only resolve difficult tests by using two obliquely illuminating lamps at once, or two sets of rays at right angles to each other: this caused two shadows, each set forming its own particular lines. The Rhomboides used to be shown, after anxious labour in this way: subsequently two slots were cut in a stop in a wide-angled achromatic condenser to produce similar effects. No one can manage the most delicate definition who disregards

* The fifth proposition of Euclid has long received this name.
† 'Phil. Tr.,' vol. ii., 1870.

shadow. It is not many years since I witnessed a skilful microscopist manipulating in the most tedious manner before he could show an object, now easy. Then arose "the kettledrum" of Mr. Reade and the Prism.

From studying the history of definition, we have that of the microscope; and *we see how good shadows really enabled a bad glass to perform like a good one.* And this was designated by the high-sounding title of microscopical skill.

What histories in histology are connected with the CENTRAL STOP! now discarded. It was much the same thing whether the central stops were placed in the condenser or placed within the objective. Looking back on those feeble microscopic powers, we calmly recognize that it was the disagreement of the central rays with the peripheral, or the bad qualities of the glasses which were concealed by cutting off the central rays. A black spot, actually produced by the black central stop, was really believed to be the proper definition of the diatom long before beads were demonstrated. A central artificial shadow imposed upon the credulity of all.

We never could make anything of the structure of the surface of the moon, without watching shadows under different degrees of obliquity. At four, six, and ten days' age, these shadows gradually stealing forth reveal the noble ruins of extinct volcanoes in all their marvellous beauty and distinctness. In the same manner studying an object under gradually deeper or more slanting shadows gives an idea of contour and configuration that nothing else can.

Several facts are noticeable.

A rouleau of beads lit up obliquely gives under imperfect resolving powers short straight lines like the rounds of a ladder, as in Fig. 1, Plate XXIII.; more perfectly they are seen as closely-packed black crescents. If the position of these crescents change as the object is made to revolve, and change symmetrically, spherical bodies may be predicated, though invisible. Again, a changing lattice-work, one set appearing to pass and repass across the other as we move the light, indicate ribs in two planes slightly inclined to one another. The violent attempts made to define the then difficult structures were seen, as just remarked, in the necessity of illuminating by two lamps or other contrivances in two distinctly different planes at one and the same time. With our improved glasses this now appears puerile. But still two sets of shadows were obtained, and this enabled the better rays of the glass to present an image to the eye.

The most difficult and delicate observations are now successfully made with nothing but central light: a pin-hole in a cap over the condenser: or with unilateral light; and even with no cap at all.

It is a strong fact that in every case two sets of lines, if made out, really heralded beading.

These were the shadows, the mixed crescentic shadows of the now recognized beading.

So also, Dr. Goring's beautiful drawings of lines, and cross-lines as seen on the Podura, really predicted the beaded structure developing those mixed shadows. It is still easy with inferior glasses to get as many as three sets of lines in many beaded objects as that in the Angulata. I have shown the crossing striæ of the Podura to many persons. The cords described in the note are, perhaps, the most interesting part of his research.*

V. The nature and characteristics of perfect definition.

If ever you are about to buy an objective, try it with the deepest eye-pieces you can find; and after making every adjustment practicable to correct the aberration of the eye-pieces (for they all differ in this respect), observe whether the image becomes woolly, indistinct, blurred, hazy; regard the edges, and examine an object with oblique as well as direct light. You shall see all these discomforts increase as the eye-pieces are deeper still. The glass will not bear such scrutiny unless of very fine quality. Do not pull out the tube: that destroys the previous correction. Get all the definition you can by using another fine objective for a condenser. Try it with the mirror, also by oblique sunlight. The better the glass the less will it be dependent upon tricks of illumination, stops, condensers, and obliquity. I have a glass which performs almost as well with the usual mirror as with the most perfect condenser. You constantly hear people say, "Oh, that is too deep an eye-piece." Of course; it is too candid. Now, what really are the obstacles to splendid definition? I fearlessly say that the chief of them may be summed up in two or three words.

Workmanship; displaced foci.

The workmanship is often the worst feature. Each uncentred glass gives out its own displacement of the circle of least confusion. I very rarely find the centres perfectly coincident. This blurs the image.

Displaced foci are always present.

Perfect definition requires that all the coloured rays, supposing

* Dr. Goring, who may justly be described as the inventor of test-objects, says ('Mic. Cabinet,' p. 152):—"The Poduræ show *a series of lines* or *cords on their surface.*" . . . . *Some have two sets of oblique lines on them*, fig. 8, plate 12. *Others are waved or curved.*

At p. 150, he sagaciously remarks—"That with the discovery of any more difficult object than that already known, *an improvement* in the microscope has soon followed. This was strikingly exemplified in the discovery of the lines in this insect: they were observed accidentally by the late Thomas Carpenter, Esq., of Tottenham."

Upon this principle, so well expressed, I venture to make the assertion that until the exclamation markings of this test are admitted to be only a provisional definition, and the beautiful beaded structure shown in the blank spaces is acknowledged to be a much finer test, no great improvement will be effected.

the light is sent down the tube in the reverse direction, should cross the axis at exactly the same precise point. That is achromatism.

It is not generally understood that each coloured ray has its own peculiar spherical aberration.

Suppose, for instance, a blue ray from a monochromatic light apparatus passes through a double convex lens, still at a focus the blue marginal rays will cut the axis nearer to the lens than the central blue pencils; that is spherical aberration for a blue ray. It would be much easier to construct object-glasses for use by *one* colour—*blue*, for instance; because no achromatism would be required; nothing but the correction of spherical aberration of the blue ray would then be requisite.

At present, in all objectives used in compound light, however fine their quality, absolute or at least almost perfect achromatism can only be obtained at the cost of a small residuary spherical aberration. Frequently, without altering the glasses intrinsically (I mean without substituting other lenses of different curve, focus, and index), colour can be got rid of by change of distance and position.

But as colour disappears the other residuum puts in its unwelcome appearance. Nothing shows this more clearly than the CIRCULAR SOLAR SPECTRUM.

Even in seeing Mr. Slack's silica cracks in his ingenious colloid silica slides, these cracks, as I once formerly remarked, appear often like coloured cylindrical threads; indeed the first time, he laughingly decoyed me into saying they were some vegetable fibre, so like to this appeared they with the Ross glass at the Society's rooms. But these cracks can be shown black with my Searcher. Nothing more particularly demonstrates the power of secondary correction behind the objective than this veritable effect.

The characteristics of splendid definition can be best seen in the use of transparent objects of a delicate structure suitable for a lower power. When, for instance, shall we ever see the *edge* of the *P. angulatum* with a high power as sharply, clearly, and brilliantly displayed as the details of the fly's tongue, upon which low objectives are usually tested? I bought a wonderfully fine inch-and-a-half from Ross, which bore the deepest eye-piece admirably. But then it was carefully chosen from many. I tested it upon *the intersection of two fine hairs:* and the four black markings caused by the interference. The same thing can be seen more beautifully with the cylindrical threads of the finest spun glass.

The $\frac{1}{80}$th of Messrs. Powell and Lealand exhibited at King's College, showed the Angulatum with woolly edges; and doubtless achromatism was obtained by some sacrifice of spherical correction. The spherules were seen peeping as it were through a white mist in which they appeared, as it were, partly dissolved.

Other splendid instances of sharpness of definition may almost always be seen by using the very lowest eye-piece obtainable, with a very excellent objective, such as a good $\frac{1}{50}$th immersion. In order to use my $\frac{1}{50}$th more conveniently, and to remove it somewhat from its dangerous proximity to the object, I have had a body made so as to reduce the conjugate focus one-half. The definition, after correction, is one of the most charming effects I have ever seen, especially with an eye-piece of long focus as 3 inches.

As an instance of the beautiful precision of this objective (under favourable circumstances), I will state *one* experience with it.

I was examining a *Podura curvicollis.* The sun was setting. A fine greenish-blue sky coloured the west. A plane mirror. *The Ross* $1\frac{1}{2}$ was in use as a condenser, unstopped off, and perfectly direct without obliquity. The entire field was filled with the ribs of the Podura. To my great delight, the whole of the white spaces, *generally seen absolutely blank*, appeared crowded with white beads in long rows, as shown within the ring drawn within Fig. 3.

If perfect definition is now required, microscopists must take a new start and destroy residuary errors by preventing the admission of obstinately aberrating rays: and using monochromatic or unique* rays (as Dr. Col. Woodward in his splendid photography).

I shall conclude this imperfect paper by pointing out the gradual, infantine, growth of magnificent microscopic defining power.

Lines preceded spherules.

*In the spaces, between the coarse markings* of the Podura Degeeria, short black cross lines were at first delineated (Mr. McIntire, 'M. M. J.,' Jan. 1870, Fig. 6, Plate XXXVII.). Next Col. Woodward photographed shadowy bars (see Fig. 2, Plate XXIII.).

Lined objects gave place to beaded (scales and diatoms).

Black dots of Quekett gave way to spherules. Nobert himself declared his XIXth Band would never be resolved, and he purposely concealed the precise number of the lines in it, but awarded the palm to Dr. Colonel Woodward, who photographed them with Powell and Lealand's *sixteenth immersion* and copper solution for a unique light of blue rays.

The same lens succeeded in turning the misty bars of the clear spaces between the markings of the Degeeria into beads, and the supposed blank spaces of the True Test Podura into similar rouleaus. And I here gratefully acknowledge the favour of their photographs being placed at my disposal by their courteous and distinguished author.

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*Postscript.*—Professor Stokes, Sec. R. S., 'Br. Ass. Reports,' 1871, has communicated the very important fact that titanic glass has the property of destroying the secondary spectrum: and a ter-

* Unique seems a more simple term than the long Greek compound.

borate of lead glass placed between crown and flint with cement has advantages in the correction of spherical and chromatic aberration hitherto unattainable. It is to be hoped that at no distant date great improvements will be effected.

Mr. Harcourt's experiments carried forward for forty years for the British Association in the construction of optical glass by means of a gas furnace.

The following substances were experimented upon, formed into glasses chiefly with phosphates on account of the pasty character of the silicates, combined in many cases with borates, tungstates, molybdates, or titanates. The glasses formed involved the elements of—

| | | |
|---|---|---|
| *Potassium.* | *Glucinum.* | *Vanadium.* |
| *Sodium.* | *Magnesium.* | *Lead.* |
| *Lithium.* | *Aluminium.* | *Thallium.* |
| *Barium.* | *Manganese.* | *Bismuth.* |
| *Strontium.* | *Zinc.* | *Antimony.* |
| *Calcium.* | *Cadmium.* | *Arsenic.* |
| *Tungsten.* | *Molybdenum.* | *Titanium.* |
| *Vanadium.* | *Nickel.* | *Chromium.* |
| *Uranium.* | *Phosphorus.* | *Fluorine.* |
| *Boron.* | *Sulphur.* | |

Mr. Vernon Harcourt made nearly 166 prisms. Titanic acid was found to have a superior power in extending the blue end of the spectrum, whilst boracic acid was found to have an opposite effect.

Professor Stokes says:—" By combining a negative or concave lens of ter-borate of lead with positive lenses of crown and flint, or else a positive lens of titanic glass with negative lenses of crown and flint, or even with a negative of very low flint and a positive of crown, achromatic triple combinations, free from secondary colour, may be formed without encountering (at least in the case of titanic glass) formidable curvatures, and by substituting at the same time a titanic glass for crown and a borate of lead for flint, the curvature may be a little further reduced."—'Rep. Br. Ass.,' 1862; Transactions of Sect., p. 1.

G. W. Royston-Pigott.

## VI.—*The Preparation of the Brain and Spinal Cord for Microscopical Examination.* By H. S. Atkinson.

(*Read before the* Medical Microscopical Society, *May* 16, 1873.)

The few remarks which I propose to bring before your notice this evening are based upon the results of some experiments which I performed last summer, in the Physiological Laboratory, King's College, by desire of Professor Rutherford, the object of the experiments being to ascertain the best method of *staining* preparations of the *brain* and *spinal cord*, previously hardened in *chromic acid.*

It may be interesting to some few of you here to night to know the method of preparing the brain and spinal cord for microscopical examination, and therefore, with your kind indulgence, I propose to give an account, as briefly as possible, of the method of preparing those tissues; first, when hardening fluids are used; and secondly, when hardening fluids are not used, but the tissue examined at once. I desire you to understand, however, that my knowledge of these methods is derived from instructions received from Professor Rutherford, and that I have no claim to any of the points to which I would direct your attention, excepting the method of staining the sections.

First, with regard to the preparation when hardening fluids are used.

I think it best to treat this division of my subject under *four* different heads, as you may be better able to follow the somewhat complicated directions which I shall be obliged to lay before you.

*First,* as to the hardening of the tissues.

*Secondly,* as to the cutting of the tissues.

*Thirdly,* as to the staining of the tissues; and

*Fourthly,* as to the mounting of the tissues.

First, as regards the *hardening of the tissues.*

The method which is found best is as follows:—

The fresh tissue, that is cord and brain, is cut into pieces as small as convenient, and placed for twenty-four to forty-eight hours in *methylated spirits of wine.* This prevents it from "*rotting.*" It is then placed in the "*hardening fluid.*" For the *spinal cord,* a solution of chromic acid of $\frac{1}{4}$ to $\frac{1}{2}$ per cent. in water answers best. If the cord be *small,* as that of a *cat* or *rabbit,* a $\frac{1}{4}$ per cent. solution is strong enough; but if the cord be *larger* than that, as that of the ox, or man, a $\frac{1}{2}$ per cent. solution must be used.

For the *cerebrum* and *cerebellum,* a mixture of chromic acid 1 part; potassium bichromate, 2 parts; water, 1200 parts, is best.

From four to six weeks are necessary for the chromic acid to render the nerve tissue sufficiently hard. It may then be cut; or if it be more convenient to cut it at a future time, the tissue should be transferred to methylated spirits and kept in it till sections can be cut.

It is important not to allow the tissues to remain in the hardening fluid too long; because they finally lose the toughness which they acquired under the influence of the acid, and become brittle. Moreover, it does not colour readily if it be too long in the chromic acid. The action of the chromic acid on the tissues is, as Dr. Rutherford observes, somewhat analogous to the process of tanning, with which all of you are familiar. In the process of tanning the tannic acid forms a solid substance with the gelatine. In the case of the hardening of nerve tissue by chromic acid there is probably an analogous change produced.

The cord then is placed in chromic acid, the cerebrum and cerebellum in the mixture of chromic acid, potassium bichromate, and water, well surrounded with fluid and kept free from dust. After a time, varying with the density of the tissues, they become of a yellowish tinge, quite hard and not yielding when gently pressed between the fingers.

The hardening fluid in the case of the cord must be changed after the first twenty-four hours, and fresh fluid added, and also changed during the process of hardening once or twice.

In the case of the cerebrum and cerebellum the strength of the solution must be doubled after the first fortnight.

*Secondly*, with regard to making *sections* of the hardened *tissues*.

The sections are sliced by means of a razor, with or without a section machine.

Some profess to make sections of tissues as well, or better, without machines as with them.

If the piece of tissue be as small as a "*bean*," and if it be embedded in a hard substance, such as paraffin or wax, a machine may be dispensed with, although even in this case the use of the machine is advantageous. If, however, we desire to make uniformly thin sections of the entire spinal cord, or brain, say of a rabbit or a cat, then the machine is of great service, for it enables one to cut slice after slice the same thickness with a rapidity and a precision which contrasts very agreeably with the repeated failures, the waste of time and tissue, which even accomplished histologists experience when they do not use a machine in these cases.

The indicator for graduating the thickness of the sections, and the freezing apparatus, which act perfectly, are Dr. Rutherford's modifications of Mr. Stirling's machine. The machine made by Hawksley has been materially altered by Dr. Rutherford, and the only machine approved by him is made by Baker.

The tissue is embedded in the machine in a mixture of lard

1 part, paraffin 5 parts. This mixture flows all round it and so supports it.

It is better than wax and oil when a machine is used, because the latter shrinks so much when it cools that it becomes loose in the box of the machine.

The mixture is heated in a water-bath, and poured into the hole of the machine, and the tissue, previously placed in absolute alcohol for ten minutes, is placed in the composition by means of forceps, and held there till it cools sufficiently to support it.

When the composition has cooled, sections may be cut by moving a razor across the top of the machine. It must be *pushed* obliquely in a direction away from right to left, and the whole section must be cut by one sweep of the razor. A *flat knife* is never used. An ordinary razor does very well, if its back be ground quite straight. It is important that both surfaces of the blade should be concave. The concavity of the surface which is below, when the razor is used in the machine, permits of the face of the knife being kept closely applied to the brass table on the top of the machine, while the concavity in the upper surface permits of the presence of a pool of spirit or other fluid for the slice of tissue to float over. The razor must be kept wet with methylated spirit or absolute alcohol. The finest sections are made by wetting the razor with absolute alcohol; but for ordinary purposes methylated spirit, or even water, answers fairly well. The sections are transferred from the knife to spirit or water, and any adherent paraffin is got rid of by gently moving them round and round with a camel-hair pencil and pouring off the fluid with the floating paraffin. They are then washed in water and placed for half an hour in a 1 per cent. solution of bichromate of potassium in water, and are then ready for staining.

*Thirdly*, with regard to the *staining* of the *tissues.*

By staining the *brain* and *spinal cord* the structure is rendered clearer than if they are examined without being stained. I propose only to deal with the staining by carmine.

Various solutions of carmine have been proposed, and after very many trials of the different solutions, it was found that a modification of the fluid proposed by Dr. Beale was best. The fluid recommended by Dr. Beale has this disadvantage, with regard to the staining of *nerve tissue*, however, in that it is much too strong, and that it stains everything evenly. Thus, for instance, when a *cord* is placed in it, the parts are all coloured alike, and consequently the *white substance of Schwann*, instead of presenting an uncoloured ring round the coloured *axial* cylinder, is coloured like the latter, and can with difficulty be differentiated from it, gives the appearance which would lead you to suppose the nerve fibril was all of one composition. And the *nerve cells*, instead of standing *boldly out*, are fused, as it were, into the mass of the *grey* *matter.* The fault, then, of this carmine fluid was, for nerve tissue, that the different structural elements were not sufficiently *differentiated.*

Now this disadvantage to its use is entirely got rid of by diluting the *carmine* solution with *water.* The brilliancy with which the sections are stained is also, I think, very much increased by using the diluted *carmine;* and in this place I may mention, that there is a certain time in the hardening of the tissue, at which time, if sections are prepared, they give much more brilliant staining than when the tissue has been left for a longer period in the hardening fluid; also, they do not take so long a time to colour. Hence, the longer the tissue remains in the hardening fluid, after a certain time, varying with the density of that tissue, the longer do the sections take to stain, and the less brilliant is the colouring.

The *method* of *staining* is as follows:—The sections taken from the $\frac{1}{2}$ per cent. solution of bichromate of potash, and washed with water, are placed in the *diluted carmine.* The *carmine* solution is prepared by diluting *Dr. Beale's carmine fluid* (composed of carmine 10 grains, strong liquor ammoniæ $\frac{1}{2}$ a drachm, glycerine 2 ounces, distilled water 2 ounces, absolute alcohol $\frac{1}{2}$ an ounce), *seven times* with distilled water, and *filtering after dilution.* It is best to have a large quantity of the *carmine solution* in which the sections are placed. They must be kept *free* from *dust,* by covering the vessel.

The period of immersion in the carmine solution varies much. It is from one to twenty-four or even forty-eight hours, depending on the length of time the tissue has been in the *hardening fluid,* and the degree of coloration required. The sections ought to be repeatedly examined to ascertain whether they are sufficiently coloured.

When they are coloured enough, the carmine solution is poured off, and the sections are washed. This is done by filling the vessel with distilled water, stirring the sections round very gently with a camel-hair pencil, and pouring the water off. This is repeated two or three times. The pigment is then fixed in the sections by immersing them in rectified spirit of *wine,* in which they may be kept till mounted.

It may be of service to communicate to you a somewhat useful *hint* with regard to carrying the unmounted sections about with you. That is, pour off the spirit, otherwise the sections will be broken to pieces by friction one upon another. At the end of your journey replace the spirit.

*Lastly,* with regard to *mounting* the sections. Having obtained your sections and stained them, the next thing to be done is to *mount* them, so as to preserve them permanently. They may be mounted in *Canada balsam* or *dammar resin.*

Before being mounted, however, they require to be *clarified*, or, in other words, *rendered transparent*, which is done by means of *creosote* or *oil of cloves*, or *oil of turpentine*. The *oil of cloves* is the best *clarifying medium*. Before using the *oil of cloves* or other clarifying media, all water must be removed from the sections. This is done by immersing them in absolute alcohol. Place the sections in a watch-glass containing absolute alcohol, in order thoroughly to get rid of all water. Then place a section on a slide, and incline it to one side so as to drain off the alcohol. Allow the section to become sodden but not too dry, then insinuate a drop of clove oil, by means of a small sable-hair brush under the section. (Sable being stiffer than camel hair, is preferable for this purpose.) Care must be taken not to allow any of the oil of cloves to run over the upper surface of the section at first, till all the absolute alcohol has been driven off into the air.

If the oil of cloves has gone over the upper surface of the preparation before the moisture (driven off by the absolute alcohol) has disappeared, you have a cloudiness in your preparation on mounting it in the dammar. When all the moisture, therefore, has been driven off, a drop of oil of cloves is placed on the upper surface of the preparation, and it is examined with a low power often, to see if it has become perfectly transparent. You then soak up the superfluous oil of cloves with *bibulous paper*, or, if there is no danger of injuring the preparation, transfer it to another slide on which is placed a drop of *dammar* or *Canada balsam* (dried and dissolved in benzole or turpentine), and place the covering glass on the preparation. To keep the covering glass in its place until the dammar has dried, a spring clip may be used.

I may now be permitted to give you a *résumé* of this complicated process.

First, then, the tissue is placed in methylated spirit, then in the *hardening fluids*,—chromic acid in the case of the cord,—chromic acid and bichromate of potassium in the case of the cerebrum and cerebellum. When hard enough, sections are cut (the razor being wetted with spirit), placed for half an hour at least in potassium bichromate, washed with water, transferred to Dr. Beale's carmine fluid diluted seven times with water. When stained enough, washed with water, and spirit added, in which they are kept till mounted by means of absolute alcohol, oil of cloves, and dammar. The sections must always be manipulated with camel-hair brushes.

I now come to the *Second* Division of my subject, namely, the preparation of the brain and cord when hardening fluids are not used.

*The brain,* in pieces the size of a pea, when fresh may be placed at once in Dr. Beale's carmine fluid diluted seven times; when sufficiently stained a portion of it may be transferred to a slide, and teazed out with needles in *acid glycerine* (that is, glycerine, 1 ounce, hydrochloric acid, 2 drops). A cover glass is then put on, and pressure exerted. This may be very easily obtained by means of a strong spring clip. It is well not to mount the preparation permanently for a few days. By this method you get very good preparations of the cells of the grey matter. (Dr. Beale.)

*The cord.* If the cord is *fresh* it is quite stiff enough to allow of sections being cut without a machine. They are placed for half an hour in a ½ per cent. solution of bichromate of potash. Stained with carmine, and mounted in *dammar*, in the same manner as the sections of hardened cord.

Dr. Rutherford finds the method of *freezing* of great service in the examination of the fresh brain and spinal cord. The sections cut from the frozen tissue are coloured in the dilute carmine, and then teazed or mounted in glycerine 1 ounce, hydrochloric acid 2 minims, or glycerine 1 ounce, glacial acetic acid 5 minims.

## NEW BOOKS, WITH SHORT NOTICES.

We have been compelled, owing to pressure on our space, to "crush out" for the present month notice of the following works:—

The Microscope and Microscopical Technology, by Dr. Frey. Translated by Dr. R. Cutler.

Manual of Human and Comparative Histology. Edited by S. Stricker. Translated by H. Power, M.B. Vol. III.

Experimental Researches on the Causes and Nature of Hay-Fever, by C. H. Blackley, M.R.C.S.

A Manual of Pathological Histology, by Dr. E. Rindfleish. Vols. I. and II. Translated by E. B. Baxter, M.D.

The Philosophy of Evolution: An Actonian Prize Essay, by B. T. Lowne, M.R.C.S., Lecturer on Physiology at the Middlesex Hospital; and

Revue des Sciences Médicales en France et à l'Étranger, dirigé par Georges Hayem. Tome I., No. 1.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Development of Cancer.*—In a late number of Virchow's 'Archiv,' Dr. Carmalt records the results of the examination of three carcinomatous tumours, removed from the skin of the nose, the cheek, and the eyelid. Thiersch, in his work on cancer, has pointed out that the epithelial cells of the sebaceous and sweat glands, and especially the cells of the rete Malpighii, are often the point of departure for cancer of the skin, and he casually includes the epithelium and the hair-follicles in the same category. In the hair-follicles Dr. Carmalt found not only an increase of the outer layer of epithelium, but also offshoots from the follicles, diverticula lined with epithelium, penetrating the connective tissue to various depths and in various directions. A section made either obliquely or parallel to the axis of the follicle, and passing through the diverticula, gave exactly the appearance of the ordinary cancer-alveoli, filled with epithelial cells. In certain preparations, it was possible to see the alveolar groupings of the cells pass into long processes lined with epithelium, which, again, opened into the hair-follicle; so that the appearance was that of a group of acinous glands with their excretory duct. Other sections presented a still more complete picture, *viz.* the enlarged follicles and their offshoots, the alveolar groups of epithelial cells, evidently in connection with the follicular offshoots, and lastly, isolated epithelial alveoli, situated more deeply in the tissues, and showingt he ordinary characters of cancer-alveoli. Carmalt thinks it is hardly to be doubted that these isolated cancer-alveoli were also originally in continuity with the hair-follicles and their diverticula.

*The Microscope in Leprosy.*—This is a subject which has been gone into very fully by Dr. Carter, of Bombay, in his paper this year read before the Royal Medical and Chirurgical Society. After dealing with some of the more medical portions of the subject, it stated that the structural changes observed are due to exudation or deposit in the skin and appertaining nerve-trunks of a firm, translucent, colourless, or pale-reddish material, which may be distinguished by the borrowed terms hyalin-fibroid and hyalin-granular. As regards the skin, conjunctiva, and adjacent mucous membrane of the mouth and larynx, this deposit (here hyalin-granular) first appears within or immediately beneath the membrane proper; accessory organs, and even the blood-vessels, are secondarily involved, but it has been noticed that the tactile corpuscles disappear before other less sentient elements. As regards the nerves, this deposit (here hyalin-fibroid) first appears between the individual nerve-tubules, and within their sheath—*i. e.* the neurilemma of the funiculus; the outer envelope of connective tissue is hardly changed. By accumulation of the new material the tubules are separated, compressed, emptied, and eventually destroyed. The microscopic characters of this leprous deposit are then referred to. The material looks exudative, but may be derived from proliferation of connective-tissue corpuscles; it undergoes slight development, and is susceptible of degeneration. In sixteen autopsies of lepers consecutively dying in hospital, no trace of deposit was noticed in the muscles, bones, or any of the viscera. The brain and spinal cord were wholly free from such deposit, &c.

*Regeneration of the Epithelium in the Web of a Frog's Foot.*—Dr. Klein, of the Brown Institute, gives an excellent account of Biesiadecki's recent experiments on the above subject. He says that he uses the web of the frog. The animal, slightly curarised, is placed on a glass plate, its web is stretched over a cork ring fixed on the plate, and a small drop of cantharidal collodion is allowed to flow over the edge of the web, so that it affects both surfaces near the edge. The part which is to be observed is covered with a small thin glass, a sufficient quantity of solution of sulphate of sodium or common water having been poured on the web. It can be examined under a magnifying power of 300–450. After two hours, the epidermis of the part to which the cantharidal collodion had been applied is raised as a blister, and the blood-vessels of the corium, arteries, and veins, as well as capillaries, appear to be somewhat dilated. The upper wall of the blister consists either only of the epidermis, or of the epidermis and the superficial layers of the rete Malpighii, or it consists of the whole epithelium. In any case, the wall of the blister must be removed entire with great care. The most suitable cases for observation are those in which, after the removal of the blister, the true corium is exposed, and the circulation in the blood-vessels is unchanged. The regeneration of the epithelium takes place thus:—Soon after the removal of the blister, the blood-vessels become dilated, the colourless blood-corpuscles accumulate first in the veins, then also in the capillaries, and an abundant emigration of them follows. After six or eight hours the corium contains numerous groups of colourless corpuscles in the neighbourhood of the blood-vessels. On observing carefully for some time the border of the exposed corium, the colourless corpuscles which have emigrated from the blood-vessels nearest to the edge are readily seen to migrate, with very active amœboid movements, to the edge, where they rise gradually, one after the other, freely to the surface. After a certain time isolated groups of colourless corpuscles are found on the free surface of the edge of the corium, performing lively movements. Soon, however, they become flat, sharply outlined, their protoplasm transparent, and their nuclei less marked. Two or three hours afterwards the whole edge of the corium is covered with one layer of such cells. After the whole defect of the epithelium is filled up by that layer, the cells become again less sharply outlined, and the corium seems to be covered by a homogeneous substance in which nuclei become visible, gradually progressing from the peripheral parts towards the centre. In the course of the following hours, below that layer a second one is formed by emigrated colourless corpuscles, and then a third one; at the same time the cells of the layer first formed enlarge, become more stiff and flat. Isolated cells are always seen to migrate from the depth towards the free surface through those layers. After twenty-four hours the defect is filled by several layers of cells, the thickness of the layers being generally greater than that of the old epithelium in the neighbourhood, as the cells of the new epithelium are softer and less flattened than the cells of the former. Between the cells of the new-formed epithelium pigment-cells make their appearance. These originate from two sources: first, from the branched pigment-cells that are to be found generally in the epithelium of the web; and, secondly, from the pigment-cells of the corium. As regards the first source, it is to be noticed, according to Biesiadecki, that the interepithelial branched pigment-cells of the neighbourhood of the defect become amœboid, undergo division, and while some of their offsprings remain amongst the cells of the old epithelium, some other ones migrate away between the cells of the new-formed epithelium. Biesiadecki takes it as probable that pigment-cells which are to be found lying round the blood-vessels of the corium become also amœboid, and migrate towards the surface between the cells of the new epithelium. Two important facts are still to be mentioned relating to the regeneration of the epithelium. First, if in the formation of the blister, the deepest layer of the rete Malpighii be left on the corium, that layer, after the removal of the blister, is generally raised and removed by the subsequent exudation. Secondly, the old epithelial cells in the immediate neighbourhood of the defect do not undergo any active changes. These two points stand in very sharp contrast to the doctrine commonly held; namely, that in the process of regeneration of epithelium, the new-formed epithelial cells are the result of proliferation of the deepest epithelial cells which have been left, or of those which border the defect.—*The Medical Record, April*, 1873.

*The Mode of Fertilization in the Grasses.*—An American writer says that Professor Hildebrand, of Freiburg, recently made to the Berlin Academy a detailed communication on this subject. He shows that there is an entire series of steps from the completely diœcious arrangement to that in which self-fertilization is the rule even if it has exceptions. There are, for instance, some examples of diœcious grasses, then a number of monœcious, after which follow some with both hermaphrodite and staminate flowers, where the latter only can serve for crossing; then, in greater number, grasses with purely hermaphrodite flowers; in some of which the pistil develops before the anthers; in others, where the pistil and anthers develop simultaneously, the discharge of pollen from the anthers lasts for an appreciably longer time; but there are some cases where the pistil and anthers appear to develop together and have the same duration, but yet under such conditions that the pollen can reach the pistil only with difficulty. And finally some grasses in which close fertilization is not avoided, but actually occurs in a large proportion of cases, and even preponderates; yet even in these instances occasional cross-fertilization does not appear to be excluded. So that fertilization in grasses, as in other families of plants, must be studied, species by species, and we cannot apply our observations of one species to another species even of the same genus. Thus the genera *Hordeum*, *Avena*, and *Triticum* exhibit great diversities in respect to fertilization in their several species.

*The Structure of Striped Muscular Fibre.*—Mr. E. A. Schäfer has communicated to the Royal Society at one of its recent sittings, a paper on the above subject, which is of so much importance that it almost promises to completely revolutionize histological science. The author, after premising that, owing to the rapidity with which changes set in after death, the subject in question can only properly be worked out whilst the muscular fibres are still living, the author proceeds to give the result of his investigations of the tissue in this condition. The animal employed was the common large water-beetle, the muscles of the legs being taken. These were examined entirely without addition, being either teazed out upon a glass slide in the ordinary way and covered with thin glass, or else prepared upon the latter, which was then inverted over a ring of putty after the method introduced by Stricker. The author describes a muscular fibre as consisting of a ground-substance appearing at first sight to be formed of two distinct substances (the one dim, the other bright in aspect, which are arranged in alternating disks disposed in successive series, with their planes at right angles to the axis of the fibre) and of a vast number of minute rod-like particles, to which he applies the term *muscle-rods*, which are closely arranged side by side and parallel to the axis of the fibre, so as to form by their juxtaposition as many series as there are disks of dim substance in the fibre. The main part or shaft of each muscle-rod is imbedded in and traverses a disk of dim substance, while the ends, which are enlarged at the extremity into little knobs or heads, extend into the bright disks. These little knobs it is which give the appearance of the line of dots which has long been described as existing in the middle of each bright stripe; when the fibre is somewhat extended this line appears double, owing to the separation of the heads of the two successive series of muscle-rods which meet in the middle of the disk of bright substance. The author describes the rods as differing somewhat both in relative position and in form, these differences being accompanied by corresponding changes in the appearance of the ground-substance. The principal changes are those of form. Thus, in what the author is inclined to regard as the state of absolute rest, the rods are uniformly cylindrical without terminal enlargements; in this case only a longitudinal fibrillation is to be seen in the fibre, all trace of transverse striping having disappeared. In the normal state of slight tension, however, the rod-heads make their appearance, and with them the bright substance by which they are surrounded, so that the dim ground-substance now presents a transversely striated aspect. In contraction of the muscle the heads of the rods become enlarged at the expense of the shaft, the extremities of each muscle-rod thus approaching one another: the enlarged heads being closely applied both to the neighbouring ones of the same series and to those of the next series which meet them in the bright stripe, the line of dots now appears as a dark transverse band with bright borders. As the contraction proceeds, and these dark bands approach one another, the bright borders encroach upon the dim stripe, which finally disappears, so that its place is taken up by a single transverse bright stripe. Consequently contracted muscle shows alternate dark and bright stripes; the former, however, are in this case due to the enlarged juxtaposed extremities of the rods, the light on the other hand being mainly composed of the ground-substance which has become accumulated in the intervals between their shafts. After giving a description of the appearances observed in transverse section, when examined in the normal state without addition, and after the consideration of those which are met with in sections from frozen muscle examined in $\frac{1}{2}$ per cent. solution of common salt, and which have been described by Cohnheim, the author proceeds to consider the nature of the ground-substance, and more especially the transversely striated appearance which it ordinarily presents. He gives it as his opinion that the ground-substance is in reality uniform in nature throughout, and that the bright bands which cross it are due to an optical effect produced by the presence of the globular heads of the muscle-rods, which have a different refractive index from that of the ground-substance. That such an explanation is possible, is shown by the examination of minute oil-globules imbedded in gelatine, which appear under the microscope as dark spots with a bright surrounding, the juxtaposition of several such dots giving the effect of a bright band. That the bright transverse bands in muscle are similarly produced by the juxtaposition of the rod-heads would appear from the following amongst other considerations:—

1. Where the rod-heads are smaller the bright bands are correspondingly narrower.

2. Where the rod-heads have become merged into the shafts, so as no longer to be seen as distinct objects, the bright transverse stripes have also entirely disappeared.

3. When in contraction the rod-heads enlarge and encroach on the shaft, their bright borders accompany them and encroach on the dim

substance, so that at last all appearance of dimness becomes entirely obliterated, the bright borders becoming blended in the middle.

4. The part of the muscle-rod where the head joins the shaft is rendered indistinct by the brightness around the rod-head; whereas if this brightness were inherent in the ground-substance, this part of the rod would stand out all the darker by the contrast.

5. The appearance of a transverse section is corroborated; for in this case the rod-heads are seen so close together that the optical effect of any one would become merged into those of its neighbours: consequently the whole of the intermediate substance would appear bright; and this is actually found to be the case.

6. The fact that both the dim and the bright substance of resting muscle appear doubly refracting would seem to indicate that they are of the same nature.

The author then proceeds to give the result of his investigations of the appearance of muscle under polarized light. He finds that, as regards muscle at rest when placed between crossed Nicols, the whole fibre appears illuminated; in the contracted state, on the other hand, the appearance is presented of illuminated stripes with dark intervals. The latter correspond with the lines formed by the juxtaposition of the enlarged ends of the muscle-rods; these consequently are singly refracting (isotropous), and so, in all probability, are the shafts of the muscle-rods; they do not, however, stand out as *black* streaks, since they are surrounded by doubly refracting (anisotropous) ground-substance, and are illuminated by the light which has previously traversed this. In the same way it may readily be understood why, in the resting muscle also, the rods, although isotropous, do not appear as such. The conclusion, then, that the author arrives at on this point is that the whole of the muscular fibre is anisotropous, with the exception of the muscle-rods. Various observers are then quoted in support of the accuracy of the description given; and the probability is pointed out, and supported by an observation of Prof. Brücke, that in all cases in which alternating disks of isotropous and anisotropous substances are observed, the muscular fibre is in a state of contraction (although not necessarily shortened)—that is to say that the anisotropous substance has become accumulated between the shafts of the rods, the isotropous disks being due to the rod-heads, between which there is no perceptible amount of anisotropous substance left remaining. The author concludes the paper by offering a conjecture as to the nature of the substances which, according to his description, compose the proper substance of muscle, and as to the probable mode in which the contraction is effected. He is inclined to regard the intermediate ground-substance as the true contractile part, and thinks that it may be allied in nature to ordinary protoplasm, the rods, on the other hand, being elastic structures, and merely serving to restore the fibre to its original length.

## NOTES AND MEMORANDA.

**Information required as to Microscopic Powers.**—A gentleman of Melbourne, Australia, who signs himself H. H. has written a letter to 'Nature,' of June 5, making some sensible inquiries on the subject of microscopic powers. We think the letter so likely to interest certain of our readers that we reproduce it in full. The writer says :—I am following up some investigations and experiments in which I require certain data, which, however, I cannot at present arrive at, not being in possession of sufficiently delicate and exact instrumental appliances. The information which I now desire to elicit from some more experienced observers than myself is of such importance as to be both useful and interesting to many of your readers, and I therefore crave your insertion of this communication. The information I require is all the more important as having a bearing upon many questions which are now attracting public attention, such as spontaneous generation, the initial stage and transitional forms of living organisms, also various researches in experimental physics, chemistry, &c. I desire to arrive at the following data :—

1. What is the estimated dimensions of most minute particles of matter which can be visible, under any circumstances or conditions, under the highest powers of the microscope? I leave out of consideration (under this head) the question whether such matter is living or dead, organic or inorganic, or in fact regardless of any of its properties whatever except its mere visibility as a minute portion of matter. Some observers speak of visible particles $\frac{1}{200000}$th and $\frac{1}{300000}$th of an inch diameter; this is surely near the limit.

2. What is the best or most accurate method of arriving at an estimate of the dimensions of such minute objects as are too small to admit of actual measurement by any of the appliances now in use? Every microscopist knows from experience that objects may be distinctly visible, not as a mere point, but having an appreciable diameter, and yet be too minute for actual measurement to any degree of accuracy.

3. Have the most recently constructed microscopic objectives, such as the $\frac{1}{50}$th or $\frac{1}{25}$th, any advantages over the $\frac{1}{16}$th or $\frac{1}{12}$th inch objectives in the determination of the data above referred to? and have immersion lenses any advantage in this respect? I find some difference of opinion on this point. Some microscopists consider that a really first-class $\frac{1}{12}$th with the use of deep eye-pieces will enable us to see anything whatever which can be seen by any other objective of shorter focus. On the other hand, it is evident that a great number of the most experienced microscopists think otherwise; and from the very fact of their purchase of such expensive high powers, argue that such lenses are found to supply what other powers cannot accomplish. It appears to me that there is too much of vague and indefinite assertion in regard to the comparative powers and qualities of microscopic objectives, and it is very desirable that some more definite results should be

arrived at. With what precision and accuracy the results of astronomical observations are made! and taking into consideration that many of these results are obtained by different methods of observation, using different instruments, and by different observers, it is astonishing that the discrepancies and errors of observation are so small. It is generally admitted that the microscope is, to say the least, equally perfect, if not more so, than the telescope; and we should therefore expect a corresponding degree of accuracy in the results of microscopical observations. There are no doubt many who, like myself, have hitherto worked with only the medium and low powers, but wish to be possessed of the improved objectives of high power, but from want of sufficient information it is difficult to make a suitable choice.

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# CORRESPONDENCE.

## A most Un-editorial Expression of Opinion.

*To the Editor of the 'Monthly Microscopical Journal.'*

Padnal Hall, Chadwell Heath, Essex,
*May* 24, 1873.

Sir,—As another result of sending the Tolles' $\frac{1}{10}$th for the aperture experiment, my attention has been called to an *editorial* article appearing in the 'Lens' for April last as a sample of the capabilities of a Journal aspiring to be the exponent of microscopical science in America. Therein my desire for a "fair trial and fair usage" is questioned, and I am accused of having performed "a trick." This style forbids all scientific discussion. If during the relative test from a *defining* aperture in air—to water—and balsam, I had shifted the adjustment (which the writer supposes I ought to have done), then I might properly have been accused of performing a "trick" instead of a simple optical demonstration. Sending this glass, without request, specially for me to try, truly placed me on the "horns of a dilemma." Judging from the remarks that it has elicited from some because the result has not gone the way that they wished it to go, I can well imagine what would have been said and the tone of triumph assumed if I had *refused* the trial.

In reference to the final graceless and uncalled-for sentence of the article, I may inform the writer and my American friends that which it is needless to state *here*, that I never had *and never shall have* any pecuniary interest in the manufacture of object-glasses as a motive for disparaging the one in question. My experiments have been carried out entirely for the sake of the pursuit, and the results would have been confined to myself had there been no Society or Journal for such communications.

Yours truly,

F. H. Wenham,

*Vice-President R.M.S.*

## Errors in a Cincinnati Letter in last Volume.

*To the Editor of the 'Monthly Microscopical Journal.'*

Cincinnati, Ohio, U.S., *May 27th*, 1873.

Editor 'M. M. J.,'—Owing perhaps to a well-nigh illegible chirography, the following corrections are necessary in my letter, p. 239.

P. 239, line 6, for "$\frac{1}{4}$th" read "$\frac{1}{16}$th"; p. 240, line 10, for "cone" read "fine"; line 21, for "cone" read "cover"; line 27, for "when" read "where."

Respectfully,
Tyro.

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# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *June* 4, 1873.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society was read, and the thanks of the meeting were voted to the donors.

The President called attention to a list of names suspended in the room in accordance with the by-laws, and expressed a hope that if any of the Fellows present were acquainted with either of the gentlemen named in the list, they would kindly use their influence to endeavour to get the arrears of subscription paid up, so that the names might not be struck out from the general list of Fellows.

The Secretary read a paper by Mr. F. Kitton, of Norwich, descriptive of some species of Aulacodiscus, and other diatoms found at Iquique, Callao, &c., by Capt. Perry of Liverpool. The paper—which was illustrated by drawings, and by specimens exhibited at the close of the meeting—will be found printed *in extenso* at page 6.

The thanks of the Society were unanimously voted to Mr. Kitton for his paper.

Mr. J. W. Stephenson said he took the present opportunity of stating that, to his surprise, he found that the mode of dividing the cone of light in his erecting binocular microscope by means of two prisms was used by Professor Riddell, of New Orleans, in the year 1853, in his form of binocular. The arrangement of that instrument differed from his own in the following respect—*viz.* that his (Mr. Stephenson's) prisms were so placed that, combined with the reflecting plate above, they acted as an erecting instrument, and by entering into the cell of the object-glass could be used for high powers whenever required; whilst those of Professor Riddell were placed above the object-glass simply to produce binocular effect. He had only just heard of this through the kindness of Mr. Frank Crisp, and he took the earliest opportunity of notifying it to the Fellows of the Society.

Mr. Stephenson then read a paper entitled "Observations on the Inner and Outer Layers of Coscinodiscus when examined in Bisulphide of Carbon and in Air." The paper was illustrated by drawings made and enlarged upon the blackboard by Mr. Charles Stewart, and by mounted specimens exhibited under the binocular microscope. The paper will be found at page 1.

A vote of thanks to Mr. Stephenson for his paper was proposed by the President, and carried unanimously.

Mr. Slack said that whilst he agreed generally with all that Mr. Stephenson had said upon the subject, he thought that Mr. Stephenson would hardly disagree with him in expressing his impression that the hexagonal framework of this diatom was composed of beads. He had carefully examined these appearances, and could find no reason for supposing them to be spurious. It might not perhaps be well known that they could be shown by a first-rate $\frac{1}{2}$-inch objective cut down to an angle of 65°, and illuminated with a large condenser having a central stop so as to give dark-ground illumination. A good objective used in this way would work well, even with a D or an F eye-piece, and this view of the diatoms was a very instructive one. Having viewed Coscinodiscus in this way, he also looked at it with a fine $\frac{1}{8}$-inch immersion, by Powell and Lealand, with various eye-pieces, the result being that he had very little doubt but that the hexagonal framework on its upper surface was really composed of two or more rows of beads, and he thought that if very great care were taken in the examination, the thicker portions of the inner layer would prove to be beaded. In no fractured specimen which he examined could he get any proof of the existence of a fractured portion of a membrane at the spots where Mr. Stephenson considered there was a foramen. He had tried to test this by means of polarized light; by placing a polarizing prism under the condenser, and using a tourmaline eye-piece, he obtained plenty of light, but though a feeble polarizing effect was seen in the framework of the hexagons, there was no indication of the existence of any membrane. In Triceratium there was undoubtedly a very decided floor to the depressions. If it should be finally decided that any diatoms had foramina, they would have to be separated from others and placed in a separate class by themselves, although it might possibly be that want of sufficient power fails to reveal them in all. It was curious that Mr. Kitton's paper should in some measure bear upon the same subject. Mr. Stephenson's method of resolving some of the difficulties was, he thought, a very admirable one, and if the example now set were to be followed out it would do much towards redeeming their characters as microscopists from the taunt that when studying diatoms they spent their time in counting a lot of dots.

Mr. Charles Stewart said that Mr. Slack had mentioned the probability of the beaded structure of the thickened ring or margin of the foramen; he had himself seen something like it, but did not think the appearance was so much like upstanding bosses, as it was like little notches. An analogous structure might be noticed in the Polycistinæ, very many of which presented a number of notches at the margin of the hole, a majority of them presenting the appearance of a curious little scollop, but how far this effect might be due to upstanding spines or to beading was not quite clear.

Mr. Slack observed that his remarks did not refer so much to the edge of the hole as to the circumstance that the top of the framework of the hexagon was composed of two or more rows of beads.

Mr. Wenham said there was one question as to an optical effect which he thought might be easily settled. It was stated that an aperture in the centre gave rise to what was known as an "eye spot." This could be decided by experiment without difficulty by mounting a piece of tin-foil upon glass, and pricking a hole in it with the point of a needle, when the same effect ought to be produced. The conditions might be altered in a number of ways, by making the holes of different sizes, or mounting the tin-foil on the other side of the glass or in different media. They had generally regarded the "eye spot" as being produced by a membrane, but the question might easily be settled, and he would endeavour to try it and give them the results.

Mr. Charles Stewart thought it could be tried in many ways. A large number of the silicious shells of Polycistinæ as well as the plates of the Echinoderms undoubtedly had holes through them, and in all cases they showed a very distinct image. The peculiarity in the instance before them was that it gave a positive image in all cases.

Mr. Stephenson supposed that nobody would deny that the Polycistinæ had foramina. The formation of an image seemed to him to be the same in principle as the formation of one in the well-known camera-obscura experiment by making a hole through an ordinary window-shutter, when objects outside were shown on a screen; the formation of the image of the sun when shining through the leaves of trees was another case in point.

Dr. W. J. Gray, in reply to a question from Mr. Slack, said he had observed that there was a covering over the hexagonal framework in Triceratium, but it did not follow that therefore the same existed in Coscinodiscus.

The President said that the formation of an image through an aperture was a necessary consequence, but it should also be borne in mind that an image of that kind necessitated a screen upon which to show it.

Mr. Wenham said he was glad to see that Mr. Stephenson had given in his paper the actual angular apertures in each instance. No doubt an image was always formed by rays of light in passing through a small hole, but it remained to be seen what effect lenses would have upon such an image formed without lenses.

The President gave notice that the library and reading room would be closed during the month of August, and wishing the Fellows a happy long vacation, the meeting was adjourned to October 1st.

Richard Branwell, Esq., M.R.C.S.L., was elected a Fellow of the Society.

Walter W. Reeves,
*Assist.-Secretary.*

## *The Scientific Evening.*

The last Scientific Evening of the season was held, by the kind permission of the authorities, in the great hall of King's College, on the 14th May, when there was a large attendance of Fellows and an excellent display of objects and apparatus.

It is gratifying to find that the opportunities afforded by these "Scientific Evenings" for the careful examination of objects and apparatus, and for friendly conversation thereupon, continue to be highly appreciated.

The subjoined list will show that various departments of microscopical science received valuable illustration from the novelty, rarity, or remarkable merit of the objects, or from new means for their exhibition.

The Society was indebted to Messrs. Horne and Thornthwaite, Baker, and How, for the loan of a large number of excellent lamps of the most approved patterns.

The Society exhibited the following from the late Mr. Farrant's collection: The Lord's Prayer, Creed, and Ten Commandments, written on glass with Peters's machine in $\frac{1}{25} \times \frac{1}{22} = \frac{1}{550}$ inch; human cuticle; and a fragment of green-glass tube.

Mr. A. Angell: Zoophytes, &c., from chalk.

Mr. W. A. Bevington: Foraminifera with a Stephenson's erecting microscope.

Dr. Cresswell Baber: *Cylindrical Epithelium* obtained during life from the interior of an *Ovarian Cyst.* The cyst tapped was probably small, as only a few drachms of a thick *colloid fluid* escaped.

*After death* the tumor, which occupied the greater part of the abdomen, was found to be a multilocular cyst, and to contain a glairy fluid.

Dr. W. J. Gray: Platino-cyanide of lithium and platino-cyanide of strontian.

Mr. Henry Hailes: Foraminifera from Tasmania.

Mr. S. J. McIntire: Section of eye of drone-fly; and the scales of *Urania leilus*, to prove that the beads are spurious.

Mr. E. Richards: Foraminifera in water, arranged with Richards' new protecting cap.

Mr. Charles Tyler: Varieties of *Dactylocalyx* and a *Flustra* from Australia.

Mr. Frederick Fitch: *Arrenurus caudatus* alive.

Mr. William Loy: Ramifications of trachea in the ovipositor of *Dytiscus marginalis;* and dissections of *Lucanus cervus.*

Mr. Sigsworth: Head of *Cysticercus fasciolaris.*

Mr. Suffolk: Ruled lens in eye-piece used in making drawings of microscopic objects. Exhibited to show the small amount of injury to definition with $\frac{1}{8}$th and 2nd eye-piece.

Mr. Slack: The curious two-celled anthers of the Bay *Laurus nobilis*, with the lids open, in living flowers.

Mr. Stewart exhibited: 1. A vertical section of the optic disk, retina, choroid, and sclerotic of a cat; the termination of the optic nerve being directed away from the light (vertebrate character). 2. Vertical section of retina and choroid of a cuttle-fish; termination of nerve directed towards the light with choroid in front (invertebrate character). 3. Section of kidney of rabbit, showing epithelium of Malpighian capsule, and the narrow neck by which it becomes continuous with the convoluted uriniferous tube.

Mr. J. W. Stephenson: Brucine; light analyzed by reflexion from black-glass plate and his Erecting Microscope.

Mr. W. W. Reeves: New species of fungi, *Acrostalagmus nigripes*, &c.

Messrs. Powell and Lealand: *Surirella gemma*, with $\frac{1}{4}$ object-glass.

Messrs. Ross: New Binocular Microscope with Wenham's achromatic prism; and a newly-arranged microscope.

Messrs. Beck: The structure of the scale of *Lepidocyrtus curvicollis* with $\frac{1}{10}$ immersion; the structure shown by the application of moisture on exposed surface. (See 'Monthly Microscopical Journal.')

Mr. Charles Baker: The Medical Microscope and the Sea-side Microscope.

Mr. Thomas Curties: British and foreign zoophytes.

Mr. Moginie: Sting and poison bag of bee, &c.

Mr. How: Transverse section of a calamite, showing cortical layer. Ditto, ditto, showing the diaphragm at node, from the Coal-measures of Lancashire.

Donations to the Library and Cabinet, from May 7th to June 4th, 1873:—

| | From |
|---|---|
| Land and Water. Weekly .. .. .. .. .. .. .. | *The Editor.* |
| Nature. Weekly .. .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts Journal. Weekly .. .. .. .. .. .. | *Society.* |
| Quarterly Journal of the Geological Society, No. 114 .. | *Society.* |
| Monthly Notices of Papers and Proceedings of the Royal Society of Tasmania, 1871 .. .. .. .. .. .. .. | *Ditto.* |
| Results of Five Years' Meteorological Observations for Hobart Town, by Francis Abbott, F.R.M.S. .. .. | *Author.* |
| Medical and Surgical History of the War, Part 1, 2 vols. | *Surgeon-General's Office, Washington, U.S.A.* |
| Three Slides of Diatoms .. .. .. .. .. .. .. .. | *Frederick Kitton, Esq.* |

## Reading Microscopical Society.*

March 4, 1873.—Captain Lang presided, and in support of his remarks, in a paper on Professor H. L. Smith's Conspectus of the *Diatomaceæ* ('Monthly Microscopical Journal,' March, 1873), as to the propriety of combining the genus *Campylodiscus* with *Surirella*, and the genus *Triceratium* with *Biddulphia*, exhibited a variety of *Surirella striatula* as much twisted, and a *Surirella fastuosa* as circular in outline as any *Campylodiscus*; and also front views of entire frustules of *Biddulphia* and *Triceratium*, showing their close affinity in form and structure.

He also exhibited a fine gathering (from Mead's pond) from Professor Smith, containing numerous rare species of *Pinnularia*, *Surirella*,

* Report furnished by Mr. B. J. Austin.

and *Nitzschia*, also slides of *Biddulphia aurita*, both selected and in their natural state of chain-like growth, gathered by Captain Perry at Callao. Captain Perry was anxious to ascertain whether such recent gatherings would yield the same forms as those found in the guano of that neighbourhood, and it is therefore worthy of observation that this *Biddulphia aurita* is a very distinct variety from that so abundant in the guano, resembling rather the modern form obtained from cleanings of *Haliotis* shells from California.

Dr. Shettle described a method of rubbing down needles so as to produce a cutting edge, and yet retain the sharp point, by running the needle edgeways through a slice of cork, allowing such portion only of the pointed edge to project as it is desirable to convert into the knife-blade. The cork, with the needle thus inserted, is then firmly fixed in a small hand-vice, the edge of the cork being brought to the edge of the vice. The needle should then be laid upon a block of metal or other hard material, and rubbed carefully with an oil-stone hone, the two sides of the needle-blade being easily produced by inclining the vice in a particular manner. The edge of the blade should always (for convenience of rubbing) be kept in one direction, and its place determined by keeping the needle much nearer one side of the vice than the other. The paper also referred to a form of handle, with tapering ferule, by which the knife-edged needle is very firmly fixed, and by the use of which a change of needle is easily effected.

## The Oldham Microscopical Society.

[We fear the Secretary of this club can have very little idea of the difficulties which beset an Editor's path, and we fancy too that he has been somewhat irregular in the discharge of his duty, for he now sends us together the reports of February, March, and April meetings of the body which he represents. Further, he has made no attempt to reduce the reports, but has sent them in full, so that, if printed, they would at least occupy eight or ten pages. We must beg of him to be more moderate in future, and to send us abstracted reports, and to let us have them at least earlier than four months after the date of the meeting.—Ed. 'M. M. J.']

On Wednesday, the 12th February, the seventh conversazione of the above-named Society was held in the club-room of the Lyceum. The subject of the evening being a geological one, the tables were spread with a large assortment of local fossils, some of them exhibiting internal structure, and upon the walls were hung numerous diagrams illustrative of fossil plant life generally. On Wednesday, the 12th March, the above Society held its eighth conversazione in the club-room of the Lyceum. The attendance of members on the occasion was numerous. In the course of the evening a paper was read by Mr. J. Butterworth, on "The Internal Structure of Fossil Plants," the subject being illustrated by numerous well-executed diagrams, and by a variety of thin sections of fossil wood, to the production of which Mr. Butterworth has given special attention for many years. After the paper had been read, and a careful examination made of

Mr. Butterworth's specimens under the microscopes, a cordial vote of thanks was passed to him, which brought the proceedings to a close. A meeting of this Society was held on the 9th of April, when a paper was read by Mr. J. R. Byrom on "Vegetable Tissues." The President, Dr. A. Thom Thomson, presided, and there was a goodly attendance of members. The walls were decorated with suitable diagrams, illustrative of the subject, and a series of beautifully-prepared objects were exhibited under the various microscopes, which contributed much to the enjoyment of the evening. Mr. Byrom's paper is the last for this season.

## Chichester and West Sussex Natural History and Microscopical Society.

On Thursday, April 17th, the members of this recently-established Society met (for the first time) in the Lecture Hall of the Literary Society and Mechanics' Institute, when the President (Dr. Paxton) delivered a most interesting and appropriate Inaugural Address. The Society already numbers nearly sixty members, many of whom confessedly are undecided in the choice of a pursuit. To assist such in the selection of one congenial to their tastes a large number of ornithological, entomological, botanical, and geological specimens, with an abundance of appropriate literature, were exhibited, competent curators were appointed to each section to give information and advice to all who desired them, and fourteen microscopes were in operation during the evening, illustrating various departments in microscopy.

Each member of the Society being on this special occasion at liberty to introduce two friends, there were nearly one hundred and fifty persons present; and so much interest was evinced by them that the committee yielded to a request to throw open the exhibition to the public from 11 A.M. to 10 P.M. on the following day, when a large number of ladies and gentlemen visited it.

## Brighton and Sussex Natural History Society.

March 27th. — Microscopical Meeting. Mr. Hollis, Vice-President, in the chair.

Mr. R. Glaisyer announced the receipt from Mr. Wonfor of eight slides for the Society's cabinet.

Mr. Wonfor announced that Mr. T. Curties, of Holborn, had sent him down, for distribution among the members, two dozen packets of seeds, interesting as microscopic objects. These he proposed distributing later in the evening. He felt sure they would join in a cordial vote of thanks to Mr. Curties. This was done by acclamation.

Mr. Wonfor then introduced the subject of the evening: "Seeds, Microscopically Considered."

One of the objects sought in setting aside an evening every month for the microscope, was the opportunity it would afford members to compare notes of observations, as well as enable those who had paid attention to any particular objects to impart to others the facts they had been able to work out. His reason for introducing a common object—"the seed"—was partly to induce some of the younger members to pay attention to it, as something worthy their study, and partly to show that an apparently insignificant object was deserving of greater notice than was generally given to it, from the physiological teachings deducible from its structure and component parts, as well as for the assistance it rendered in classifying the plants to which it belonged.

As some were aware during 1862–3–4 he devoted considerable time and attention to the collection and examination of the seeds of wild plants, when, he might safely say, he examined some hundreds; in fact, he seldom took a walk without bringing home half-a-dozen fresh examples, a considerable percentage of which were so beautiful as to warrant their being mounted for the cabinet. Since then he had, from time to time, paid more attention to the seeds of cultivated plants. One lesson he had learnt, among others, was the family likeness which ran through seeds belonging to some of the great natural orders, and another the assistance the microscopic characters afforded in determining the difference between species which approximated closely to each other in some particulars.

In its ripe state a seed consisted of a nucleus, or kernel, and an outer skin called the *testa*, variable in colour, texture, and markings. Sections, such as those so admirably made by their friend, Dr. Hallifax, showed that three distinct layers might be detected in the *testa*, corresponding to the three coats of the ovule. The nucleus consisted of two parts—the albumen and the true-growing parts. The albumen differed in quantity and consistency, being soft in some, and as hard as bone in others, and was composed of starch, lignine, oil, nitrogenous and saline substances, contained in cells. The embryo was either straight or curved, and consisted of the *radicle*, which developed into the root; the *plumule*, which produced one or more seed leaves, and the *gemmule*, or first leaf-bud.

Turning to the external appearance of the seed, the microscope revealed " a thing of beauty," as shown by the great variety of markings. Without entering into minute descriptions, he would indicate some of the families whose seeds he had found contained some of the most interesting examples. As before mentioned, the family likeness was so great that it was often possible to indicate the family to which a seed belonged before knowing its name.

The Chili nettles, the *Loasaceæ*, contained very interesting and beautiful seeds, contrasting with which were the *Portulaceæ*, Purslane family, some of which bore a striking resemblance to the shells of fossil ammonites, many of them shining with quite a metallic lustre. Among the *Lobeliaceæ* would be found interesting specimens, not the least interesting being *Isotoma axillaris*, presenting an amber-coloured crystalline appearance, with exquisite markings. The *Papaveraceæ* well repaid examination, while the *Scrophulariaceæ*—a very large family, containing the foxgloves, mulleins, figworts, the toadflax, snapdragon, eyebright, paulonia, &c.—were all characterized more or less by hexagonal markings, with more minute reticulations within them. One cultivated plant, *Nycterinia capensis*, was of a delicate primrose colour, and was covered with minute rounded granulations. Perhaps the most interesting group was the *Caryophyllaceæ*, some of the finest examples of which might be found among our English wild plants, such as the catchflies, chickweeds, stitchworts, campions, and soapworts, one of the most beautiful being the "ragged robin," *Lychnis flos-cuculi*.

Other families would supply beautiful objects; one must not be omitted, the orchids, which had been compared to gold coins in silk purses; with the exception of these, which might be viewed as transparent objects, the rest should be mounted dry. He had brought down between one and two hundred different kinds for examination. These he proposed should be arranged under the microscopes in groups for comparison of the different families.

Dr. Hallifax mentioned the facility with which those who had studied seeds could separate not only those closely allied, but also hybrids.

Mr. Sewell inquired whether seeds required drying with heat to prevent fungus growth.

Mr. Wonfor stated that care should be taken to gather them dry, and make sure they were free from moisture when mounted. Many of those he had brought down for exhibition had been mounted ten years.

Mr. Hennah said he could vouch for the truth of Mr. Wonfor's assertions, for looking at the seeds he had himself brought down, he found the greater part were mounted by Mr. Wonfor, and bore the date 1863.

The meeting then became a conversazione, when Messrs. Hennah, W. H. Smith, F. E. Sawyer, Sewell, R. Glaisyer, Wonfor, and Dr. Hallifax exhibited seeds, the latter gentleman showing some of his admirable sections.

April 10th.—Ordinary Meeting. Mr. G. Scott, President, in the chair.

Messrs. Balean, J. Jeffcoat, and the Rev. C. Payne were elected ordinary members, and Mr. J. C. Ward an honorary member.

The receipt was announced for the library of the last Proceedings of the Eastbourne Natural History Society, and of the Second Report of the Sub-Wealden Exploration.

Mr. Wonfor then read a paper entitled "Suggestions towards the verification of the Fauna and Flora of the county of Sussex."

After detailing the work done by the Society since its formation, and pointing out how it had from time to time, as it felt strong enough, increased its sphere of action and usefulness, Mr. Wonfor considered the time had arrived when the Society should undertake the great work of verifying the natural history of the county. He then pointed out the advantages derived from a study of, or even attention to, some one branch of natural history, indicating the pleasures which a true naturalist, as compared with a mere collector, derived while in pursuit of plant or animal, not resting satisfied with a dried plant, a stuffed bird, or a set-out insect, but endeavouring to discover something in the life-history and economy of each.

The suggestions resolved themselves into what the Society collectively and the members individually could do. He proposed the

Society should become the conservator of such lists of plants and animals as its own members, the members of similar societies, or naturalists generally, either within or without the county, might transmit to it. That these lists be collated and compared; the question of publication, and in what way, to be determined at some future time. That to carry out this object, naturalists generally, and its own members in particular, be asked to note down the name, with *approximate* locality, and the circumstances whether the species was rare, local, or common, with any other points of interest. That such members as had already paid attention to some branch of natural history be asked to work at it systematically in the county. That those who had only a general interest, and especially the younger members, should devote themselves to some one branch, and endeavour to work it up, putting down, as before suggested, facts of locality, time, &c.

To those asking "What to work at?" he replied, any branch would prove interesting, but he would indicate certain paths likely to afford novelties either to science, or at least to the Fauna and Flora of Sussex.

The marine zoology and botany of Sussex, with its extensive coast line, would well repay any ardent workers, as not only many species hitherto unknown to the district would be sure to reward the investigator, but almost everything had to be learnt of the times of appearance and transformation of most of the sea-dwellers along the coast.

Much remained to be done in Botany, both Cryptogamic and Phænogamic.

The Mosses and Lichens had been admirably done by such indefatigable students as Mitten and G. Davies. One branch of Cryptogamic Botany, though, was unworked, *viz.* the Fungi. Though Ralfs, Smith, and Jenner had done much for the minute Algæ, many parts of the county, at present unworked, would be sure to yield good results.

In zoology a field of inquiry lay open among the land and freshwater mollusks. No county was so rich in insects; the beetles, spiders, and diptera, required workers, as did also the Tineæ among the Lepidoptera, while careful observation would certainly add to localities for moths and butterflies, and, possibly, increase the number of known species. The late Dr. Ormerod and Mr. Unwin had added to our knowledge of the Hymenoptera, auguring what might be done in other departments of entomology. Captain Knox and others had told how rich Sussex was in birds, and, with the new "Wild Birds' Act" in operation, there was no fear of a diminution of species.

In any department the microscope would be found an invaluable aid; in fact, it ought to form a part of the equipment of every naturalist.

As assistances to work, the library contained admirable monographs in almost, if not every, branch, rendering the identification of species comparatively easy. There were two lessons all should learn, never to be ashamed of ignorance when they did not know a plant or animal, and not to fancy their dignity suffered by asking some one, who had made any particular branch a study, to name any specimen for them. All our best naturalists were wont to refer novelties to themselves to those who worked at a special branch.

The co-operation of non-members might be secured by making them honorary members, under the powers of a rule which enabled the Society to make such any person residing out of Brighton or Hove who contributed specimens or interesting matter.

There was one other suggestion, that all who might be induced to co-operate in carrying out the good work be asked, while securing a specimen for their own herbarium, cabinet or collection, to obtain a duplicate and forward it to the Society for the Brighton Free Museum, or such other local museums as might from time to time arise in other parts of the county. Local museums should be rich in local objects, and while he hoped to see in time the Brighton Museum a great educational institution, well supplied with objects, arranged, as was intended, with a view to their educational value, he also hoped it would be the nucleus from which similar institutions might spring up in other parts of the county.

After a very cordial vote of thanks to Mr. Wonfor, a discussion followed, in which Messrs. O. A. Fox, T. H. Hennah, R. Glaisyer, Sewell, F. W. Phillips, J. C. Onions, Wonfor, and the President took part, the opinion being generally expressed that if Mr. Wonfor's suggestions were carried out they would mark an important era in the Society's history. Eventually it was resolved "that the Society approves the suggestions embodied in Mr. Wonfor's paper, and requests the committee to consider the best mode of carrying out the same, and to report to a future meeting."

## South London Microscopical and Natural History Club.

An ordinary meeting of this Club was held on Tuesday, May 20th, at Glo'ster Hall, Glo'ster Place, Brixton Road. Robert Braithwaite, Esq., M.D., F.L.S., presided.

Mr. E. P. Pett read a paper "On the Aphis, or Green Fly." After a minute description of the form and structure of the insect, the reader passed on to the consideration of the modes of reproduction. Much had been written on the subject, but all authorities appeared to agree that in the spring the warming sun and air acted upon the eggs laid in the previous autumn. In due course the eggs were hatched, and the young aphis emerged, wingless. After changing its skin three or four times, it commenced, without interposition of a male, to give birth to living wingless young, who in their turn became mothers, and so the lineage descended for many generations. Occasionally an aphis at its birth appeared similar to its predecessors, but at the last change but one of its skin it possessed rudimentary wings, which became fully developed on the final change taking place. The multiplication of these insects was extremely large, though different statements were made as to the average rate at which young were produced; one authority stating it at three, and another at fourteen *per diem*. Schrank, starting from Bonnet's observations, calculated the progeny of a single aphis during one summer at 23,740,000;

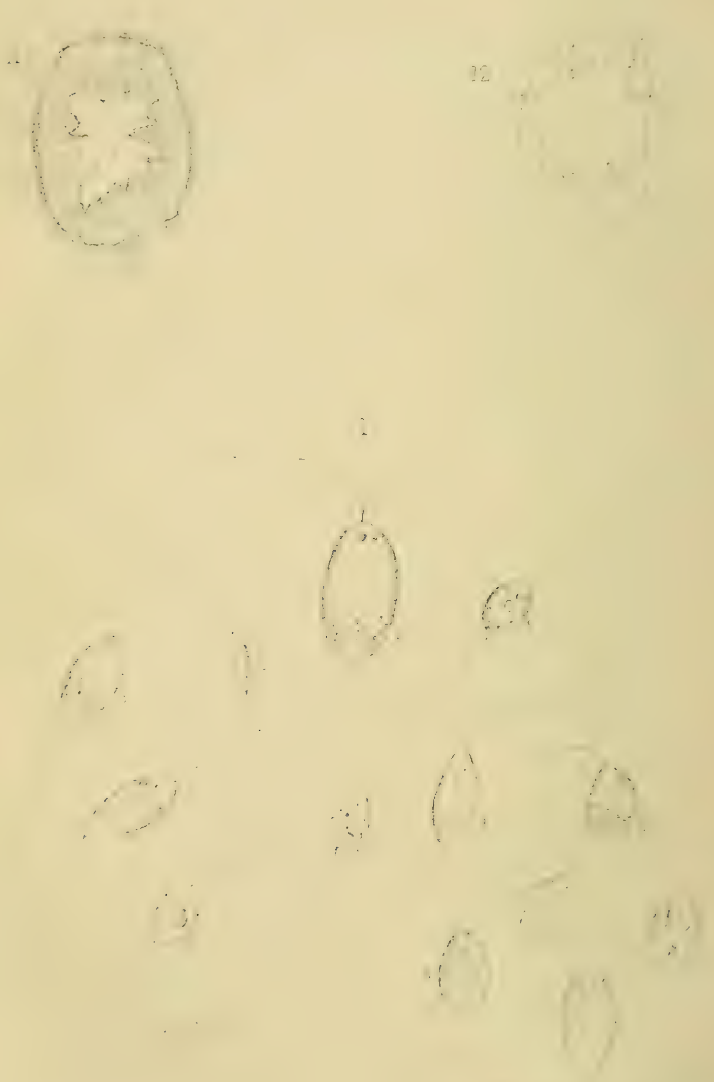
whilst Reaumur says the offspring of a single aphis will amount to 5,904,900,000. In wingless females, Mr. Pett had frequently counted from thirty to fifty young in various stages of development, whilst the winged individuals contained from twenty to thirty young.

Aphidæ were generally regarded as stupid and devoid of maternal feeling: instances might, however, be seen where their actions seemed to show at least some care for their offspring. When the sap failed in any particular branch or leaf on which a family had been feeding, Mr. Pett had seen the parent emigrating with her family on her back and clinging to her antennæ. He was curious to see if this was at the will of the parent, the caprice of her children, or by mutual consent, and to this end he took a mother who was walking along with four of her children on her back, and placed them separately on a glass slip on the stage of the microscope, parent and offspring being close together. The mother immediately extended one of her front legs, so as to form an inclined plane, by means of which the little ones mounted, three to her back, and one to her antennæ. With a fine camel-hair brush two of the young were then removed from their resting place. The mother immediately instituted a search for her lost children; and having found them, the same process of ascending to a place of safety was gone through.

Wherever there was vegetation—roots, branches, leaves, flowers, &c.—it was more than probable that aphidæ would be found, although exceptions to the devastations of this tiny plague of the field and garden might be found. Amongst the most destructive of these insects were the hop and potato fly, both akin to the rose-aphis. A species usually confined its attacks to one family of plants; probably deriving therefrom nourishment which it failed to obtain elsewhere: thus aphidæ taken from an arum lily died when placed on a rose-tree, and those taken from a rose-tree dwindled and died off when placed on the lily. The classification of the various kinds of aphidæ was spoken of as being at present in a rather imperfect condition; and the various trees on which aphidæ, presenting distinctive features, had been found, were enumerated.

Mr. Pett recorded a parasitic fungus with which the aphis was frequently attacked, usually after changing a skin. The insect became pearly-coloured, and ultimately of a reddish-yellow tint: its skin was then covered with a slight down, and it speedily died. Lady-birds would feed largely upon aphidæ, while perhaps their worst insect enemy was the larva of the lacewing fly. The gardener was, however, the most inveterate foe of the "green fly": he may fumigate, powder, or syringe his plants, but still fail entirely to rid himself of his pests; and probably the best mode of securing ourselves against the damaging attacks of the aphis was to encourage the preservation and multiplication of insects that prey upon it, such as the lady-bird and lacewing fly.

Various drawings, illustrative of the subject, executed by Mr. Pett, were, during the reading of the paper, exhibited to the members; and at the conclusion a hearty vote of thanks was unanimously accorded to Mr. Pett for his interesting paper.



Development of a Cercomonad.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1873.

## I.—*Researches on the Life History of a Cercomonad: a Lesson in Biogenesis.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.

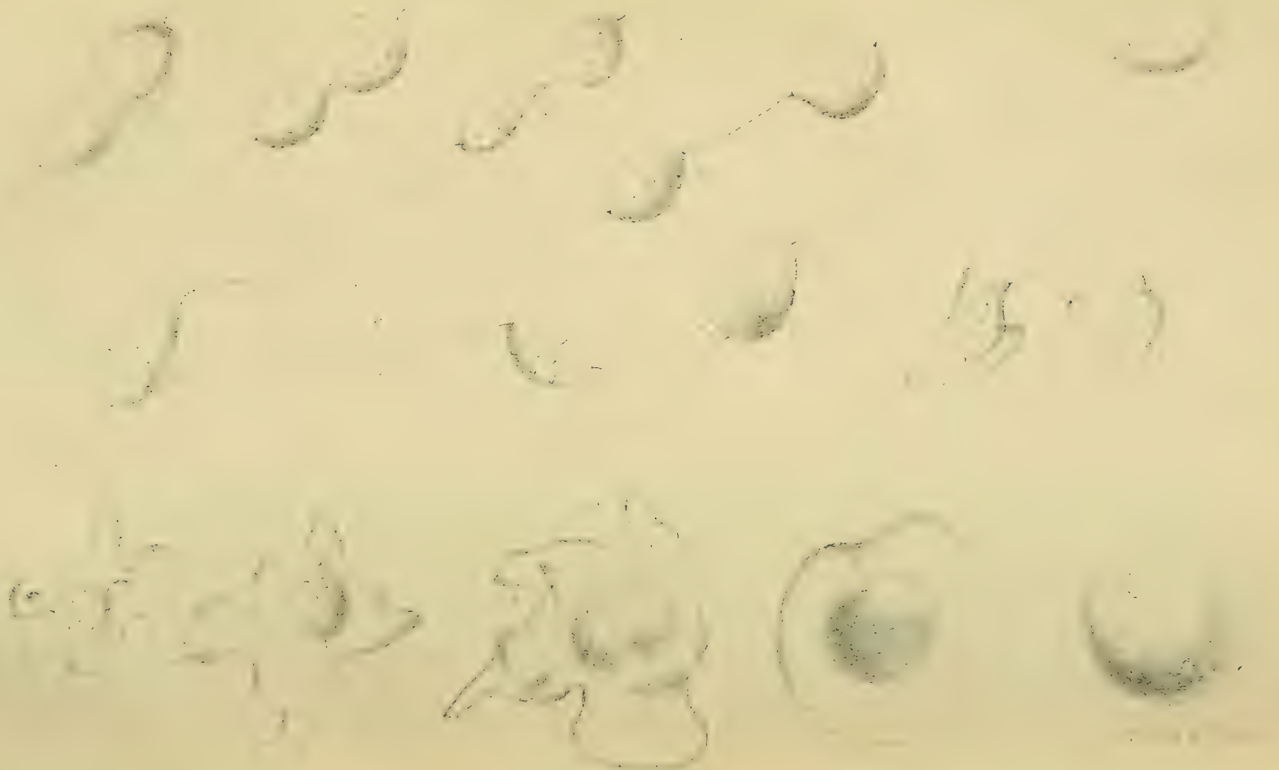
Plates XXIV., XXV., and XXVI.

The question as to whether vital forms of the lowliest and minutest kind may have their origin in a new, and as yet unexplained, arrangement of invital material, is one that can never find a legitimate and final reply in the class of experiments employed to test it within the last thirty years. A careful student of the literature of the subject will see that the *results* obtained by the same and different experimenters, with similar infusions and solutions, are so uncertain, and often contradictory, as to leave the whole question open to bias; and an almost equal array of so-called "experimental facts," from nearly equally trustworthy observers, may be quoted on either side. This may be all pleasant enough in a "wordy war," but it does not even approximate to a decision of the issue, and points to insufficiency in the experiments employed. The appearance or non-appearance of organic forms in certain infusions placed in sealed flasks or tubes, or otherwise conditioned, is held to be decisive of their production *de novo*, or otherwise; but in point of fact, we know *nothing*—absolutely nothing—of the life history of the greater number of the forms produced. To attempt to decide, therefore, from the experiments as yet published, that their production in gross masses in inorganic infusions proves that inorganic elements produced them, may be to beg the whole question. Inferring from what we *know* of nature's modes of reproduction, we have a right to expect not a *de novo* production, but a production from genetic elements. But when we remember the relation in size, throughout nature, between the ova and spermatozoa and the organism producing them, the fact that no such elements are visible (if they exist) in Bacteria or monads is probably a mere necessity of our present instrumental power. At least this is inevitable, that before we can be scientifically certain that these lowly forms do or do not originate in non-vital elements, we ought to know their life history; and if this be desirable in the question of Abiogenesis, it must be absolutely essential before we even approach that of Heterogenesis. We must patiently follow them without a break in observation, through all their changes, and then, by repeating these observations, decide on the stability or otherwise of the form. For some years our attention has been individually directed to this subject; and three years since the advisability of combined work commended itself to us. For work of this kind to be effective, we believe there must be more than one observer, in order that the observations may be unbroken as far as possible, and also to secure a mutual as well as a double confirmation.

The powers we have employed are Powell and Lealand's $\frac{1}{50}$, $\frac{1}{25}$, $\frac{1}{16}$, $\frac{1}{12}$, $\frac{1}{8}$, and Ross' $\frac{1}{6}$ and $\frac{1}{4}$. We commenced upon a monad which is at present undescribed, but which is, under some circumstances, found in enormous quantities in the fluid resulting from the maceration of a cod's head. Our mode of procedure we shall not now describe, as we purpose doing so at length eventually. Suffice it to say that we employ an arrangement by means of which a drop of the infusion under examination may be kept wet, and its contained organisms preserved in life and health for an indefinite length of time, when examined even with the $\frac{1}{50}$. We have on several occasions kept the same drop under examination, with its living inmates multiplying, for from eight to fourteen days, and during the whole of that time the object-glass has not been more out of focus than two or three turns of the fine adjustment.

The form on which our constant labour has been bestowed passes through a remarkable series of changes, all and every one of which might be taken for a distinct and independent creature, but that we have tracked it through all its transitions, and seen it pass from the one into the other. We find that these changes are the reverse of capricious; they are always alike. The stability of their recurrence is as complete as that of an entomological form. But we have yet one very important investigation to complete. On this we were working with the highest powers, when our attention was arrested by the appearance in the field of a form very similar to the one with which we were so long familiar, but manifesting an entirely dissimilar behaviour. The new-comer was, roughly speaking, like the old form in size and shape, but instead of two flagella at one end, it had a single flagellum at each end, and it was multiplying by fission with great rapidity. Reasoning from what we knew, we felt assured that this was not a capricious development of our old friend whose life history we had almost compassed. But we had no right to *positively* assume this without investigation; and the researches that ensued led to the observations now recorded.

The field as first seen presented the appearance figured in Plate XXIV., Fig. 1. Among a number of the forms on which we had been working, possessing the two flagella at one end, marked *a*, were several cercomonads with an equal flagellum at each end, marked *b*, moving about the field with great activity; and some of



Development of a '[illegible]onad'

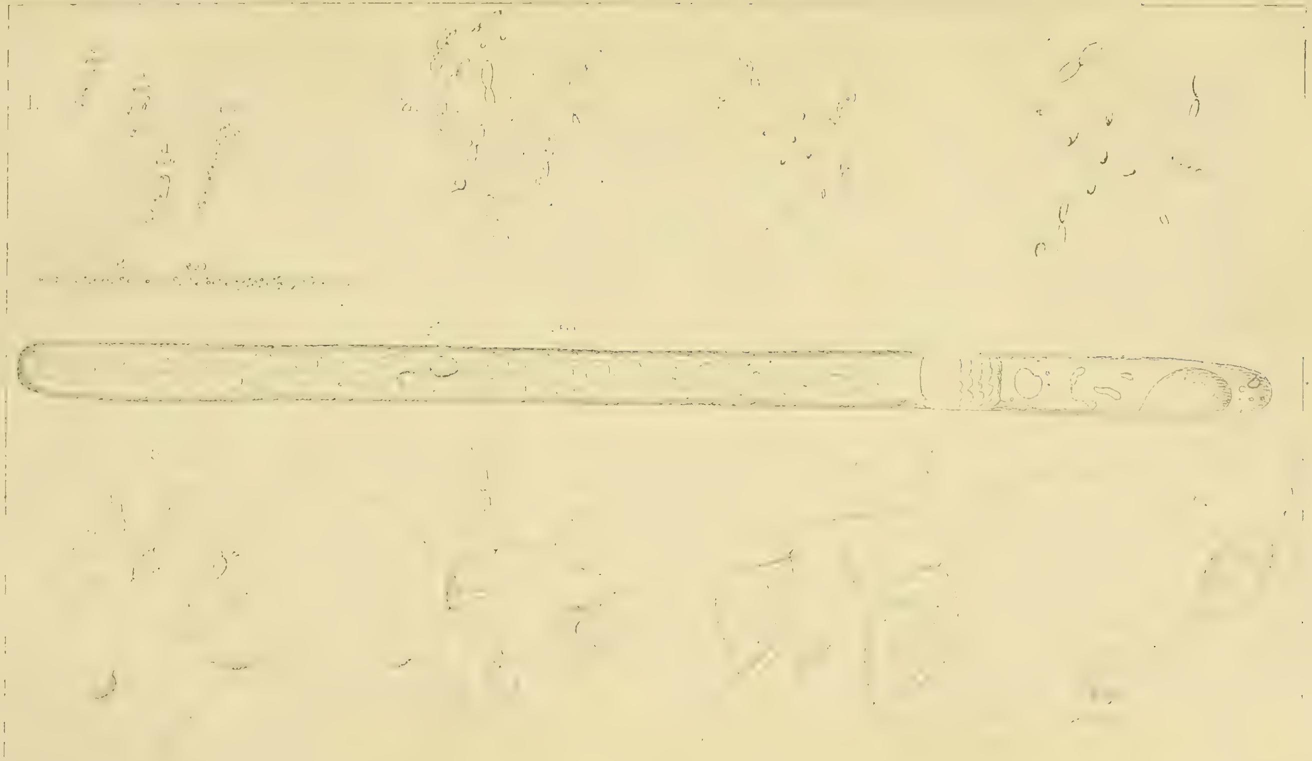
them were dividing by fission in a manner, as far as we know, before unobserved, as drawn at *c, c*. We now made them the special objects of attention, directing our examination first to their mode of division. The first indication that it was about to take place was, that the body became squarer, more plastic, and sub-amœboid, a slight but sudden constriction of the sarcode ensued, as seen in Fig. 1, Plate XXV.; this rapidly proceeded to the greater constriction shown in Fig. 2. At this stage a *stretching* of the constricted portion of the sarcode took place, as in Fig. 3, the flagella *a* and *b* lashing with great force. As the sarcode stretched it became finer, no more being extruded from the now perfectly-divided bodies. In Fig. 4 it is drawn with the sarcode slightly thicker than the flagella, and in Fig. 5 it has reached its final length, after which, by the rapid and strong motion of *a* from *b*, the attenuated sarcode *c* snaps at *c*, as in Fig. 6, leaving the separated bodies *a* and *b* free, and each possessed of a new flagellum *c, d*. Taking an average of forty cases, we find that this entire process is completed in four minutes and forty seconds.

Now becomes manifest the importance of continuous and patient watching; for we found that this mode of increase might continue without any change whatever for eight days at least. But this is a sufficiently long period to justify an observer in concluding that it was the *only* mode by which increase was secured. If we had not been constantly watching the other form in the same field, we might have arrived at this conclusion, but our attention was at length called to a new phenomenon. Many of the organisms in question all at once appeared to be pouring out a delicate sarcode, as in Fig. 7, *a, b*, Plate XXV. Nevertheless, they moved with great freedom, and the flagella rapidly vibrated. This sarcode increased in size all round the organism, but was of extreme tenuity, and as it increased in quantity the latter moved only by pseudo-podia, although the flagella were still comparatively active. In the course of seven hours there were several in the field, moving in all directions, and at length two approached and touched each other, as in Fig. 8, Plate XXV.; a rapid blending of the sarcode now ensued, and the flagella disappeared: they were constantly watched; the amœboid sarcode of each blended with the other, and at length the bodies touched and began to unite, as in Fig. 9, Plate XXV. Their union was now rapid until it reached the condition drawn in Fig. 10, Plate XXV., and at length it became a mere cyst, figured at Fig. 11, Plate XXV., with a very decided investment. Having reached this condition and become slightly yellow in hue, an apparent thinning of the integument of the cyst ensued, and it became suddenly rent all round, and retracted towards the centre, as in Fig. 11, Plate XXIV. Up to this time we had employed without intermission a magnifying power of 2500 diameters. (Powell and

Lealand's $\frac{1}{50}$ and A eye-piece.) With this we perceived that the burst cyst was pouring out what at first appeared like a viscid mass of oily matter, but which, when followed into its more dispersed condition, presented the appearance of minute granulation. A draw-tube 8 inches in length was now put on with the B eye-piece, and on delicate focussing it was palpable beyond all question that a dense mass of granules, inconceivably small, was being emitted from the cyst, as drawn in Plate XXIV., Fig. 11.* This observation appeared to us so important, that it was determined, if possible, to repeat it. This we did: following a pair from the condition drawn in Plate XXV., Fig. 7, to the final bursting of the cyst, as in Plate XXIV., Fig. 11. We have made careful drawings of each stage, but give only the film of the cyst after it had spent itself, in Fig. 12, Plate XXIV.

It became now a matter of great interest with us to study the future of these infinitesimal spores. With the $\frac{1}{25}$ the most accurate observer could not have discovered their presence if he had not previously seen them with the $\frac{1}{50}$. Indeed, we should have failed wholly to see them but for their enormous aggregation and motion in a mass. A relative idea of their size may be given. The *Bacterium termo* of Cohn is familiar to all microscopists. His *Bacillus Ulna*, a larger form, almost equally so. In Plate XXVI., Fig. 1, *a*, the former, is drawn as magnified 600 diameters; the latter magnified with the same power at *c*, *d*, *e*, while at *b* one of the *B. termo* at *a* is magnified 2500 diameters, and at *f* the *Bacillus Ulna* drawn at *e* is also magnified 2500 diameters. Fig. 2 represents a portion of a field under the same magnification with *B. termo*, and some of the granules emitted from the cyst interspersed. Fine as these dots are, they are all too coarse to be other than diagramatic. This field was now watched without intermission for six hours, when a portion of it in which gradual increase in the size of the granules had been seen was drawn at Fig. 3, the same power of course being employed; and the increase in size being well seen by a comparison with the *B. termo* in the field. The increase was now more rapid, Fig. 4 representing the change that ensued in another hour and a half; where there is not only an increase in size, but a tendency to the sub-ovate form of the parent. At the expiration of nine hours (in all), in which the field had never for one moment been unwatched, they had grown into the forms shown at *a*, *b*, *c*, *d*, and *e*, Fig. 5, and *flagella* were distinctly seen. How they were acquired eluded our most vigorous research, both then and subsequently. The first movement seen, which was before flagella were *discovered*, was similar to, but much slower than, that of a watch "balance-wheel"; this was shortly changed into a

* The relation between the granules and the cyst in size must be considered only approximate; they cannot be drawn with sufficient minuteness.



wriggling motion, when the flagella were discovered, and they very soon moved freely over the field. Their increase in size was now rapid, arising probably from the greater amount of pabulum secured by motion, and certainly aided by a considerable vacuolation, as shown in Fig. 6, drawn at the expiration of ten hours; and at the end of a little less than twelve hours, the normal parent form was taken, as seen in Fig. 7, which in about forty minutes after had begun to divide by fission; one that was watched from the beginning is drawn at Fig. 8, when in the middle of this process.

Thus the entire life cycle of this form is seen. When mature, it multiplies by fission for a period extending over from two to eight days. It then becomes peculiarly amœboid; two individuals coalesce, slowly increase in size, and become a tightly-distended cyst. The cyst bursts, and incalculable hosts of immeasurably small sporules are poured out, as if in a viscid fluid, and densely packed; these are scattered, slowly enlarge, acquire flagella, become active, attain rapidly the parent form, and once more increase by fission.

We were now desirous of ascertaining to what extent the mature form and the sporule were pervious to the action of heat. There was considerable difficulty in this, since it was necessary to know that the sporules *were* in the heated drop, and this could only be positively asserted of that which was in the field of the instrument. An ordinary slide, containing adult forms and sporules covered in the ordinary way, was allowed to evaporate slowly, in seven instances, and placed in a dry heat which was raised to 121° C. It was then slowly cooled, and distilled water allowed to insert itself by capillary attraction. On examination all the adult forms were absolutely destroyed, and no spore could be definitely identified. But after being kept moist in the growing stage for some hours and watched with the $\frac{1}{50}$, gelatinous points were seen in two out of the seven cases, which were recognized as exactly like an early stage of the developing sporule, which were watched until they had reached the small flagellate state shown in Fig. 5, Plate XXVI. The remaining five were barren of result.

In boiling, the difficulties were still greater, from the uncertainty beforehand of the presence of sporules in a sufficiently large drop of the infusion casually taken. After, however, a considerable number of experiments, we find that a temperature of 66° C., given to the infusion, destroys all adult forms; but we have found *young monads appear and develop* in an infusion which has been raised to 127° C.; suggesting that the sporule is uninjured in a temperature considerably above that which is wholly destructive of the adult.

We are aware of the valuable generalization of Cohn and Horwath, founded upon accurate experiments, that all living things are destroyed at a temperature of 62° C. when equal diffusion of heat is secured, and so far at least as adult forms go, we are prepared, as a general rule, to accept it. We question its application to sporules, where they exist; and nothing but patient inquiry and experiment on these, as such, can definitely settle the question. But the unequal diffusion of heat from causes known and unknown, must always in individual experiments cause variety of results. Cohn himself confirms the fact that Bacteria are not always killed by *boiling* in flasks, and that *Bacillus subtilis* (the lactic acid ferment organism) survives the boiling of the solution in which it is contained; and that in every case the boiling should be continued for an hour.

We think that the above experiments justify us in questioning the statement of a recent partisan of Abiogenesis, "that even if Bacteria do multiply by means of invisible gemmules, as well as by the known process of fission, such invisible particles possess no higher power of resisting the destructive influence of heat than the parent Bacteria themselves possess."* This *may* be true of Bacteria, but it certainly remains to be proved; while its inapplicability to *all* sporules is apparent.

## II.—*The Angular Aperture of Objectives.*

By Robert B. Tolles, U.S.A.

Any objective when adjusted to the maximum point and measured in air, having less than 180° angular aperture, such objective, being applied in the microscope to observation of an object in the ordinary balsam slide, necessarily has less than 82° of aperture practically for that balsamed object. Now, avowedly English objectives, and presumptively *all* of single-lens fronts of common crown glass, are of rather less than 180° air angle. Therefore it is evident that I did not "challenge" test of English objectives as against any other whatever built on that plan. There was nothing invidious in my remark, and fairly considered I think not any such appearance. It was cautionary merely. Thus (as stated in my note, which happened to follow Mr. Wenham's in your last issue) I obtained a view with definition of a fine object unquestionably with the advantage of a pencil of above 100° of actual practical balsam angle, *measured with the object in focus at the moment.*

Well, measured in air, this objective would show not distinguishably more than any English (or other) objective of the ordinary construction corrected for air angle approximating 180°. In Mr. Wenham's tank in balsam a proper difference would be shown

* Bastian, 'Proc. Roy. Soc.,' March, 1873.

between the common and the "peculiar" kinds, *i. e.* ref. index of the "balsam" used being the same as "glass,"—the one of usual construction would show less (probably) than 82°, while the one I alluded to in the note which Mr. Wenham comments upon would give over 100°,—I got 110°.

The latter being transferred to the microscope (no change of adjustment) *would practically have no more* than closely the same angle as the first, for with both the outside *plane surface* of the slide limits the balsam pencil to angular cone resultant from an air angle of 180°, and which cone is less than 82° *in* the glass and in the balsam.

All this is "piper's news," of course, but it seems a story necessary to be told again. But to proceed beyond this—with the semi-cylindrical, or some equivalent "thing," we have that angle *ultra* of anything derivable from 180° in air, and which is utilized by this objective (the one reported of in my last, 'Monthly Microscopical Journal' for June, current), and not touched with the other sort.

What I utter is a matter of knowledge, not speculation. I have seen well with that additional 20° of pencil, though as yet by chance not verified by others' eyes. All the 80° and more being shut out, the *outside* portion illuminated the object, and the objective with that ultra portion of the cone (and limited to it) gave a good image of the object. *But the cylindrical appliance was necessary* to show that the *part beyond* 80° was used.

One word more, to show there is no room to question. When used as a means of measure of balsam angle of the objective, the light being *put down the microscope body*, the cylindrical surface is *dulled* to act as a screen on which the angle-limits are marked, and with *no deviation*, all the while, mind you, the *object being in focus*. Mr. Wenham's explanation does not apply. It seems to me this is one of those "true facts" he is bound to recognize.

I do not care to impeach the Wenham tank. The objectives I tried in that tank *all* showed *more* angle in "balsam" than was shown by the "wretched adaptation" that vexes so Mr. Wenham. Thus, the $\frac{1}{5}$ of the tank list ('Monthly Microscopical Journal,' No. XLV., p. 106), the tank showed it to have 110°, while the so-smartly tabooed "cylindric affair" gave it 107° instead! My cause was helped most by the showing of the tank.

The tank will give perhaps correct exhibit of angle for any liquid, but to test the question at issue here the *balsam* must have that refractive index involving interior total reflexion at incidence of 40° and a fraction, or, in other words, like "glass," as repeatedly laid down by Mr. Wenham—not much turpentine, neither must it be a resin. Such is the difficulty with the tank. Your balsam as to index will not agree with case as stated.

Now, the semi-cylinder of common crown glass is just the thing to produce that case, and *admit of no other.* The plane surface of the semi-cylinder controls. If the balsam is like the glass as to index no deviation takes place between them. If thin with *turpentine,* considerable deviation, but the reading on the cylindrical surface the *same.* If water, the angle will appear the same. This is according to actual trials when the apparatus was first mounted.

Therefore, and also, the *semi-cylinder* would correctly represent the balsam angle whatever the medium between objective front and cover—only if (taking the case of the $\frac{1}{5}$-inch objective named herein) the medium be so rare as to give to glass an angle of interior total reflexion less than the objective's angle in balsam. Thus, with air between objective front and cover, only 80°, or closely to that, was obtained, although in balsam tank the objective showed 110°.

I may as well add, that if anyone in England thinks proper to construct an objective *to gain* this angle in balsam, of course my remark as to comparative limit of angle would not apply. *I have pointed out how* in the case of the four-system objective ('Monthly Microscopical Journal,' March, 1872).

The 110° $\frac{1}{5}$ of the tank list is such an objective ('Monthly Microscopical Journal,' No. XLV., p. 106), but it was not built for the maximum in balsam by a long way. The same objective without change of construction is the identical one-fifth tested in Washington by Dr. J. J. Woodward and associated gentlemen, and declared to have "over 100° of balsam angle."

The means of making an objective of *three* systems to have as much angle, I have suggested at least in the same paper, and since produced, as stated in my note in your last issue.

In conclusion, I have to remark, that as all my measurements of angle given in the tank list, and since, have been always made with adjustments for cover, giving *good definition,* therefore, Mr. Wenham has not yet answered.

In 'Monthly Microscopical Journal,' No. LI., p. 123, Mr. Wenham says,—" Mr. Tolles, in admitting that he closes the lenses within the position of proper definition, gives us the key to his fallacy." This "admitting" I pointedly deny, and call on Mr. Wenham to point out *where he* finds such admission.

BOSTON, *June 20th,* 1873.

## III.—*Remarks on the Confirmation given by Dr. Colonel Woodward to the "Colour Test."*

By Dr. Royston-Pigott, F.R.S., &c., &c.

I had the honour, on May 31, 1869, of transmitting to the Council of the Royal Microscopical Society a paper printed seven months afterwards, December, 1869, which contained the following statement:—

"I cannot here too strongly call attention to the beautiful phenomena which I have always endeavoured to obtain as a fine and reliable test of approaching aplanatism (freedom from spherical aberration) and heralding a fine definition.

"In examining striated bodies—

"Longitudinal bands glisten with a ruby tint upon a green or yellowish ground;" and describing the beads on the Podura scale, I stated, p. 300—

"The upper beads are best seen either green upon a pink ground or pink upon a greenish ground," and p. 302—

"The most difficult definition is that of the substratum of beads glimmering through the membrane nearest the light: on the other side they are generally of a very bright yellow green colour, contrasting prettily with the deep ruby colour of the upper beads." —1869.

It is gratifying to find the truth of this observation verified by so distinguished and accurate a gentleman as Dr. Woodward, in his communication for the November number of the 'Monthly Microscopical Journal,' 1871, in which he alludes to the colour testing in very decisive language.

He says in substance, that purchasers of objectives, in general, demand approximate achromatism above all else; that in order to obtain it, "the corrections for spherical aberration are inevitably sacrificed;" that in obtaining on a white screen by sunlight a picture of *Pleurosigma Formosum*, it was found quite impossible to get a distinct view of the beads unless the colour corrections were such that they appeared, "brilliant red on a greenish ground, and that when the object-glass is more nearly achromatic, not merely photographs are unsatisfactory, but with white light it will be found impossible to separate the beads distinctly on the screen." He then mentions a variety of excellent glasses by different makers, which all resolved the 19th band of Nobert, but all of these had the same colour corrections.

The announcement which I had the boldness to make so early as May, 1869, that in "the best glasses there is a certain residuary aberration," raised a storm of opposition hardly yet subsided. I afterwards described this in the 'Philosophical Transactions'* as a nebulous yellow fog, which no objective adjustments are able to dissipate, p. 592; and from experiments therein detailed the residuary lateral aberration was calculated to be $\frac{1}{52000}$ of an inch. The details sent us by Colonel Woodward now seem amply to justify the following passage of my first paper, p. 304:—

"I know it is very difficult to throw aside the creed and belief of forty years, and I have hesitated a long time (seven years) to bring forward my views, being perfectly convinced that a battle of the glasses will have to be fought.

"I point to the immersion lens as an irrefragable proof of the deficiencies of the corrections of the old-fashioned glasses to grapple with some of the exquisite difficulties of microscopic research, and if my efforts shall in any way advance the excellence of defining power, especially in the higher range of investigations, I shall feel in the end amply rewarded; my work has been earnest and sincere."

The passage, however, which I wish particularly to call attention to, now that the principle is better acknowledged and understood, is in the April number, 'Monthly Microscopical Journal,' 1870:—

"I have lately seen the Formosum beading coloured with red, orange, yellow and blue. The beading has appeared wreathed with a golden bronzing; individual beads separated from their fellows appeared remarkably distinct.

"Destruction of colour reduces the field to a spiritless picture. (Dr. Woodward now says they cannot be seen at all on the whole screen he employs unless they are made to appear red on a greenish ground.)

"Now, if we view diatoms *en masse* with the unaided sight, they appear resplendent with a variety of prismatic hues when under a strong light.

"Yet these colours, so evident to the unassisted eye, *en masse* vanish in *achromatic vision:* should these charming colours," I then asked, "be destroyed"?

In a paper communicated last June to the 'Quarterly Journal of Microscopical Science,' I made the following deductions from the observations there related:—

. . . On referring to a diagram, I said the great bulk of the rays, except the red or reddish yellow, converge accurately to B, whilst the residuary rays of reddish orange converge to R, so that we either in general get achromatism with residuary spherical aberration, or reddish yellow rays when the spherical aberration is destroyed.

*Spherical aberration, as detected by the methods I have recommended, displays itself in a colourless milkiness or whitish smoke-*

* Vol. ii., 1870.

*like cloudiness. Dissipate this by better spherical corrections, and it is immediately replaced by all brilliant points becoming irradiated with the orange-red halo.*

In the same paper (July, 1872) I further stated—

"If the orange-red rays be absorbed by the ammonio-sulphate of copper solution transmitting monochromatic light, definition actinically is much more easily produced. But the eye, receiving ordinary compound light, whilst the aberration is neglected to the advantage of the achromatism, cannot possibly see as well, as the actinic monochromatic rays potentially define and depict. A point may be ascertained in the axis where the aberration may almost be extinguished when it is no longer confused with uncorrected red and yellow rays. In other words, the blue rays may be brought accurately to the same mathematical point in the axis, whilst all other (coloured) rays would vary more or less. In the finest objectives now made I find when the spherical aberration is corrected there is a strong secondary spectrum, chiefly consisting of a mixture of the red and yellow rays. But when this is corrected, by using all possible precautions, so as to render the achromatism almost absolutely perfect, *then the aberration re-appears.* In point of fact, the focal point where the spherical aberration vanishes, does not correspond, or is not identical with, the focal point when the coloured rays are blended into white light. But as all makers (and indeed everyone else) more readily detect imperfect achromatism than residuary aberration, the latter is sacrificed to the former. All the glasses with which I am acquainted err in this respect, or aberrate," p. 266.—'Quarterly Journal of Microscopical Science,' July, 1872.

Dr. Woodward expresses himself forcibly in the same direction, p. 299, No. xxvii., 1872:—

"I have observed that those glasses which were quite under-corrected for colour, not merely gain the best photographs, but did the best work by lamplight." . . . "Now, in view of the irrationality of dispersion, absolute achromatism is impossible, and *in aiming* to approach it as closely as may be, the corrections for spherical aberration are inevitably sacrificed. It appears important that this fact should be more generally known." He then proceeds to describe a simple test. It is gratifying to find my views expressed in July so fully confirmed in November by our American savant.

There is one point upon which I desire to make an observation. Dr. Woodward says, in quoting a passage from my paper of February, 1872, "It seems probable to me, therefore, that the distinguished makers last named have made no substantial progress since 1869; and this view is confirmed by Dr. Royston-Pigott, who mentions (p. 66) that Powell and Lealand had placed at his disposal 'for a fortnight,' a $\frac{1}{16}$th, 'similar, though perhaps slightly superior, to the celebrated immersion $\frac{1}{16}$th, signalized by Dr. Woodward,' but which they were 'unwilling to dispose of on account of its excellence.'"

I think it is only fair to the reputation of Messrs. Powell and Lealand to state that I assigned no reason for this unwillingness, and in justice to them I may be permitted to quote the whole passage, thus (page 67):—

"In face of these observations with the best glasses, I presume, in existence (Powell and Lealand's own $\frac{1}{16}$th, *which I think* they are unwilling* to dispose of, and their new $\frac{1}{50}$th immersion, made expressly for the writer), I may make bold to predict that a double set of beading will also be observed on the Angulatum, Rhomboides, and other difficult diatoms."

It will be interesting to find that Colonel Woodward succeeds in photographing the double sets of beading described in the February number, with the admirable glass now in his possession, made by Mr. Tolles. The Formosum will assuredly yield its secrets to so excellent and artistic an instrument.

I have very little doubt that if anyone be willing to offer Messrs. Powell and Lealand double the price of their $\frac{1}{16}$th—the same as charged for Tolles' immersion $\frac{1}{18}$th, by Mr. Stodder, $175, or 34*l.* sterling—they would be able to produce a glass proportionately improved in some of the minor details. It would truly be unphilosophical to believe that the best glass is absolutely so perfect as to admit of no further improvement.

Dr. Woodward's known courtesy towards those who happen to differ from him, will, I am sure, excuse my pointing out to him this passage of his as conveying more than I intended.

One remarkable fact was noticed by me on the 7th November last: that the beading on the *Degeeria domestica* Podura was perfectly distinct moistened with water, though usually difficult of observation in this condition.

The colours of the beading on several *wetted* insect scales were of a fine sapphire, blue, and red, when most distinctly defined; thus again verifying the importance of the "colour test."

Notwithstanding the bold denials by persons insufficiently informed as to the existence of errors in our best glasses of the old fashion, it is gratifying to observe that strenuous efforts are now made to improve them by new constructions. The capabilities of Titanic optical glass, specially manufactured, encourage the hope that not only the secondary spectrum will vanish, but also the spherical residuary aberration.

* I was informed that as it was a glass made by Mr. Powell, junior, he wished to keep it for his own use. But it did not seem relevant to make known this reason.

### *On Using the Colour Test to determine Focal Depth, and the Nature of certain Structures.*

The following observation illustrates the value and importance of the colour test in examining minute structures.

The object employed was a minute triangular fragment of *P. Formosum,* mounted in Canada balsam, exhibiting nine beads on one side and six on the other, the longer side being formed of a portion of the mid-rib. The focus was gradually, but very delicately deepened. Appearances—

$F_1$. First focus. All the boundary beads appeared blue and in close contact. An inner row in contact, but pale and indistinct.

$F_2$. Second Focus. Outlying beads reddish, in close contact. Inner row touching them, blue.

$F_3$. Third Focus. Outer beads indistinct. Inner red.

$F_4$. Fourth Focus. Boundary beads vanished. Inner beads dark red.

The planes of these several foci corresponded to focal changes of about 1–60,000th of an inch; so that the focus deepened each time by about half the depth of the vertical diameter of a bead.

*Conclusions.* 1. Never having had before the good fortune to discriminate so clearly the double set of beading of the Formosum formerly described by me, as in this broken fragment, I am now enabled to declare, that this application of "the Colour Test" determined the fact that the two sets of beading occupy different planes, *i. e.* the outer boundary beads were elevated above the inner rows.

2. That instead of the Formosum being composed of beading usually reckoned at about 33,000 to the inch, the beads are set much more thickly (cannon ball-wise), one layer of beads being piled upon the other layer, so as to admit the beads of the lower layer to appear between those of the upper. By means of Browning's delicate Recording Micrometer, which under the power used gave 924 divisions for a thousandth of an inch on the stage, I ascertained the diameter of the beads in contact was about the one fifty thousandth of an inch. A good idea of the structure of the Formosum would be obtained by examining a layer of shot loosely spread out, but confined by a frame upon which another layer had been superimposed.

Having constructed a measuring machine which was so delicate as to admit of changing the central spot of Newton's Rings through the nine orders of colours, in which I counted altogether twenty-three changes, under the microscope I hope to be able to survey with great accuracy the position of the molecules of structure, assisted by the colour test here described, and so determine more accurately the depth of the different layers.

I announced the intermediate beading of diatoms in May, 1869, to which I here more particularly wish to draw attention, as they exist in different planes of focal vision. I wish to add, that the minute beading of the mid-rib described by me, did not appear more than about one hundred thousandth of an inch in diameter under a power of 1000; one of Browning's micrometer divisions then corresponds to one millionth upon the stage.*

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## IV.—*Remarks on Mr. Carruthers' Views of Prototaxites.*

By J. W. Dawson, LL.D., F.R.S.

In the 'Monthly Microscopical Journal' for October, 1872, Mr. Carruthers, of the British Museum, has published a paper in which he endeavours to show that my *Prototaxites Logani* from the Devonian of Gaspé is a gigantic sea-weed, for which he proposes the generic name *Nematophycus.* Though I saw this article some time ago, other avocations have prevented me from attending to it until now.

The tone and manner of the article, I may say in passing, are unnecessarily offensive; and the author bolsters up his argument by unfair assumptions that I am ignorant of some of the most familiar facts of structural botany, facts which were well known to me while he was yet a school-boy, and which are stated or implied in many of my papers on fossil plants. Possibly, however, Mr. Carruthers is already aware of his bad taste in this matter, and it will be to me a sufficiently ungracious task to expose, as I must do in the interest of truth, the worthlessness of the explanation which he offers of the nature of Prototaxites. I shall reply to his objections under the following heads:—(1.) The mode of occurrence of Prototaxites. (2.) Its microscopic structure. (3.) Its probable affinities.

* With regard to these minute quantities, and to remove doubts which may arise in some persons' minds as to the possibility of seeing such very minute linear quantities, I may say that a minute of arc corresponds to the breadth of the 344th part of an inch as seen at ten inches, which is at least four times as thick as a human hair at that distance. Now the one hundred thousandth of an inch under a power of 1000 is precisely the same thing as a thousandth of an inch under a power of *one*, or seen naturally at ten inches. But we can see hairs much finer than this—say three times—therefore, with regard to arc, we can see with a power of 1000 the $\frac{1}{3}$ of $\frac{1}{100000}$ *i.e.* with a power of 3000 about the millionth. To find the angle in seconds $1'' = 0 \cdot 000004848 = \frac{5}{1000000}$th, nearly.

$$\left.\begin{array}{r}\text{The angle under a power of 3000,}\\ \text{at a distance of ten inches, is for}\\ \text{a millionth of an inch} \quad .. \quad ..\end{array}\right\} = \frac{3000}{10 \times 1000000} = \frac{3}{10000}$$

Divide this by the value of one second and we get six seconds in the angle subtended by $\frac{1}{1000000}$th under a power of 3000.

1. *Mode of Occurrence.*—This alone should suffice to convince any practical palæontologist that the plant cannot be a sea-weed. Its large dimensions, one specimen found at Gaspé Bay being three feet in diameter; its sending forth strong lateral branches, and gnarled roots; its occurrence with land plants in beds where there are no marine organisms, and which must have been deposited in water too shallow to render possible the existence of the large oceanic Algæ to which Mr. Carruthers likens the plant. These are all conditions requiring us to suppose that the plant grew on the land. Further, the trunks are preserved in sandstone, retaining their rotundity of form, even when prostrate; and are thoroughly penetrated with silica except the thin coaly bark. Not only are Algæ incapable of occurring in this way, but even the less dense and durable land plants, as Sigillariæ and Lepidodendra are never found thus preserved. Only the extremely durable trunks of coniferous trees are capable of preservation under such circumstances. In the very beds in which these trees occur, *Lepidodendra*, tree-ferns and *Psilophyton* are flattened into mere coaly films. This absolutely proves, to anyone having experience in the mode of occurrence of fossil plants, that here we have to deal with a strong and durable woody plant.

These considerations were dwelt on in my published descriptions of Prototaxites, but they naturally have more weight in my judgment than in that of Mr. Carruthers. Geologists and palæologists at least will be able to appreciate them.

2. *Microscopic Structure.*—It would be tedious to go into the numerous scarcely relevant points which Mr. Carruthers raises on this subject. I may say in general that his errors arise from neglect to observe that he has to deal not with a recent but a fossil wood, that this wood belongs to a time when very generalized and humble types of gymnosperms existed, that the affinities of the plant are to be sought with Taxineæ, and especially with fossil Taxineæ, rather than with ordinary pines.

Mr. C., after describing Prototaxites according to his own views of its structure, expresses the opinion that "the merest tyro in histological botany" may see that the plant could not be phænogamous. But if the said tyro will take the trouble to refer to the beautiful memoir on the Devonian of Thuringia, by Richter and Unger,* and to study the figures and descriptions of *Aporoxylon primigenium*,† *Stigmaria annularis*, *Calamopteris debilis*, and *Calamosyrinx Devonicus*, he will find that there are Devonian plants referred by these eminent palæontologists to gymnosperms and higher Cryptogams, which fall as far short of Mr. Carruthers' standard as Proto-

* Trans., Vienna Academy, 1856.

† I have elsewhere compared Aporoxylon with Prototaxites, 'Journal Geol. Soc.,' 1862, p. 306. Report on Devonian plants.

taxites itself. Nothing can be more fallacious in fossil botany than comparisons which overlook the structures of those primitive palæozoic trees which in so many interesting ways connect our modern gymnosperms with the cryptogams.

It is scarcely necessary to reply to such a statement as that the fibres of Prototaxites have no visible terminations. They are very long, no doubt, and both in this and their lax coherence they conform to the type of the yews. In Mesozoic specimens of *Taxoxylon* which I have now before me, the fibres are nearly as loosely attached and as round in cross section as in Prototaxites. In these, as in Prototaxites, water-soakage has contributed to make the naturally lax and tough yew-structure less compact, and to produce that appearance of thickness of the walls of the fibres which is so common in fossil woods.

Disks or bordered pores in Prototaxites I did not insist on, the appearance being somewhat obscure; but Mr. Carruthers need not taunt me with affirming the existence of such pores in the walls of cells not in contact. Pores, if not bordered pores, may exist on such cells, and the wood-cells of Prototaxites are in contact in many places, as may easily be seen; and even where they appear separate, this separation may be an effect of partial decay of the tissues.

Mr. Carruthers converts the spiral fibres lining the cells of Prototaxites into tubes connecting the cells. This is a question of fact and vision, and I can only say that to me they appear to be solid, highly refracting fibres; and under high powers, precisely similar to those of fossil specimens of Taxoxylon from British Columbia, and to those seen in charred slices of modern yews. I may further say that Mr. Carruthers' figure (Plate XXXII.) is in my judgment to a great extent imaginary.

But what of the arrangement of these fibres. It is true that, as I have stated, they appear in some cases to pass from cell to cell, and I hesitated to account for this appearance. Mr. C. might, however, have spared himself the remark that "if Dr. Dawson knew anything whatever about a vegetable cell, and the formation of the spiral fibre in its interior, he would not have written such nonsense"—(a specimen, by the way, of the amenities of British Museum Science, as represented by Mr. C.). The possibilities of such an appearance, as yet, perhaps, unknown in the plant-rooms of the Museum, result from the following considerations: (1.) In more or less crushed fossil plants, it is not unusual to see what are really internal structures appearing to pass beyond the limits of the cell-wall, from the mere overlapping of cells. I have good examples in the Mesozoic Taxoxylon already mentioned. (2.) In fossil woods the original cell-wall is often entirely destroyed, and only the ligneous lining remains, perhaps thickened by incrustation of mineral matter within. In this case the original lining of the cell may seem to be an external structure. I have examples both in Mesozoic conifers and in carboniferous plants. Long soaking in water and decay have thus often made what may have been the lining of wood-cells appear as an intercellular matter, or an external thickening of the walls. (3.) In decayed woods the mycelium of fungi often wanders through the tissues in a manner very perplexing; and I suspect, though I cannot be certain of this, that some fossil woods have been disorganized in this way. At the time when my description was published, I felt uncertain to which of these causes to attribute the peculiar appearance of Prototaxites. I have now, from subsequent study of the cretaceous Taxineæ of British Columbia,* little hesitation in adopting the first and second explanations, or one of them, as probable.

Mr. Carruthers does not believe in the medullary rays of Prototaxites. The evidence of these is the occurrence of regular lenticular spaces in the tangential section, which appear as radiating lines in the transverse section. The tissues have perished; but some tissues must have occupied these spaces; and in fossil woods the medullary rays have often been removed by decay, as one sometimes sees to be the case with modern woods in a partially decayed state. Mr. Carruthers should have been more cautious in this matter, after his rash denial, on similar grounds, of medullary rays in Sigillaria and Stigmaria, contrary to the testimony of Brongniart, Goeppert, and the writer, and the recent exposure of his error by Professor Williamson. That the wood-cells have been in part crushed into the spaces left by the medullary rays is only a natural consequence of decay. The fact that the medullary rays have decayed, leaving the wood so well preserved, is a strong evidence for the durability of the latter. The approval with which Mr. C. quotes from Mr. Archer, of Dublin, the naïve statement that "the appearance of medullary rays was probably produced by accidental cracks or fissures," would almost seem to imply that neither gentleman is aware that radiating fissures in decaying exogenous woods are a consequence of the existence of medullary rays.

Perhaps the grossest of all Mr. Carruthers' histological errors is his affirming that some of my specimens of Prototaxites show merely cellular structures, or are, as he says, "made up of spherical cells." Now, I affirm that in all my specimens the distinct fibrous structure of Prototaxites occurs, but that in parts of the larger trunks, as is usual with fossil woods, it has been replaced by concretionary structure, or by that pseudo-cellular structure which proceeds from the formation of granular crystals of silica in the midst of the tissues. Incredible though it may appear, I know it to be a fact, as all the

* Report of Geol. Survey of Canada, now in course of publication. The collections contain wood showing the structure of yew, cypress, oak, birch, and poplar, all from rocks of cretaceous age.

specimens I gave to Mr. Carruthers had been sliced and studied by myself, that it is this crystalline structure which the botanist of the British Museum mistakes for vegetable cells.* I think it right to state here that I not only gave Mr. C. specimens in these different states of preservation, but that I explained to him their nature and origin.

It is unnecessary to follow further the histological part of the question, as my object is not so much to expose the errors of Mr. Carruthers as to illustrate the true structure of Prototaxites.

3. *Affinities.*—In discussing these I must repeat that we must bear in mind with what we have to deal. It is not a modern plant, but a contemporary of that "prototype of gymnosperms" *Aporoxylon*, and similar plants of the Devonian. Further, the comparison should be not with exogens in general, or conifers in general, but with Taxineæ, and especially with the more ancient types of these. Still further, it must be made with such wood partly altered by water-soakage and decay and fossilized. These necessary preliminaries to the question appear to have been altogether overlooked by Mr. Carruthers.

My original determination of the probable affinities of Prototaxites, as a very elementary type of taxine-tree, was based on the habit of growth of the plant—its fibrous structure, its spirally-lined fibres, its medullary rays, its rings of growth, and its coaly bark, along with the durable character of its wood, and its mode of occurrence; and I made reference for comparison to other Devonian woods and to fossil taxine-trees.

Mr. Carruthers prefers to compare the plant as to *structure* with certain chlorospermous Algæ, and as to *size* with certain gigantic Melanosperms, not pretended to show similar structure. This is obviously a not very scientific way of establishing affinities. But let us take his grounds separately. He selects the little jointed calcareous sea-weed *Halimeda opuntia* as an allied structure, and copies from Kutzing a scarcely accurate figure of the tissue of the plant as seen after removal of its calcareous matter.† He further gives a defective description of this structure; whether taken from his own observation or from Kutzing, he does not say. Harvey's description, which I verified several years ago, in an extensive series of examinations of these calcareous Algæ, undertaken in consequence of a suggestion that Eozoön might have been an organism of this nature, is as follows:—"After the calcareous matter of the frond has been removed by acid, a spongy vegetable structure remains made

* In fossil-woods, the carbonaceous matter, being reduced to a pulpy mass, sometimes partly becomes moulded on the surfaces of hexagonal or granular crystals, in such a manner as to deceive, very readily, an observer not aware of this circumstance.

† A more characteristic figure is given in Harvey's 'North American Algæ.'

up of a plexus of slender longitudinal unicellular filaments constricted at intervals, and at the constrictions emitting a pair of opposite decompound, dichotomous, corymboso-fastigiate horizontal ramelli, whose apices cohere and form a thin epidermal or peripheric stratum of cells." It will be seen at once that this structure has no resemblance whatever to anything existing in Prototaxites, even as interpreted by Mr. C., and without taking into the account the fact that *Halimeda opuntia* is a small calcareous sea-weed, divided into flat reniform articulations, to which this structure is obviously suited, as it would be equally obviously unsuited to the requirements of a thick cylindrical trunk, not coated with calcareous matter.

In point of size, on the other hand, Mr. Carruthers adduces the great *Lessonia* of the Antarctic seas, whose structure, however, is not pretended to resemble that of Prototaxites except in the vague statement of a pseudo-exogenous growth. Lessonia I have not examined, but the horny *Laminariæ* of our North American seas have no resemblance in structure to Prototaxites.

Nothing further, I think, need be said in reply to Mr. Carruthers' objections; and *Nematophycus* may be allowed to take its place along with a multitude of obsolete fucoids which strew the path of palæontology. As to Prototaxites, it is confessedly an obscure and mysterious form, whose affinities are to be discussed with caution, and with a due consideration of its venerable age and state of preservation, and probably great divergence from any of our modern plants; and it is to be hoped that ere long other parts than its trunk may be discovered to throw light on its nature. Until that takes place, the above remarks will be sufficient to define my position in regard to it; and I shall decline any further controversy on the subject until the progress of discovery reveals the foliage or the fruit of this ancient tree, belonging to a type which I believe passed away before even the Carboniferous flora came into existence.

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## V.—*On Ancient Water-fleas of the Ostracodous and Phyllopodous Tribes (Bivalved Entomostraca).*

By Professor T. Rupert Jones, F.R.S., F.G.S.

(*Continued from p.* 193, *vol. iv.*)

### *Part II.*—Cypridinadæ.

In the seas, chiefly of warm climates, numerous Ostracods are found which possess a subglobular or subcylindrical bivalved carapace, notched in its antero-ventral region, to allow of the play of the extruded lower antennæ, as locomotive organs, with a lateral movement. The notch varies considerably in size and shape in different genera. It makes a mere crescentic slit or triangular opening in the two united valves of some; but, when the valves gape in front, it makes a cruciform opening; and if the notch be strongly developed, the antero-dorsal portion of the carapace projects as a beak, forming more or less of a hood. The muscle-spot on each valve, or place of attachment of the great transverse muscle, is distinctly marked with a patch of small lucid spots, sometimes having a radiate arrangement.

Milne-Edwards, Baird, Dana, Costa, Sars, Grube, Brady, and others, have treated of these forms with their soft parts; and for English readers, Dr. Baird's description and figures in the Zoological Society's Proceedings, and G. S. Brady's illustrated memoirs in the Zoological Society's Transactions, vol. v., and the Linnean Society's Transactions, vol. xxvi., and his subsequent papers in the Zoological Society's Proceedings, 1871, &c., will supply useful particulars.

In the fossil state there are abundant evidences of the former existence of Cypridiniform Ostracods, chiefly in the Palæozoic rocks, especially the Carboniferous Limestone and the Coal-measures. As indicated in my "Monograph of the Tertiary Entomostraca of England" (Palæontographical Society), 1856, pp. 2 and 9, the name *Cypridina* had been misapplied by palæontologists to fossil *Cytheræ*, in some cases, and not given to veritable members of the genus, on account of the characteristic notch not having been represented by the engraver in the figure of M. Milne-Edwards' *Cypridina Reynaudii.**

There are fossil carapace-valves so nearly corresponding with those of a living *Cypridina*, that, as far as the valves can guide us, there are no characteristics whereby to judge between one species and another, except those of general shape, form of notch, amount of overlap, pattern of muscle-spot, superficial ornament, and relative thickness.

Among the recent *Cypridinadæ* themselves the limbs and other soft parts supply the main data for specific valuation. Perhaps *Bradycinetus* alone is characterized by speciality of carapace, the others having valve-characters of variable and mutual modification. Indeed, it is difficult to allocate to the more definitely studied genera of existing monographists all the so-called "Cypridinæ" of earlier authors, for want of exact information as to the soft parts of the respective animals, male and female. *Polycope* and *Cytherella*, which are not "Cypridinads," but belong to two allied families, possess recognizable carapaces.

As at present known, the recent *Cypridinadæ* comprise—1. *Cypridina*, M.-Edwards; 2. *Asterope*, Philippi; 3. *Philomedes*,

* 'Hist. Nat. des Crustacés,' vol. iii., p. 407, Plate XXXVI., Figs. 5–9; and 'Hist. Nat. Anim. sans Vertèb.,' ed. 2, vol. v., p. 178.

Liljeborg; 4. *Bradycinetus*, Sars; 5. *Eurypylus*, Brady. The nearly related *Conchœcia*, Dana, and *Halocypris*, Dana, constitute the *Conchœciadæ*. *Heterodesmus* is a distinct form allied to the *Cypridinadæ*. *Polycope* is the type of a different family; and *Cytherella* is the type of another.

In the fossil state the valves alone remain for our examination; and however similar they may appear to those of this or that genus, doubt must always be entertained as to the relationship of the animal to existing forms; for it may have exhibited a very different construction of other parts of the frame. Yet the fossil forms must be placed in some kind of category; and in preference to a purely artificial arrangement of all the fossil forms of *Cypridinadæ* and their immediate allies under such a provisional genus as "Cypridinopsis," I venture to express such evidence of their relationship to existing forms as is recognizable, by placing them in the existing genera, or under genera supposed to be in alliance with them, as already planned by De Koninck and others.

Among the fossil, subglobose, ovate-oblong, anteriorly notched, bivalved Entomostraca, we find some with oval outline, distinctly notched, at the middle of the front end, by a sinus, with a projecting or hooked peak. Although the valves are thicker than those of the existing true *Cypridinæ*, and though the lost soft parts probably differed somewhat, these forms are placed under that genus for the sake of convenience, thus serving palæontological purposes and avoiding multiplication of terms. As an example of this group, *Cypridina Phillipsiana* is figured in Pl. LXI., Fig. 8, vol. iv., with its long shallow sinus and small beak, and its large radiate muscle-spot. We know of twelve other species from the Carboniferous strata of the British Islands, including *Cypridina primæva* (*Daphnia*, *M'C.*), which closely approximates in shape to the existing *C. norvegica* and others of a nearly oval outline. *C. radiata*, from the Scotch and English Coal-measures, also oval in profile, has peculiar star-like vascular patches in its valve-structure. Its real outer surface has a small but coarse blebby reticulation; the convex tops of these bladder-like meshes rub off, and leave irregular hexagonal raised lines. This surface flakes off, and exposes the radiate inner structure of the shell.

Another group of allies are also notched and beaked in front, but are subovate in profile and acuminate behind; moreover, the lower part of the front margin has a tendency to be exaggerated, or produced like the prow of an ancient trireme, or a modern armour-clad "Ram" or "Monitor." In the oblong *Cypridinæ* above mentioned this antero-ventral margin was liable to decrease, so that in *C. brevimentum*, common in the Mountain Limestone, the strong beak stands out from a chinless front. And in another group the chin is altogether wanting, and the antero-dorsal angle projects as an important feature in the rhomboidal outline of the valves (*Rhombina*), which are rare in the same rock. The "Monitor" group is named *Cypridinella*, with seven species; its most symmetrical and ovate form is *C. Cummingii;* some showing the extreme of its prow-like feature are *C. monitor* and *C. vomer*. All occur in the Carboniferous Limestone of Europe and the British Isles.

In the next group, *Cypridellina*, we have the form of *Cypridinella* (for which, by-the-by, we have no near recent representative) with a superadded feature, namely, a subcentral tubercle, or swelling at or near the centre of each valve. There are eight species, with several varieties all like the foregoing, from the Mountain Limestone. Some few of these closely imitate *Cypridinella*, others go off in divergence of shape, especially in the prow, which inclines to be vertical.

When, in addition to the tubercle, a nuchal furrow is present, we see the *Cypridella* of De Koninck (revised): for among the associated fossil forms there are several very closely related to *Cypridina* in general characters, but differing from it in having the faces of the valves raised up in one or more tubercles, and in being impressed near the middle of the dorsal region by a short, vertical, and often curved sulcus, generally immediately behind the chief and most persistent tubercle.* The tubercles may be three or four in number, giving the valves an irregularly quadrate shape. Usually there is only one tubercle, at or about the centre of each valve, and even that may be almost obsolete; and so also the furrow is sometimes so faint as scarcely to be recognized. The notch and peak are usually large and distinct. These forms, which are exceedingly variable, lie under *Cypridella*, a name given by De Koninck to one of the most marked of them (*C. cruciata*, not yet found out of Belgium). *C. Edvardsiana* (*Cypridina*, De Koninck) resembles a *Cypridinella* in shape, but is swollen here and there into tubercles, fewer in the young than in the old state, and is impressed also with the nuchal sulcus. We have figured *C. Koninckiana*,† in which, as usual, there is but one tubercle. In De Koninck's *Cypridella cruciata* the tubercles and dorsal sulcus are very strong, and a subquadrate outline results. There are gradations through *C. Wrightii* and *C. obsoleta*, to the smooth ovate forms of *Cypridinella*, and even to the acute ovate outline (in *C. cyprelloides*) found in the next group. *Cypridella* belongs to the Mountain Limestone of Europe and the British Isles. *Sulcuna*, from the same limestone, presents some few forms characterized by a general resemblance to some *Cypridellæ*, but so deeply indented by an oblique dorsal sulcus as to present sloping outstanding processes on the antero-dorsal regions.

* As in *Primitia*, see before, vol. iv., p. 191. † Plate LXI., Fig. 9, vol. iv.

*Cyprella*, another of these interesting Lower Carboniferous genera, is a very near ally, but distinguished by its usually more tapering shape, and especially its annulate ornament. Among the few species and varieties known, we have either a long or a short ovate outline, apiculate behind, notched and beaked in front. The valves are transversely ringed with slight furrows and step-like rings, like the annulated body of a chrysalis. This annulate sculpture covers either the hinder moiety only, or the whole of the carapace. In Plate LXI., Fig. 10, vol. iv., is figured *C. subannulata*, which is probably, however, only a local variety of *C. chrysalidea*, De Koninck.

Of the fossil *Cypridinella*, *Cypridellina*, *Cypridella*, and *Cyprella*, we of course know nothing as to their soft parts, which probably differed very much among themselves and from those of *Cypridina;* and we have no recent carapace at all closely representing those extinct forms, which, however, both by general and special features, claim alliance with the *Cypridinadæ.* There are, however, some fossil carapace-valves which so well correspond with certain recent specimens, that we have little or no hesitation in referring them to known genera. Thus, Dr. Rankin, of Carluke, has found in the Carboniferous strata of his neighbourhood a small ironstone nodule containing some well-preserved shells curiously like the carapace of *Bradycinetus Macandrei* (*Cypridina*, Baird), both as to the general shape and the form of the beak. We call this species *B. Rankinianus.*

So also in the Carboniferous Limestone of Cork, Ireland, Mr. Joseph Wright has met with some little valves so nearly resembling those of the male *Philomedes interpuncta*, that we refer them to that genus, dedicating the species to the memory of the eminent British entomostracist, the late Dr. Wm. Baird. In the Silurian beds of the Pentland Hills a Cypridina-like fossil has been found, but is not fully described yet; and another in the old quartzite pebbles of Budleigh-Salterton.

Another step among the relics of past life, preserved in the Palæozoic rocks, leads us to other allies of *Cypridina*, in which the carapace was often large, subglobose, or nearly quadrate, and the front edge of each valve was indented at the upper third, leaving a slight beak, and making a long, shallow sinus or depressed area down more or less of the front of the carapace. In *Entomoconchus* of M'Coy this sinus had a narrow, vertical gape under the little projecting angle, and a smaller gape lower down, or antero-ventral. Two other species, also from the Mountain Limestone, are known. In *Offa* the sinus is simpler and the gape smaller still. The former genus has supplied some bedded masses of valves to the Carboniferous Limestone of Yorkshire and Ireland. The latter is rare in the same limestone at Cork.

## *Part III.*—Polycopidæ, Cytherellidæ, M. Barrande's new Genera, and Entomididæ.

The recent genus *Polycope* has no notch, though sometimes there is a slight indication of its place. So in some fossil valves we have either little indication, or none at all, of the Cypridinal notch. These I group under the generic name of *Polycope*, with the same proviso and reservation as I adopt in using "Cypridina" for some of the notched forms. *Polycope* is represented by three species in the Lower Carboniferous rocks of Settle, Carluke, Cork, and Meath.

*Cytherella* has strong, thick, oval, or oblong valves, one fitting at its edge into the other, and has existed from the Carboniferous Period to the present day. *C. brevis* * is one of the few species of that genus yielded by the Lower Carboniferous strata.

*Cytherellina* is of Silurian age and obscure in character; thickness of shell and internal impressions remind us of the foregoing genus. *C. siliqua* † is the only known species.

*Æchmina* ‡ is also Silurian, both British and American, and obscure in its relationship.

M. Barrande's *Nothozoe* (from the Silurian of Bohemia §) has simple oval valves, with thickened ventral margin; his *Callizoe* (from the same) is narrower, indented antero-ventrally, and has a group of tubercles in that region. *Aristozoe*, Barrande, not uncommon in several forms in the Silurian of Bohemia, sometimes (as in *A. prælonga*) approaches our Carboniferous *Rhombina* in shape, but has a group of low tubercles in the antero-dorsal region, and a faint nuchal furrow behind them, as in some *Leperditiæ*, but more strongly marked. *Orozoe*, Barrande, also from the Silurian of Bohemia, is similar in general features, but has two large tubercles in the dorsal region, with the furrow between them, as occurs in some small *Primitiæ*. Altogether, these sometimes large Bohemian Ostracods, though not destitute of signs of alliance, differ largely from *Leperditiæ* on the one hand, and *Cypridinæ* on the other.

Quite different from the foregoing genera, and evidently belonging to a separate family, in which the organs of locomotion did not require anterior gape, notch, or hood, are the *Entomididæ*, comprising (1) *Entomis*, in which the dorsal or nuchal furrow is very strong, but reaches only half-way across the valve, with or without a tubercle on one of its margins; and (2) *Entomidella*, in which this sulcus crosses the valve obliquely, dividing off the anterior third as a separate region; ‖ recognized as *Entomidella* in 'Annals Nat. Hist.,' June, 1873, p. 416. *Entomis concentrica*

* Plate LXI., Fig. 4, vol. iv. † Plate LXI., Fig. 5.

‡ *A. and E. cuspidata*, Plate LXI., Fig. 6.

§ 'Sil. Syst. Bohême,' vol. i., suppl. 1872.

‖ See Plate LXI., Fig. 12, termed *Entomis divisa* at p. 185, vol. iv.

(De Koninck) has a most interestingly sculptured surface, each valve being sculptured with concentric elliptical lines, like the minute plicæ or ridges of the skin on the inside of the human finger-top. In *E. biconcentrica*, Jones, each moiety of the valve has this concentric ridging.* *E. Koninckiana* and *E. Burrovii*, Jones and Kirkby, have the ridging coarser, more open and vertical, that is, transverse to the valves, except on the ventral region, where it is nearly parallel with the margin. The former has fewer of the transverse riblets than the latter species; both have oblong outlines, and so also has *E. obscura*, Jones and Kirkby, which is smooth or faintly reticulate. All of these belong to the Mountain Limestone. Several *Entomides*, formerly termed *Cypridinæ*, occur in and characterize the "Cypridinen-Schiefer" of the Devonian series; several are known in the Silurian strata of Bohemia, according to M. Barrande, and there are a few in the same rocks in Scotland, especially the little *E. aciculata*, Jones, from the Pentland Hills, which has the subcentral tubercle produced as a sharp spine.†

## *Part IV.*—Cypridæ and Cytheridæ.

Among the Palæozoic Bivalved Entomostraca occur many that are indistinguishable, by means of their carapace-valves, from some members of the *Cypridæ* ‡ and *Cytheridæ*.§ In the Coal-measures we meet with valves like those of *Candona*; and in many Carboniferous shales and limestones *Bairdia* ‖ is recognizable by its peculiar triangular and apiculate valves, one overlapping the other, as well also in the Permian Limestones in abundance, and even in Silurian Limestones (Kildare). There are many Ostracodous valves in the Palæozoic rocks ¶ comparable with *Cythere*,** or some of the allied genera; and hosts of them occur in the Carboniferous strata. *Thlipsura* †† was doubtless a closely related form, but is pinched in posteriorly. *Carbonia* ‡‡ has the simple form of an oblong *Cythere*, but shows the sunken lucid spots of the *Leperditiadæ*.§§

## *Part V.*—Phyllopoda.

Other truly bivalved Entomostraca found in the older strata belong to the *Phyllopoda*, such as *Estheria* ‖‖ and *Leaia*,¶¶ for which the reader is referred to my Monograph of the fossil Estheriæ, Palæontograph. Society, 1862, and to the 'Geol. Mag.,' vol. vii., p. 219. *Estheria* lives now, and there is little difference between

* See Plate LXI., Fig. 13, p. 185, vol. iv.
† See 'Ann. N. H.,' *loc. cit.*, p. 416. ‡ Vol. iv., p. 186. § P. 187.
‖ Plate LXI., Fig. 1, p. 185, vol. iv. ¶ P. 186.
** Plate LXI., Fig. 3, *loc. cit.* †† Plate LXI., Fig. 2, p. 185.
‡‡ Plate LXI., Fig. 2, p. 185. §§ 'Geol. Mag.,' vol. vii., p. 218.
‖‖ Plate LXI., Figs. 23 and 24, and p. 185, vol. iv. ¶¶ Fig. 22.

the valves of the extinct and the recent forms. Their structure and ornament afford excellent subjects for microscopical work, and the geologist prizes the *Estheria* as being evidence of brackish water having alternated with marine, and as indicating (in the Trias, for instance,) the occurrence of lakes and lagoons where the main mass of fossils and their surroundings seem to speak of marine conditions.* For *Leaia*, I know of no closely corresponding recent analogue, and even among fossils the still enigmatical *Myocaris* † and *Ribeiria* ‡ are the only probable relations as yet observed.

*Postscript.*—At page 185, vol. iv., for *Cyprella subannulata* read *Cyprella chrysalidea*, De Koninck, var. *subannulata;* for *Entomis divisa*, read *Entomidella divisa;* for *Beyrichia Wilckenziana*, read *Beyrichia Wilckensiana.*

At page 186, to the Cypridæ *add* the genera Goniocypris and Metacypris.

At page 187, to the Cytheridæ *add* Polycheles.

*Delete* Cylindroleberis, this being a synonym of Asterope.

The genus *Entomis* should be separated from the *Cypridinadæ*, and with *Entomidella* be arranged as a distinct family (*Entomididæ*).

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## VI.—*The Pathological Relations of the Diphtheritic Membrane and the Croupous Cast.* By Jabez Hogg, Surgeon to the Royal Westminster Ophthalmic Hospital, President of the Medical Microscopical Society of London, &c.

(*Read before the* Medical Microscopical Society, *June* 20, 1873.)

Considerable misapprehension appears to prevail with regard to the pathological relations of the felt-like membrane usually secreted in diphtheria, and the filmy viscid substance thrown off in croup. You will therefore, I think, agree with me that the subject is one of sufficient importance to bring under discussion in the Medical Microscopical Society; the question being one of equal interest in a medical and a histological point of view.

One often hears of practitioners endeavouring to decide between an affection, croup with a mucous-looking membrane, and diphtheria with a true membrane, and in which the constitutional disturbance is quite remarkable; and then, apparently without having formed an opinion as to their true nature, or arrived at a settled conclusion

* Jones, 'Quart. Journ. Geol. Soc.,' vol. xix., pp. 147, 153, &c.
† Salter, 'Geol. Mag.,' vol. i., p. 11.
‡ Sharpe, 'Geol. Soc. Journ.,' vol. ix., p. 158.

as to their exact etiology, proceeding to treat them in opposite ways. Sir Thomas Watson, in the last edition of his 'Practice of Physic,' seems to give in his adhesion to the unity of all membranous affections. I doubt very much whether he is right; he is, probably, far nearer the truth in separating catarrhal croup, or simple laryngitis, although during life the symptoms are not distinguishable. His division of this disease is as follows:—

Croup { Catarrhal<br>
Membranous { Simple<br>
Diphtheritic { Qy. distinct or the same.

And for the first time he gives a place to diphtheria, and says of it, that "the proper place for this disease in any methodical nosology would be among the specific fevers." In the new nomenclature of diseases, drawn up under the sanction of the College of Physicians, croup and diphtheria are classed under one heading, the latter as a disease "not local," and requiring "a definition"; while both are separated from laryngeal catarrh, laryngitis, and *laryngismus stridulus.*

The epidemic visitation of diphtheria during the years 1858 and 1859, for the first time attracted the attention of the profession to the pathological indications and histological anatomy of the false membrane. Members of the Pathological Society of London at the period exhibited specimens of membranous exudations. A few microscopical examinations were made; these in my opinion were somewhat unsatisfactory, or, at all events, not at all conclusive as to the pathology of the disease; indeed it was by no means made clear that any considerable difference exists between the membrane, nearly always associated with diphtheria, and the mucus or albuminous film thrown off in certain croupous affections of the throat, and it seems hard to comprehend that, while the pathological indications in the one case partakes of an inflammatory nature, in the other it is a simple *non*-inflammatory tenacious exudation of little importance.

Conflicting statements, therefore, appear in the writings of those who exhibited various specimens at the meetings of the Society; as, for instance, "the false membrane was made up of a network of fribrillated lymph, in which epithelium was entangled"; and then, again, as if in seeming contradiction, "only a very delicate film, in which quantities of cells and granules are entangled, but nothing like a fribrillated structure was found." The explanation of this divergence of opinion is only explicable on the supposition that no one then had a notion of the true relation of the "*felt-like*" membrane to a specific form of disease, or that the histological characters of the membrane were totally unlike those of the simple and almost structureless film thrown off during a non-inflammatory affection of the throat. Consequently a considerable confusion of thought as well as of language even now exists among medical men on the etiology of the affection diphtheria; and it has been asserted, only quite recently, that nothing more than "a clinical tradition" separates diphtheritic and croupous complaints; indeed, it is boldly stated and taught, both in this country and on the Continent, "that croup, accompanied by false membrane in the larynx and trachea, is always a diphtheria, whether in the child or in the adult."* And again, "that while both diseases are highly contagious and inocuable, they are one and the same disease, neither peculiar to children nor adults, as they are equally sporadic, epidemic, and endemic."† My answer to this statement is, that while one disease, *diphtheria*, is most decidedly epidemic and endemic, often widespreading and affecting a large proportion of adults, and probably belonging to a specific form of fever; the other, *croup*, is essentially sporadic, often a local affection, not communicable, or only so in a small degree, as when a family predisposition exists, mostly occurring during childhood, and rarely after it is fairly passed. The contradictory evidence of clinical medicine compels us to put it aside, and look entirely to histological anatomy for a solution of the difficulty raised by the physician. I maintain that a sharp line can be drawn between the diphtheritic membrane and the croupous cast, and surely if this be demonstrated, no one will venture to say that "clinical tradition" alone separates diphtheria and croup. I will first glance at the naked eye appearances of the diphtheritic membrane. As the name implies, it is a dense, compact, opaque, yellowish-white or reddish-grey coloured mass, of from half a line to five or six lines in thickness. It is usually firmly adherent to the subjacent membrane, upon which it is moulded; is more or less friable, so that when traction is made upon it with a pair of forceps it comes away piecemeal, or in a layer somewhat resembling felt or chamois leather. If forcibly detached, a breach of continuity of surface is made, and bleeding generally follows its separation, as the mucous membrane is much congested. Frequently general tumefaction from excessive corpuscular infiltration occurs, and then compression of the vessels, and an arrested circulation, ends in decomposition. Ulceration of the superficial and deeper-seated structures produces paralysis of the nerves, probably of the vagus, and quickly kills the patient. By no unaided effort on the patient's part during the extreme paroxysm of the attack is the membrane ever thrown off.

In striking contrast to the foregoing brief description of diphtheria and its membranous exudation, the croupous cast is semi-transparent, delicate, and tender to handle; often gelatinous

* Sir Thomas Watson's 'Practice of Physic,' vol. i., p. 903, 1872.

† Prof. M. Roger's 'Chemical Lectures,' 1872.

or white-of-egg-like, and of a pale yellow colour ; easily separable from the subjacent surface, as an imperfect cast of the part on which it is formed. It is only as a post-mortem deposit, or when it has been steeped in a weak ammoniacal solution of carmine, that it is seen otherwise than a viscid secretion, or a single layer of cells. It is generally thrown off in a membranous form, or rather separated during a fit of coughing, when the patient finds almost immediate relief from the more urgent symptoms of the attack. It is never so intimately connected with the subjacent mucous membrane as to cause bleeding if detached by force, although there may be some tumefaction about the parts. In short, it is a simple epithelial layer or cast of the superficial structure, closely resembling the skin shed by some of the lower animals—the growing amphilia, for example—an outgrowth of epithelium cells undergoing degeneration of protoplasm, and entangling granular molecules. Such a cast is, however, thrown off with difficulty by a feeble delicate child.

The histological characteristics of the felt-like membrane of diphtheria are even more strongly marked than those just enumerated. A small portion of a fresh exudation requires a good deal of careful teazing out to fit it for microscopical examination under a power of 350 ×. The normal tissues are seen to be replaced by an aggregation of compressed cells, molecules of fat, connective and fibrous tissue, a few crystals, muco-purulent or glandular corpuscles, foreign bodies, as starch granules or other portions of food, and spores of the *Oïdium albicans.* It is surmised, therefore, that the dense felt-like membrane is made up of superficial and deep tissues ; mucous membrane, voluntary and involuntary muscles and glands, and produces great tension and decomposition or ulcerative destruction. Even the cartilages are at times involved, and the cells become fusiform. That the mucous membrane itself is affected by the infiltration as well as the more superficial structures is quite evident by the loss of sensibility in nerve fibres.*

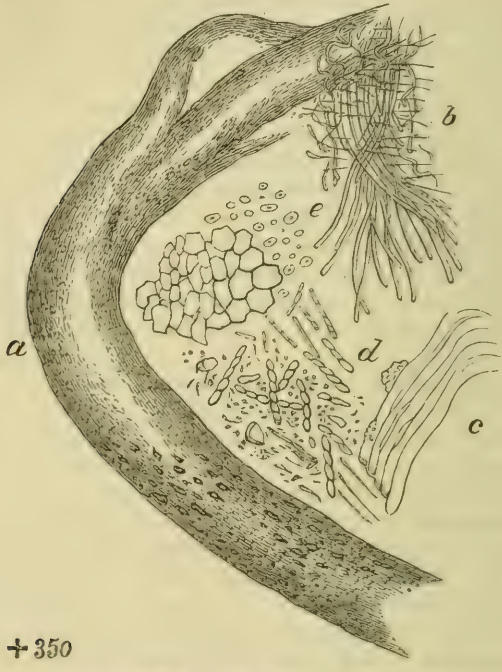
The drawing, Fig. 1, made from a dried preparation, exhibits most of the changes spoken of. For showing the structures involved in the morbid state produced by the disease, it is better, after careful removal of the membrane, to immerse it for a short time in a staining fluid, and then dry it. Fine sections cut from such specimens must be mounted in dammar or balsam.

In preparations made from other cases of diphtheria I noticed considerable tumefaction, which appeared to compress the vessels and arrest circulation and nutrition ; not a trace of columnar epithelium will be seen in any specimen. Those who have failed to find evidences of connective and fibrous tissue in the diphtheritic

* Virchow is also of opinion that an exudation takes place into the substance of the mucous membrane, and produces tension and subsequently ulceration.

membranous exudation, in my opinion must have confined their examinations to the false membrane thrown off in the early stage

Fig. 1.




Section of a Diphtheritic Membrane.

*a*. Vertical section composed of condensed tissue and shrunken epithelium cells. *b*. Fibrous or connective tissue teased out. *c*. Involuntary muscular fibres. *d*. Oïdium albicans. *e*. Pavement epithelium and altered cartilage cells.

of the disease; that is, when it consists almost exclusively of an excessive corpuscular infiltration. In some cases, glandular cells are chiefly found: the glands swell, pour out their rapidly-formed contents, together with transparent pus-like corpuscles, but which scarcely behaves like the pus corpuscle when a reagent, as acetic acid, is applied. At this period of the affection nothing like spores, *Oïdium albicans*, will be found in the membrane.

A portion of a croupous cast, examined under a power of 350 × is seen to consist of pavement and cylindrical, or columnar, epithelium, and a transparent albuminous substance entangling the scattered contents of epithelial cells, molecular matters, fat and mucous corpuscles; and a few foreign bodies, as starch granules, involved in a homogeneous matrix. When stained by a weak solution of ammoniacal carmine or aniline dye, and carefully spread out with needles; the columnar epithelium retains its cilia; each cell being filled with clear protoplasmic and nucleated contents. It is, therefore, highly probable that these casts are not long retained, but are rather thrown off soon after their formation. Fungus spores are rarely found in these films, which appear to partake of the nature of an excessive cell proliferation of the epithelial surface rather than of a transudation, or true exudation. Although such casts differ a good deal in colour and consistency, connective or fibrous tissue never enters into their composition; but the transparent mucus often exhibits the striations or wavy lines peculiar to this material.

Such casts are thrown off from the intestinal canal, and pre-

Fig. 2.


Section of Croupous Cast.

*a.* Homogeneous matrix, in which altered epithelium granular and fatty matters are entangled. *b.* Columnar epithelium, with cilia retained. *c.* Detached epithelium fat and mucous corpuscles.

sent similar appearances. Fig. 2 represents a drawing made from a croupous cast, and stained with carmine.

The several examinations that I have made clearly show that diphtheria and croup demand a nosological separation, as they most undoubtedly differ from each other, both in their general and histological characters. To speak of "a diphtheritic croupous membrane" is to mislead the student in medicine, for there can be no doubt that one affection is essentially of an inflammatory destructive nature, "a specific fever," epidemic or endemic in its course;* while the other is a local manifestation of a simple disease, entirely wanting in the features of an inflammatory exudation. It is, however, not denied that occasionally similar casts may be found in connection with an inflammatory affection of the larynx, but this is a question upon which I do not now propose to enter.

* Dr. Oscar Giacchi tells us that even on the hills of Arno, where a pure balsamic air distinguishes the country, diphtheria rages epidemically. The robust peasantry are victims to it equally with the inhabitants of towns. His opinion is that the disease does not belong to the specific fevers, and that the paralysis which occasionally supervenes is a neurosis, while the albuminuria is the result of parasitic infiltration.—'On the Nature and Treatment of Diphtheria,' by Oscar Giacchi, 1872. Other observers think that its contagious nature depends upon a parasitic fungus or algæ; some again maintain that in cases where a fungus is found, "its presence is probably explained by the view that the false membrane is a nidus favourable to its development."

# NEW BOOKS, WITH SHORT NOTICES.

Manual of Human and Comparative Histology. Edited by S. Stricker, assisted by others. Vol. III. Translated by Henry Power, M.B., Lond., Examiner in Physiology to the University of London. The New Sydenham Society, London, 1873.—The third and last volume of Dr. Stricker's valuable work is now before us, and we think that though the time has appeared long since the first made its appearance, the editor is, nevertheless, to be congratulated upon the result. And, furthermore, we owe Mr. Power our thanks for, as in the former volumes, a translation whose merits and advantages cannot very well be overrated. In so far as we can speak of the present volume we must confess to a little disappointment, not so much in regard to the matter as to the absence of material which we expected the third volume would open out to us. But, doubtless, this circumstance could not be helped; and with some few objections to certain parts of the entire work, and to the entire absence of any common plan, we cannot but award the highest praise to Dr. Stricker for the patience which he has shown and the ability he has displayed in producing a work which, all things considered, is without a doubt the finest treatise of the kind which has yet been issued by any printing press in the world. Histological readers will recollect that it is exactly twenty years since the first volume of Kölliker's manual appeared in this country, under the combined editorship of Professor Huxley and Mr. Busk. And they will probably remember how that work was then considered much too far advanced for anyone but the special student of Histology. Yet, Kölliker is as much behind-hand now, when compared with Stricker, as he was foremost at the date we mention. It would not be fair to object to the incompleteness and inequality of the materials comprising the volumes of this work, without acknowledging that the editor sees these defects as clearly as anyone else. For he says, a "review of the whole work, however, compels me to admit that it does not present the same uniformity that it would otherwise have done had it been the outcome of a single master-mind. Some pages glow with the results of the long-continued industry of the best investigators of our time; and sometimes, again, these nodal points, so to speak, appear joined together by the labour of younger hands. It lacks, however, that whitewash with which our master-builders, following the usual custom, are wont to cover their constructions in order to hide from the eyes of the observers all the piece-work of their men—the good bits equally with the bad." The only question is, whether, if Dr. Stricker had taken sufficient trouble, he might not have found for *each* subject one who was eminently distinguished in it. We consider he might have; but it is useless now to think about it.

The subjects dealt with in the present volume are not numerous, but some of them are of considerable importance. For example: Herr T. W. Engleman has the organs of taste allotted to him. Herr J. Kessel has described the external and middle ear, exclusive of the

Eustachian tube, which is dealt with by Professor Dr. Rüdinger of Munich; who has also in his charge the membranous labyrinth. The auditory nerve and cochlea are written upon by Herr W. Waldeyer; and the olfactory organ, by Professor Babuchin. Next comes the eye, and of this the retina is done by Professor Max Schultze. This is unquestionably the first and ablest essay in the whole book; indeed, with the exception of two or three others, it is the only important paper in the volumes. As our readers are perhaps aware, the author is editor of the first microscopical journal in the whole world, a work whose illustrations are executed with a skill so superior as to make our best plates appear two hundred years behind the work of German draughtsmen. It was in this periodical (Max Schultze's 'Archiv') that most of the author's papers on the organs of vision have appeared during the past few years, and in the essay he has written for Herr Stricker's volume he has given a condensation of his valuable labours in about 75 pages. This we regard as the most valuable part of the present work, and one to which we commend our readers' serious consideration.

Other papers follow this one, such for instance as Professor Iwanoff on the Tunica vasculosa; Herr Leber on the blood-vessels of the eye; Schwalbe, Iwanoff, and Babuchin on the Lymphatics, the vitreous humour, and the lens of the eye; Alexander Rollett on the cornea; Stricker, Stieda, and Klein on the conjunctiva and sclerotic; Boll on the lachrymal glands; Dr. Chrobak on the uterus; Dr. Reitz on the placenta; Grundwald and Stricker on the oviducts and fallopian tubes. Last comes an excellent article by Stricker on the development of the simple tissues; a paper by Dr. Ernst Fleischel on the non-pedunculated Hydatid; and by Edward Albert on the structure of the synovial membrane. The papers on the development of the tissues and on the conjunctiva and sclerotic, are exceedingly good, and contain facts which will be quite novel to many of our readers. The type and illustrations of this English edition are excellent, the cuts being, as they should be, most carefully worked out.

The Microscope, and Microscopical Technology: a Text-book for Physicians and Students. By Dr. Heinrich Frey, Professor of Medicine in Zurich, Switzerland. Translated from the German and edited by George R. Cutter, M.D.; 343 engravings on wood. From the 4th and last German edition. New York: W. Wood and Co., 1872.—The work which is now under notice extends over 600 pages of large 8vo, and is amply illustrated with woodcuts. It has been well, though very literally, translated by its editor, who has here and there added a few notes on points with which he as an American was specially acquainted; but he has, unfortunately, been insufficiently familiar with the splendid work of this country to have played his part fairly as an accomplished editor of a work which appeals in its present form to English as well as to American readers; a circumstance which we trust the publishers will not forget when the next edition is being prepared for the press.

Dr. Frey is modest enough in his Preface, when he expresses the hope that his "little work may serve as a guide for students and physicians till the time at least when a better pen shall produce a better substitute." But in spite of this wish, we must at the outset confess that its fulfilment is we fancy far enough away; for assuredly to the medical man and student no better work has been offered to the English reader. It is of course a treatise specially devoted to the medical reader, and in all that relates to his wants, it is peculiarly complete and modern. The book is divided into two parts, the first being devoted to descriptions of the instrument, the theory of its construction, its various forms at present in use; the apparatus for measuring and drawing; the binocular stereoscopic and polarizing microscopes; testing the microscope; the preparation of microscopic objects, fluid media, and chemical reagents; modes of staining, impregnation with metals, drying and freezing, methods of injecting; and, lastly, the mounting and arrangement of microscopic objects. Under almost all of these several departments the information supplied is most complete, and in many cases novel. We cannot of course examine every section, but we may take a few, which strike us as being of special importance, under consideration. It is rather strange that the editor, not the author, makes an assertion to the effect that Dr. Carpenter, in the last edition of his 'Microscope and its Revelations,' has made a misstatement in regard to M. Nachet's student's microscope. He states that the improved stage which M. Nachet has adopted, and which Dr. Carpenter especially praises as being M. Nachet's, was really "invented by Zentmayer in 1862, but which was copied by a Paris maker, to whom Dr. Carpenter gives the credit of being the inventor. In speaking of this instrument (Nachet's student's microscope), Dr. Carpenter says, 'The chief peculiarity of this instrument, however, lies in the stage, which the author has no hesitation in pronouncing to be the most perfect of its kind that has yet been devised.' The instrument from which Nachet copied the circular stage was made by Zentmayer, in 1864, for Dr. W. Keen, of Philadelphia, who showed it three different times to M. Nachet, and had it packed by him in the spring of 1865 for transportation." This is a serious assertion, and one too of which we question the accuracy; but we have not the smallest doubt that Dr. Carpenter, who must have based his account in great measure on M. Nachet's statement, will at once admit the error if it is so.

In reference to the use of microscopic lamps, the volume appears strangely defective; but one form, and that a most elementary one, has been included in the description. And this seems the more strange in an English edition of a foreign work. Clearly in regard to this matter, as well as various others, the American editor has not had much experience. Still we are indebted to him for his account of a very ingenious section-cutter, which he has described from the Proceedings of the American Ophthalmological Society for July, 1871. It is the invention of Dr. Edward Curtis, a distinguished microscopist of New York; and as it is fully described and figured in the present volume, we now dwell on it no longer; it seems a most convenient instrument.

In the chapter upon fluid media, we find some valuable information which has been furnished by Herr Schultze. Among other points are his remarks on the use of camphor in the preservation of animal tissues, a substance which seems even in very small quantities to have a marvellously powerful preservative effect. His observations on the subject of iod-serum are important. Herr Schultze says it "consists of the amniotic fluid of the embryo of Ruminantia, to which a concentrated tincture of iodine, or a strong solution of iodine in hydriodic acid, is added. About six drops are to be added to the ounce, while shaking the mixture. The colour of the solution is at first wine-yellow, but after a few hours it becomes paler; this paleness afterwards increases, and the subsequent addition of a few drops of the iodine solution becomes necessary. Our mixture forms an excellent fluid for the examination of delicate fresh tissues, and is also a very good and very preservative macerating medium, acting in this way even for hours or days. We must here give a piece of advice which is of great importance in the numerous macerations of this kind which are necessary, namely, to have the piece which is to be placed in them very small, and the quantity of fluid as large as possible." This solution, which is extremely simple, appears admirably adapted to the purpose for which it is devised.

In reference to the process of staining tissues, very many methods are given, some of which are novel, and others well known to most of our readers. We fancy that the system of double-staining, which was discovered by Herr E. Schwarz, will be novel to many workers. He "places the tissues in a mixture of 1 part creosote, 10 parts acetic acid, and 20 parts water. The preparations are to be immersed in this mixture while it is boiling for about a minute, and are then to be dried for two or three days. Thin sections are to be made and immersed for an hour in water slightly acidulated with acetic acid, and then washed out in distilled water. Next they are to be put in an extremely dilute watery solution of ammoniacal carmine, and after being again washed in water are exposed for two hours to a solution of picric acid (0·066 grm. to 400 ccm. of water). The sections are then placed on a slide, the superfluous acid is allowed to flow away, and a mixture of 4 parts of creosote to 1 part of turpentine, which has become resinous from age, is dropped on to it. In about half an hour the specimen, which has become transparent, is to be mounted in Canada balsam." Now, by this process, which, it must be confessed, is a lengthy one, a peculiar effect is produced. Epithelial and glandular cells, muscles, and the walls of vessels, show a yellowish colour, with reddened nuclei, while the connective tissue is not coloured by the picric acid, and only presents the carmine colour.

In the second part of the work the reader is taught how to prepare for examination the several tissues which make up man's body, and then how to observe the various structures so brought out. This is, to our minds, the most valuable part of the entire book; for the descriptions of the tissues are for the most part extremely recent, and are sufficiently elaborate, while the cuts in illustration are numerous, excellent, and many of them unfamiliar even to the student of Histology.

There is, however, cause to complain of the treatment of English makers of the microscope, who have no fair position in the list which is given at the end of the volume, and have not a place given to them in the substance of the work. Had such men as Woodward or Richardson been the editors, this would have been very different. However, we have to express our gratitude to the editor for what he has done, and to declare our immense satisfaction with Professor Frey's labours.

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## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Origin of Leucocytes.*—This important subject has been very fully dealt with by M. Feltz, who has recently published a memoir, which completes the views expressed in his earlier paper published in the 'Journal de l'Anatomie.' In this (the first paper) the author concludes that the globules of pus which infiltrate the peritoneum do not proceed from the leucocytes of the blood which escape through the walls of the capillaries, nor from the epithelium of that membrane which desquamates at the end of a relatively short period; and that after their fall leucocytes may still be seen to be produced in the substance of the serous membrane. He now infers from further researches (of which the conclusions are stated in a note addressed to the Académie des Sciences, Feb. 17) that in the peritoneum, as in the cornea, the connective tissue which forms the web of the membrane is crossed by a network of canaliculi, the fusiform enlargements of which form what are called the *cellular elements of the connective tissue*, the *connective nuclei*, or *plasma-cells.* These networks consist normally of a simple organic matter (*protoplasma* of Remak Schulze, &c.), and under irritation, the circulating blood being increased in amount and modified in its plasma, a parallel modification and augmentation of the protoplasma ensue, whence the considerable development of the network of interstitial canaliculi, and of the element styled *plasmatic*, and its organization into leucocytes. He thinks it not doubtful that this protoplasm becoming free, as well as by a direct individualization or genesis, as by segmentation and gradual organization of the fusiform swellings, gives rise at once to the form of leucocytes. He hopes soon to be able to give irrefutable proofs of it in the pulmonary alveoli.

*The Liver Ferment.*—In Pflüger's 'Archiv' there is a paper on this subject, which is abstracted in the 'Medical Record,' by Dr. Ferrier. The paper is by Herr Von Wittich. In reference to a recent paper by Tiegel, who found that blood corpuscles under process of destruction in the liver generated a diastatic ferment, Von Wittich shows that such a ferment can be obtained from blood serum by precipitation with alcohol, and subsequent extraction with glycerine, in the absence of blood corpuscles. In addition he finds that the liver-parenchyma itself, when quite freed from blood, yields an active diastatic ferment to glycerine. He allows with Tiegel that it is difficult, if not impossible, to obtain the ferment absolutely free from sugar; but a series of comparative experiments prove beyond doubt that a sugar-forming ferment exists in the liver-cells. A calf's liver was washed out for four hours by a stream of water directed through its vessels. At the end of that time the water passed through quite colourless, but still contained sugar. A portion of the liver soaked in alcohol and extracted with glycerine, was shown to contain a ferment acting on starch, by the fact that, when equal quantities of the glycerine extract were mixed in the one case with distilled water, and in the other with starch paste, and allowed to stand for an hour, the latter reduced a much larger quantity of Fehling's solution than the former. The same liver washed for two hours longer yielded water which now contained not a trace of sugar, but on being allowed to remain for another hour, the water which was then passed through it contained sugar. A glycerine extract of the liver so treated still yielded a very appreciable amount of a ferment acting energetically on starch. It would therefore appear that the thoroughly washed and blood-free liver still contains a ferment, and that this ferment is formed in the liver-cells. The ferment formed here is partly poured out with the biliary constituents; as fresh bile, as Von Wittich has shown, also possesses a diastatic action.

*The Microscopic Structure of Trap Rocks* has been treated at some length by Professor Hull, F.R.S., in a paper in the 'Geological Magazine' (April). The paper should be referred to as it is of some length, meanwhile we may give the author's opinion as to the points specially noticeable in reference to the above specimens, which may be regarded as fair representatives of the Limerick Carboniferous melaphyres, which are as follows:—

1st. The glassy felspathic base with cells and tubes.
2nd. The small quantity of augite, this mineral only occurring in the form of scattered crystals or grains.
3rd. The abundant infusion of chlorite, or more rarely epidote, not only filling in cavities and interstices between the crystals, but also replacing, in many cases, the original minerals themselves (augite, olivine, &c.).
4th. The abundance of calcite, also due to percolation, and of secondary formation.

*The Blood-vessels of the Membrana Tympani.*—On this question a very able paper appears in the 'American Journal of Medical Science.' Dr. Burnett, who is the author, describes the arrangement of the blood-vessels in the tympanic membrane of the dog, cat, goat, and rabbit. These are arranged in a double series of loops, one of which is composed of vessels which run from the periphery directly toward the handle of the malleus, and at a point from one-half to a third of the distance between the periphery of the membrane and the handle of the malleus return abruptly upon themselves, thus forming a series of vascular loops round the edge of the membrane. The second series of loops run from the handle of the malleus toward the periphery of the membrane. In consequence of this arrangement a portion of the membrane between the annulus tympanicus and the handle of the malleus remains free from capillaries in its normal condition. In the guinea-pig these vascular loops do not exist, but the vessels are arranged in the form of a net with coarse meshes of a quadrangular or pentagonal form. In this animal, moreover, the radiate are strongly developed in comparison with the circular fibres of the membrana tympani. The arrangement of the nerve in these animals is described as "fork-shaped," the prongs embracing the loops, while the handle unites with a similar projection from the opposite series of loops. In the human tympanic membrane the arrangement of the blood-vessels resembles that of the guinea-pig in the absence of loops. The vessels themselves, however, are coarser, and the meshes finer than in that animal. The radiate and circular fibres are, moreover, equal in amount. The conclusions from these observations are the following: 1. There is a distribution of vessels in the membrana tympani of man peculiar to him. 2. There is a distribution of vessels in the tympanic membrane of the dog, cat, goat, and rabbit, constant in as well as peculiar to them. 3. The arrangement of these vessels in the guinea-pig is peculiar to it. The author then gives instructions for the preparation of the membrane.

*An un-Microscopic Specimen of an almost Microscopic Group.*—A paper has been read before the Royal Society,* which was sent by Herr R. Von Willemöes-Suhm, Naturalist to the 'Challenger' Exploring Expedition. It is upon a new genus of Amphipod crustaceans founded on the capture of a large new Amphipod, perfectly transparent, and with enormous faceted eyes. The author shows that among the Amphipods known to us, *Phronima* is its nearest relation. But there are so many points in which this genus differs from *Phronima*, that it cannot form a member of the family Phronimidæ; and he therefore proposes to establish for it a new family, Thaumopidæ, belonging to the tribe of *Hyperina*. The form of the *head* is totally different from that of *Phronima;* the antennæ are not situated near the mouth, but at its front, and the enormous faceted eyes occupy its upper surface. The first two pairs of thoracic appendages are not, as in *Phronima*, ambulatory legs, but maxillipeds, so that only five pairs of legs are ambulatory in *Thaumops*. The *thorax* is composed of six segments—the first of which has, on its under side, the vulva and one pair of maxillipeds; and the second, representing two segments, bears two pairs of appendages, the larger maxilliped and the first pair of ambulatory legs. The *abdomen* consists of five segments, with three pairs of pedes spurii, the caudal appendages being attached to the fourth and fifth segments. The animal being beautifully transparent, the *nervous system* could be carefully worked out without dissecting it; the position of the nerves going out from the cephalic ganglion, as well as that of the five pairs of thoracic and the three pairs of abdominal ganglia, could be ascertained. The *eyes*, having at their borders very peculiar appendages, were examined, and a description is given in the paper here abstracted, of the structure of the large crystalline

* 'Proceedings of R. S.,' April.

bodies which are to be seen in them. Organs of hearing and touch have not been discovered. The *mouth* is covered by a pair of maxillæ and a small labium. There is a recurved œsophageal passage leading into a large cæcal stomach, and an intestinal tube departing from near the end of the œsophagus and running straight to the anus. The *heart* is an elongated tube extending from the second to the fifth segment, with probably three openings. Three pairs of transparent sac-like gills are attached at the base of the second, third, and fourth pairs of feet. *Genital Organs.*—The single specimen taken is a female. The ovary, probably composed of two ovaries, has a rose-colour, and the genital papilla is situated at the under part of the first segment; it is covered by two small lamellæ, which in this case did not sustain the eggs, which were found to be attached to the first pair of ambulatory legs. The animal seems to carry them in a manner similar to the pycnogonid *Nymphon*. *Development.*—The eggs contained embryos having already the antennæ, the five pairs of legs, and the abdominal feet; they show that *Thaumops* has to undergo no metamorphosis, and that the young ones leave the eggs with all their appendages well developed.

*The so-called Syphilis Corpuscles.*—These bodies, which our readers are familiar with by this time, have been the subject of many papers in the German journals. But none of them has been as good as that little sketch which Dr. E. Klein has given in the 'London Medical Record' of April 9th. It is an account of Biesiadecki and Lostorfer's researches. Lostorfer, in the beginning of the past year, alleged that he had made the important discovery that, in preparations of the blood of syphilitic individuals, there develop within a week small bright corpuscles — syphilis corpuscles — which in four or six days reach the size of a coloured blood-disk, and in six or eight days become vacuolated. This observation of Lostorfer has been declared by many observers to be incorrect. Biesiadecki (*Untersuchungen aus dem Pathologisch-Anatomischen Institute in Krakau.* Vienna, 1872), following Lostorfer in a large series of experiments, has come to the conclusion that the assertions of Lostorfer are, with some slight modifications, correct. The mode in which Biesiadecki proceeds in his observations is similar to that employed by Lostorfer. By means of a pointed needle, a small drop of blood is taken from the perfectly clean finger, brought on a clean glass slide, and covered with a glass. By a slight pressure on one edge of the cover-glass with the nail of the finger, the blood can easily be made to spread out so that the blood corpuscles lie only in one layer, without being broken up and destroyed. Preparations in which the blood corpuscles have not spread out into one layer, or in which they appear to be squeezed, are to be put aside as useless. A number of preparations are brought into a moist chamber, where they are kept at a temperature of 14–18° C. (57–64° Fahr.). In most of the preparations which have not become dry at the edges of the cover-glass, taken either from syphilitic or other patients, *e. g.* arthritic or rheumatic, there appear on the second, third, or fourth day, numerous needle-shaped or rhombic hæmoglobin crystals, varying in diameter from that of a blood-disk to twice or three times as large. In blood preparations of syphilitic patients the following changes take place, beginning from the fourth day. In the yellowish-coloured plasma there appears a cloudy opacity, which is due to the presence of small flakes. These latter are seen to contain extremely small spherical bright granules, which generally possess a filamentous appendix. The fifth day the number of these granules becomes much greater; they become much larger, perfectly bright, spherical or irregular-shaped, whereas at the same time the filamentous appendix disappears. These granules make their appearance all over preparations; they are not limited to certain foci. The most of them are to be found on those places in which the plasma is still unclosed. There exists some difference, however, as regards the time in which these granules appear; in some of the preparations taken from the same patient at exactly the same time, they come into view, some on the fourth, others from the fifth to the seventh day, others still later and, in a limited number, or, lastly, not at all. After the twelfth day, up to which time their number has increased immensely, no material change can be made out, even up to the twentieth day, except that some become a little larger, brighter, and more sharply outlined. In preparations of the blood of patients suffering from different diseases (endocarditis, acute rheumatism, Addison's disease, gout, jaundice, pneumonia, tuberculosis, variola, puerperal peritonitis, septicæmia), the above-described corpuscles make their appearance only in an extremely limited number. Consequently, a preparation of blood which contains only a few of those corpuscles is unavailable for a diagnosis; whereas a preparation that contains a great number of them can be said to have been taken from a syphilitic patient. Biesiadecki succeeded in this respect, just as Lostorfer, in being able to point out in a series of mixed preparations, submitted to him and prepared in the above-mentioned manner, which of them had been taken from syphilitic patients, and which not; except in one preparation, in which Lostorfer's corpuscles were present abundantly, and which was taken from a patient suffering from pustula maligna; it could not be ascertained, however, whether this patient did not suffer from syphilis. Biesiadecki does not agree with Lostorfer in his assertion, that the corpuscles in question become vacuolated after a certain lapse of time, having been able to find such vacuolated bodies in syphilitic blood as well as in the blood of the small-pox already on the second or third day. Biesiadecki regards them as residua of coloured blood-disks, and not as transformed syphilis corpuscles. Biesiadecki shows that these latter are not fat, not sarcina, not granules of colourless corpuscles, and not fungi, as Lostorfer was inclined to assume, but that they are granules of precipitated paraglobulin; for *a*, if a current of carbonic acid be allowed to pass through a preparation of diluted serum (plasma?—*Rep.*) of a dog, similar corpuscles to those above described make their appearance; on replacing carbonic acid by oxygen they disappear; *b*, if through a blood preparation, in which numerous syphilis corpuscles have developed, a current of oxygen be allowed to pass, the small ones disappear, whereas the larger ones diminish considerably in size; *c*, the syphilis corpuscles do not dissolve in ether, but they dissolve almost entirely in a large quantity of saline solution (one part of concentrated saline solution in two parts of water). All these are properties which belong to paraglobulin. In blood preparations, therefore, which are kept in a moist chamber, that is, in which, on the one hand, the plasma becomes gradually diluted by absorption of water, and in which, on the other hand, as it must be supposed, carbonic acid is developed by decomposition, all the conditions are present under which paraglobulin may be precipitated. That Lostorfer's corpuscles are to be met with abundantly generally only in blood preparations from syphilitic patients, seems to show that their blood contains either more paraglobulin or less fibrinogeous substance than other blood.

*Microscopic Appearances of Silica in the Galway Granites.*—In an able paper "On the General Microscopic Structure of the Irish Granites," which he read before the Royal Geological Society of Ireland at a late meeting, Professor Hull, F.R.S., gave the following account of the appearance of the silica in rocks. He says it occurs without crystalline form enveloping all the other minerals. It is structureless, but full of cells, which are visible with a high power. With polarized light, and on rotating the upper prism, the silica presents the usual gorgeous play of colours, being broken up into distinct patches of irregular form, each refracting different prismatic colours. Some of the patches show round their edges parallel wavy bands of prismatic colours, marking out the individuality of the patches, and indicating the manner in which the particles consolidated in independent masses of various sizes—sometimes exceedingly small. These cells are often so minute that three successive series are brought into the field upon changing the focus of the microscope by means of the mill-headed screw, with a magnifying power of 350 diameters. Along with the cells are numerous long "belonites" or "trichites," sometimes perfectly straight, and stretching in all directions through the mass of the silica. With the 1-inch object-glass these can be generally observed; but with the $\frac{1}{4}$-inch and the No. 2 eye-piece, magnifying 350 diameters, they are very well brought out, sometimes in extraordinary numbers. Even with this power their apparent thickness is not so great as that of the finest needle, with an apparent length from an inch downwards. In one or two instances they appear to be barbed, but this may be owing to the meeting of two trichites at a point: there are also examples of trichites slightly bent or curved. Sometimes the silica contains cells only without trichites. What the nature of these needle-like objects may be I have no means of judging from this slice. Cavities are exceedingly numerous in the silica. Some of these resemble the forms described and figured by Mr. Sorby in his admirable and well-known paper "On the Microscopical Structure of Crystals." They are of various shapes, enclosing the little globule of fluid which just comes into view with a magnifying power of 350 diameters, and must be less than $\frac{1}{1000}$ of an inch in diameter. Others, however, are much larger, and sometimes do not appear to contain any bubble; and, as Mr. Sorby suggests, the fluid may have escaped. Stone cavities, or appearances which I take to be such, as figured and described by Mr. Sorby, are also numerous in the silica. Along with the confused broken materials which they contain are also minute black specks. The form of these stone cavities is often very irregular and ill-defined. Occasionally perfect spheres occur, which may be assumed to be gas cavities. They are, however, rare.

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## NOTES AND MEMORANDA.

**Elections of Naturalists to the French Academy.**—At the meeting of the Paris Academy of Sciences, on the 7th ultimo, three elections to the section of Anatomy and Zoology took place. The places to be filled were those of M. Agassiz, elected a Foreign Associate, and MM. Pictet and Pouchet, deceased. In the first case, M. Steenstrup obtained 38 votes and Mr. Darwin 6; in the second, Mr. Dana obtained 35 and Mr. Darwin 12; in the third, Dr. Carpenter obtained 35, Mr. Darwin 12, and Mr. Huxley 1 vote. Messrs. Steenstrup, Dana, and Carpenter were therefore declared duly elected. The treatment of Messrs. Darwin and Huxley is well understood, and must be appreciated by their supporters.

**A New Form of Microscope.**—At one of the meetings of the Microscopical Society of Illinois, Dr. Adams read a letter from Professor Sanborn, of Boston, Massachusetts, on a new form of microscope, to be used for the examination of parts of the observer's own face. The instrument consists of an ordinary microscope tube bent twice at right angles, forming thereby a body and two arms. Inside the tube at the angles are affixed prisms or mirrors. The objective being adjusted in one arm and the eye-piece in the other, the light traversing the axis of the objective is reflected by the mirror or prism in the first angle and thrown on the mirror in the other angle, whence it passes through the eye-piece. The instrument is held in position by a clamp fixed to the middle of the body, and firmly screwed to a table or rest. The observer assumes the reclining position, and, adjusting the eye to the eye-piece, brings the objective to bear on the part of the face under examination. Sunlight is used for illumination, and the objectives are, of course, low. It is the purpose of the Professor to study in this way the pathological processes involved in vesication, &c. Anyone possessing a microscope can, at slight expense in procuring a tube and mirrors, avail himself of this means of study, by using his own objectives and oculars.

**Mounting with Balsam.**—In 'Science Gossip' for June, a correspondent, Mr. C. L. Jackson, thinks that Mr. F. Kitton (a distinguished authority) is quite wrong in saying the most important condition in mounting with balsam is to keep the balsam free from chloroform. Until he began to use chloroform very freely in the different processes of mounting with balsam, he could make very little progress. Mr. Kitton's plan he found very troublesome, and to result in many failures. His own plan is to mix chloroform with the balsam until it is sufficiently fluid to drop nicely from the neck of an ounce vial. He prepares the objects in the usual way, soaks them in turpentine from a few minutes to several weeks, according to the nature of the object, then places them on the slide, drops the balsam on, covers with small round glass, and sets aside for some days; by the end of this time, which varies according to the nature of the object, all the air-bubbles will have made their way from under the glass, unless actually enclosed in the substance of the object, which indicates either too short time in the turpentine, or, in some cases, the absolute need for the use of an air-pump. The slides can be left in this state until these are sufficient to bake, which he manages in the following way:—He has a tin or copper box 12 inches square by $2\frac{1}{2}$ inches deep, flat on the top: this holds three dozen slides. He fills the box with water, places the slides on the top, and on each a flat bullet or large shot. He then puts a gaslight under the box, and keeps the water nearly boiling for about forty-eight hours: the slides will then be sufficiently baked, and may be cleaned and finished off by putting a ring of black or other varnish round the edge of the glass circle. He found the use of the spring clips very objectionable, as he was always getting too much or too little pressure on his slides; but by having various sizes of bullets and shot, he can put just the weight he requires on each slide. The bullets are flattened by striking them with a hammer. This process prevents all possibility of getting the balsam to the boiling point, and at the same time gives as much heat as is required. He has exchanged many slides with correspondents, who have, without exception, expressed a very favourable opinion of them. He should say that, if the object is very thick, and consequently the balsam thick round it, it should bake rather longer. The two funnels are merely to allow of the expansion and contraction of the water as the heat varies. His objections to Mr. Kitton's plan (though in his hands, no doubt, through practice it is successful) are that air, if it gets in, will be difficult to get out, instead of going out itself, as in his plan; that it affords no opportunity of carefully examining the object when on the glass to remove any dust or hairs; that the wire clip will often crush a valuable object, or not subject a strong object to sufficient pressure, and that the slide will sometimes get too hot over the lamp and spoil all the work.

## CORRESPONDENCE.

### Erratum in Mr. Tolles' Article in May Number.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—In your Journal for May last, p. 213, there is an error. Fourth line from the top, for "closest" read "At closed, to the extent of one-half of its whole adjustment."

Respectfully yours,

Robt. B. Tolles.

## Mr. Reuel (*not* Renel) Keith.

*To the Editor of the 'Monthly Microscopical Journal.'*

War Department, Surgeon-General's Office,
Dr. H. Lawson. Washington, D.C., *June* 17, 1873.

Sir,—In my article on the "Aperture of Object-glasses" in the June number, my friend Mr. Reuel Keith, of Georgetown, appears as Renel Keith. I suppose my MS. was not sufficiently plain, but beg that this correction may be made in your next.

Very respectfully yours,

J. J. Woodward,
*U. S. Army.*

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## Mr. Tolles' Objective.*

War Department, Surgeon-General's Office,
Dr. Henry Lawson. Washington, D.C., *May* 19, 1873.

Dear Sir,—Since sending my paper on "Angular Aperture," Mr. Tolles has sent me the $\frac{1}{10}$th measured by Mr. Wenham. (See 'Monthly Microscopical Journal,' January, 1873, p. 29.) Measured by the method mentioned in my paper, I get a balsam angle of 70° at the open point, and 84° when the lenses are closed *to the point of maximum angle*, which is reached some time before the screw collar is fully closed. Unlike Mr. Wenham I find this glass performs admirably at the point of maximum angle *provided the covering glass is thick enough.* I tested it on *Grammatophora subtilissima* in balsam under a cover $\frac{1}{75}$th of an inch thick, and obtained what I am obliged to call admirable definition. My friend Mr. Wenham must surely have used too thin a cover for the position of maximum angle, or he could not have arrived at the conclusions expressed in his note.

I have the honour to be, very respectfully,

Your obedient servant,

J. J. Woodward,
*Assistant-Surgeon U.S.A.*

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## Inexperienced Artists *v.* Experienced Ones.

*To the Editor of the 'Monthly Microscopical Journal.'*

78, King William Street, E.C., *July* 3, 1873.

Sir,—In the current number of the Journal, in Dr. Pigott's paper on "High-power Definition," page 21, the following sentence appears: "The drawing then taken by a lady of talent, *unaccustomed to the microscope*, was everything I could desire." This idea has been introduced repeatedly by the author, both in his written and spoken communications; and as the following quotation appears apposite, I venture to bespeak attention to it:—

* This letter was received about the 3rd of June, and should have appeared in the July number of this Journal had we thought for a moment that Col. Woodward intended it for publication. We have since heard from Col. Woodward, in answer to a communication of our own, who expresses his surprise at its absence from the July number. We therefore, with many apologies, insert it in the present Journal.

"It is supposed that nothing more is requisite for microscopical investigation than a good instrument and an object, and that it is only necessary to keep the eye over the eye-piece, in order to be *au fait*. Link expresses this opinion in the preface to his phytotomical plates:—'I have generally left the observations altogether to my artist, Herr Schmidt; and the unprejudiced mind of this observer, who is totally unacquainted with any of the theories of botany, guarantees the correctness of the drawings.' The result of such absurdity is, that Link's phytotomical plates are perfectly useless; and in spite of his celebrated name, we are compelled to warn every beginner from using them, in order that he may not be confused by false views. Link might just as well have asked a child about the apparent distance of the moon, expecting a correct opinion on account of the child's unprejudiced views."—Schleiden's 'Principles of Scientific Botany,' London, 1849, p. 584.

I should not have quoted the above, but for Dr. Pigott's evident fondness for inexperienced *versus* practised workers, to confirm his views.

I am, Sir, your obedient servant,

B. Daydar Jackson.

## The Woodward, Tolles, and Wenham Controversy.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—When Col. Woodward's paper appeared in your Journal, I wrote the following paragraphs as a commentary; but I deferred forwarding them, that Mr. Wenham, who was principally concerned, might in the first place answer for himself; and in part also from knowing that substantially my observations must coincide with his. As, however, I purpose making a subsequent communication on a more advanced part of the subject, I now forward my observations as made from my own point of view, so that it may be unnecessary to recur to this controversy in any future communication.

There are no errors, scientifically speaking, in Col. Woodward's paper, at least such as to require notice. And as his experiments confirm the results predicted by theory, and already verified in London, it may be hoped that we have now heard the last of this strange controversy.

But there is a serious error of an historical or statistical kind which cannot be passed without notice. Although an "object-glass" does not and cannot give more than the specified aperture, yet it is possible to construct an optical machine by which an indefinitely larger angle may be extracted. By a misapprehension which it is not very easy to account for, Col. Woodward, and the two friends who were called in by him, imagine that this latter fact was unknown to Mr. Wenham and to myself; and that the announcement of it will come upon us as a surprise. But nothing could be wider of the truth. The construction which they think new is old, well known here and familiar years ago—in fact, a mere commonplace. But it had nothing to do with the question in dispute. This question concerned not "possible" constructions, but object-glasses only. It was restricted to these by its very terms. Is the known superiority of immersion-glasses due to their greater angle? This was the question proposed. To this it was answered—the cause is certainly not the greater angle; for the angle is *not* greater, but confined necessarily within the self-same limits. On this point issue was joined; Mr. Tolles declaring that, whatever theory might say, as a matter of fact his own glasses could have and did have a much wider angle. In token of which he finally selected one of these glasses to be tested, the whole issue being staked on the result. This was the question, the only question, in controversy. There was no misunderstanding about it on either side, for the glass selected and sent was an *object-glass.* It was tested, as we all know; first in England, then "on appeal" in America, with the results which likewise we now all know.

In course of the discussion, however, Mr. Tolles did, no doubt, introduce, as a kind of "second string," the possibility of the optical machine referred to having an extra hemisphere, which he afterwards put together. But on this subject there never was for a moment any controversy. When introduced, I at once pointed out that this, though manifestly possible, was nothing to the present question. And Mr. Wenham added, that not only was it not the question, but that the construction, such as it was, was his own, published by him many years ago, and practically carried out for the very purpose of testing the effect of the increased aperture so obtained; in testimony of which he reprinted his paper in the January number of this Journal, where it may now be read. Yet, notwithstanding this double publication, by a curious inversion of the facts he has been credited with ignorance of his own invention; while Mr. Tolles, assuming the merit of the idea to himself, finds his claim allowed, as Cleon, in *The Knights*, threw out his rival by taking the cake the other man had kneaded, and serving it up as his own:—

*πανουργότατά πως παραδραμὼν ὑφαρπάσας*
*αὐτὸς παρέθηκε τὴν ὑπ' ἐμοῦ μεμαγμένην.*

The construction produced by Mr. Tolles differs in no way from the other, except that the extra hemisphere, which was formerly left unattached, he disguises by cementing it to the front of the object-glass.

When, therefore, Dr. Woodward writes that Mr. Wenham has "overlooked the possible case," I can only account for the mistake of this most eminent microscopist, by supposing that he has inadvertently omitted to read the article in the January number.

Such a structure—should it ever come into common use, which is not likely—will not be an "object-glass" in the ordinary sense of the word. To call it an objective "of four systems" is a misuse of language, because it implies a false idea of its structure. It implies that the four systems are systems in the same sense of the term, which is not true. A one-inch, *e. g.*, has two systems; and in the very same sense of the word a quarter has three, because the third system while correcting the action of the front leaves the conditions of vision exactly the same. And in this way we might have, if necessary, a dozen corrective systems behind these. But here the fourth "system" is not a system in this sense at all. It is placed in front of an already perfect object-glass, and thus destroys the previous conditions of vision; for example, its property of seeing objects in air or *in vacuo*. Whether united or kept in two parts, the structure will still be simply an objective *plus* a hemisphere in front.

It is to be observed that the results of this discussion are entirely negative. The question as to what is the cause of the superiority observed in immersion glasses has been answered only so far as concerns what is *not* the cause. At first it had been assumed, by a curious oversight in theory, that these glasses were exempt from the reduced angle of dry glasses; and that their indefinitely wider angle accounted for the difference. That both are reduced is the point which it has required so much discussion to establish. Now that such easy methods have been invented for the measurements, anyone can verify them for himself. For all really useful purposes a rough measurement is quite sufficient. A few degrees either way would not be of the slightest consequence, as these, even supposing them real and not apparent, could have no effect in accounting for the difference of performance. The real question remains—what *is* the cause of this observed difference, and how far on optical principles can we account for it. On this question—a much more difficult one—I purpose offering the results of my investigations in a future paper.

Your obedient servant,

S. Leslie Brakey.

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## The Use of the "Aquatic Nozzle."

*To the Editor of the 'Monthly Microscopical Journal.'*

Pennsylvania Park, near Exeter, *July* 12, 1873.

Sir,—I have long watched what is expressively called "the battle of the glasses;" but it appears to me that one very decided recommendation of the new (immersion) mode of using the highest powers, has not, as yet, been distinctly stated.

It is as follows:—

For many years past the makers have been working up the "angle" of achromatic objectives higher and higher; because the increased angle was supposed to have certain advantages; which may be the case. It has, however, one very great *dis*advantage, which is that as the angle is *in*creased, the focal distance is *de*creased; thus rendering it impossible to focus an object, unless the covering glass is of that exquisite thinness which the "preparers" are not always sufficiently attentive to use.

Hence the late Judge Tyrrell, the inventor of the first "finder," and who was a very intimate friend of mine, when I told him, many years ago, that I had a sixteenth to show him, expressed surprise, saying, "A sixteenth! I never yet saw one that was usable!"

I soon, however, convinced him he was "taking the thing by the wrong handle," as the saying is; and that he *ought* to say he had never yet seen *objects* prepared with sufficiently thin glass to enable the sixteenth to reach them. When that is the case, a sixteenth is just as "usable" as an inch, &c. But to continue. In process of time a great improvement was announced in the said $\frac{1}{16}$th; and as I always wish to keep up with the "march of improvement," I accordingly ordered one of the "last improved," which was extolled as having an angle of 175°!

Well, I found it *did* "perform" beautifully upon all that it could *reach:* but, alas! I had the mortification to find that about half of my extensive collection of Diatomaceæ were thrown out of use, as far as this tremendous 175° was concerned: and it became necessary to fall back upon the old and more moderately angled glass.

Consequently, all my high-power objects have long been labelled $\frac{1}{16}$th and O. $\frac{1}{16}$th; the latter implying *old* sixteenth.

Things were in this state when the new "aquatic nozzle" was invented; and on having one of them adapted to my new $\frac{1}{16}$th I had the pleasure of finding that the new mode not only brought out the diatoms with much more brilliancy, and also considerably greater amplification (converting a $\frac{1}{16}$th into about $\frac{1}{20}$th), but, best of all, it so much elongates the focal distance, that all my O. $\frac{1}{16}$th objects became perfectly usable, and much better seen than formerly.

Thinking the simple fact now stated may possibly be of use to others who have "unusable" sixteenths, I have ventured to send you this letter; and am, Sir,

Yours very respectfully,

HENRY U. JANSON.

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# PROCEEDINGS OF SOCIETIES.

## MEDICAL MICROSCOPICAL SOCIETY.

The fifth ordinary meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital on May 16th, at 8 P.M.; Jabez Hogg, Esq., President, in the chair.

The minutes of the last meeting having been read, and the names of gentlemen proposed as members having been announced, Mr. Atkinson read a paper on "The Preparation of the Brain and Spinal Cord for Microscopical Examination," which will be found in full at p. 27.

In the discussion which followed, the President considered that for staining, Beale's carmine solution diluted fourteen times with water was strong enough, and preferred rosaniline and logwood for staining. The use of spring-clips in mounting increased the chance of air-bubbles.

Mr. White thought bubbles might be avoided by using balsam dissolved in chloroform.

Dr. Pritchard agreed with Mr. White. Before dissolving the balsam in chloroform he dried the former at a temperature of 200° Fahr.

Mr. Paul was in the habit of placing his chromic acid specimens in glycerine to render them still harder.

Mr. Needham used the carmine solution reduced to one-fourth its strength: it stained best after being made three weeks.

Mr. Groves preferred blowing through a pipette upon the specimen for cleaning it, to using the camel's-hair pencil.

Mr. Atkinson, in reply, had found the carmine solution diluted seven times with water too weak. He thought rosaniline not a permanent colour. In his carmine solution he retained as much ammonia free as possible, while spring-clips he thought useful in dislodging air-bubbles.

Dr. Osler then read a paper upon "The Action of certain Reagents—Atropia, Physostigma, and Curare—on the Colourless Blood Corpuscles."

The reagents made use of were, a fresh solution of sulphate of atropia, a fresh solution of sulphate of physostigma, 1 per cent. strength, and a rather stronger solution of curare; a $\frac{1}{2}$ per cent. saline solution was used to dissolve them. In the case of newt's or frog's blood, about four times as much reagent as blood was made use of, while for human blood the proportion of reagent to blood was 5 : 1. The specimens were examined on a Stricker's stage at a temperature of 39° C. The experiments were undertaken to show, if possible, in the corpuscles the antagonism between the reagents, which had been already demonstrated by Dr. Fraser.

A solution of 1 part of sulphate of atropia to 2000 of water allows the normal amœboid movements of the corpuscles, while a 1 to 3 per cent. solution definitely alters the form and structure of their processes; for it is in these that the changes noticed lie. Generally in about ten minutes the corpuscle is seen to throw out processes, bud-like, long and thin, or tuberous; the number of processes being indirectly as their size, while the outline of the corpuscles may change two or three times in a minute. Sometimes the processes are retracted, but not always, and they may remain without any change of shape, while some corpuscles in the field never alter nor move at all: all, however, retain their spherical form. The processes are mostly hyaline, but sometimes granular, and have a sharply-defined line where they join the body of the corpuscle; a fusion of the granules they contain may restore their original transparency. The phenomena described do not always occur upon the addition of the reagent, being sometimes more evident than at others.

A number of experiments were here narrated in detail, but of which it is impossible to give an abstract, showing the action of atropine on the corpuscles; but the result was to the effect that all motion ceased in the corpuscles, on the application of the reagent, sooner in the blood of the newt and frog than in that of man, and sooner also the stronger the solution used.

The blood of frogs and newts poisoned with atropine showed normal amœboid movements without any modification whatever.

The action of physostigma is somewhat different. A solution of the strength of 1 to 800 of water allows the normal movements of the white corpuscles. A solution of 1 to 1000 of water stops all motion in two hours; while one of a strength 1 to 300 of water, all but completely prevents the formation of processes, and causes the movement to be of an undulating and heaving character; a rather stronger solution produces changes the same as atropia. As a rule, less corpuscles are affected by a given amount of the reagent than in the case of atropia.

The red corpuscles are changed by a 1 to 2 per cent. solution of the reagents, their surfaces become irregular, from involutions and cuppings of the surface; but scarcely two corpuscles are affected alike.

The explanation of the changes above mentioned is difficult: that they are of a vital nature seems certain; the hyaline processes strongly reminding the observer of some of the Pseudopods in the Rhizopodæ. The normal prolongations of a white corpuscle are formed of its hyaline substance (protoplasm), together with the granules it contains: but these resulting from the application of atropia and physostigma are free from granules: similar processes can be seen in the yolk spherules of the Batrachia. The result of these experiments would show that no antagonism exists between atropia and physostigma, at least as far as their action on blood corpuscles is concerned: and in proof of this, blood treated with the reagents mixed showed just the same changes as when used separately.

Experiments to show the action of curare upon blood corpuscles produced only negative results; the normal movements going on as usual: yet where a ½ per cent. solution was used these ceased in ten minutes.

The President remarked that such observations as Dr. Osler's might increase our knowledge of the action of drugs; and referred briefly to the microscopic observations made recently on blood corpuscles in syphilis.

Dr. Payne then read a paper "On Certain Points in the Histology of the Omentum."

The fenestrated portion of the human omentum consists of fibrous bands or trabeculæ, in which are embedded connective-tissue corpuscles, and on which is spread a continuous and most uniform layer of endothelial plates: it is with the latter that the present notice is concerned. The best mode of examining them, that of staining with silver, is generally inapplicable in the human subject in consequence of the time which elapses before examination is possible, but the structures can be very well seen either without any reagent at all or after staining with carmine. The attention of the author of the paper was first drawn to the subject on examining the omentum in persons dying of acute tuberculosis, with miliary tubercles in the peritonæum. In these cases were found, around the tubercles, epithelial cells in various phases of change: some with nuclei, some almost divided so as to show two cells, and some groups of cells, the shapes of which showed they had been produced by cell division or multiplication. These have been described by several authors (Rindfleisch, Kundrat, &c.) as showing the origin of tubercle. There were also seen large compound cells, like "myeloid or giant cells," and small masses of adenoid tissue.

Similar proliferative changes are seen in acute inflammation, and the appearances in the neighbourhood of small cancerous growths are likewise very similar. In the one case they have been recognized as a source of pus cells; in the other, of new cancerous growth.

The important fact, however, is that appearances just like those described above may be found in the normal omentum, *viz.* evidences of cell proliferation, many nucleated or giant cells and masses of adenoid tissue. It appears then that the morbid changes that accompany inflammation (as well as the formation of tubercle) are not only essentially alike, but are identical with processes that are always going on in the omentum, and not indicative of any special disease.

The inflammatory changes or those of specific diseases differ from the normal chiefly in their greater abundance and activity, and are doubtless simply due to hyperæmia and consequent increased nutrition. It is probable that appearances, which are strictly normal, have sometimes been described as those of disease.

In consequence of the late hour no discussion followed.

The President, having announced the next meeting for June 20th, at 8 P.M., the meeting resolved itself into a conversazione.

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The sixth ordinary meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital, on June 19th, at 8 P.M.—Jabez Hogg, Esq., President, in the chair.

The minutes of the last meeting having been read, the President read a paper on the "Histological Difference between Croup and Diphtheria." The paper is published in full at p. 78.

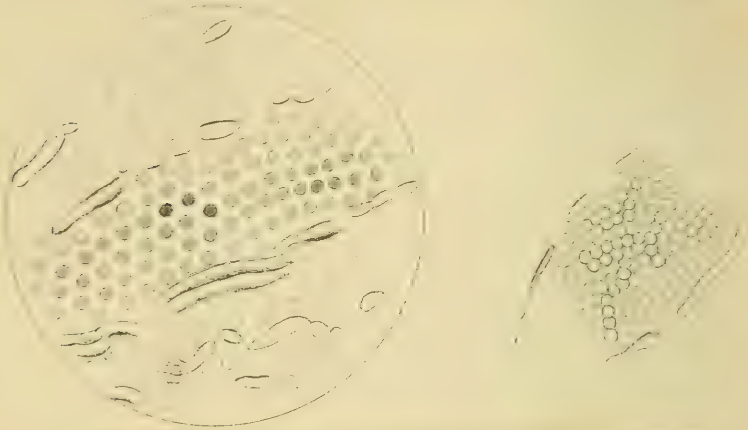
In the discussion that followed, Dr. Pritchard considered that the presence of the *Oïdium albicans* in diphtheritic membrane indicated only a deteriorated condition of the blood, as it was to be found at times in cases of blood poisoning. He also thought that inclusion of the nerve fibrils in a mass of inflammatory tissue scarcely sufficient to explain the paralysis of diphtheria.

Dr. Bruce, in cases of croup that he had examined, had never found epithelium in the membranous exudation, as Mr. Hogg had described; but had observed infiltration of the sub-mucous connective tissue with exudation cells.

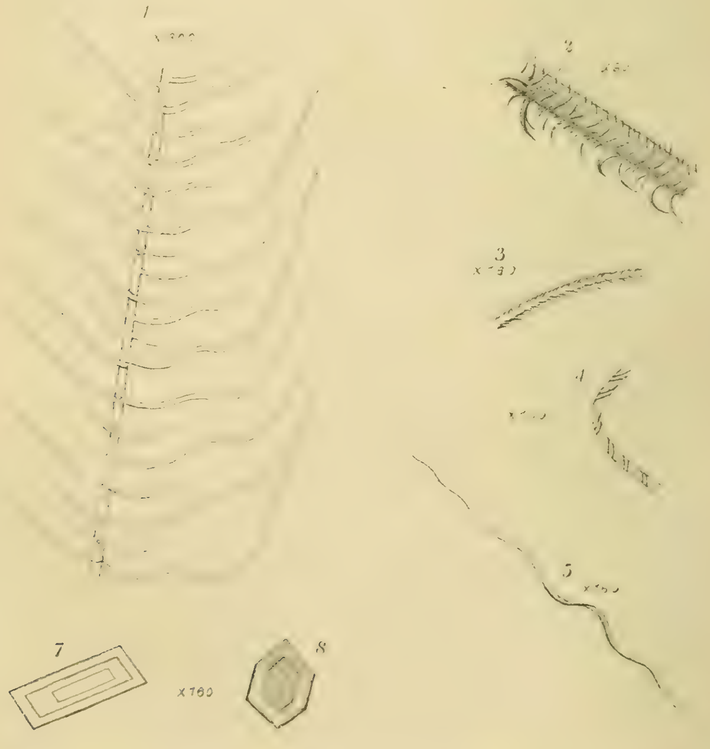
Mr. Golding Bird agreed with the last speaker as to the presence of exudation cells (white blood corpuscles) in the croup membrane; and these he had noticed arranged in a linear manner of two or three deep between the corrugations of the apparently structureless membrane. He had never noticed epithelium.

The President, in reply, stated that his specimens had been chiefly obtained from patients by means of emetics, and was almost inclined to agree with a member who had stated that another epidemic of diphtheria was needed before the histological differences between croup and diphtheria were fully understood. He thought the paralysis of the latter affection owing to disintegration of the nerve fibrils, and not to pressure.

The meeting then resolved itself into a conversazione. The President exhibited specimens of croup and diphtheritic membranes, illustrative of his paper.



Beads of Crushed Podura Degeeria.




Organic bodies in fire-opal

THE

# MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1873.

## I.—*On Organic Bodies in Fire Opal.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Taken as read before the* Royal Microscopical Society.)

Plate XXVII. (Lower portion).

Some time since I was indebted to W. G. Lettsom, Esq., for a slice of fire opal from Zimapan, in the centre of the Northern Mining District of Mexico, in which he told me I should find a number of crystals and other curious bodies.

The crystals were scattered about in various positions, and were most numerous in portions of the slide that did not contain many organic-looking objects. I am told they have been recognized as *Tridimite.* They are, as Figs. 7 and 8, Pl. XXVII., show, hexagonal, and, according to Von Rath's analyses, Tridimite is composed of silica with a little oxyd of iron, alumina, and magnesia. The following closely-corresponding analyses are taken from *Poggendorff's Annalen*:*—

| | | |
|---|---|---|
| Silica acid | 96·1 | 95·5 |
| Ferric oxyd | 1·9 | 1·7 |
| Alumina and magnesia | 1·3 | 1·2 |
| Loss | 0·66 | 0·66 |
| | 99·96 | 99·96 |

The opal slice exhibited several cracks and flaws, readily distinguishable with almost any illumination, but there were numbers of objects, sometimes scattered irregularly and sometimes grouped together, that did not present either aspect, but which might be taken for minute veins filled with a whitish material. Most of the aggregations had a more or less cylindrical appearance, such as might be imitated by drawing a number of curved lines at approximately equal distances, and leaving irregular gaps at the bottom. These might have been taken for purely mineral formations, and indeed were so by some experienced mineralogists; but in parts a more organic character could be noticed, and by diligent search throughout the slide and comparison of various objects, it appeared that the complex and unintelligible forms were composed of simpler ones, more or less injured and crushed together. Fig. 1 represents

* cxxxv., 1868, pp. 437-454.

one of several specimens isolated from others, and in which an organic structure will scarcely be denied. It will be seen that a number of branches spring from a slender upright stem, usually exactly opposite, but occasionally alternate. At the point from whence these branches spring there is a slight thickening of the stem, scarcely amounting to a knot, and no positive structure of any kind can be discerned either in branches or stem. In examining the slide, whole branches and fragments were frequently seen separate from any stems, and in some of the compound masses the stems were often wanting in places, or too indistinct for certain vision. Fig. 1, Plate XXVII., is magnified about 300.

Another formation, as shown in Fig. 2, has a decidedly organic aspect. It is a stem with short hair-like branches in more or less confusion, × 90. Fig. 3 is an exceedingly delicate formation, like a slender feathered shaft, × 160.

Fig. 4 represents a double spiral, but this could not be seen sufficiently well to be quite certain of the real shape, × 160.

Fig. 5 represents one of many threads of various lengths with a spiral twist, × 160.

Fig. 1 must not be taken as representing a complete object; it is only a portion of a much longer one, and the slide gave no certain indications of how great the full length of this and other objects might be. In point of size Fig. 1 occupies a mean place, some similar objects being twice as big, and others much less.

To the question, What are these things? the writer is not in a position to give any determinate answer. Their aspect is that of minute vegetable fossils, possibly Algæ, but this is merely conjectural. The object of submitting this paper to the Society is to draw the attention of others who may have opportunities of observing slices of fire opal from Mexico or elsewhere, to the probability of their discovering objects similar or diverse, but which may throw further light on the subject. It has been thought better to give no name to the objects until their nature is understood.

Opals are hydrates of silica, and there is nothing in their composition or probable mode of formation that renders their containing fossils improbable. Dana states: "Opal consists of soluble silica and 5 to 12 per cent. of water."

Several specimens of Fig. 1 are so placed in the opal slice as to admit of very distinct vision. Fig. 2 represents the plainest piece of that form. Some like it of considerable length were bigger, and others less.

No objects of the kind could be found in a slice of precious opal in the matrix from another part of Mexico.

## II.—*On the High-Power Definition of Organic Particles. No. II.*

By G. W. Royston-Pigott, M.A., M.D., F.R.S., &c.

Plate XXVII. (Upper portion).

The definition of a minute organic particle, as a subject of discourse, can never be exhausted till the microscope has reached the last stage of improvement: probably in generations to come.

To a great many microscopical people the denial of such definition is their stronghold, or citadel of defence. Errors and imperfections have for a long time prevented their definition.

To declare, once for all, that the "tadpole markings" of the Podura can only best be seen according to the notions of object-glass makers when the glasses are spherically over-corrected, doubtless will be received as rank heresy. This unfortunate error in the making and testing of glasses pervades all Europe and America. If a standard is all wrong, what becomes of the test? Galileo saw, instead of Saturn's ring, a planet with two smaller ones touching it: that was all he could do with his antiquated opera-glass.

Mr. Stephenson has given us a splendid example of what can be done by departing from the usual "humdrum" of work. Boldly introducing the principle of developing new powers of refraction in structure by choosing a refractive medium of the highest refractive index; we see how much the defining power of the microscope has been exalted. Professor Walcott Gibbs constructed spectroscopes with a strong solution of phosphorus in bisulphide of carbon, and obtained vastly superior powers of resolution in the spectra. In the true philosophic spirit, we should hail all attempts to detect error and elucidate truth with no niggardly hand, and the thanks of our Fellows are warmly due to our collaborateur Mr. Stephenson. Some three years ago I gave or lent Dr. Maddox a slide in which I had rendered minute particles much more distinctly visible by introducing a naphthaline solution of india-rubber; and the very low refractive index of the glass spicules was described in the 'Quarterly Journal of Microscopical Science,' supplied kindly to me by Dr. Bowerbank.

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EXPLANATION OF PLATE XXVII. (Upper portion).

*Beads of Crushed Podura Degeeria.*

Fig. 1.—Appearance of Crushed Podura Degeeria Beading, upper set.
„ 2.—Appearance of Crushed Podura Degeeria in another portion of the same scale.

The colours of the beading varied with the slightest change of focus, three different colours being distinguishable at one time in the same field. Exhibited to Mr. Slack, Sec. R.M.S., F.G.S., at my house, latter end of July, 1873.

I here, however, wish to record an observation made lately in the presence of Mr. Beaumont, F.R.S., 33, Norland-square, and copied by Mr. Hollick, Plate XXVII., upper portion, (for whom I had sent) on the spot. Exhibiting the appearance (described by a little girl, who had never looked down a microscope before, as like "strings of seed pearls"), I accidentally came upon a heap of what appeared to be a crushed Podura scale (of the coarse kind). Mr. Beaumont expressed his astonishment in no measured terms. A large portion appeared distinctly studded over with closely-packed spherules resembling the finest conceivable definition of the *Angulatum*.

The covering glass is very strong and thick, and had undergone some kind of pressure, as often happens (when the glass is too thick) in the endeavour to focus down.*

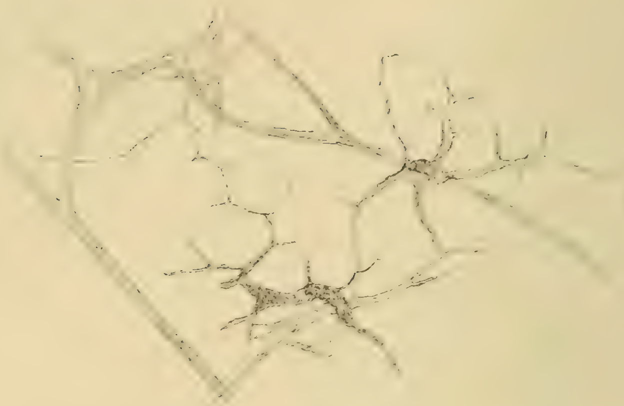
I had got the finest definition of the black edging of the sides of the ribs I had ever seen, with orange beads between, when on moving the slide about I could hardly believe my own eyes. Two layers of beading had apparently been compressed into one in the manner of two layers of small shot, arranged with the same regularity, as shot kept in contact. These beads when seen at their best were dark blue, and some of them appeared golden-orange. All the crushed structures had sharp black outlines. Apparently, as Mr. Beaumont pointed out and Mr. Hollick copied, some long tubular-looking bodies scattered about, with extremely jet-black outlines, contained several beads.

I have found a good effect from using Rangoon oil; also the chloride of gold, dissolved in glycerine, 2 grains to the drachm.

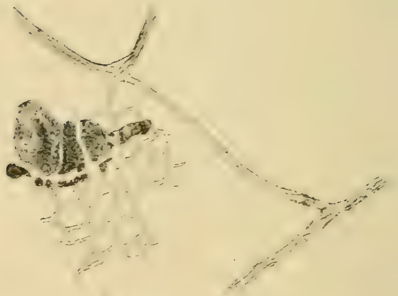
But the strong fact was this: the usual correction for seeing the "tadpoles" at their best failed to exhibit distinctly those beads, and the other beautiful effects. Therefore to see the minute structure at its best, required, as I am prepared to prove, a different correction—a diminution of the spherical over-correction before established.

* A Dalmeyer *twelfth* penetrated easily through this glass cover; but a twelfth, marked a Ross, would not at all.


1.



2.



3.

4.

W. West & Co. lith.

## III.—*On the Apparent Relation of Nerve to Connective-tissue Corpuscles, &c., in the Frog-Tadpole's Tail.*

By R. L. Maddox, M.D., H.F.R.M.S.

Plates XXVIII. and XXIX.

Last year I recorded in the pages of the 'Monthly Microscopical Journal' (vol. viii., p. 101), a few of many experiments made on the common Frog-tadpole, to try and preserve the tissues without much loss or change in their natural appearance; not only for the purpose of ordinary observation, but also for photomicrography. This year the experiments were renewed in different ways, yet on the whole, since mounting, the preparations do not appear very satisfactory. Some of the specimens prepared after the previously described plans, especially 6 and 8, kept very fairly, others not as well, the tissues softening much in the acetate of potash; it is feared this mounting medium was rather strongly alkaline.

The purpose of the present communication is to note the appearance of the apparent connection of some of the nerve branches with the connective-tissue corpuscles, also a fine network of rather large meshes, situated upon and near to one of the cutaneous gland cells, &c.

A careful examination of a very large number of specimens, mounted after various methods of staining and preparing, was made for the purpose of endeavouring to satisfy myself, whether any intimate relation could be found between any of the small branches of the cutaneous nerves and the connective-tissue corpuscles in the tail of the tadpole, as I believe is stated by Eberth, and as has been pointed out to exist between these two tissues in the cornea, by Kuhne, and verified by Moseley, who gives figures of the same in the 'Quarterly Journal of Microscopical Science,' vol. xi., p. 262.

Three different examples were remarked by myself where there appeared at least if not fusion, a very close union. The largest and

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EXPLANATION OF PLATES XXVIII. AND XXIX.

Plate XXVIII.

Fig. 1.—Shows a nerve branch of a considerable size, forming union, if not real fusion, with a large elongated connective-tissue corpuscle; several vessels, connective-tissue corpuscles, pigment cell, and cutaneous gland, are given to show the general relations.

Plate XXIX.

Fig. 2.—Towards the top of the figure a nerve branch is seen passing *apparently* through the body of a connective-tissue corpuscle.

" 3.—A nerve network lying upon and close to a cutaneous gland cell.

" 4.—A delicate subepithelial nerve network, with two small connective-tissue corpuscles lying between the larger nerves; and a pigment cell to the right hand. In the branches of the fine network is seen a small ovoid cell, the distinct relation of which to the fine branches could not be satisfactorily determined.

most distinct is given at Fig. 1, where a moderate size nerve branch is seen coming off from a larger trunk, and running for some distance amongst the tissues, until it seems to thrust itself against the body of a very marked connective-tissue corpuscle and there stop. In the other cases the nerve branches were much finer, though apparently as perfectly united to the corpuscles.

Finding these occurrences so rare, and as Nature does not generally deviate from unity of plan, or is mostly true to herself, I felt much hesitation in accepting these appearances as affording examples of perfect union between the substance of the two tissues.

It may be argued in the figure given, this is a case where the nerve branch has returned upon itself, for at the apparent junction the nerve is seen to be blunted and larger than on the other part of its course; also at about midway of its length is a very distinct loop, one side of which may be formed by the returning fibres escaping from the sheath for a short distance, and then re-entering it to pass back to the main trunk.

As these cases are so few, we have to be guarded in our conclusions, but the greater number of instances that can be furnished may at last establish these relations as a rule. As the parts had not been disturbed by tearing asunder the tissues, the relationship had not been interfered with, and no compression had been used, for the previous loss of many slides, by endeavouring, through slight pressure, to bring out more clearly some obscure points, generally ended in rendering the specimen worthless.

In Fig. 2 is represented a nerve branch passing through a connective-tissue corpuscle, or if it do not pass through, the nerve and corpuscle appeared to be in absolute contact by their outer surfaces; the latter view I expect is the more correct one. In these often flat-branched structures, even the most careful focussing with the use of high powers, owing to the refraction of these bodies themselves, will sometimes not admit of a positive conclusion, for the underlying object or nerve branch may have so imbedded itself in the overlying part as regards the plane of vision, as to be virtually in its centre yet beneath its boundary.

In the figures given by Moseley in the afore-named Journal, little doubt can be left of the real fusion of the two elementary tissues; hence this is a point well worth research, the result of which may be to invest the connective-tissue corpuscles with an office higher than some are disposed to attach to them, and show how they may be directly influenced by any nerve current, and at the same time relieve them from the suspicion of being only post mortem effects.

These remarks are not intended for a moment to throw discredit on the observations of others, for opinions are much divided amongst excellent observers on these points. Dr. Beale, in his paper "On the Relation of Nerves to Pigment and other Cells or Elementary Parts" (vol. vii. p. 45 of this Journal), renounces the view held by others, and I believe Dr. Klein also does not admit the intimate union or fusion of these tissues in the cornea. On the other hand, Pouchet has given us some good observations, in the Journal for 1871 (vol. vi., p. 285), on the connection between nerves and chromoblasts. He states that in all cases the tissues were stained with chloride of gold, and speaks only of the specimens prepared according to the directions given at p. 286, "without asserting the existence of a *direct* relation between the nerve fibres and the chromoblasts," yet the characteristic appearances given in the figures point rather to the affirmative.

It seems hardly feasible to attempt to reason upon what is or may be the action of the nervous influence on these or the connective-tissue corpuscles generally, until we have more unity of opinion as to the true nature of each of these interesting little bodies. Possibly by researches drawn from creatures lower in the scale of life, as in the transparent marine mollusca, &c., and upon a far greater number of instances than are at present cited, a correct hypothesis of the relationship between these tissues may be established.

At Fig. 3 is given the appearance of a subepithelial plexus of fine nerve branches lying upon and near to a cutaneous gland cell. The closest examination failed to discover the entrance of any of the branches of the network entering the substance of the small cell, nor have I been more successful in some sections made through the large cutaneous cells situated in the skin of the frog near the nares. Eberth, I believe, first pointed out the nerve plexus to the cutaneous glands of the frog.

Fig. 4 represents the appearance of a very fine subepithelial network, carefully drawn (though without the aid of the camera lucida, as employed in the other figures), and which I take to be the same as described by Dr. Klein, and beautifully figured in his 'Beiträge zur Kentniss der Nerven des Froschlarvenschwanzes,' for a reprint of which I am indebted to his kindness.

It was only after the examination of a very large number of specimens of the tail of the tadpole, also of the young smooth newt, that here and there distinct views of this network were obtained, and even then I must confess it considerably taxed the patience. In most of the recent specimens prepared somewhat after the methods described last year, the minute terminal network or anastomosis of the very finest branches of the connective-tissue corpuscles appeared to be largely mingled with this network, and to prevent securing as clear views of it as desired; but after gold staining, adopting the plan of immersing the tail in absolute alcohol acidified with acetic acid, as directed by Moseley, larger clear spaces were procured where this network would be more distinctly seen, and from one of which the above figure was drawn. In the preparations made without the use of gold, the fine nerves were most easily seen by suddenly lifting the focus a trifle by the fine focussing screw, when these fine threads became apparent by the peculiar refracting power of their material; then by carefully adjusting the illumination they could be refocussed for more easily. A few days since I tried to photograph this network, but did not succeed then to my own satisfaction, otherwise I had intended to forward a figure from the negative instead of the above drawing.

Some one had recommended the use of a solution of hydrate of chloral in the examination of some kinds of living specimens when under the microscope; adopting the hint, I may here state, that I found it to answer remarkably well in quieting the movements of the tadpole, in the strength of 5 grains to 1 ounce of distilled water. For insect larvæ and infusoria its application generally quickly renders them quiet, and no doubt to those studying the rotifers in which the motions are so exceedingly rapid this solution, of possibly a different strength, would prove most useful. I may here also note that it was adopted successfully in the strength of 10 grains to 1 ounce of water on a large slug, for it is difficult to kill these creatures by ordinary means so that the tissues shall remain in an extended state; indeed the mollusca generally become very contracted when immersed in any stimulating fluid for the purpose of killing them quickly; so possibly this article may be a more or less merciful means of treating these and other creatures, without interfering with the normal appearance of the tissues.

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## IV.—*On a New Sub-stage for the Microscope, and on certain Appliances for Illumination.* By Edwin Smith, M.A.

The microscope to which I have recently added the very convenient improvements in question is a Crouch's Students' Binocular, with circular revolving stage. In the first place, I have found much advantage from a second diaphragm behind the one usually accompanying the Webster's condenser. Some such condenser is indispensable, if an equally illuminated field is to be given to both eyes, with a ⅔-inch or ½-inch objective. The *two* diaphragms work together to afford a great range of effects, both for spot-lens and for ordinary thorough light. Two large openings being brought opposite to each other, give perfect command of the secondary stage below without, my having to remove the condenser, except the doublet-lens, which I have made to take out, without unscrewing, through the large aperture of the revolving stage itself. This simple arrangement enables me to pass from one kind of illumination to another, or from the upper to the lower stage, with the greatest facility. It is well known that the condensing lens, which is a help with a $\frac{2}{3}$-inch or $\frac{1}{2}$-inch objective, especially if used binocularly, may be worse than useless for lower powers, say a 2-inch, except when required for dark-ground illumination; and ought therefore to be capable of easy removal, independently of the perforated diaphragms, which are always wanted ready in their place.

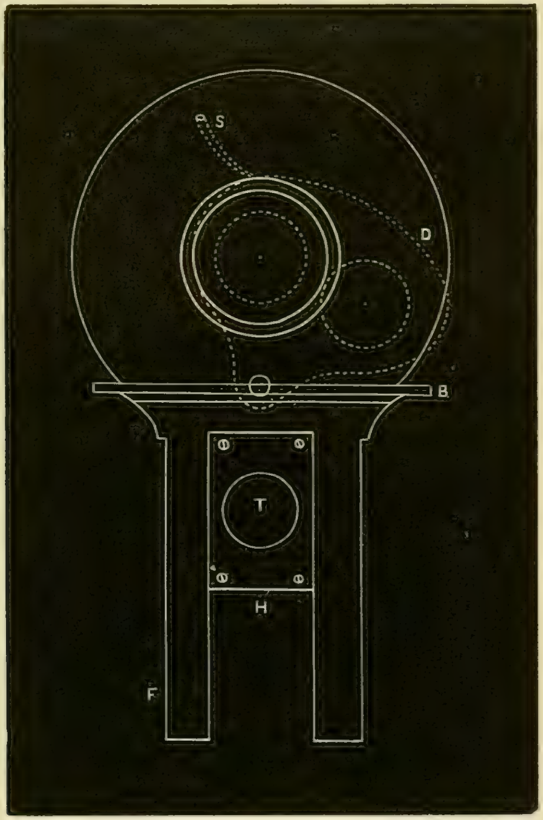
Secondly, my purpose in adding a sub-stage was to gain racking space for the use of a 4-inch or 5-inch objective, and at the same time to provide for the quick resort to the polarizing apparatus whenever it might seem desirable, or to the light-modifier, which is such a comfort when a lamp is employed. The sub-stage slides by a short tube upon the bottom tube of the main stem of the stand, and has a deep notch which firmly nips the square part of the stem and keeps everything duly centred. The brass plate, on which objects are supported by a sliding fork and bar, is circular, of the same diameter as the revolving stage above, and concentric therewith, having itself a central aperture $1\frac{1}{2}$ inch across. Three diaphragms turn one under another upon a fixed pivot below the plate, each diaphragm having two apertures rather more than $\frac{7}{8}$ inch in diameter. These apertures are furnished with shallow revolving cells, and one of them carries the polarizer. The shape of the diaphragms is something like the sector of a circle, and is such as to avoid needless weight, while allowing the whole set to be turned aside, so as to leave the large opening of the sub-stage quite clear. The upper edge of each diaphragm is the segment of a circle, and slides smoothly past a catch-spring which serves for a click. The diaphragms, each having two apertures, are fitted as follows:—the one nearest the mirror with a Nicol prism as polarizer, and a light-modifier of pure blue glass; the second with a plano-convex lens and a neutral selenite; and the third with two selenites respectively blue and yellow, and red and green. All revolve by means of milled collars. They can be combined in various ways with ease, as each selenite may be used alone, or the neutrals may be combined with either of the other two, or all may be turned aside, and the polarizer alone left, or light-modifier, as may be required. The plano-convex lens appears to improve the performance of the polarizer with low powers; and it suggests other uses which I have not yet worked out.

Thirdly, it still remained to provide for the analyzer. A short Nicol prism seemed on the whole the best, if only it could be quickly applied or removed, and if it could be so placed as to give the best effect in association with the Wenham prism. I also wished to retain the advantage of the Brooks' nose-piece, without any tiresome screwing and unscrewing every time the analyzer had to be used. I managed the matter in this way. The short tube which attaches the nose-piece to the body is just long enough to hold the tube which carries the analyzing prism, and allows the latter to be slipped in close up to the Wenham prism, and rotated by a slightly projecting milled collar. Room for this collar is at once obtained by filing down that of the nose-piece attachment. When the analyzer is not wanted, it can be easily drawn out and replaced by a plain tube for ordinary work. The object-glasses are now as near to the Wenham prism with the analyzer on as without it; and the fields for both eyes can be completely illuminated either with common or with polarized light. The definition too is good; but, if the operator desires, he can still place the analyzer by means of a simple fitting above the eye-piece as usual, sacrificing, of course, the binocular view. This, however, is a change which he will not often care to resort to when the definition is so good without it. For full illumination, it is sometimes well to interpose the stand-condenser between the light and the mirror, so as to obtain a straight strong beam. It should also be remembered, that with any power higher than a 1-inch, it is desirable to employ the upper stage in combination with the Webster condenser. This illuminates the field for both eyes equally. By the above arrangement I can use the polarizing apparatus *binocularly* with every power from a 4-inch up to a $\frac{1}{2}$-inch, and obtain in each case a thoroughly satisfactory result.

For white-cloud illuminator, in conjunction with the stand-condenser placed so as to concentrate the light upon it, I use a hollow disk of roughened porcelain, attached by a pivot to the margin of the ordinary mirror, and capable of being turned aside when not wanted.

For diffused light I simply grind upon a slab of sandstone *one side* of the glass chimney of the lamp. This side can readily be turned towards the mirror when desirable.

Lastly, permit me to direct attention to the convenience of the Morris' object-holder for showing with low powers the iridescence upon an insect's wings, and other points of interest requiring a certain slope to reveal them, and which cannot always be displayed in the mounting. But if the ball-and-socket joint be made a little larger, it can be *perforated* so as to allow of transparent illumination, while the steadiness of the holder is increased. For the practical carrying out of the preceding improvements I am much indebted to the intelligent co-operation of Mr. Selby, of the firm of Gray and Selby, opticians, Nottingham, who will be happy to make similar apparatus upon my patterns at a reasonable cost. I shall be glad to render my humble and friendly assistance to anyone who may desire to test the utility of these aids to practical microscopic work.




Sub-stage:—D, position of one of the diaphragms when in use. S, catch-spring. B, bar, sliding up or down by a fork F, which is held by the bevelled edges of the fixed piece H. T, short tube by which the stage is attached to the microscope.

NOTTINGHAM, *Aug.* 11, 1873.

## V.—*The "Colour Test" and Dr. Pigott.*

By F. H. Wenham, Vice-President R.M.S.

In the last issue of this Journal we have another characteristic Podura reiteration by Dr. Pigott. As I imagine that the number of believers in his ideas of structure is as small as the subject, I have not been inclined to notice that already controverted, and the patience of microscopists must eventually become exhausted by such repetitions of the same theme. As he, however, appears to claim without foundation some remarkable discoveries, I venture upon a few reminders relating to this theory of beads and accompanying colours.

He says that having had the "boldness" in 1869 to state that in the best glasses there is a certain residuary aberration, "this raised a storm of opposition hardly yet subsided." This assertion is not borne out by the facts, and is quite ridiculous. No such storm was raised, to subside at last into a tranquil acquiescence of any discovery whatever.

It may be remembered that after several of these reappearances, I, as nearly the only opponent, noticed his essays, and questioned the inferences alone that were assumed, never doubting that object-glasses were improvable, as my papers and work in this direction testify. My argument was that Dr. Pigott had devised no method of any utility for deciding such errors, and that his inferences were drawn from an erroneous interpretation of the structure of known test-objects; and when he further ventured to publish some large diagrams, I asserted that these were evidence that he was not even optically acquainted with the construction of object-glasses that he would have us believe he was the means of improving.

As regards the now much-vaunted colour-test, I acquiesce in its value, though it is no new feature; for, in fact, this style of correction is *coeval with the application of the single-front lens*, introduced by me more than twenty years ago, with the statement to opticians that this appearance of ruby-tinged prominences on a pale-blue or green ground was the criterion by which object-glasses would be found to bear the highest eye-pieces, even with an elongated draw-tube. The glasses which demonstrated this are in my possession still. This colour correction was not then liked. It was I that had the battle to fight! Nothing but the most colourless appearances, as that given by the now nearly obsolete triple-front, found favour. However, in justice to Mr. Andrew Ross, I may state that he at once so far adopted my views, as to make a number of $\frac{1}{8}$ths with a single-front lens, which, being very fine, had a good run at the time, but as they were expensive to make, having three achromatic systems behind the single front, they were abandoned for commercial reasons for the more simple form.* Mr. Ross explained to me at the time that his reason for adopting the "supplementary" achromatic was in order to get rid of some of the reddish tinge of the Podura markings, and approach nearer to the then approved black-and-white style, conspicuous in what are sometimes nicknamed "wishy-washy" glasses. At that time, though writing scraps relating to microscope matters, I said nothing concerning object-glasses, because information was entrusted to me that I had then no right to divulge. I am of opinion that still further improvements may arise with a different quality of glass, and am in daily expectation of receiving some *crown* now in manufacture, of a new composition, having but little spectrum, or, in other words, relatively of a very high refractive and low dispersive power; and where there are four elements of crown to be corrected by one of flint, the advantages should be correspondingly great. If secondary spectrum is to be corrected by any peculiar composition forming the negative element, we should properly begin by having as little of it to deal with as possible. I have long been aware of the Harcourt Titanium glass, but hitherto have not been able to obtain an atom of it.

After passing over the region of self-quotation and inverted commas in Dr. Pigott's paper, a phrase appears in the following sense:—"If anyone will give P. double price—the same as charged by T., I have no doubt that P. would be able to produce a glass proportionately improved." If Dr. Pigott really knows what the aberration is, and the means of correcting it, he ought in the interests of science to state it, or persons inclined to criticize might consider such sentences more appropriate for a trade prospectus than a scientific paper.

The mere assertion that there is a "certain residuary aberration" is unsatisfactory, and seems to have been raised from the region of phantoms, and its shadow-form is the result of a wrong interpretation of structure from illusory beadings. These Dr. Pigott has great skill in displaying as a reality, enhanced by drawings made by persons who may be clever in the ordinary use of the pencil, but clumsy and inaccurate in the delineation of microscope subjects, for the want of that knowledge of interpretation and discrimination of structure which alone can constitute the micro-draughtsman an artist; and yet such persons were first employed on the plea that they were free from prejudice?

* For the information of those possessing these particular $\frac{1}{8}$ths, I may state that I have found that by approximating the same front lens by the adjustment that they also act as immersion lenses, the water film giving an advantage in definition and increase of light. Mr. Ross was not aware of this at the time that they were made, for the value of the immersion was not then known or altogether ignored.

I now refer to the culminating point that Dr. Pigott alone has the "good fortune" to discriminate so decidedly—that the note markings on Podura are "spurious," and an optical illusion caused by the oblique crossing of "rouleaus of beads" on opposite sides of the scale. We know how this has been doubted, and I can now refer to scales torn in all manner of ways, to ribs twisted round, to scales treated with reagents, or seen where blotches of dried-up gum have partly encroached upon the markings. We have scales shocked to bits by Leyden discharges, and finally my friend Mr. J. Beck has taken the pains to show to numbers of persons the curious influx and efflux of moisture caused by breathing on the scale, the evaporation proving directly to the sight not only that the markings are real longitudinal ribbings, but as elevations existing mostly, if not entirely, on one side of the scale. As such proofs are quite ineffectual with Dr. Pigott, who in the face of them all still insists upon claiming a credence upon which his aberration is based, I may say that I know not one microscopist of any note who has investigated the subject that believes in him. If such a *rara avis* is to be found, then I must withdraw this remark, and invite him to come forward in a discussion that is futile with Dr. Pigott, who, without some such direct testimony, cannot secure belief in his creed by mere repetitions only of the same thing. I do not offer this as a defiant challenge, but for a temperate argument with one willing to accept and not suppress the truth. I believe, as I always have done, in the form of the Podura ribbings, and when such sentences are applied as a consequence of disputing the bead illusion and maintaining the rib structure, that "common sense at once revolts against accepting this appearance as even a rough approximation to the truth," those whose investigations have given them a right to hold a different opinion must necessarily object to such dictation, and feel disposed to controvert theories forced upon them in this style.

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## VI.—*Experiments on the Development of Bacteria in Organic Infusions.* By C. C. Pode, M.B., Demonstrator to the Regius Professor of Medicine, and E. Ray Lankester, M.A., Fellow and Lecturer of Exeter College.

The following passage from Dr. Charlton Bastian's 'Beginnings of Life'* induced us to make experiments similar to those mentioned in it, with the view of testing the correctness of Dr. Bastian's conclusion as to matter of fact:—

"On the other hand, the labours of very many experimenters

* Vol. i. p. 429.

have now placed it beyond all question of doubt or cavil that living *Bacteria*, *Torulæ*, and other low forms of life will make their appearance and multiply within hermetically-sealed flasks (containing organic infusions) which had been previously heated to 212° F., even for one or two hours. This result is now so easily and surely obtainable, as to make it come within the domain of natural law." And in a note is added, "In a very large number of trials I have never had a single failure when an infusion of turnip has been employed; and from what I have more recently seen of the effects produced by the addition of a very minute fragment of cheese to such an infusion,* I fully believe that in 999 cases out of 1000, if not in every case, a positive result could be obtained." Though this is one of a great number of statements made by Dr. Bastian upon which he bases speculations as to the prevalence of spontaneous generation or archebiosis, we think it necessary to state that we have not considered that (which is a question of interpretation) as the point at issue, but merely the question of fact as to the appearance of *Bacteria* in what may be considered, according to our present lights, infusions duly guarded from inoculation. The point under discussion is one as to a fact in the natural history of *Bacteria*, in a further study of which we are occupied at the instance of the Radcliffe Trustees; and we believe that a more precise knowledge of the life-history, life-conditions, and various forms of these organisms is necessary before the hypothesis of their spontaneous generation can serve as a safe guide in scientific investigation.

The experiments recorded below were made with infusion of hay and with infusion of turnip, sometimes with the addition of a few fragments of pounded cheese. It is necessary at once to call attention to three precautions which we have taken, and which we think are indispensable:—1. Recognizing the fact that the presence of lumps is a possible source of error, we excluded these from our infusion, either by filtration or by decantation. 2. To ensure the satisfactory exposure of the whole contents of the tube to the boiling temperature, we, as a rule, *completely* submerged our experimental tubes in boiling water for a period varying from five minutes to half an hour. 3. The substances used in preparing the infusions being necessarily of a very heterogeneous nature, we always examined samples of the infusions before and after boiling, at the time of closing the tubes, and were thus able to determine whether any *change* had taken place in the visible particles contained in the fluid after a lapse of time.

The microscopes used by us throughout, working side by side with samples from the same infusion, were a Hartnack's Stative VIII. objective No. 10 à immersion, ocular 4, belonging to the anatomical department of the University Museum, and a large Powell and

* See Appendix C, pp. xxxiv–xxxviii.

Lealand belonging to the Radcliffe Trustees, which is provided with a $\frac{1}{12}$th and a $\frac{1}{50}$th objective. The former of the two English glasses was more usually employed than the latter, on account of its greater convenience in manipulation.

*Appearances in freshly-prepared Infusions.*—Since the objects seen in such infusions are remarkable, and have doubtless sometimes led to error in subsequent examination of infusions, we may draw attention to them now. In such freshly-prepared infusions we have not unfrequently seen appearances agreeing very closely with some of those figured by Dr. Bastian in his book as coming into existence *after* boiling, sealing, and preservation in a warm chamber. A freshly-prepared and boiled strong infusion of hay may present shreds of vegetable fibre, a considerable number of dead *Bacterium termo* (some two or three to the field), minute, highly refringent spherules, varying from the size of a blood corpuscle to the smallest size visible; and such spherules are often present in pairs, forming figure-of-8-shaped bodies, both smaller and larger than *Bacterium* and of different optical character. Further, dumbbell-shaped bodies are not unfrequently to be observed of similar form and size to *Bacteria*, but coarser in outline; they dissolve on addition of HCl, which *Bacteria* do not.* All these bodies exhibit constant oscillatory (Brownian) movements. The addition of new cheese to such an infusion (as shown by examination of a simple infusion of new cheese taken by itself) adds a considerable number of highly refringent spherules of various sizes (oil-globules) and finely granular flakes, also a few *Bacteria* and (if the cheese be not quite new, almost certainly) fungus-mycelium and conidia in quantity.

Fresh-boiled turnip-infusion alone may contain so very few dead *Bacteria* that none are detected with the microscope, or only one in a drop. It presents a great number of minute, highly refringent spherules, varying in size from $\frac{1}{5000}$ inch downwards, all in most active oscillatory movement. Shreds and filaments of various sizes and character also are found, and a few finely granular flakes about $\frac{1}{1000}$ inch in diameter. The addition of cheese brings in, of course, the objects enumerated above as belonging to it.

*Visibility of Bacteria.*—It is perhaps necessary to say, before proceeding further, that we have satisfied ourselves that, in infusions of the optical character of those used, the multiplication of *Bacteria* makes itself obvious by a cloudiness. Hence, though we have not remained content with that evidence, the retention by such a limpid infusion of its limpidity is a proof of the absence of *Bacteria*. We also should mention, what is well known already,

* In the most carefully guarded of the experiments published by Dr. Child a few years since in the 'Proceedings of the Royal Society,' a very small number of bodies similar to these were obtained; and we suggest that they were of the same nature.

that in a closed tube or bottle, after such a cloud (of *Bacteria*) has developed, the *Bacteria* at a certain period cease to multiply and settle down as a fine powder, leaving the fluid again clear. Such precipitated *Bacteria* remain unchanged in the fluid for a long period (weeks certainly, perhaps years), and can be readily shaken up and at once recognized by microscopic examination; they are, moreover, not destroyed by boiling: hence it is not possible to miss the detection of a development of *Bacteria* in a limpid turnip-infusion, examined daily for three weeks or more by the naked eye, and finally, after agitation, by means of the microscope.

SERIES A. Nov. 23rd. *Experiments with Hay-Infusion.*—An infusion was prepared by pouring water of about 90° C. on to chopped hay. The infusion was of a dark sherry colour; reaction slightly acid. The glass tubes used in this and subsequent experiments were about five inches in length, of half-inch bore, rounded at one end and drawn out to a capillary orifice at the other. The infusion in these and subsequent experiments was introduced by heating the tube and plunging its capillary beak beneath the surface of the experimental liquid during the cooling of the expanded air, until the tube was about one-third or half filled. Tubes 1, 2, 3 were half filled with the hay-infusion previously filtered, the liquid was boiled in the tube, and the capillary beak fused, as nearly as possible, during ebullition.*

Tubes 4, 5 were similarly treated, with the difference that a small quantity of cheese, in a very fine state of division, had been added to this portion of the hay-infusion before its introduction into the tubes.

Tubes 6, 7. Quantity and character of the infusion as in 1, 2, 3, but the tubes sealed without previous ebullition.

Tube 8. Quantity and character of the infusion as in 4 and 5, but the tube sealed without previous ebullition.

Tubes 9, 10, 11. Quantity and character of the infusion as in 1, 2, 3, but rendered slightly alkaline with KHO. Sealed approximately during ebullition.

All these tubes (1 to 11) were after closure completely submerged in boiling water for fifteen minutes, and were then preserved in a hot-air bath, varying in temperature from 30° C. to 35° C.

*Microscopic and Naked-Eye Appearances of the Hay-Infusion*

* The tubes were sealed at the moment of removal from the flame over which they had been boiling. In every case a subsequent recurrence of ebullition was observed during the cooling of the upper part of the tube. Dr. Roberts, of Manchester, has suggested that the occurrence of *Bacteria* in tubes thus sealed may be explained by their indraught, together with a certain amount of air, at the moment of closure; but the experiments of Sanderson, recently confirmed by Cohn, have shown that contamination of fluids by *Bacteria* only takes place through the medium of impure surfaces or liquids.

*at the Time of Sealing the Tubes.*—The infusion in tubes 1, 2, 3, 6, 7 was clear and pellucid; that in tubes 4, 5, 8, 9, 10, 11 was hazy.

Microscopic examination gave the result indicated above, as to the appearances of freshly-prepared hay and hay-and-cheese infusion.

*Subsequent Appearances of the Infusion in Tubes* 1–11.—The tubes with infusion which was pellucid at the first were found to retain this character for several weeks, being preserved in the air-bath, and examined from day to day. The hazy infusions were opened after four days, and their contents found to be unchanged.

A portion of the same hay-and-cheese infusion, boiled and purposely contaminated by preservation in an uncleaned beaker, was found after four days to be teeming with *Bacterium termo* exhibiting vital movements. The pellucid infusions were subsequently examined with the microscope at different times, and found to be unchanged.

Series B. Nov. 25th. *Experiments with Turnip-and-Cheese Infusion.*—An infusion was made with 700 grms. sliced white turnip and 1000 grms. water, to which about 1 grm. finely-minced new cheese was added, the jug containing the mixture being maintained for four hours on a sand-bath at a temperature of 45°–55° C.

The infusion was now filtered; sp. gr. of the infusion 1011·1. Reaction slightly acid.

Tubes 12, 13, 14. Sealed cold. Submerged in boiling water for thirty minutes.

Tubes 15, 16, 17, 18, 19. Sealed approximately during ebullition. Submerged in boiling water for thirty minutes.

The tubes were preserved in the air-bath as in Series A.

*Microscopic and Naked-Eye Appearances of the Infusion at the Time of Sealing the Tubes.*—The liquid in all the tubes was perfectly clear and limpid. A few shreds and flakes were obvious, which appeared to be derived from the filter paper and from the slight precipitation of albuminous matter. The microscopic appearances were those above described as characterizing such infusions.

*Subsequent Appearances of the Infusion.*—The infusion in all the tubes was found on examination from day to day to retain its limpidity. Subsequent microscopic examination of all the tubes at various periods subsequent to the closure of the tubes (from four days to three weeks) yielded no indication whatever of a development of *Bacteria* or other organisms, nor of any change. A portion of the same infusion placed in an uncleansed beaker for comparison was milky and swarming with *Bacteria* after three days.

Series C. Nov. 28th. *Experiments with Turnip-and-Cheese Infusion.*—The infusion similar in all respects to that in Series B, but prepared with a somewhat larger proportion of turnips; therefore of higher specific gravity, which was not numerically determined.

Tubes 20, 21, 22, 23. Boiled and sealed approximately during ebullition. Not subsequently submerged.

Tubes 24, 25. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water during thirty minutes.

The tubes were preserved in the air-bath as in Series A and B.

Series D. Nov. 30th.—An infusion prepared as in Series B and C, but brought to a sp. gr. 1031 by evaporation after filtration.

Tubes 26, 27, 28, 29. Sealed cold. Subsequently submerged in boiling water for thirty minutes.

Tubes 30, 31. Boiled and sealed approximately during ebullition. Not subsequently immersed.

Tubes 32, 33. Boiled and sealed approximately during ebullition. Subsequently submerged in boiling water for thirty minutes.

*Appearances in the Infusions, Series C and D, at the time of Sealing and Submerging.*—The appearances in the freshly-prepared infusion were similar to those described above as characterizing such infusions.

Subsequent naked-eye examination of the tubes did not reveal the slightest change; they remained limpid. Specimens from each group were opened and examined with the microscope after four days, and the microscopic characters found to be unchanged: the liquid was perfectly sweet. The remaining tubes were examined at intervals before the end of December, being maintained during the whole time at a temperature of 35° to 40° C. in the air-bath; they equally proved to have remained unchanged when opened and examined with the microscope, and were also free from unpleasant smell.

Series E. Nov. 28th.—Six porcelain capsules were heated to redness, and nearly filled with the turnip-infusion used in Series C. They were placed on the air-bath under a glass shade.

Capsules 1, 2. The infusion was unboiled.

Capsule 3. The infusion was boiled in the capsule.

Capsule 4. The infusion was introduced after it had been boiled for five minutes in a superheated test-tube.

Capsules 5 and 6. The infusion was that used in Capsule 4, but a drop of distilled water was added to each of these two capsules.

After four days the infusion in capsules 1, 2, 5, and 6 was found to be teeming with *Bacterium termo* and Bacterian filaments.

Capsule 3 was found to be cracked, and hence was discarded (it swarmed with *Bacteria*).

Capsule 4 was perfectly free from organisms, and remained so during a fortnight, when a fungus-mycelium made its appearance on the surface.

Series F. Dec. 10th.—A strong infusion of turnip and cheese, prepared as in Series B (sp. gr. 1013), was boiled in an eight-ounce flask for five minutes. Three common test-tubes were superheated and placed in a beaker to support them.

No. 1. The infusion was poured in, and with it one drop of distilled water.

No. 2. The infusion was poured in and thus left.

No. 3. The infusion was poured in and again boiled for two minutes.

These and the flask containing the remaining infusion were left on a shelf for one day; on Dec. 11th, there being no cloudiness in any of the four, they were placed on the top of the hot-air bath. On Dec. 13th No. 1 was found to be swarming with *Leptothrix*-growths and free *Bacterium termo*.

No. 2 also was cloudy, and swarmed with what Cohn calls the rosary-chains. No. 3 was absolutely free from all development of life, and was perfectly sweet and limpid; so also was the fluid in the original flask, a large one capable of holding eight ounces. How is the development of *Bacteria* in No. 2 to be explained? The original fluid remains pure; the fluid in No. 3, which was reboiled, remains so too; the tube itself, No. 2, had been heated red-hot, and could not be a source of contamination. One's attention was therefore directed to the conditions of the passage of the fluid from the flask into the tubes; and here an explanation at once offered itself. The large flask *had not been superheated;* its lip was still dirty, laden with *Bacteria* ready to contaminate fluids as they poured from it; hence the contamination of the fluid in test-tube No. 2. The validity of this explanation cannot be disputed, because it is known that such glass surfaces, unless specially cleansed, invariably contaminate infusions exposed to them.

Series G. Feb. 11th.—The publication of Dr. Burdon Sanderson's letter, describing some experiments made by Dr. Bastian, induced us to make a further series of experiments with important modifications. We had expressly avoided the introduction of anything like visible lumps of solid cheese or turnip into our infusions during their ebullition, believing that such lumps were a possible source of the exclusion of *Bacteria* or their germs from the killing influence of the boiling temperature. This precaution we had supposed (in the absence of any statement to the opposite effect) to have been taken by Dr. Bastian in the experiments adduced by him in the 'Beginnings of Life.' The presence of such lumps was publicly suggested in discussion at the British Association Meeting at Liverpool as a source of fallacy, and has been demonstrated to be so by Dr. Ferdinand Cohn in experiments made with peas and infusion of peas.* Further, we had limited the bulk of our infusions and the size of our experimental tubes, in view of the obvious consideration that the larger the mass and area to be guarded against contamination the greater the chance of failure in that respect. Thirdly, it had not occurred to us to make use of vessels in these experiments of a form so inconvenient and difficult to thoroughly guard against effects of "spluttering," and to thoroughly heat by boiling, as the retort. Nor could we guess, in the absence of any directions on that point from Dr. Bastian, that it was desirable to exclude the rind of the turnip from the preparation of the infusion. The correspondence in 'Nature,' however, indicated that "pounded" cheese (necessarily in a condition of solid lumps) was added (in some cases) to his *experimental vessels after the turnip-infusion*, and was present during ebullition. It also appeared that retorts capable of holding two ounces were the vessels used; whilst, on grounds not given, it was considered advantageous by Dr. Bastian to peel the turnips before slicing them.

The following experiments were accordingly made:—

An infusion of turnip (minus the rind) was prepared and filtered; it had sp. gr. 1012·7. In the experiments Nos. 34 to 47 two-ounce retorts were used, and the bulb half filled with the experimental infusion.

No. 34. The infusion neutralized with KHO. About two grains of pounded cheese in pellets added to the retort.

Nos. 35, 36. Infusion not neutralized. About two grains of pounded cheese in pellets added to the retort.

Nos. 37, 38, 39. The simple infusion.

No. 40. The simple infusion, to which were added a few drops of an emulsion of cheese prepared with some of the turnip-infusion and new cheese, the emulsion having been filtered.

No. 41. The simple infusion.

Nos. 34 to 40 were boiled for five minutes; they were then preserved in the air-bath at a temperature of 35° C., and sealed approximately during ebullition. Four of them, including No. 36, were subjected to a further boiling of fifteen minutes in a water-bath after sealing.

No. 41 was boiled for five minutes and placed on a shelf with its mouth open.

### *Subsequent appearances in Retorts Nos.* 34–41.

On Feb. 15th Nos. 34, 35, 37 were opened and found to be perfectly sweet and free from a development of *Bacteria* or other organisms.

* 'Beiträge zur Biologie der Pflanzen,' Breslau, 1872.

No. 41 was observed to be perfectly limpid, and is so still (March 17th).

On Feb. 27th Nos. 36, 38, 39, and 40 were opened. With the exception of No. 36, they were perfectly sweet and free from organisms.

No. 36 had a slightly fœtid odour and swarmed with rather long *Bacteria*—that is, *Bacteria* longer than the common *B. termo*, which develops in infusions open to atmospheric air, but not quite of the form of the *Bacillus subtilis*, of the butyric fermentation, which is stated to appear in some infusions, *e.g.* milk, to which the access of atmospheric air has been entirely prevented. It is to be noticed that in this series the only retort in which *Bacteria* made their appearance was one of those in which small *lumps* of cheese were present during the subjection of the flask to the process of ebullition and subsequent immersion in boiling water.

This result induced us to make a further series of differential experiments, bearing upon the influence of the state of aggregation of the cheese introduced into the turnip-infusion.

Series H. March 8th.—A turnip-infusion was prepared as in Series B; found after filtration to have sp. gr. 1113·5.

Tubes similar to those used in Series A–E, and half filled, were used.

Tubes 42, 43, 44. The simple infusion was poured into the tube, so as to half fill it; a lump of cheese the size of a pea was then added. Sealed cold.

Tubes 45, 46, 47. To the turnip-infusion, before introduction into the tubes, an emulsion of cheese prepared with turnip-infusion and strained through a piece of cambric was added. The tubes were then half filled with this mixture and sealed cold.

Tubes 48, 49, 50. The same as 42, 43, 44, but sealed approximately during ebullition.

51, 52, 53. The same as 45, 46, 47, but sealed approximately during ebullition.

All the tubes, 42 to 53, were completely submerged during five minutes in boiling water, and subsequently preserved in the air-bath at 35° C. temperature.

On March 13th the contents of the twelve tubes were examined with a microscope. No. 45 had been broken in the boiling. The five remaining tubes which had been prepared with cheese in the finely-divided condition were found to be entirely devoid of life, the infusion microscopically and otherwise unchanged. Of the six tubes prepared each with a small lump of cheese, no organisms were detected in 42 and 44; but in 43 and 49 a few elongate *Bacteria* were observed (in the proportion of about two to the field of a Hartnack's system 10). In 48 and 50 the fluid was swarming with elongate *Bacteria* and true *Bacillus*. The lumps of cheese in those tubes in which life appeared had softened and spread out to a certain extent on the side of the tube. The cheese-lumps in Nos. 42 and 44 retained their original form.

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From the result of these later experiments, made in consequence of the fuller information given by Dr. Sanderson as to Dr. Bastian's mode of treating turnip and cheese so as to obtain phenomena supposed to be in favour of the doctrine of Archebiosis, we consider that the importance of excluding visible lumps from the experimental infusions is clearly indicated, as also is the comparatively greater trustworthiness of the small tube as opposed to the larger retort for use as an experimental vessel. We moreover consider that we, in our earlier experiments (November and December), carefully following Dr. Bastian's directions, as far as he had given any in the 'Beginnings of Life,' but using at the same time proper care as to cleanliness and due boiling, obtained a series of results contradicting Dr. Bastian's statements as to the spontaneous generation of *Bacteria* in infusion of turnip to which a fragment of cheese had been added.

Further, certain of the experiments above recorded, and others made at the same times with open vessels and simple turnip-infusion, compel us to dissent emphatically from the conclusion of the following statement contained in a recent paper by Dr. Bastian ('Nature,' Feb. 6th, p. 275):—"Taking such a fluid therefore in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask, with the assurance that it contains no living organisms. Having ascertained also, by our previous experiments with the boiled saline fluids, that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion *invariably* becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria*." The italics are our own.

We find not only that such an infusion remains free from *Bacteria* when thus treated (subject, of course, to certain failures in the precautions taken) for "one or two days," but if contamination by the admission of coarse atmospheric particles capable of carrying *Bacteria* be guarded against, it will remain so for weeks and probably so for years. In consequence of this absence of development of *Bacteria* we have cultivated *Torulæ* in such a turnip-infusion, so as to obtain them entirely free from the former organisms.*

* At this moment, May 20th, the turnip-infusion in the open retort (No. 41) is free from all organisms, and is perfectly limpid and sweet.

In conclusion, we would point out that failure in manipulation, contamination in unsuspected ways, such as that due to the preservative influence of lumps, and, again, the mistaking of particles in an infusion which have been there from the first for organisms originated *de novo*, do not exhaust the list of conceivable explanations of phenomena which have been attributed to spontaneous generation. When the knowledge of the natural history of *Bacteria* has advanced somewhat further, there will be a possibility of such explanations presenting themselves in ways at this moment unsuspected.

Whilst awaiting Professor Huizinga's fuller account of his experiments, we may point out that the hypothesis of an inhibitory influence of increased density should be supported by experimental evidence, and that it cannot apply to tubes closed before boiling. The neck of the flask closed with asphalt may (so far as conditions are stated by him at present) harbour *Bacteria*, as in our Series F. But especially we would urge upon him and others that it is undesirable, as yet, to introduce into the discussion other organic mixtures. Turnips and cheese may be very bad material for experiment; but it would be well, as far as possible, to settle the matter, or the way in which the matter is to be viewed with regard to them, before going off to other particular cases.

It would be a very excellent thing if all further reference to this subject could be postponed for a year or two—that is, until further study of *Bacteria*, such as that inaugurated by Sanderson and Cohn, has given us surer ground to tread upon.—*Proceedings of the Royal Society*, No. 145.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*The Organization of Asterophyllites.*—In an abstract published in a recent number of the P.R.S., Professor Williamson gives the following account of his own views of its structure and relations:—On two occasions he directed attention, in the 'Proceedings of the Royal Society,'* to the structure of some stems which appeared to him to belong to the well-known genus *Asterophyllites*, briefly pointing out at the same time their apparent relations to a strobilus of which he had previously published figures and descriptions † under the name of *Volkmannia Dawsoni*. In the present memoir he gives a detailed exposition of the various parts of the plant, including the roots, rootlets, stems, branches, leaves, and fruit, in different stages of their development. This is done chiefly in two modifications of the primary type—one from the Lower Coal-measures of Oldham in Lancashire, the other from those of Burntisland. In its youngest state, the Oldham form first appears as a mere twig, having a central fibro-vascular bundle enclosed in a double bark. The vascular bundle consists entirely of vessels which are chiefly, if not wholly, of the reticulated type. When divided transversely, it presents a triangular section, the triangle having long narrow arms and very concave sides. The bark is already differentiated into two layers, and has its exterior deeply indented by three lateral grooves—one opposite to each concave side of the vascular triangle. The outer layer is prosenchymatous, with vertically elongated cells; the inner one consists of cylindrical parenchyma arranged in radial lines, the cells being also elongated vertically. As the plant grew, successive vascular layers were added exogenously to the exterior of the vascular axis. Each layer consisted of a single linear row of vessels, which were of large size opposite the concavities of the triangle, and small where they approached its several angles. The radial arrangement of those in the several growths was equally regular; they were disposed in single radiating series, new laminæ being intercalated peripherally as the stem grew. These radiating laminæ were separated by small medullary rays. Owing to the fact mentioned, that the laminæ radiating from the concave sides of the central triangle consisted of much larger vessels than those radiating from its angles, three or four such growths sufficed to convert its concave sides into slightly convex ones, whilst a few more such additions converted the vascular axis into a solid *cylindrical* rod. At this stage its transverse sections appeared definitely divided into six radiating areas—three of large open vessels radiating from the sides of the primary triangle, and three of small ones proceeding from the sides and extremities of the angles. When these growths have thus given a cylindrical form to the vascular axis, a change takes place in its further development. Concentric growths again begin to form, but in them all the vessels are of almost equally small diameters: hence the abrupt termination

* Vol. xx., pp. 95 and 435.

† 'Transactions of the Literary and Philosophical Society of Manchester,' third series, vol. v., 1871.

of the three areas of large vessels in the younger growths produces a distinct circular boundary line, marking a special stage in the genesis of the stem. From this point the additions go on uninterruptedly, the vessels of each radiating lamina or wedge increasing slowly in size from within outwards as the stem advances towards maturity. During these further developments the bark has continued to be separated into two well-defined forms. An inner layer consists of very delicate elongated cells with square ends (prismatic parenchyma); these are seen in the transverse section arranged in radiating lines proceeding from within outwards. The outer bark consists of narrow, elongated, prosenchymatous cells, having very thick walls; at intervals, corresponding with the spaces between the successive verticils of leaves in the ordinary examples of *Asterophyllites*, we find distinct nodes where the bark expands into lenticular disks. The vascular axis passes through these nodes without undergoing any visible change, either in the position of its vascular layers or in giving off vessels to the nodes or their appendages. The thin peripheral margin of each node sustains a verticil of the slender leaves of *Asterophyllites*, of which there are about twenty-six in each verticil. The aspect, dimensions, and arrangements of these leaves correspond exactly with what is seen in the ordinary specimens found in the Coal shales. Transverse sections of them exhibit a single thick central midrib, but no traces of vascular tissues have hitherto been found in them.

The laminæ of the vascular axis are separated by numerous medullary rays of small size; these rarely exhibit more than four or five cells in any vertical series, and usually but one or two. The exterior of the bark is deeply indented in each internode by three very deep superficial grooves, each one of which occupies the side of the stem corresponding with a concavity of the central triangle of the vascular axis. These grooves, which are sometimes double instead of single, extend from node to node, but do not indent the nodal disks. Owing to the great depth to which these penetrate the bark, they give a very characteristic tripartite aspect to each transverse section of these stems.

The Burntisland type agrees with the Lancashire one in all its leading features of structure and growth; but its vessels are all barred instead of being reticulated, and the author has not met with such beautiful examples of its nodal disks as he has done in the case of the other form, neither has he seen its leaves attached. On the other hand, he has found specimens of much larger diameter than any that have hitherto been detected in Lancashire, exhibiting the characteristic peculiarities already referred to in an exquisitely beautiful manner. The author has also obtained one section from this locality in which a branch is given off. The vessels of this divergent organ are derived from the central portion of one of the segments of small vessels, seen in the transverse sections, which proceed from one of the angles of the central triangle.

Having elucidated the details of the aërial stems, the author proceeds to examine such organs of fructification as appear to belong to these plants, commencing with the *Volkmannia Dawsoni*, which he described at length in the Transactions of the Philosophical Society of Manchester in 1871. This is a verticillate strobilus with a central vascular axis, of which latter transverse sections exhibit a close correspondence with the triangular bundle of *Asterophyllites*, being also triangular, with concave sides and truncate angles. But in order to adapt this primary fibro-vascular bundle to the requirements of the fruit, each of the truncate angles is enlarged, so as to make the entire section an almost hexagonal one. This axis is surrounded, as in *Asterophyllites*, by a double bark—an outer prosenchymatous one, and an inner one of more delicate cellular structure. At each node this bark expands into a lenticular disk fringed with stiff narrow bracts, which extend upwards and outwards beyond the sporangia. The latter rest upon the bractiferous disks and the basal portions of the bracts, each verticil being fertile. The sporangia are closely packed in about three concentric circles, and attached by sporangiophores, originating from each side of the base of each bract. The sporangia have cellular walls; they are full of large spores, each of which has its surface prolonged into a number of very long radiating spines. This fruit the author unhesitatingly identifies with the aërial stems previously described.

He then examines various so-called *Volkmanniæ* found in the Lancashire Carboniferous shales, of which the internal structure is not preserved, but which, being found with leaves attached to them, admit of no doubt as to their belonging to *Asterophyllites*. These are regarded as being identical with *Volkmannia Dawsoni;* hence the author accepts the latter fruit as giving the internal organization of the ordinary Asterophyllitean strobilus. The fruit, which has been previously described by Binney, Carruthers, and Schimper, under the names of *Calamodendron commune*, *Volkmannia Binneyi*, and *Calamostachys Binneyana*, is then investigated. The above authors had associated it with *Calamites;* but its internal structure is shown to have nothing in common with that type; it consists of alternating verticils of barren and fertile appendages. The former are nodal disks bearing protective leaves; the others are verticils of sporangiophores, usually six in each verticil, and which closely resemble those of the recent Equisetaceæ; they project at right angles from the central axis, and expand at their outer extremities into shield-like disks, which sustain a circle of sporangia on the inner surface of each shield. The sporangia consist of a very peculiar modification of spiral cells; they are filled with spores which have been described as provided with elaters, like those of *Equisetum;* but the author rejects this interpretation, regarding the so-called elaters as merely the torn fragments of the ruptured mother-cells in which the true spores have been developed. The vascular axis is shown to be *solid*, and without any cellular elements, being wholly different from that of *Calamites*, in which the vascular axis is a *hollow* cylinder containing an immensely large, cellular, and fistular pith. In one fine example of *Calamostachys Binneyana* the author has found the central fibro-vascular bundle surrounded by an exogenous ring. This, too, exhibits no resemblance whatever to the corresponding growths of *Calamites;* on the other hand, it corresponds closely with conditions occurring in some parts of *Asterophyllites*, with which group the author believes the fruit to be related, notwithstanding the peculiarity of its sporangia and sporangiophores. The author is confirmed in his conclusion that this fruit is not Calamitean by his having already described the structure of a true Calamitean strobilus, from an example in which the central axis retains most accurately the arrangements of tissues characteristic of Calamitean stems (Manchester Transactions, 1870). A type of stem to which the author had previously assigned the provisional generic name of *Amyelon* is now shown to be the root or subterranean axis of *Asterophyllites*, specimens being described in which clusters of rootlets are given off, in irregular order, from various points of the exterior of the branching roots. The latter have no medulla; but in the centres of several of them the author finds the peculiar triangular fibro-vascular bundle so characteristic of *Asterophyllites*; and in all remains of the same trifid origin of the vascular layers may be traced in the peculiar curvatures assumed by the vascular laminæ as they proceed from within outwards. The bark consists of two layers: the inner one is composed of ordinary parenchymatous cells, often of considerable size: the outer one consists of irregular piles or columns of cells, disposed perpendicularly to the surface of the bark, and with their tangential septa in close contact and in parallel planes. The lateral or radial boundaries of these piles of cells are more strongly defined than the transverse septa. In tangential sections of this outer bark, each of these radially-disposed columns of parallel-sided cells appears as a single thick-walled parenchymatous cell, whose aspect, in common with that of its neighbours, is that of ordinary coarse parenchyma. Such sections exhibit no indication of the radial elongation of these cells seen in radial and transverse ones. On re-examining the inner bark, we discover the explanation of these appearances. Many of the larger and more peripheral of the cells of the latter are seen to be undergoing division by the development within their walls of secondary cell-partitions, which are parallel with those of the radially-disposed columns. It appears obvious that each of the latter was primarily one of the cells of the inner bark, which has become elongated radially, and at the same time divided into a linear series of compressed cells by the growth of a succession of secondary divisions, all of which were more or less tangential to the periphery of the stem.

The author directs special attention to the genetic activity of this inner bark; the cells of its inner surface were obviously instrumental in producing the successive circumferential additions to the primary vascular axis, whilst those of its outer surface increased the diameter of the outer bark in the way just described.

After comparing these plants with living forms, the conclusion is arrived at that the nearest parallel to the structure of their stems is to be found in *Psilotum triquetrum*; whilst their general affinities are regarded by the author as Lycopodiaceous rather than Equisetaceous. The exogenous aspect of their successive vascular growths is, if possible, more conspicuous than in most of the other Carboniferous Cryptogams.

The structure of the stems described is identical with that of those found at Autun by Professor Renault, and assigned by him to *Sphenophyllum;* thus the close affinity of this genus with *Asterophyllites* appears to be finally established. The *Calamites verticillatus* of authors is probably the arborescent stem of one of these plants.

*The Anatomy of Necrosis.*—A paper on this subject was lately read before the Medical Society of Albany (N.Y.), by Dr. W. Hales. The general plan of his remarks was first to treat of necrosis as it occurs in connective tissue, and to compare the processes which nature adopts in dealing with the same affection in the more compact and unyielding tissues, as the osseous and tendinous. The subject he illustrated by a series of diagrams of the microscopical appearances of normal and inflamed bone, and a large number of photographic slides made directly from pathological specimens in the museum of the Albany Medical College, thrown upon a white wall by means of the oxycalcium lantern. The college museum is extremely rich in the variety and number of its specimens, being one of the finest collections in the State. The mode of separation of the sequestrum, the formation of the involucrum, the presence of the living wall of granulation tissue between the septic elements of decaying tissues and the open mouths of absorbent vessels, and the almost complete analogy existing between the various structures in accomplishing the separation of dead parts and the reproduction of the new, were spoken of at length. The microscopic and pathological anatomy of the subject was fully illustrated. The minute structure of the parts at the different stages of the affection, and the appearance of actual specimens in the various phases of necrosis, were exhibited. The modifications of the vascular supply in different tissues, and their various powers of anastomosis, were fully discussed.

*A Trace of Sexual Organs in the Hymenomycetes.*—M. A. Œrsted has, says M. Anton de Bary (in a paper in 'Grevillea' for June), discovered a trace of sexual organs in the Hymenomycetes where, perhaps, no one had previously looked for them. He has seen, in fact, in *Agaricus variabilis*, Pers., oocysts or elongated reniform cells, which sprang up like rudimentary branches of the filaments of the mycelium, and enclose an abundant protoplasm, if not even a nucleus. At the base of these oocysts appear the presumed antheridia, that is to say, one or two slender filaments which generally turn their extremities towards the oocysts, and which more rarely are applied to them. Then, without ulteriorly undergoing any appreciable modification, the fertile cell, or oocyst, becomes enveloped in a lacework of filaments of mycelium which proceed from that which bears it, and this tissue forms the rudiments of the cap. The reality of some kind of fecundation in this circumstance, and the mode of the phenomenon, if there is one, are at present equally uncertain. If M. Œrsted's opinion is confirmed, naturally the whole of the cap will be the product of fecundation. As long ago as 1860 M. Karsten presumed that such was the case. His observations on the first development of *Agaricus campestris*, as far as we can judge by the rather obscure account given in 'Bonplandia' (1862, pp. 63), would agree with M. Œrsted. "It is impossible not to perceive the similitude between the phenomena seen by M. Œrsted and those I have described in *Peziza confluens.*"

*Mode of preparing the Tympanic Membrane.*—The method of preparation of the tympanic membrane which has been found most effectual by Dr. M. Watson is the following:—The membrane is removed as soon after death as possible, and steeped for a few seconds in concentrated acetic acid. It is then placed in a solution of chloride of gold (0·5 per cent.), which should be kept at a temperature somewhat above that of the blood for half an hour, after which the membrane should be placed for twenty-four hours in glycerine, or in water slightly acidulated with acetic acid, and exposed to the light till it assumes a delicate purple colour. By this means, the loops and the nerves accompanying them are rendered visible. The specimen may be preserved in glycerine acidulated with acetic acid.

*A Fungus Parasitic on the Mouse.*—At the meeting of the Academy of Natural Science of Philadelphia (April 22nd), Professor Leidy exhibited a mouse with several whitish masses adherent to the ears, side of the face, and nose. The mouse had been caught in the children's department of Blockley Hospital. The white matter examined beneath the microscope proved to be composed of sporular bodies, single, double, or in short chains of a dozen or more. They measure about the $\frac{1}{650}$ of a line in diameter. The fungus is a Torula or Oïdium, and resembles that found in Aptha. Perhaps the disease in the mouse is the result of feeding upon articles imbued with adherent portions of apthous matter from the mouths of children.

*Cancer in the Neighbourhood of the True Skin.*—Dr. Ogston says of this, that when it occurs on the prepuce, the lips, the face, the hands, &c., one of the earliest expressions of the disease is a recognizable amount of hypertrophy of the epidermis over the tumour, and a binding down of this structure, so that it cannot be moved backwards and forwards over it. This is equally true whether the cancerous proliferation of epithelium progresses more superficially on and between the papillæ of the skin, so as to give rise to an elevated epithelioma, or extends among the subcutaneous tissue or deeper parts, so as to form a cancerous nodule of the ordinary deep description; in both classes of cases, the skin is bound down to the tumour from the very commencement, and generally presents an alteration of appearance visible to the naked eye of a close observer. The cuticle appears rough and scaly, and the true skin beneath shows through it with a purplish red tinge, so that the portion affected offers a contrast to that in its vicinity—not very prominent, it is true, but unmistakable on close observation. The hairs are sometimes stunted and broken, the hair-follicles hypertrophied, and the sweat-ducts present a thickening of their epithelial lining.—*Edinburgh Medical Journal.*

*Subepithelial Endothelium of the Mucous Membranes.*—Dr. E. Klein says in the 'Medical Record,' that M. Debove asserts that the mucous membranes possess, beneath the superficial epithelium, a single layer of flat cells, consisting of protoplasm and united with each other by a very delicate intercellular substance. If a portion of small fresh intestine be deprived of its epithelium, and impregnated with solution of nitrate of silver, there appears a beautiful network of more or less sinuous dark lines on the surface of the villa, exactly like that on the surface of the serous membranes. A similar endothelium makes its appearance beneath the epithelium of the Lieberkühnian glands of the intestine, so that the membrana propria of the gland is a mere endothelial membrane. [V. Czerny found in silver-stained preparations some years ago, that the membrana propria of the sweat-glands of the skin consists of endothelial plates.] In the bronchi, a similar layer of subepithelial endothelium is to be found; it consists of polygonal plates, which in silver-stained preparations are bordered by straight lines. Deboué believes it probable that the epithelium of the bronchi does not continue into the infundibula, but that the cellular lining of these latter is a continuation of the above-mentioned endothelium. The subepithelial endothelium of the mucous membrane of the bladder consists of very large polyhedral cells, bordered by straight lines. From all this, says Dr. Klein, it appears that the subepithelial endothelium corresponds to what has been very often described as a structureless basement membrane.

*The Resolving and Penetrating Power of certain Objectives.*—Professor Ardissone publishes in the 'New Italian Journal of Botany,' the following Tables showing the relative resolving and penetrating power of objectives by four different French and German makers. In the determination of the separating or resolving power he employs the diatoms ordinarily used as test-objects, and for the reason that they are more generally accessible than Nobert's Test-plates. In publishing these Tables, Professor Ardissone does not intend to pronounce a judgment upon the relative *value* of the work of the different makers. He very justly states that the separating or resolving capacity is only one of the qualities of a good objective. The same is also true of the quality of penetration. In the Table, N. refers to *Nachet*, G. to *Gundlach*, H. to *Hartnack*, and Z. to *Zeiss*.

| Grade of Difficulty. | Test. | Balsam or Dry. | Minimum Power of Objective. | |
|---|---|---|---|---|
| | | | Direct Light. | |
| I. | Isthmia enervis .. .. .. .. .. | B | N., O | — G., I. |
| | Triceratium favus .. .. .. .. | ,, | ,, | — ,, |
| | Coscinodiscus omphalanthus .. | ,, | ,, | — ,, |
| II. | Biddulphia pulchella .. .. | ,, | ,, | — ,, |
| | Amphitetras antediluviana .. | ,, | ,, | — ,, |
| | Pinnularia nobilis .. .. .. .. | ,, | ,, | — ,, |
| III. | Triceratium arcticum .. .. | ,, | N., I. | — G., II. |
| | Aulacodiscus orientalis .. .. | ,, | ,, | — ,, |
| IV. | Navicula lyra .. .. .. .. .. | ,, | ,, | — ,, |
| | Arachnoidiscus ornatus .. .. | ,, | ,, | — ,, |
| V. | Cocconeis punctatissima .. .. | ,, | ,, | — ,, |
| | Rhabdonema arcuatum .. .. | ,, | ,, | — ,, |
| VI. | Synedra superba .. .. .. | ,, | H., IV. | — G., III., IV. |
| | Pinnularia interrupta .. .. | ,, | ,, | — ,, |

| Grade of Difficulty. | Test. | Balsam or Dry. | Minimum Power of Objective. |
|---|---|---|---|
| | | | Direct Light. |
| VII. | Stauroneis Phœnicenteron .. | B | H., VII. — N., III. |
| | Pleurosigma balticum .. .. | ,, | ,, — ,, |
| | Grammatophora marina .. .. | ,, | ,, — ,, |
| VIII. | Synedra splendens .. .. .. | ,, | ,, — ,, |
| | ,, fulgens .. .. .. .. | ,, | ,, — ,, |
| | Pleurosigma attenuatum .. .. | ,, | ,, — ,, |
| IX. | Synedra pulchella .. .. .. | ,, | H., VIII. — N., V. |
| | Pleurosigma angulatum .. .. | D | ,, — ,, |
| | ,, acuminatum .. | B | ,, — ,, |
| X. | Nitzschia sigmoidea .. .. .. | D | Z., F. — N., V. |
| | Surirella gemma (transverse) .. | ,, | ,, — ,, |
| XI. | ,, ,, ,, | B | H., IX., X. — G., VII. |
| | Nitzschia amphioxys .. .. | ,, | ,, — ,, |
| | Pleurosigma strigosum .. .. | D | ,, — ,, |
| XII. | ,, Spencerii .. .. | ,, | ,, — ,, |
| | | | Oblique Light. |
| | ,, ,, .. .. | ,, | N., V. — Z., F. |
| | | | Direct Light. |
| | ,, angulatum .. .. | B | H., IX., X. — G., VII. |
| | | | Oblique Light. |
| | ,, ,, .. .. | ,, | H., VII., VIII. — G., V., VI. |
| XIII. | Nitzschia sigmoidea .. .. .. | ,, | H., IX. — G., VII. |
| | Grammatophora subtilissima .. | ,, | ,, — ,, |
| XIV. | Cymatopleura elliptica .. .. | ,, | H., X. — ,, |
| XV. | Pleurosigma fasciola .. .. | ,, | ,, — ,, |
| XVI. | Surirella gemma (longitudinal) | D | ,, — ,, |
| | | | Artificial Light. |
| XVII. | ,, ,, .. .. .. .. | B | H., X. — G., VII. |
| | | | Monochromatic Light. |
| | ,, ,, .. .. .. | ,, | H., VII. — G., V. |
| XVIII. | Frustulia saxonica .. .. .. | ,, | H., IX. — Z., F. |
| XIX. | Nitzschia curvula .. .. .. | ,, | H., X. — G., VII. |
| XX. | Amphipleura pellucida .. .. | ,, | ,, — ,, |

TABLE II.—GRADE OF PENETRATION.

| Objective. | | Central Light. | Oblique Light. |
|---|---|---|---|
| Gundlach .. .. .. .. | I. | II. | — |
| Nachet .. .. .. .. | O. | II. | — |
| ,, .. .. .. .. | I. | IV. | — |
| Gundlach .. .. .. .. | II. | V. | — |
| ,, .. .. .. .. | III. | VI. | — |
| ,, .. .. .. .. | IV. | VI. | — |
| Hartnack .. .. .. .. | IV. | VI. | — |
| Nachet .. .. .. .. | V. | VIII. | XII. |
| Hartnack .. .. .. .. | VII. | VIII. | XII. |
| Gundlach .. .. .. .. | V. | X. | XII. |
| ,, .. .. .. .. | VI. | X. | XII. |
| Hartnack .. .. .. .. | VIII. | X. | XII. |
| Zeiss .. .. .. .. .. | F. | X. | XII. |
| Hartnack .. .. .. .. | IX. | XII. | XIII. |
| ,, .. .. .. .. | X. | XII. | XVI. |
| Powell and Lealand .. (*immersion*) | $\frac{1}{12}$ | XII. | XVI. |
| Gundlach .. .. .. .. | VII. | XII. | XVI. |
| ,, .. .. .. .. | VIII. | XII. | XVI. |
| ,, .. .. .. .. | IX. | XII. | XVI. |
| ,, .. .. .. .. | X. | XII. | XVI. |

*Remarks on Triceratium fimbriatum.*—Dr. A. Mead Edwards says that in the number of the 'Lens' for April, 1872, vol. i., page 100, is a paper by Dr. Woodward, on the double markings of Triceratium, wherein he figures two valves, one whole, the other broken, as, both of them, belonging to *Triceratium fimbriatum*. *First*, as to the species. It was founded on what is now generally considered very insufficient grounds by Dr. G. C. Wallich, in 1858,* and Ralfs † has ranked it under the older name of *T. favus*. "For my part, although I have never seen Dr. Wallich's original specimens, I must say I think it cannot be separated from that species. Möller in his Typen-Platte has chosen to retain the name, attaching it to a four-sided form, and giving Brightwell as the founder. I do not wish to be too severe on Mr. Möller, who has given us such beautiful specimens of his mechanical skill, but I have known of more than one beginner at the diatoms led astray by errors which have crept into his slides. The form he names *T. fimbriatum* cannot be, with justice, separated specifically from *T. favus*, Ehr., as Dr. Woodward's plate shows plainly. The finer set of markings can be shown in every valve of *T. favus* which has not been too long acted upon by chemicals. As to the other specimen figured in the plate, and which is in the cabinet of Dr. Johnston, I have seen and examined it critically. Dr. Johnston lent me the specimen in 1866, and I took several photographs of it. I was particularly interested in it as it came from the Moron earth, and I had found the same species some time before in the Monterey deposit, but with six sides. About the same time Mr. C. G. Bush, of Boston, found a three-sided form of the same in the Monterey material, and sent it to me for photographing. I obtained one or two pretty good negatives of it, and sent it back to him. Soon after I was sorry to hear that the balsam had contracted, drawing the cover down and breaking the diatom. I have never been able to find another three-sided form of this, as I consider it, distinct species. I also lost my six-sided form, and for awhile was in despair. Thereafter, however, I found in the Monterey material a beautiful and perfect six-sided valve, besides several fragments. The group including Dr. Johnston's, Mr. Bush's, and my specimens, I consider deserves to rank as a separate species, and I have provisionally, in the manuscript of my report on the specimens collected by the California State Survey, called it *Triceratium ponderosum*. Therefore, I would ask as a favour of diatomists, that, until my said report sees the light, when I will give my reasons for so ranking these forms, they be called by the name I have proposed for them."

*Distinguishing Fibres in Mixed Goods by the Microscope.*—Mr. Charles Stodder has the following in a number of the 'Scientific American,' published some time since. He says, in answer to an editorial inquiry, "Unquestionably the microscope is the best means of accomplishing the purpose of your correspondent; it is the simplest, quickest, easiest, and surest. All and each of the fibres named

* 'Quarterly Journal of Microscopical Science,' vi., page 242.
† 'Prit. Infus.,' 1861, page 855.

in the article are constructed—built up, so to speak—in different manners, so distinct from each other that a moderate magnifying power, say 400 diameters, of a decently good instrument, will show at once what they are. Anyone with a very little skill in manipulation can obtain the result. The differences have been described and figured in the books, but there is no need of books. Everyone can obtain genuine fibres of either kind, with almost less trouble than referring to a book, for comparison with those found in the fabric, and the original comparison is of far more value than the authority of a picture. No chemical test is known to distinguish flax from cotton fibre; but their difference in the microscope may be seen at a glance. Jute fibre has more resemblance to flax, but can be distinguished with a little more study. The materials of paper may also be ascertained, in part at least, by the microscope: for example, your number dated March 15, is printed on paper containing no cotton or linen; it is mostly wood fibre, with 'pitted' and 'scalariform' ducts, not peculiar to any kind of wood, with possibly fibres of manilla, esparto, or ramie, of which I have not the means of comparison. But the microscope cannot do everything. There is a certain fabric in use purporting to be made entirely of cows' hair. The question came up: Is there any sheep's wool in it? This could not be answered. For, while the bulk of each is easily distinguished, there are some hairs from each animal that cannot be known from the other. In this case, so far as is known, chemistry is equally powerless."

*The Phthisis Controversy.*—Dr. Joseph Coats thus sums up the results of this serious discussion. In the 'Medical Record,' June 4, he says, if we now finally ask, "What is the outcome of this discussion?" the answer may be in some respects difficult. It can at least be said, it was agreed by all the speakers that we have in phthisis pulmonalis, besides the ordinary products of inflammation within the lung-alveoli, also a cellular growth in the walls of the alveoli, which cellular growth is variously called lymphoid, adenoid, and reticular growth. The chief discussion was as to whether this growth is to be called tubercular or not; whether it is a specific new formation of a definite anatomical structure, or whether it is simply the result of chronic inflammation. If the discussion has done no more than state this ground clearly, it may not be entirely without result.

*Histological Characters of Two New British Algæ.*—The following are given by Mr. E. M. Holmes as the microscopical characters of two new British Algæ. The paper with full details appears in 'Grevillea,' July, 1873. The first is *Callithamnion hormocarpum;* of this the microscopical characters are: articulations at the base of the stem, coated with branched and jointed filaments; articulation of the plumules, 6–8 times as long as broad; those of the pinnæ four times, decreasing to twice as long as broad; flexuose and attenuated towards apex of the ultimate pinnules. Pinnules of the pinnæ either simple, once forked, or repeatedly forked, and tufted at the apex; axils very narrow, so that the less-branched pinnules appear pinnate, and the more densely branched ones appear furcate. The author says that the *fructification* is of two kinds: 1st, Tufts of branched moniliform cells, of a darker colour than the cells of the frond, each cell surrounded by a hyaline border; these tufts are situated on the rachis of the plumules and pinnæ, but are never formed from the terminal branchlets. 2nd, Elliptical cells, two or three in number, forming a whorl round, and semi-immersed in, the upper part of the articulations of the plumules. Both the tufts and the whorls of cells appear to contain granular matter, but show no appearance of being tetraspores. This very remarkable plant bears some resemblance to *Seriospora Griffithsiana*, but differs from it in its want of gloss, different colour, in the moniliform cells never being terminal, and not formed from the branchlets, but an independent growth on the rachis, and in the presence of the whorled elliptical cells. It is interesting to find that there is a specimen in Mrs. Griffiths' collection of Algæ belonging to the Linnean Society, which was gathered at Salcombe, in 1840, and which presents the same character of tufted cells, &c. This specimen is marked "Seirospora?"

The second species is *Nitophyllum thysanorhizans*, N.S.—*Microscopical Structure:* Cells polygonal, becoming smaller and quadrate at the margin of the frond; the cellular processes are composed of large elongate polygonal cells, which become smaller and very dense toward the point from which the roots arise. A network of minute veins traverses the whole of the frond, and is especially noticeable in the ultimate segments, the veins being formed of a single row of narrow, somewhat cylindrical cells. Tetraspores distinctly tripartite, collected into definite rounded sori in the apices of the ultimate segments. Capsular fructification not yet met with. *Habitat:* Thrown up on a mud-bank at Torpoint, and at Mount Edgcumbe, near Plymouth. Perennial? This interesting little plant has probably been overlooked for many years as a variety of *Rhodymenia bifida*, under which name I have several times received it, and have also seen it among the Algæ collected by the late Dr. Cocks, and now in the possession of the Linnean Society. This mistake has most likely arisen from the similarity of its branching to that of *R. bifida*, and perhaps also from the rare occurrence of its tetraspores. From *R. bifida*, however, and from *R. cristata*, which it also resembles, it is abundantly distinguished by its *definite* sori, and *tripartite* tetraspores. From *Nitophyllum punctatum*, to the narrow forms of which there is a close resemblance in colour and general appearance, it is separated by the tetraspores forming sori in the *apices* of the frond *only*, and by its *fimbriate margin*.

*The Gonidia of Lichens.*—In the 'Annales des Sciences Naturelles' (Botanique), vol. xvii., M. E. Bornet records a series of observations on the gonidia of lichens, made on species belonging to sixty different genera. The conclusion to which his examination has led the writer is that the relations of the hypha to the gonidia of lichens are of such a nature that they exclude the possibility of one of these organs being produced from the other; the theory of parasitism being the only one which can give a satisfactory explanation of these relations. He believes that in every known instance the gonidia of lichens can be produced upon a species of Alga. See also 'The Academy' (July).

*The Epidermis of the Tway-blade.*—Mr. Gulliver, F.R.S., who is so well known for his researches into the structure of plants, lately gave a lecture to one of the provincial societies on the above subject. He stated, says 'Science Gossip' (July), that though the epidermal cells of plants often afford good diagnostic characters, it is remarkable that they have been little used. The object of the present communication was to show that these cells of *Listera ovata* differ from those of other orchids. In this species, the epidermal cells on the under surface of the leaf have remarkably sinuous boundaries, so as to form a good example of that common kind of epidermis which botanists have named Colpenchyma, while on the upper surface of the leaf of that same plant the cells have smooth margins, more or less polygonal from mutual pressure of roundish or oblong cells. Thus, besides the stomata on the under side of the leaf, the epidermis of the two sides differs so plainly and curiously as to present very pretty microscopic objects. At the same time, for comparison, examinations were made of the corresponding tissue of *Orchis mascula*, *Orchis fusca*, *Ophrys muscifera*, and *Ophrys aranifera*, in every one of which the epidermal cells, on both the upper and under sides of the leaf, were much alike and—save the stomata on the under surface—resembling the same cells on the upper side of the leaf of the Tway-blade. To define the exact value of this character would require an examination of the wilderness of exotic orchids as well as all our native species; but the remarkable character now described suggests a wide and probably fertile field for future cultivation. At present we know that, among the Duckweeds, *Lemna minor* is easily distinguishable, by its sinuous epidermal cells, from *Wolffia arrhiza*, though these two plants were formerly considered as identical.

*The Embryo Pig's-head.*—Mr. W. K. Parker, F.R.S., has given a very valuable paper on this subject to the Royal Society. It will, we suppose, be published in the 'Transactions.' Meanwhile, we take the following account from the last number of the 'Proceedings of the Royal Society.' The number of embryo skulls examined is something surprising.

The most important results of the present investigation may be stated as follows:—

1. In a pig-embryo, in which the length of the body did not exceed two-thirds of an inch, and four postoral clefts were present, the craniofacial skeleton was found to consist of:—(*a*) The notochord, terminating by a rounded end immediately behind the pituitary body.

(*b*) On each side of the notochord, but below it, there is a cartilaginous plate, which in front ends by a rounded extremity on a level with the apex of the notochord, while behind it widens out and ends at the free lower margin of the occipital foramen. These two plates, taken together, constitute the "investing mass" of Rathke. In this stage they send up no prolongations around the occipital foramen; in other words, the rudiment of the basioccipital exists, but not of the exoccipital or superoccipital.

(*c*) The large oval auditory capsules lie on each side of the anterior half of the investing mass, with which they are but imperfectly united: there is no indication of the stapes at this stage.

(*d*) The *trabecular* or first pair of præoral visceral arches enclose a lyre-shaped pituitary space; they are closely applied together in front of this space, and, coalescing, give rise to an azygous prænasal rostrum. They are distinct from one another and the investing mass.

(*e*) The *pterygo-palatine* or second pair of visceral arches lie in the maxillo-palatine processes, and are therefore subocular in position. Each is a sigmoid bar of nascent cartilage, the incurved anterior end of which lies behind the internal nasal aperture, while the posterior extremity is curved outwards above the level of the angle of the mouth. The pterygo-palatine cartilages are perfectly free and distinct from the first præoral and from the first *postoral* arch.

(*f*) The *mandibular* or first pair of postoral visceral arches are stout continuous rods of cartilage which lie in the first visceral arch behind the mouth. The ventral or distal ends of these arches are not yet in contact; the dorsal or proximal end of each is somewhat pointed and sharply incurved, pushing inwards the membrane which closes the first visceral cleft and is the rudiment of the membrana tympani.

(*g*) The *hyoid* or second pair of postoral arches are in this stage extremely similar to the first pair, with which they are parallel. They are stout sigmoid rods of cartilage, which are separated at their distal ends, present an incurved process at their opposite extremities, and are not segmented.

(*h*) The *thyro-hyal* or third postoral arches, which correspond with the first branchial of the branchiate vertebrata, are represented by two short cartilaginous rods which lie on each side of the larynx.

(*i*) The olfactory sacs are surrounded by a cartilaginous capsule, which has coalesced below with the trabecula of its side; while, within, the mucous membrane lining the capsule presents elevations which indicate the position of the future turbinal outgrowth of the capsule.

In this stage the posterior nares are situated at the anterior part of the oral cavity, as in the Amphibia, and the roof of the mouth is formed by the floor of the skull, the palatal plate of the maxillæ and palatine bones being foreshadowed by mere folds. The outer end of the cleft between the first and second præoral arches is the rudiment of the lachrymal duct, while its inner end is the hinder nasal aperture. The gape of the mouth is the cleft between the second præoral and first postoral arch. The auditory passage, representing the Eustachian tube, tympanum, and external auditory meatus, is the cleft between the first and second postoral arches. The proximal end of the mandibular arch, therefore, lies in the front wall, and the hyoid in the hinder wall of the auditory passage.

2. In an embryo pig, an inch in length, (*a*) the notochord is still visible; (*b*) the investing mass, the halves of which are completely confluent, has become thoroughly chondrified, and is continued upwards at each side of the occipital foramen to form an arch over it.

(*c*) The auditory capsules are still distinct from the investing mass, and a plug on the outer cartilaginous wall of each has become marked off as the stapes.

(*d*) The hinder ends of the trabecular arches have coalesced in front of the pituitary body, but they are not yet confluent with the investing mass.

(*e*) The pterygo-palatine rods have increased in size ; they have not become hyaline cartilage, but are beginning to ossify in their centre.

(*f*) In the mandibular arch the proximal end has become somewhat bulbous, and is recognizable as the head of the malleus, whilst the incurved process, still more prominent than before, is the *manubrium mallei*. The rest of the arch is Meckel's cartilage ; outside this a mass of tissue appears, which is converted into cartilage, rapidly ossifies, and eventually becomes the ramus of the mandible.

(*g*) The proximal end of the hyoidean arch, similarly enlarging and articulating with the corresponding part of the mandibular arch, becomes the incus, the incurved process attaching itself to the outer surface of the stapes and becoming the long process of the incus. The incus, thus formed out of the proximal end of the hyoidean arch, becomes separated from the rest of the arch by conversion of part of the arch into fibrous tissue, and by the moving downwards and backwards of the proper hyoid portion of the arch. A nodule of cartilage left in the fibrous connecting band becomes a styliform *interhyal* cartilage, while the proximal end of the detached arch becomes the *stylo-hyal*.

(*h*) The *thyro-hyals* have merely increased in size and density; they closely embrace the larynx by their upper ends.

(*i*) The olfactory capsules are well chondrified ; their descending inner edges have coalesced with each other, and below with the trabeculæ to form the great median septum: the turbinal outgrowths are apparent.

In this stage the alisphenoids and orbito-sphenoids appear as chondrifications of the walls of the skull, quite separate from the investing mass, and from the trabeculæ.

The floor of the pituitary space chondrifies independently of the trabeculæ and investing mass, but serves to unite these four cartilaginous tracts.

3. In an embryo pig, $1\frac{1}{3}$ inch in length (*a*, *b*, *c*), the primordial cranium is completely constituted as a cartilaginous whole, formed by the coalescence of the investing mass and its exoccipital and superoccipital prolongations, the modified trabeculæ, the subpituitary cartilage, the auditory capsules, and alisphenoidal and orbito-sphenoidal cartilages, and the olfactory capsules. The notochord is yet to be seen extending in the middle line from the hinder wall of the pituitary fossa (now the "*dorsum sellæ*") to the posterior edge of the occipital region.

(*d*) The trabecular arches form the sides of the sella turcica, the presphenoid, and the base of the septum between the olfactory capsules; in front, where they form the azygous "prænasal," they are developed backwards as "recurrent bands," elongations of their free recurved "cornua."

(*e*) The pterygo-palatine arches, still increasing in size, but not chondrifying, are rapidly ossifying; they are half-coiled laminæ bounding the posterior nasal passages.

(*f*) The mandibular arch and the rudimental ramus have become solid cartilage, and the latter is ossifying as the dentary; the distal part of each mandibular rod unites with its fellow for some distance.

(*g*) The hyoid arches are each fully segmented as incus, with its "orbicular" head, interhyal, stylo-hyal, and cerato-hyal.

(*h*) The thyro-hyals are merely larger and denser.

(*i*) The olfactory capsules have the turbinal outgrowths all marked out as alinasal, nasal, and upper, middle, and lower turbinals.

4. In pigs of larger size the form and proportions of the parts of the cranium become greatly altered, and ossification takes place on an extensive scale, but no new structure is added.

5. It follows from these facts that the mammalian skull, in an early embryonic condition, is strictly comparable with that of an Osseous Fish, a Frog, or a Bird, at a like period of development, consisting, as it does, of

(*a*) A cartilaginous basicranial plate embracing the notochord, and, like it, stopping behind the pituitary body.

(*b*) Paired cartilaginous arches, of which two are præoral, while the rest are postoral.

(*c*) A pair of cartilaginous auditory capsules.

(*d*) A pair of cartilaginous nasal capsules.

Further, that in the Mammal, as in the other Vertebrata, the development of the skull of which has been examined, the basicranial plate grows up as an arch over the occipital region of the skull, and coalesces with the auditory capsules, laterally, to give rise to the primordial skeleton of the occipital, periotic, and basisphenoidal regions of the skull. The trabeculæ become fused together, and, uniting with the olfactory capsules, give rise to the presphenoidal and ethmoidal parts of the cranium; and the moieties of the skull, thus resulting from the metamorphosis of totally different morphological elements, become united, and give rise to the primordial cranium.

As in the Salmon and Fowl, the second pair of præoral arches give rise to the pterygo-palatine apparatus. In the Frog this arch is late in appearance, and is never distinct from the trabecular and mandibular bars, serving as a conjugational band between them. The mandibular arch, which in the Salmon becomes converted into Meckel's cartilage, the os articulare, the os quadratum, and the os metapterygoideum, in the Frog into Meckel's cartilage and the quadrate cartilage (which early becomes confluent with the periotic capsule), in the Bird into Meckel's cartilage, the os articulare, and the os quadratum (which articulates movably with the periotic capsule), in the Pig is metamorphosed into Meckel's cartilage and the malleus, which is loosely connected with the tegmen tympani, an outgrowth of the periotic capsule.

Meckel's cartilage persists in the Fish and in the Amphibia, but disappears early in the Bird, and still earlier in the Mammal. The permanent ossifications of the mandible are all membrane bones in Fish, Frog, and Fowl; but in the Mammal (exceptionally) the ramus has a cartilaginous foundation. The hyoidean becomes closely united with the mandibular arch, and then segmented, in the Fish, into the hyo-mandibular, the stylo-hyal, cerato-hyal, and hypohyal—the hyo-mandibular, or proximal segment, articulating with the outer wall of the periotic, and many of the segments of the arch becoming dislocated.

In the Frog the hyoid also becomes segmented, but only after extensive coalescence with the mandibular arch. The proximal segment becomes the suprastapedial (hyo-mandibular) with its extrastapedial process, and, extending inwards as mediostapedial and interstapedial, articulates with the stapes, developed by segmentation from the outer wall of the auditory capsule. The stylo-hyal is dislocated, and becomes connected with the auditory capsule below the stapes (opisthotic region).

In the Bird the hyoidean arch remains distinct from the mandibular. Whilst in its primordial condition it coalesces by its incurved apex with the auditory capsule in front of the promontory, before the stapedial plug is segmented. It then chondrifies as three distinct cartilages—an incudal, a stylo-hyal, and, distally, a cerato-hyal. The stapes becomes free from the auditory capsule, but remains united with the cartilaginous part of the incus (mediostapedial); the ascending part is largely fibrous (suprastapedial), and the part loosely attached to the mandibular arch is the elongated extrastapedial. The short stylo-hyal afterwards coalesces with the body of the upper or incudal segment by an aftergrowth of cartilage (the *interhyal* tract); a long membranous space intervenes between it and the glossal piece (cerato-hyal). Thus the "columella" of the Bird is formed of one periotic and three hyoidean segments.

In the Pig the hyoidean arch is distinct, but articulates closely with the mandibular; its upper segment (hyo-mandibular) is converted into the incus, and becomes connected with the stapes. The stylo-hyal is dislocated, and coalesces with the opisthotic region of the auditory capsule.

*Retrograde Changes in the New Formation of Blood-vessels in Bone and Cartilage.*—In the 'Medicin Jahrbücher' (vol. iv., 1873) Herr Heitzman asserts, says Dr. E. Klein in the 'Medical Record' (August 6th), that in long bones of young dogs the material contained in the vascular canals is, up to the blood-vessels, gradually transformed into bone-tissue. The blood-vessel itself, after having changed into a solid protoplasmic cone, finally also gives origin to bone-cells and bone ground-substance. In bones artificially inflamed, an abundant formation of new vessels of a capillary character takes place from the elements of the decalcified tissue as well as from those lying in the absorption cavities, *viz.* derived from bone-cells. The former Heitzman observed in a scapula of a cat in the third day of inflammation, produced by injuring its posterior margin with a forceps; the latter in that of a dog in the fourth day of inflammation, produced by perforating its centre. In both instances Heitzman was able to follow the transformation of hæmatoblasts, not only into coloured blood-corpuscles but also into blood-vessels. The latter takes place in either of the following manners :—(*a*) Hæmatoblastic elements, having become vacuolated, lengthen themselves, coalesce at their extremities, and after the disappearance of their corresponding septa a blood-vessel is finished, which in many instances contains new-formed blood-disks. Or (*b*) a number of hæmatoblasts—the offspring of bone-cells—which lie in the absorption cavities, become fused together so as to form multinucleated masses ; the central part of these gives origin to blood-disks, whereas the peripheral nucleated portion represents the wall of a blood-vessel, a row of such structures having coalesced with each other. Having formerly stated that in the calcification region cartilage-cells give origin to coloured blood-corpuscles, Heitzman now finds that blood-vessels are formed at those places as well, the central yellow shining portion—hæmatoblastic portion—of the cartilage-cells being transformed into a vesicle filled with blood-corpuscles. These vesicles are in general pear-shaped ; one of their extremities, that which is drawn out into a thin solid process, being directed towards the centre of the bone. From the fusion of several such vesicles a blood-vessel proceeds. In a similar way a rapid formation of blood-vessels from cartilage-cells is to be found in inflamed as well as in new-formed cartilage, *e. g.* in the callus of fractures.

*The Reproduction of Duckweed in Winter.*—Professor Biscoe gives a paper in the 'American Naturalist' (May), illustrated by drawings, of microscopic work undertaken with a view of testing the mode by which the minute white " winter fronds" of *Lemna polyrrhiza* develop into the well-known green summer flowering and rooting fronds. He finds that the rudiments of both leaf-buds and roots are to be detected, by careful dissection, in the apparently dead winter fronds.

*Professor Agassiz's New Mode of Teaching.*—At the opening of the new School of Natural History, at Penikese Island, the other day, Professor Agassiz, in his opening address, said :—" Our chief work will be to watch the aquarium. I want you to study principally marine animals. The only way to do that properly, is to have them alive by your side. In a very few days I shall place at your disposal a series of these appliances. I have ordered one for every person admitted to the school, so that each of you will have means to make these investigations. I have never had, in my own laboratory, better opportunities for work than I place at your disposal. Our way of studying will be somewhat different from the instruction generally given in schools. I want to make it so very different, that it may appear that there is something left to be done in the system adopted in our public schools. I think that pupils are made too much to turn their attention to books, and the teacher is left a simple machine of study. That should be done away with amongst us. I shall never make you repeat what you have been told, but constantly ask you what you have seen yourselves."

*Development of the Ovule and Fertilization in Primulaceæ.*—Professor P. M. Duncan read a very important paper on this subject at the meeting of the Linnean Society, held June 19th, which is thus abstracted by 'The Academy.' He controverts the published views of Duchartre that the "free central" placenta of Primulaceæ is formed perfectly free within the cavity of the ovary, and never at any time has any connection with the ovarian wall, and finds on the contrary that the placenta and ovarian wall separate from one another by a process of differentiation. The ovules are of a very simple structure, consisting of nothing but a single integument covering the embryo-sac; there is no inner integument and no nucleus. The lower part of the style consists of dense tissue absolutely impermeable to the pollen tubes; and even if these were able to enter the ovary in this way they would be quite unable to reach the micropyle of the ovule, from its close contiguity to the placenta. Professor Duncan has traced the course of the pollen tubes from the base of the style through the loose tissue of the placenta itself, from which they emerge in the immediate neighbourhood of the micropyles of the ovules, which they then enter.

*The Mycelium of Agarics.*—A paper on this interesting subject is given in 'Grevillea' (July) by M. J. De Seynes. The mycelium, he says, the elementary composition of which is very simple, found under the soil, or under the *débris* of dead leaves or branches, affects different appearances, generally white, sometimes yellow, and also red. It is at times filamentous or silky (*nematoid* mycelium of M. Léveillé),* at times like felt (*hymenoid* mycelium of the same author); finally, at times it becomes compact and solid, for a long time regarded as a perfect fungus, and was called *Sclerotium;* this is the *scleroid* or tuberculous mycelium of M. Léveillé. This author has also signalized the *malacoid*, or pulpous mycelium belonging to some *Physariacei*, or to some *Trichiacei*, the fungoid nature of which is actually contested.† The nematoid mycelium, which is more frequently found amongst Agarics, varies extremely in appearance, at times presenting itself like some rayed threads of silk, and prickly; at times ramified or dichotomous, like some radicular fibres, and at times so thin that it is easily pulverized; it certainly has its characteristic value. Hoffmann draws from its absence, or its concrete form, a conclusion which appears to us quite just. "That there is more difference," says this author, "than the kind of development in *Amanita* without a mycelium, which recals the *Gasteromycetes*, and among which the mycelium is replaced by the veil, and some Agarics, with a permanent mycelium in the form of *Sclerotium*, as for example, *Agaricus tuberosus*." ‡ One can, perhaps, place more value on the permanence or annual disappearance of the mycelium, than to the perennial, or to the annual or biennial life of the stem of Phanerogams; where the form of the organs of vegetation so notably differs, it follows that they are monocotyledons or dicotyledons; the

* 'Annales des Sci. Nat.,' 2nd ser., t. xx., p. 78, &c.

† The observations of Wigand (Pringsheim's Jahrbücher) appear to me to shake strongly the hypothesis of M. de Barry, as to the animal nature of these small productions.

‡ Hoffmann, 'Icon. Analyt. Fung.,' Heft i., 1861.

mycelium may affect different modes of development, as in the two examples cited by Hoffmann. The concrete mycelium or sclerotium is rather scattered amongst the Agarics, as the remarkable researches of Léveillé have demonstrated it, and removed all doubts on the subject. In his recent work,* M. Tulasne gives a rather instructive history of *Sclerotium*, which appears to be most complete on the subject.

M. Léveillé has indicated the mode of sclerotial formation. which has greater analogy with the rhizome, as is remarked in *Agaricus fusipes;* the base of the pedicel is permanent, and produces the following year some new Agarics, becoming more or less branched. The mycelioid nature of the *Sclerotium*, and its assimilation to the organs which, in the Phanerogams, take the place of veritable stems, is a proof more in favour of the theory, first noticed by Palissot de Beauvois, and then by Dutrochet, of the identity of the mycelium with a stem of thallus.

## NOTES AND MEMORANDA.

**Herr Gegenbauer, now at Heidelberg.** — Dr. Gegenbauer, of Jena, the well-known Comparative Anatomist, has been nominated ordinary Professor of Anatomy and Director of the Anatomical Institute in the University of Heidelberg.

**Crystals in the Seed-coat of the Elm (Ulmus campestris).**— 'Science Gossip' (August) says that at this season, or a little earlier, the fruit of the elm is shed and scattered in profusion on the ground, often so as to make patches in our paths. Each fruit is a capsule, somewhat oval, very flat, and about as big as the thumb-nail. The seed is contained near the centre of this compressed and winged capsule or samara, and the outer coat of the seed is the seat of the crystals. Every cell of this part contains a short and brilliant crystal, in form cubical, lozenge-shaped, or prismatic, and presenting a long diameter of about $\frac{1}{2666}$th, and a short diameter of $\frac{1}{3555}$th of an inch. They are beautiful microscopic objects, and perhaps may be found well adapted for experiments with polarized light. The crystals are composed chiefly of oxalate of lime.

**An American Criticism on Dr. Maddox's Simple Mount.**—This we cannot allow to escape us, though it appeared some time since, the fact being that we have only now had our attention directed to it. The writer in the 'Lens' says that anything coming from Dr. Maddox in the microscope line may be anticipated to be good, and no one can be surprised that he says, "It works quickly, easily, has considerable range, and no sensible slip." By slip he undoubtedly means what the mechanic terms back-lash; a fault that is so annoying to the microscopist, and almost universally found in objectives imported from Europe. Hundreds of American microscopists will confirm Dr.

* 'Selecta Fungorum Carpologia,' p. 107.

Maddox's opinion of his "simple mount," for essentially it is the same as Tolles devised, and has used for some ten years past. There are some minor details of construction in which the two differ, *viz.* Dr. M. introduces a spiral spring of two turns, Tolles a spring of several turns. Maddox's spring lifts the tube, Tolies' depresses it. These differences are not essential. Maddox's spring acts against one steel pin screwed into the inner tube. This pin must be liable to wear in its bearing in the thin inner tube; and besides, the pressure of the spring acts on one side only of the tube, having a tendency to press it sideways. These defects are remedied in Tolles' mount. But Dr. Maddox takes no notice of the most important point in this arrangement, that is, moving the inside bases instead of the front base. Mr. Wenham many years ago devised some means of moving the middle and back bases, leaving the front base stationary. Although he spoke of this plan as a great improvement on the old one, although it has been highly commended by those who have had objectives specially mounted so since, yet it has not been adopted by the English makers, or by any American except Tolles. Why? The only explanation seems to be, that such construction, if done well by first-class workmen—and it must be done as only the best workmen can do it, or it will not be satisfactory—will cost from one to three guineas extra for each objective.

**Monochromatic Light in the Study of Diatoms.** — Professor J. Edward Smith says:—"I have recently been using monochromatic light for the study of the finer diatoms. A rude appliance for this purpose can be arranged in a very few moments, as follows:—Take a piece of thin board, say 15 × 20 inches, and provide several pieces of plain cleaned glass, either light-green or blue; spectacle glasses will answer. Cut a hole of proper size through the board, and at about the height of level of microscope stage; this aperture to be occupied by the coloured glasses, using the combination which proves to give the best definition. At present I am using one pale-blue outside and four interior ones of light-green, all placed in contact. The combination should be deep enough to prevent any blazing effect when the full beam is turned on. Such a contrivance, so placed as to transmit the solar rays to the mirror of the instrument, will prove to be far superior to any lamplight illumination, and no condensers required. With it, and a Tolles' $\frac{1}{8}$th dry, or $\frac{1}{10}$th wet objective, I have easily shown *Amphipleura pellucida* on balsam in beads, under high eye-piecing, and with lowest eye-piece the transverse and *longitudinal* 'striæ' are easily seen. Nos. 18, 19, and 20 of Möller's Probe Plate, which have resisted my protracted efforts by lamplight, yield at once to this illumination. Probably other combinations of coloured glass may be found superior to that described."

**Young Octopods at the Brighton Aquarium.**—It is to be hoped that the Brighton Aquarium people will some of them see the splendid opportunities which are being daily presented of studying the development of rare animals. We understand that the octopus has deposited its spawn in the Aquarium, and that it has been regularly hatched. We trust that the several operations in the changes of the ovum have been carefully watched.

**Dr. Pettigrew's New Appointment.**—We are glad to learn that Dr. J. Bell Pettigrew, F.R.S., has been appointed Lecturer on Physiology at the School of Medicine, Surgeons' Hall, Edinburgh. Dr. Pettigrew is well known by his able researches into the structure of the heart and stomach and by his valuable investigations of the organs of flight in animals, and his recent lectures on the apparatus of the circulation. We congratulate the school and the lecturer on the appointment.

**A Chair of Normal and Pathological Histology** has been founded by the Spanish republican government in the University of Madrid, and, according to the 'Medical Record,' endowed with a salary of 5000 pesetas (210*l.*). The medical faculty of the University of Valencia has protested against the establishment of a similar chair in that institution, on the grounds, *inter alia*, that the subjects are already taught by the several professors.

**What is the Thread Blight?**—At a recent meeting of the Royal Horticultural Society, the Rev. M. J. Berkeley stated that he had provisionally referred the thread blight which had attacked the tea plantations in India to *Corticium repens* Berk.

**A New Slide for the Microscope.**—At a late meeting of the American Philosophical Society Mr. Holman exhibited a slide for the microscope, designed for the better observation of substances suspended in fluids, especially the different corpuscles of the blood. The slide contained two concavities on its face, which were connected by a groove, and covered by a thin plate of glass. It was highly sensitive to changes of temperature.

## CORRESPONDENCE.

### Reply to the Note in July Number, headed "Information required as to Microscopic Powers."

*To the Editor of the 'Monthly Microscopical Journal.'*

Wayland, New York, U.S., *July* 25, 1873.

Sir,—With your permission I will offer a few remarks by way of reply to the questions of H. H., in the July number of the 'Monthly Microscopical Journal,' page 39.

However important the first question may be, it is one which it is extremely difficult at the present time to answer. Considerable margin must be left for error in any estimate that may be made of the dimensions of the most minute particles of matter. The smallest particles that I have yet been able to detect with any degree of certainty are estimated at $\frac{1}{200000}$th of an inch diameter. The observations were made with a new $\frac{1}{50}$th immersion of 165° angle aperture, made by R. B. Tolles, Boston, Mass.

The method used by me "of arriving at an estimate" is a slight modification of the old double-sight mode. I paste a piece of paper of suitable colour on the end of the object slide, on which a series of dots are made of various known sizes. The magnified molecules in the instrument are readily compared with these dots or circles by double sight, and the magnifying power being known, by a simple calculation, their sizes are approximately ascertained. This is not given as the best possible method, but as one having some advantages.

In regard to the third question, *viz.* "Have the most recently constructed microscope objectives, such as the $\frac{1}{50}$th or $\frac{1}{25}$th, any advantages over the $\frac{1}{16}$th or $\frac{1}{12}$th inch objectives in the determination of the data above referred to? And have immersion lenses any advantage in this respect?" I would suggest that the great uncertainty in determining the size of extremely minute particles renders any comparison based on such observations alone, almost, yea altogether, worthless. It seems to me therefore that it would be better to depend on the recognized tests of definition, at least for the present, and draw the legitimate inference that an objective that gives the finest definition on these will also do the best work on the "particles."

The $\frac{1}{50}$th above referred to has in my hands done better work on the most difficult test scales and diatoms than has up to this time been done by any "$\frac{1}{12}$th or $\frac{1}{16}$th," as far as is known to me through the published performance of other lenses or otherwise. First-class $\frac{1}{5}$ths to $\frac{1}{20}$th are showing the transverse striæ of *Amphipleura pellucida*, *Navicula crassenervis*, *Frustulia saxonica*, and *Nitzschia curvula*. The $\frac{1}{50}$th reveals *longitudinal* lines on all these, much finer than the transverse, and evidently genuine. Under favourable conditions the resolution into the so-called beading is distinctly effected on the first three named. The diameter of each dot on *Amphipleura pellucida* is probably not over the $\frac{1}{240000}$th of an inch. *Hyalodiscus subtilis Bailey* is instantly seen covered, throughout the hyaline portion, with nicely defined hexagons in place of the fine "rulings."

This is done with sunlight and the ammonio-sulphate of copper cell. With ordinary day or lamp illumination, either central or oblique, on scales or Bacteria the performance is also *excellent*.

This objective works either dry or wet by turning the adjusting collar, but its immersion work is preferred.

The preceding facts appear to me to demonstrate the superiority of the best $\frac{1}{50}$th over the medium powers, for the kind of investigation H. H. is engaged in.

Yours respectfully,

G. W. Morehouse.

## "Audi Alteram Partem."

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—I thank you for inserting my letter (in your last number, page 100) upon the new, or immersion, mode of using achromatics.

But, as there are always two sides to every question, it is but fair and right to state what may be said on the other side.

And first, it is a decided objection that the interposed drop of water greatly prevents our judging of the actual distance of the outer lens from the covering glass; and in consequence of this, very vexatious accidents may happen—*exempli gratia*, one of my first essays with the new plan was upon the "*Diatomaceen Typen Platte*"; and I would caution those who possess that somewhat costly little curiosity to take care how they examine it with the "aquatic"; for not being, then, aware how much the focal distance was increased by the new mode, and it being, moreover, difficult to know exactly *how* near the glass was, for the reason already stated, I naturally thought the glass was *without* the focus, when it really was *within* it; and therefore turned downwards, very "gingerly," until I was suddenly appalled by a horrid crash! and, on examination, found to my sorrow that the "aquatic" had thrust his "nozzle" right through the covering glass! And this leads me to observe that the said *Typen Platte* is not made in the same way as our English diatom slides are. The latter are made by merely placing the diatoms on the slide, pouring on a little balsam, and then, having indurated it by heating over a spirit-lamp, the covering glass is pressed down upon it; and thus the slide, balsam, and covering glass become as though one solid mass; and such a slide will bear to have the object-glass turned down upon it, until the safety-spring (with which all good microscopes are furnished) yields, and so gives notice that there is contact.

But let everyone beware of doing this with the *Diatomaceen Typen Platte*. The latter, I understand, is made by "spinning" a ring of balsam upon the slide. Then, when that is hardened, some very thin balsam or varnish is poured on; and the covering glass (to which the diatoms have been previously attached) is gently placed upon the aforesaid ring. Now, by this mode, there is a space *between* the slide and covering glass which is filled with balsam, or some equivalent, in nearly a fluid state; and the covering glass being about as thin as a bank-note, it will not bear the slightest contact of the achromatic without destruction! Under this head I may mention one important fact. When the aforesaid accident has occurred, and the unfortunate "Platte" appears "lost beyond redemption," it may still, in a measure, be restored in the following manner:—

Provide a disk of the thinnest glass, of the exact size of the outer glass of the *Platte* (which is four-eighths and a sixteenth, English measure), and then, having dropped a little hot and very fluid balsam upon the fractured glass, gently press on the new cover. This will heal up the cracks, rendering them nearly invisible; and the "Platte" takes a "new lease of life."

I would willingly end here; but should like to state objection

No. 2 against the new mode. It is as follows:—I may be mistaken, and hope I am so; but it appears to me that although the new mode shows objects with greater *brilliancy* than before, yet it is gained at the loss of perfect *achromatism*. For example, if I examine a valve of the *Pleurosigma Formosum*, under my "new sixteenth," arranged in the usual way, the beautiful "markings," which under a lower power (say $\frac{1}{2}$ or $\frac{1}{4}$ inch) appear as fine crossed lines, are resolved into regular rows of *dots;* but without any particular colour. They are merely dark dots, and nothing more. But on applying the "aquatic" the said dots are immediately converted into brilliant little gems, resembling rows of rubies! making the object far prettier, doubtless; but the question is, is it *right?*

Perhaps some of your numerous readers will kindly give me their opinion? Meanwhile, I shall be happy to exhibit these effects to anyone who may be coming this way, and may consider it worth his while to call.

Many years ago, a microscopical friend said to me, "The best test for perfect achromatism in a low power ($\frac{1}{2}$ or $\frac{1}{4}$ inch) is the minute pops (or 'glands' as they are called) seen between the lines in a longitudinal slice of coniferous wood; common deal for example." And, truly, I never yet saw a $\frac{1}{2}$ inch that would show them without colour.

But here is the question. Is the said colour *real?* or is it caused by want of perfect achromatism in the object-glass? or is it the result of some kind of dichroism, or semi-polarization?

I should really be glad to know.

I will merely add that the *P. Formosum* above-mentioned is prepared in Canada balsam, in the usual way.

Yours respectfully,

H. U. Janson.

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## The Aperture Question.

*To the Editor of the 'Monthly Microscopical Journal.'*

Padnal Hall, Chadwell Heath, Essex, *Aug.* 1, 1873.

Sir,—Mr. Tolles will, I hope, excuse me from making any further references to past sentences, which can scarcely affect a position that must now be well understood. Whether in his measurements he did close the lenses to a degree or so beyond what I had done, cannot alter the principle. I have therefore finally to thank him for the equanimity and good humour that he has maintained in the controversy.

As a word in answer to Col. Woodward's omitted letter (dated May 19), I may state, that I had not the slightest wish to disparage the glass sent here for trial; for it has always been my desire to avoid the publication of comparisons either against or for any existing makers, by which their respective partisans too often appear as advertising mediums.

When Mr. Tolles' glass was handed to me, I at once noticed the neatness and accuracy of the workmanship; but it must be borne in mind, that in this particular case the glass was heralded long before its arrival with the announcement that it was to prove a peculiar condition or advantage which no English glass would be found to have. Naturally a number of microscopists were curious to see its performance, and comparisons became inevitable. The variety of test objects used were such as are usually sold, mounted under covers something near $\frac{1}{500}$ thick. I admit that I did not try anything as thick as $\frac{1}{75}$th; few glasses of this power are made for such covers, I am therefore quite ready to allow that, under these conditions, the performance might have been different.

Your obedient servant,

F. H. Wenham.

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## Inexperienced Artists *v.* Experienced Ones.

*To the Editor of the 'Monthly Microscopical Journal.'*

London, *Aug.* 11, 1873.

Sir,—Amid the numerous singular letters which have appeared in " *re* Pigott," none are more singular than that by Mr. B. D. Jackson, in your last number.

Mr. Jackson evidently mistakes the reasoning of the two authors, one of whom is estimating the value of positive and the other of negative evidence.

Dr. Pigott's argument may thus be put.

If a person, totally unacquainted with an object or a subject in dispute, make a drawing clearly depicting certain structure stated by A. B. to exist, it ought, *à fortiori*, to be clear as noonday to the practised observer, and it necessarily follows, if the unskilled observer represent with his pencil that which A. B. has described with his pen, that the latter has not drawn on his imagination.

The argument in Schleiden is exactly the reverse of this.

If a person, ignorant of the structure of an object, make a drawing, *omitting* certain well-known organs or developments, it is not to be inferred that such structure does not exist. On the contrary, such *omissions* are in the highest degree probable, and therefore such drawings are, from their *probable omissions*, valueless.

As a matter of positive evidence none can be stronger than that of a disinterested witness, and our late President, Mr. Reade, tells us in his paper "On the Diatom Prism" that he had the "evidence of an unprejudiced witness," for a boy, looking at Formosum through his instrument, saw what looked like a " plate of marbles"—this was conclusive as to the optical appearance.

Let us suppose that in Central Africa Dr. Livingstone is assured by a savage tribe that a civilized stranger has passed that way, the evidence will be unsatisfactory by itself; but let these " Inexperienced Artists" make a sketch, however rough, of a microscope in the traveller's possession, and all doubt vanishes. Such illustrations might be furnished without end, conclusively showing that, although we do not value the savage's drawings for one purpose, for others they are invaluable.

I think your readers will see that the quotation from Schleiden is anything but "apposite"; both authors are perfectly right, they are treating of different subjects, and their apparent divergence is purely imaginary.

Yours, &c.,

LEX.

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## PROCEEDINGS OF SOCIETIES.

### BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

July 24th. — Microscopical Meeting. Mr. J. J. Sewell, Vice-President, in the chair.

The subject for the evening, "Cements," was introduced by Dr. Hallifax.

All, he said, who had mounted objects had found some objection to the different cements and varnishes recommended in the manuals on the microscope. Against some the objection was that, after a time, they cracked and peeled off; while others gave off exhalations which not only clouded the covering glass, but often ruined the object; this latter fault belonged especially to all cements containing oily substances. Dr. Carpenter, no mean authority on such a subject, advised that all valuable slides should be varnished annually for the purpose of preserving them, thus showing, as the result of his experience, the untrustworthiness of the cements in general use. There was one cement recommended in books against which, as far as his experience went, such objections as those he had mentioned could not be raised; this was sealing-wax varnish or cement. This arose, he believed, from the nature of the composition; the best sealing-wax, according to Ure, being composed of 20 per cent. of Canada balsam, 50 per cent. of shellac, a small quantity of balsam of tolu, and the rest colouring matter. He had for years employed a cement and varnish made by dissolving the best sealing-wax (powdered) in alcohol, and had experienced neither leakage, chipping, flaking, nor exhalations; in fact, it was the most trustworthy cement he had employed for years.

Some little time since Mr. Wonfor showed him some slides, which had been sent him by Mr. Curties, the cells of which consisted of concentric rings of different colours, presenting to some a fancy appearance. At the same time it struck him they were composed of a new cement. After various experiments he came to the conclusion that Canada balsam formed a considerable item in the composition. It would be seen by the slides, which Mr. Wonfor and he would exhibit, that if he had not produced an identical cement, he had made one similar in appearance, and, he believed, perfectly trustworthy. It had been produced by mingling with Canada balsam different pigments; and if it were urged that the addition of litharge to gold size made it less trustworthy, it should be borne in mind gold size was an oily preparation, while Canada balsam was not.

Canada balsam, by long experience, had been found to be the simplest, purest, most manageable and best working of all the *media* in which to mount objects; and if, at the same time, it could be made to work with a brush, it then could be utilized as a cement.

Canada balsam was known to be soluble in various substances, such as ether, chloroform, and turpentine, none of which, from various reasons, he should recommend; but there was another solvent, which he had used for some time, *viz.* benzole. Since employing it as a solvent, he had found that Dr. Bastian had spoken very highly of it in the pages of the 'Monthly Microscopical Journal.' When thinned by the admixture of benzole, it dried rapidly; it also readily mingled with insoluble substances; in fact, formed paints.

Bearing in mind that lead was used in the manufacture of paints, and that white-lead was the basis of some cements for repairing china and glass, he thought, if he blended white-lead with balsam he should obtain a trustworthy cement. Taking, then, Canada balsam and white-lead as the basis of his experiments, he had produced the results he had handed round. So tenacious was it that he had found the greatest difficulty in removing a covering glass fixed by this *medium*, and the specimen of two pieces of glass united by its means showed its tenacity. There was one additional advantage in this cement, it would take any colour, *viz.* such pigments as were used by the colourman in making paints.

His mode of operation, in making the white cement, was to rub down, on a piece of glass, used as a slab, white-lead with Canada balsam, thinned with benzole, until it would run freely with a brush. For a thicker cement he added more lead. To obtain the colour seen on some of the slides he had rubbed down, in a similar way, the powdered pigments obtainable at any colourman's. If some objected to colour, or the addition of an insoluble substance, then balsam thinned with benzole could be used alone. In many of the slides exhibited he had put the white cement over old mountings and then added the coloured rings. He found the cement and varnish dried quickly and acquired a high polish. Other balsams or resins might be found which might do as well, but he preferred Canada balsam, because it was very durable and worked easily; the white-lead gave it body, firmness, and drying properties.

Some might think the subject trivial, but when one heard on all hands of spoilt slides, through the use of untrustworthy cements, anything likely to turn out a secure cement was worth consideration.

Mr. C. P. Smith mentioned, in illustration of the untrustworthiness of gold size thickened with litharge, that Jenner had spent almost a lifetime in preparing diatoms and desmids, using gold size thickened with litharge as a cement. At his death the whole collection was found to be worthless.

Mr. W. H. Smith thought methylated spirit, not the *finish* of commerce, would be better in making sealing-wax varnish, being cheaper and 4 per cent. stronger than alcohol; the addition of a small quantity of balsam of tolu would render it softer.

Mr. Benjamin Lomax felt sure white-lead and balsam would prove permanent; they would form a superior kind of paint.

Mr. Wonfor spoke of sealing-wax varnish as being unchanged after several years' use. Some made seven years ago was still as good as when made. Dr. Hallifax and he found an advantage in using a small quantity of chloroform with the alcohol in dissolving the sealing-wax. When talking with the Doctor about the new cement, he understood him to say he had used benzoline, so he obtained the ordinary form used for illumination, and employed it in the same way as Dr. Hallifax had the benzole. He found it worked easily. It was far cheaper and dried readily. As a proof of the last-named quality, some of the slides he had exhibited had not been made six hours and were now nearly dry.

In proposing a vote of thanks to Dr. Hallifax, both the chairman and Mr. G. D. Sawyer thought the information both useful and practical. That the subject was important might be gathered from the examples quoted by Mr. Smith and others.

Dr. Hallifax thought their thanks were due to Mr. Wonfor for introducing the subject to him and confirming his observations and experiments.

Mr. Wonfor repudiated being anything more than a pupil of an admirable instructor, from whom he had constantly received very useful hints in mounting and preparation.

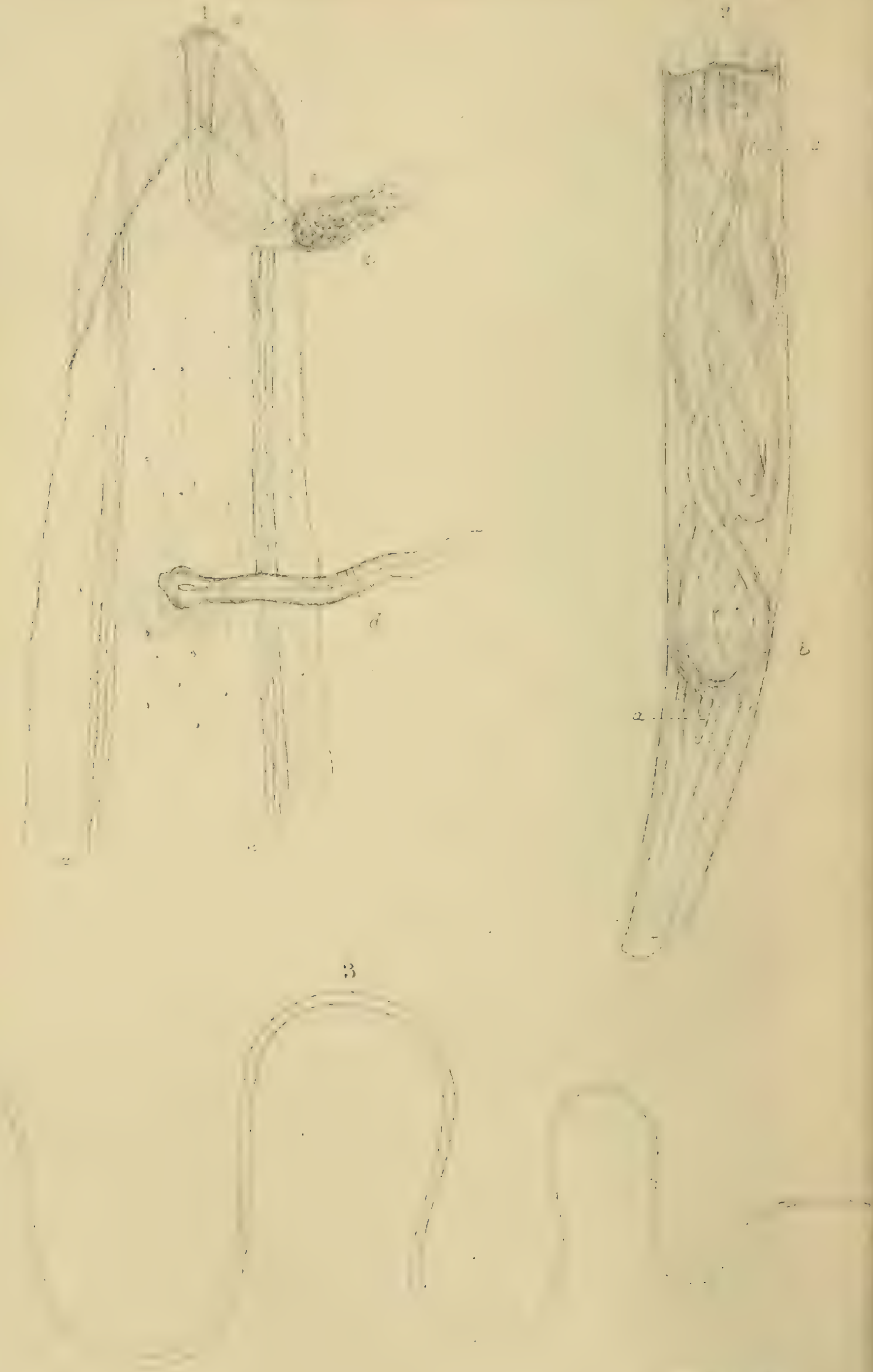
Previous to the above some rare plants, gathered in Sussex, and presented by Mr. G. Davies and Mr. Birch Wolfe, were exhibited, including *Lolium temulentum*, poisonous darnel, *Rhinodina exigua* var. *horiza*, and *Specularia hybrida*.

## Medical Microscopical Society.

The seventh meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital on the 18th of June, Jabez Hogg, Esq., President, in the chair.

The necessary business of the Society having been transacted, the meeting resolved itself at once into a conversazione, no paper being read. Numerous and interesting microscopical specimens were exhibited by the members.

The next meeting will not take place till the ensuing winter session, and will be held on Friday, Oct. 17th, at 8 P.M.

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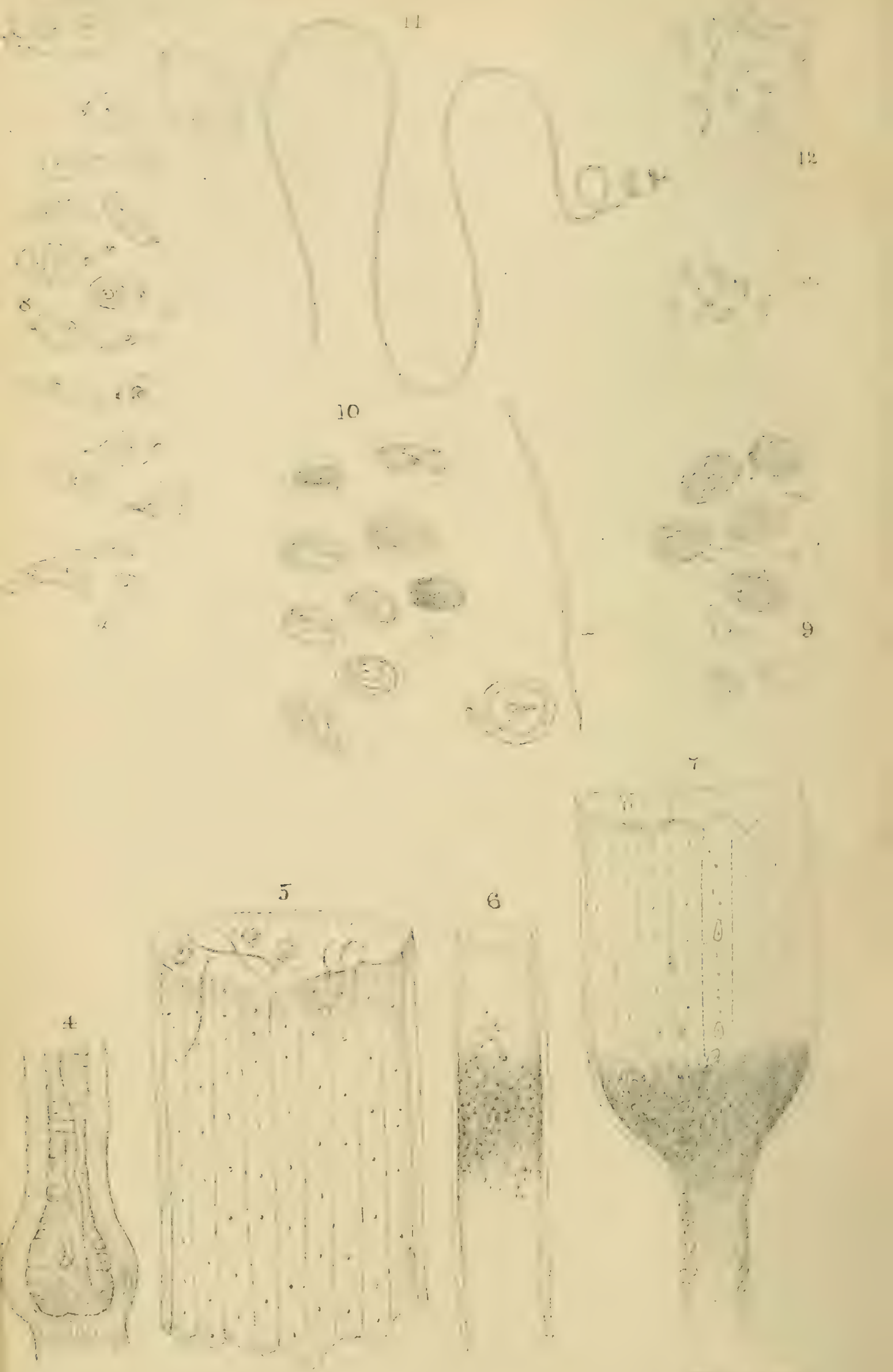

THE

# MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1873.

## I.—*A Description of the Thread-worm, Filaria immitis, occasionally infesting the Vascular System of the Dog, and remarks on the same relative to Hæmatozoa in general, and the Filaria in the Human Blood.* By Francis H. Welch, F.R.C.S.E., Assistant Professor of Pathology, Army Medical School, Netley, Southampton.

Plates XXX., XXXI., and XXXII.

Since the discovery of microscopic filariæ in the human blood in India by Dr. T. Lewis, Army Medical Department, fully detailed in the Annual Report of the Sanitary Commissioners of the Government of India, 1871, Appendix E, a much greater interest has naturally been thrown upon the presence of similar or congener forms in the blood of animals, and hence any collateral evidence which natural history can furnish tending to the elucidation of the

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EXPLANATION OF THE PLATES.

Plate XXX.

Fig. 1 × 25 diameters.—The anterior end of female worm; the musculo-cutaneous tube split up to within a short distance of mouth and folded back; the alimentary and generative tubes turned out.

(*a*) mouth, (*b*) œsophagus, (*c*) alimentary canal with contents, (*d*) vagina, (*e*) water vascular canals.

Fig. 2 × 25 diameters.—Tail end of female, showing (*a*) cæcal termination of the alimentary tube, (*b*) looped commencement of the ovarian tubes, (*c*) cupped-shapen cuticular orifices of the water vascular canals.

Fig. 3.—Female worm, natural size, curled up for convenience of sketching. Head, the left end. Tail, the right end in Plate.

Plate XXXI.

Fig. 4 × 75 diameters.—Vagina, showing the longitudinal and circular muscular layers, and the canal, containing free embryos, becoming bulbous before opening on the exterior of the body.

Fig. 5 × 75 diameters.—Uterine canal taken from about the centre of the worm, and containing ova and embryos. The wall is extremely delicate, with longitudinal striæ and so-called calcareous corpuscles.

Fig. 6 × 75 diameters.—Alimentary canal taken from about the centre of the worm. The wall is delicate, with longitudinal and circular striæ. The contents: fat-granules, and apparently blood colouring matter.

Fig. 7 × 75 diameters.—Uterine canal, taken 4 inches from the tail end of the female worm, showing its connection with one of the ovarian tubes, and the presence in both of germ cells, more highly magnified in Fig. 8.

life history of hæmatozoa is acceptable. With this end in view, in the 'Lancet,' March 8, 1873, I gave a short account of some worms sent to Netley by Dr. Lamprey, A.M.D., with a brief statement that "they were taken from the heart of a dog at Shanghai on May 20th, 1865, and found in both ventricles and for some distance along the course of the aorta;" and to my description of the worms I appended a few remarks on the presence of nematodes in the blood of animals generally, relative to the filaria in the blood of man and the ova and larvæ of a nematoid worm in the urine. These worms had been preserved in alcohol since 1865, and were all females containing ova and embryos, yet they were not sufficiently numerous or perfect to allow of a complete inquiry into all the anatomical

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EXPLANATION OF PLATES—*continued.*

Fig. 8. *a* × 300, *b* × 475 diameters.—Germ cells found in the ovarian tubes and the uterine canal occupying the lower third of the body of the worm. The degree of maturity of the cells is traced from below upwards in the Plate. Free sperm cells are present, as well as others attached to the germ cells (*b*).

Fig. 9 × 300 diameters.—Germ cells—ova, taken from the middle third of the uterine canal. In most the yolk is in a state of segmentation, a few are abortive. Intermingled are free spermatozoa (*c*).

Fig. 10 × 300 diameters.—Contents of the uterine canal, towards its termination in the vagina, and in the upper third of the body of the worm. Abortive germ cells; segmented yolk of irregular shape, surrounded by the egg-wall; coiled-up embryos, loosely retained within the capsule; free young worms (*a*).

Fig. 11.—Male worm, natural size; anterior two-thirds of body curled up for convenience of sketching; posterior third, spiral tail end, in the normal condition.

Fig. 12. *a* × 300, *b* × 475 diameters.—Sperm cells common to the entire sperm-producing tube throughout the body length of the worm.

PLATE XXXII.

Fig. 13 × 25 diameters.—Head end of the male worm, showing its general outline and the following parts:—(*a*) mouth, (*b*) œsophagus at its junction with alimentary canal, (*c*) sperm duct coiled on itself, (*d*) cuticle, (*e*) muscular layer, (*f*) free granular material within the musculo-cutaneous enveloping tube, "tube charnu."

Fig. 14 × 25 diameters.—Tail end of the male, spirally arranged. The extreme end viewed laterally, and showing, within the tip, in the concavity of the coiled body of the worm, the exserted spiculum and the generative appendages.

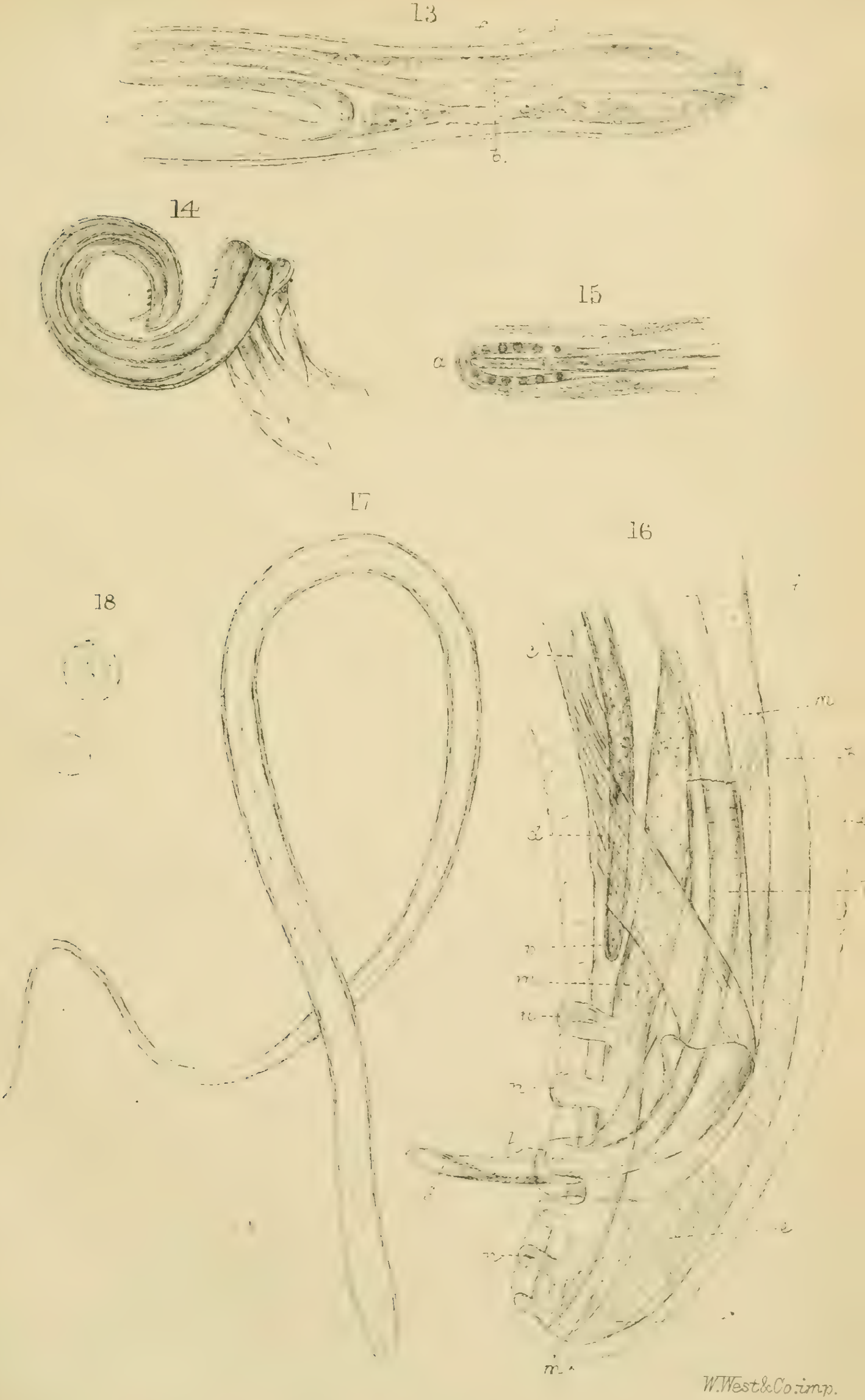
Fig. 15 × 25 diameters.—Tail end of the male worm viewed from above, showing its vertical compression as compared with the lateral cylindrical contour of Fig. 14, the rows of delicate imbricated epithelium, the arrangement of the internal seminal tubes, and the bases of the generative appendages.

Fig. 16 × 300 diameters.—Lateral and slightly oblique view of the male generative organs.

(*a*) cuticle, (*b*) longitudinal muscular layer, (*c*) oblique muscular layer, (*d*) muscular layer for retraction of penis, (*e*) muscular layer for protrusion and withdrawal of penis, (*f*) muscular layer for protrusion of penis, (*g*) spiculum or penis, (*h*) sheath of penis, (*i*) common sperm duct, continuous with (*l*) vas deferens, and (*m*) horse-shoe duct of (*n*) generative appendages (vesiculæ seminales?), (*p*) cæcal termination of alimentary canal.

Fig. 17 × 850 diameters.—Free young female worm, taken from a blood clot in the left ventricle of the heart.

Fig. 18 × 850 diameters.—Outlined red and white blood corpuscles taken from the same blood clot for comparison with the young brood.

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details; enough facts were obtained, however, to demonstrate that the worm was a *filaria*, and that the name hitherto applied—*spiroptera*—was misplaced, a point subsequently corroborated by Dr. Cobbold in a letter to the 'Lancet,' March 15th, 1873, consequent on my paper, and in which he mentioned that the term *Filaria immitis* had been given to the worm in Germany. Since this time, through the kindness of Dr. D. Macdonald, F.R.S., Assistant Professor of Naval Hygiene, Army Medical School, I have received the heart and one lung taken from a dog at Yokohama, Japan, by Staff-Surgeon H. Hadlow, R.N., containing numerous and comparatively recent specimens of the blood-worm—male, female, and free young; and the results of the examination of these are embodied in the present paper. I have applied the name *Filaria immitis* to these on the assumption that they are similar to those found in Germany and referred to by Dr. Cobbold, and I propose in this communication to detail the anatomy of the worms, and subsequently to append a few remarks on the hæmatozoa in general—animal and human.

*Mature Female Worm.*—Body long, thread-like, cylindrical (Plate XXX., Fig. 3), averaging 11 inches in length, but varying from 8 to 13 inches, with a body diameter of $\frac{1}{20}$ inch in the centre, $\frac{1}{50}$ inch at the head, and $\frac{1}{100}$ inch at the tail; head blunt and rounded; tail bluntish, yet tapering towards a point; both gradually merging into the central maximum body thickness; colour, milk-white, opaque, but rendered translucent by immersion in glycerine. Worm coiled up, yet easily straightened out; tail straight; animal markedly resembling a piece of white twine, with an occasionally annulated condition of the centre or tail end, and especially in those fully distended with ova and embryos. With an ordinary hand-lens, while holding the worm up to the light, a differentiation of its structural components could be made, into the parietes, and internal viscera. The latter were made up of two dark tubes—one smaller (alimentary canal), traceable from the head throughout the entire body length, with the exception of the immediate tail end; the other larger (generative canal), apparently commencing about half an inch from the head, and terminating about an inch and a half from the tail, the latter interval being filled in by a convolution of small tubes. By the aid of glycerine and higher magnifying powers, further details were brought out, as follows:—The parietes were composed of cutaneous and muscular strata. The former consisted of a corium externally covered by imbricated longitudinal layers of a beautifully delicate minute epidermis, much resembling that on ophidians; the latter was made up of three layers in the following relation from the corium inwards: two oblique intersecting each other at an acute angle, a longitudinal, a circular, all varying in the degree of development in different situations. The combined muscular and cutaneous tunics constituted about one-eighth part of the total body diameter, and formed a "tube charnu" to the loosely-lying contained viscera (Plate XXXII., Fig. 13). The mouth was a small circular aperture in the centre of a papilla, occupying the most prominent portion of the rounded anterior extremity of the worm (Plate XXX., Fig. 1, *a*; Plate XXXII., Fig. 13, *a*); and the longitudinal muscular layer diverging from it was very strongly pronounced. Continuous from this was the œsophagus, about $\frac{2}{10}$ inch in length, and $\frac{1}{200}$ inch in thickness, with strongly pronounced walls made up of a longitudinal and circular muscular layer (Plate XXX., Fig. 1, *b*; Plate XXXII., Fig. 13, *b*). At the junction of the gullet with the stomach was a clearly-defined sphincter. The stomach, or rather alimentary tube, into which the œsophagus opened, expanded from the sphincter into a delicate membranous canal nearly $\frac{1}{100}$ inch in thickness (Plate XXX., Fig. 1, *c*), pursued a straight course along the body length of the worm, diminishing at the centre to $\frac{1}{130}$ inch, and could be traced to within half an inch of the tail end, where it terminated in a cæcal extremity about $\frac{1}{400}$ of an inch in diameter, lying between the convoluted ovarian tubes (Plate XXX., Fig. 2, *a*); or, in the case of the male worm, either above or below the sperm duct. Its delicate wall was made up of very fine longitudinal and circular fibres, and retained within it fat globules and granules, and not uncommonly red colouring matter, doubtless derived from the blood of the host (Plate XXXI., Fig. 6). The alimentary canal was encircled throughout its entire length by the reproductive organs. It will thus be seen that there was no anal aperture, a circumstance possibly connected with the life history of the worm passed within the vascular canals of the host, and with the nutriment obtained from a vital fluid comparatively free from effete products; the alimentary excreta of the parasite being thus reduced to a minimum, if not an actual nullity. In Plate XXX., Fig. 2, the tail end of the female is shown, the outline of the parietes in one aspect being straight, on the other side being comparatively sharply curved, with the result of throwing the tip in the direction of the straight longitudinal line of the body—a feature more strongly pronounced in the male.

The reproductive organs consisted of a vagina opening externally, an uterine canal, and ovarian tubes. The vulva was a small circular or somewhat oval aperture, about $\frac{1}{200}$ inch in diameter, situated on the anterior end of the worm, generally about $\frac{3}{10}$ths of an inch from the oral aperture, but varying from $\frac{2}{10}$ inch to, in one instance, $1\frac{1}{2}$ inch from the head. Its presence was with great difficulty observed in the external surface of the body, even when, as in Plate XXX., Fig. 1, *d*, it had been clearly traced up from within; but when detected (from occasionally being surrounded by a somewhat elevated ring of tissue), the cutaneous envelope of the body was seen to merge into its inner surface, while the longitudinal, and possibly the circular, muscular fibres of the body curved in with the canal, and were continuous with its outer layer. The vagina (Plate XXX., Fig. 1, *d*; Plate XXXI., Fig. 4) consisted of two clearly-defined muscular layers encircling a narrow canal—a strong outer longitudinal layer continuous with that of the body, a strong inner circular layer stopping short within the vulva, and at the bulbous expansion of the canal, which was observed to be occupied in more than one instance with free embryos (Plate XXXI., Fig. 4). The vagina was rather more than $\frac{1}{200}$ inch in thickness, and about $\frac{1}{2}$ inch in length, terminating internally in two uterine canals, which, after pursuing an individual course for about $\frac{3}{4}$ inch, merged into a single membranous tube $\frac{1}{50}$ inch in diameter, traceable, curved around the alimentary canal and doubled back upon itself, throughout the body of the worm to within 3 inches of the tail end, where it was continuous with the ovarian tubes. The uterine canal, more than four times the diameter of the vaginal, was seen to have its wall made up of a delicate transparent fibrous texture, marked by regular longitudinal lines and faintly oblique or circular ones, and dotted over with so-called calcareous corpuscles (Plate XXXI., Fig. 5). Folds of the delicate wall were numerous, indicating a greatly increased capacity of the tube when required, and fine bands of tissue passed from the outer surface of the wall to the inner surface of the body parietes, thus retaining the canal *in situ*. The continuity of the uterine canal with an ovarian tube is shown in Plate XXXI., Fig. 7, the former merging into the latter like the body of a wine-glass into its stem. The mode in which, however, *all* the ovarian tubes are connected with the common uterine cloaca is not so apparent. At 4 inches from the tail end of the worm two tubes are to be noted, one, small—intestine; one, large—uterus. At 3 inches there are four; one intestine, three germ-bearing; all about the same size. Within this distance and the inferior end of the worm, the cæcal termination of the intestine can be observed, and three loops of ovarian tubes. I could not discern the coalescence of the ovarian tubes with each other, yet from the clear merging of the uterine canal into one tube, I am led to infer that the loops discharge their products into the latter, and hence into the common germ-accumulating canal. The structure of the ovarian walls was similar to that of the uterine sac. The contents of the germ-bearing system varied accordingly to proximity, or otherwise, to the vaginal discharging canal. Commencing with the ovarian tubes, these were seen to be lined throughout with a well-defined layer of epithelium-like cells (Plate XXXI., Fig. 7, *a*), represented detached and further magnified in Plate XXXI., Fig. 8, *a*. These masses of protoplasm were irregular in shape, but not uncommonly assumed an ovoidal form somewhat flattened and drawn out at each end; except the strongly-marked nucleus the mass was transparent. They were somewhat smaller than those found in the lower end of the uterine canal, and figured at the centre, and to the upper end of Plate XXXI., Fig. 8. Here the average size was $\frac{1}{750}$ inch in length, by $\frac{1}{1000}$ inch in breadth; the shape was oval, or approaching that form, the nucleus double or triple, with well-defined nucleolus; the mass was occasionally divided into two by a transverse line, and granular spermatozoa were detected attached to the germ body as at *b*, Plate XXXI., Fig. 8, or free among the masses as at *c*, Fig. 9. It is apparent that at this point in the uterine canal the germ had met with the spermatic fluid, the mass thrown off from the ovarian tubes had become fecundated, assuming an oval outline, and that growth and development had commenced in the multiplication of the nucleus. In the middle third of the uterine canal the ova were in a state of yelk segmentation (Plate XXXI., Fig. 9), mingled with a few free spermatozoa and a few abortive or non-impregnated masses. Higher up the segmented yelk was irregular in shape, with the flexible egg-wall loosely surrounding it; the form of the embryo was faintly mapped out in some, in others it was clearly defined, while on approaching the double uterine canal, the cavity was observed distended with free embryos coiled up or straightened out (Plate XXXI., Fig. 10, *a*). Throughout the canal abortive germ masses were to be met with, and the egg-wall, or limiting membrane of the mass, was transparent, flexible, yet easily ruptured by pressure; some of the ova were as small as $\frac{1}{1250}$ inch + $\frac{1}{2100}$ inch; but as a rule an increase in size took place on impregnation, and on the distinct maturation of the embryo. The ova and embryos were so innumerable that the entire worm was mainly made up of one large germ-containing bag.

Besides the alimentary and generative system, was the water vascular system. Passing from one extremity of the worm to the other, and clearly shown from the inside by splitting up the musculo-cutaneous parietes, were four main tubes attached to the inner muscular layer (Plate XXX., Fig. 1, *e*), their walls thick but transparent, and freely studded with calcareous corpuscles in common with the "tube charnu" generally, and also the uterine canal. These main tubes were apparently connected with the surface by a series of oval cuticular depressions with circular circumferences, having a central aperture at the bottom of the cup (Plate XXX., Fig. 2, *c*), communicating with the tube within. Sometimes these breathing orifices were linearly arranged, but generally scattered, and more numerous towards the tail end of the animal. Within the musculo-cutaneous parietes these main tubes appeared also connected with some very delicate ones ramifying between and around the contained viscera, and especially numerous towards the ovarian convolutions; and in the centre of the body of the worm were on a former occasion mistaken by me for diverticula, or smaller delicate ovarian tubes, from the main uterine canal.*

*Mature Male Worm.*—Like that of the female, the body is long, cylindrical, thread-like, straight from the head to within two inches of tail, which is spirally arranged in gradually-decreasing circles terminating in a sharpish free point (Plate XXXI., Fig. 11). Average length of body, 7 inches; thickness, $\frac{1}{40}$ inch; somewhat thinner towards the head than the centre of the body, but markedly thinning off towards tail. The firmer, thinner, whipcord-like aspect of the body and spiral tail, easily, to the unaided eye, distinguishes the male from the female; the former being to the latter in relative frequency as 1 to 8. The "tube charnu," alimentary system, and water vascular canals present no deviation from the description of the same in the female worm; it is only when we come to the generative system and the organ for the transfer of the sperm fluid to the germ-containing canal that a divergence becomes necessary. Commencing with the sperm-producing tract, it was found to consist of a membranous tube originating in a blind but somewhat pointed extremity about $\frac{1}{2}$ inch from the head; passing up for a short distance it was reflected on itself (Plate XXXII., Fig. 13, *c*), and thence traversed the entire length of the worm's body, lying either parallel to or encircling the alimentary canal, from which it would with facility be distinguished by the diminished calibre of the latter (one-half that of the spermatic canal) and the light-refracting quality of the oily contents, ultimately terminating in a vas deferens at the base of the spiculum. The wall of the sperm canal consisted of exactly the same delicate striated and corpuscular membrane forming the uterine wall, the contents only differing. Towards the upper cæcal end of the tube there was a strongly-defined minute epithelium-like layer extending over the entire inner surface, the elements being somewhat larger than, but in character similar to, the free masses lying in innumerable quantities throughout the entire sperm canal. These masses generally had oval club-heads, terminating in the opposite direction in a slim pointed tail (Plate XXXI., Fig. 12); their aspect was that of minute, oval, caudated epithelium; they were strongly nucleated, occasionally tailless, but more frequently than that having two tails opposite the one to the other. Their average size was $\frac{1}{1300}$ inch in length × $\frac{1}{3000}$ inch in breadth, yet it was not difficult to find others of half these dimensions; and it will be remembered that these spermatozooids were found also in the uterine canal, both free, and attached to the germ masses. It is apparent that the sexual particles in both male and female were nucleated masses of protoplasm thrown off from a germ-producing basement tubal membrane, and not inclosed in a capsule or cell-wall as in most of the higher organisms.

* 'Lancet,' March 8, 1873.

Turning now to the tail end of the worm, the seat of the intromittant organ, it was observed that when viewed laterally it gave a decreasing cylindrical contour (Plate XXXII., Fig. 14), when from above downward a much broader spatulated character (Plate XXXII., Fig. 15), induced by a spreading out of the sides as from vertical compression, a feature conspicuous throughout the entire spiral portion of the tail end of the animal. As is seen in Plate XXXII., Fig. 14, the outline of the body following the concavity of the last spiral twist is straight terminating in the tip, while opposed to this the contour is that of a sharp convexity also terminating in the tip—an excess of that characterizing the female tail, and within the tip, at a distance from it of $\frac{1}{300}$ of an inch on the straight surface, the spiculum emerges. It is obvious that this arrangement of the parts—the curve of the tail, the flatness of the surface to be brought in direct contiguity with the female, and the thinned-out lateral edges capable of adapting themselves to the sloping sides of her body—must greatly assist in the act of copulation and in the retaining of the male in close contact with the female during the necessary period. The penis or spiculum is a curved, narrow, silicious, somewhat brittle, intromittant organ, bluntly pointed at the free end, bulbous with one, and possibly, two (one on each side) root-like projections at the base (Plate XXXII., Fig. 16, *g*), having a groove along the concavity receiving the vas deferens and stopping short of the tip, and lodged within a membranous sheath (Plate XXXII., Fig. 16, *h*). The sheath is an elongated capsule connected with the cuticle at the genital fissure, is pierced behind by the vas deferens, and has connected with it strong layers of muscular fibres concerned in the protrusion, withdrawal, and elevation of the spiculum, and derived by modification from the muscular layers of the "tube charnu." Thus, a broad band (Plate XXXII., Fig. 16, *d*) is continued from the oblique layer, and is traceable to the base of the spiculum for its retraction after protrusion; another band (*e*) from the circular layer would tend to protrude the spiculum if retracted, or partially retract it and elevate it if protruded; while a third band (*f*) from the longitudinal layer would directly draw it forward when retracted and protrude it; these layers are doubtless double, one on each side of the worm. The spermatic canal (*i*) was observed to terminate in a much reduced calibre tube, a vas deferens, just short of the base of the sheath, being joined at the same time by a horse-shoe tube, one branch of which passed on each side of the sheath and met its opposite member at the extreme end of the tail (Plate XXXII., Fig. 16, $m^{\times}$; Plate XXXII., Fig. 15, *a*), being connected with the generative appendages (Plate XXXII., Fig. 16, *n*). These appendages (possibly vesiculæ seminales) were twelve in number, in two parallel rows running longitudinally to the body of the worm, and on each side of the spiculum. They consisted of a globular, papilla-like body, rather less than $\frac{1}{1000}$ inch in thickness, not projecting beyond the epidermis, by their attached end continuous with the horse-shoe duct (*m*), and opening into it by a smaller duct traversing the gland in its long diameter. The substance of these glandular bodies was identical in character with the material occupying the horse-shoe duct into which they discharged themselves, and no external aperture was detected. Their connection with the duct opening into the seminal tube evidently associates them with the generative function, and their secretion mingled with the spermatic fluid would find its way by the vas deferens to the groove of the spiculum and so into the uterus of the female.

Hence, then, from these anatomical details, we may sum up these mature worms as having a filiform musculo-cutaneous cylindrical envelope, containing an alimentary, generative, and water vascular system, the sexes distinct and the reproductive organs largely predominating, mouth circular and papillary, intromittant organ of the male sub-caudal, genital orifice of the female situated on the anterior end of the worm within 2 inches of the oral orifice, alimentary canal cæcal.

*Free Young Worm composing the Brood.*—These were microscopic, free within the vascular canals of the host equally with the parent worms, and so numerous that a piece of blood clot the size of a pea, taken from the left ventricle of the dog's heart and broken up in a teaspoonful of glycerine, gave twelve specimens to two drops of the fluid from a pipette. The young worm was of filiform shape, identical in relative thickness of parts of body with the mature worms, head rounded, tail pointed, average length $\frac{1}{84}$ inch, thickness $\frac{1}{4000}$ inch, proportion of breadth to length 1 to 47, proportion of tail to total length 1 to 8, body structure translucent with delicacy of texture or granular from fatty degeneration or fat particles within the alimentary canal and then removed by the addition of liquor potassæ. Following the contour of the external surface was an inner line which mapped off a transparent parietes, corresponding to the "tube charnu" of the mature worm and merging into the general translucency of the tail end of the young animal. On the addition of magenta colouring the textures became more easily distinguished the one from the other, and undoubted transverse striæ were noted along the concave margin of the curled body of the worm, as well as often faintly elsewhere. By the colour staining of the body, especially after the dispersion of the fat-granules by the potash solution, a light-refracting inversion of the cuticle over the head, indicating the mouth, was perceptible, and from it an alimentary canal was feebly, though indubitably, marked out, while one or two darker lines were traceable along the length of the body of the worm, stopping short of the tail. The dead animal was easily broken across, when the distinction between the parietes and the inner mass was no less apparent than when the body continuity was preserved, but no differentiation of the inner mass into organs was determinable. In Plate XXXII., Fig. 17, an entire worm, highly magnified, has been drawn by the camera lucida, while, for comparison, a red and white blood corpuscle lying under the same microscope slide have been outlined on the same Plate (Fig. 18). The average length of embryos within the body of the mother was $\frac{1}{100}$ inch by $\frac{1}{2800}$ inch breadth, but a considerable latitude in size was observed and a proportionate thickness to length, varying from 1 to 16 to 1 to 28; hence, as compared with these, the free young worm was longer and narrower, the thickness of the body more uniform, there was less of the transparency of the tail end, more clear foreshadowing of internal organs and body striation, more rounded outline of the anterior end of the worm. Among the free young, moreover, while retaining the relative proportion of length to breadth, there were some as small as $\frac{1}{150}$ inch long, while a few were as large as $\frac{1}{50}$ inch, clearly suggesting growth since birth. Another feature was conspicuous as regards the tail end—a division of the young into two categories, the one with the posterior extremity of the worm tapering off cylindrically into a point, the other with this portion flattened from side to side, or from above downwards, and spirally twisted, apparently distinguishing even at this early stage the female from the male. In this flattened spirillum the aspect of the worm at first sight strongly suggested the existence of an enveloping membrane to the tail end, similar to that observed in the human filaria during life.

From these remarks it will be seen that the microscopic young worm clearly and accurately foreshadowed the mature animal.

*General Observations.*—In the 'Veterinarian,' Jan. 1873, the editor asserts that "nematodes are common enough in the blood-vessels of the young ass, colt, and some other animals." MM. Grube and Delafond originally detected minute worms in the canine blood, about $\frac{1}{100}$ inch in length, and "less than a blood corpuscle in diameter." They found them in the vessels in all localities, but none in the lymph, chyle, secretions, and excreta. Injected into the blood of a non-contaminated dog they were traceable at the end of three years; they lived 89 days when transferred to the blood of two rabbits, but died when placed in the serous and cellular tissues of dogs; it was clear that their habitat was the blood. On one occasion naked-eye worms, supposed to be the parent worms of the microscopic forms, were found lodged in a clot in the right ventricle of the heart, 4 females, 2 males, from 5 to 7 inches in length, and from $\frac{1}{25}$ to $\frac{1}{15}$ inch in diameter, and to these they gave the name of *Filaria papillosa hæmatica canis domestici*. This instance of mature worms, coexistent with the microscopic animals, was noted but once in 29 affected dogs: the ascribed parentage is apparently doubted by Leuckart, who states that, with the exception of *T. spiralis*, no nematode is known to infect its own bearer, and that young hæmatozoa in dogs and frogs have never been known to develop into mature helminths. Instances of the so-called *Spiroptera sanguinolenta*, not apparently determined as such, but linked to the *Filaria immitis* by their recorded features, are far from uncommon in dogs in China and Japan, and also, according to Dr. Cobbold, in France and Germany. In the 'Field,' 1872, p. 162, is a letter from a Mr. Dare recording the deaths of three spaniels, imported from England, from this cause, in China. The worms were found in the right side of the heart only, and in the branches of the pulmonary artery; they measured from 6 to 11 inches, with a diameter of $\frac{1}{40}$ inch; and the suggestion is there made that the germ or ovum was received with the food or drink, and passed by the thoracic duct into the venous circulation. In Dr. Lamprey's case the animal was an English pointer born in China; the dog was fat and apparently in good health, and the suddenness of the death led to the opening of the body and the detection of the worms. They were coiled together, resembling a ball of ligature thread, and filled the ventricles to such an extent as to excite astonishment at the possibility of blood passing between or around them, and to, or from, the heart's cavities. In the instance from which the specimens described in the present paper were taken, the right auricle and ventricle were full, the mature worms passing between the columnæ carneæ into the pulmonary artery which was firmly impacted with them, and reaching the larger subdivisions of the vessel in the lung; probably there were at least thirty of them in all. In the left ventricle was a firmish blood clot which had entangled innumerable free young worms, but no parent ones were present in the left cavities; and in the right ventricle particles of blood clot evinced there also an abundance of the young brood, a feature no less conspicuous by taking portions of the muscular tissue of the heart, or portions of lung tissue, free from the mature worm. The lung was engorged with blood, and in a condition bordering on apoplexy. In the letter accompanying this example from Yokohama, Staff-Surgeon Hadlow, R.N., remarks,—"It (*i. e.* the worm) is always found in the right side of the heart, and often extends through the pulmonary vessels to the lungs. I have several times examined the inferior cava and intestines with all the care I could, but without finding anything to suggest how the parasite found entrance, or in what form." On a subsequent occasion, however, he discovered mature worms also in the inferior cava. In all the instances in which the fact was inquired into, the presence of the worms did not in any way interfere with the general nutrition of the dog nor impair the muscular powers; their sole deleterious nature was displayed mechanically, inducing sudden death by actual rupture of the heart or obstructing the pulmonary or cerebral circulation. The young brood appeared innocuous. It is clear that the loss of valuable sporting animals by sudden death while in apparent full health, and the idea of "foul play" as the cause, have led to the elucidation of this branch of canine pathology, and an inquiry into the life history of the parasite.

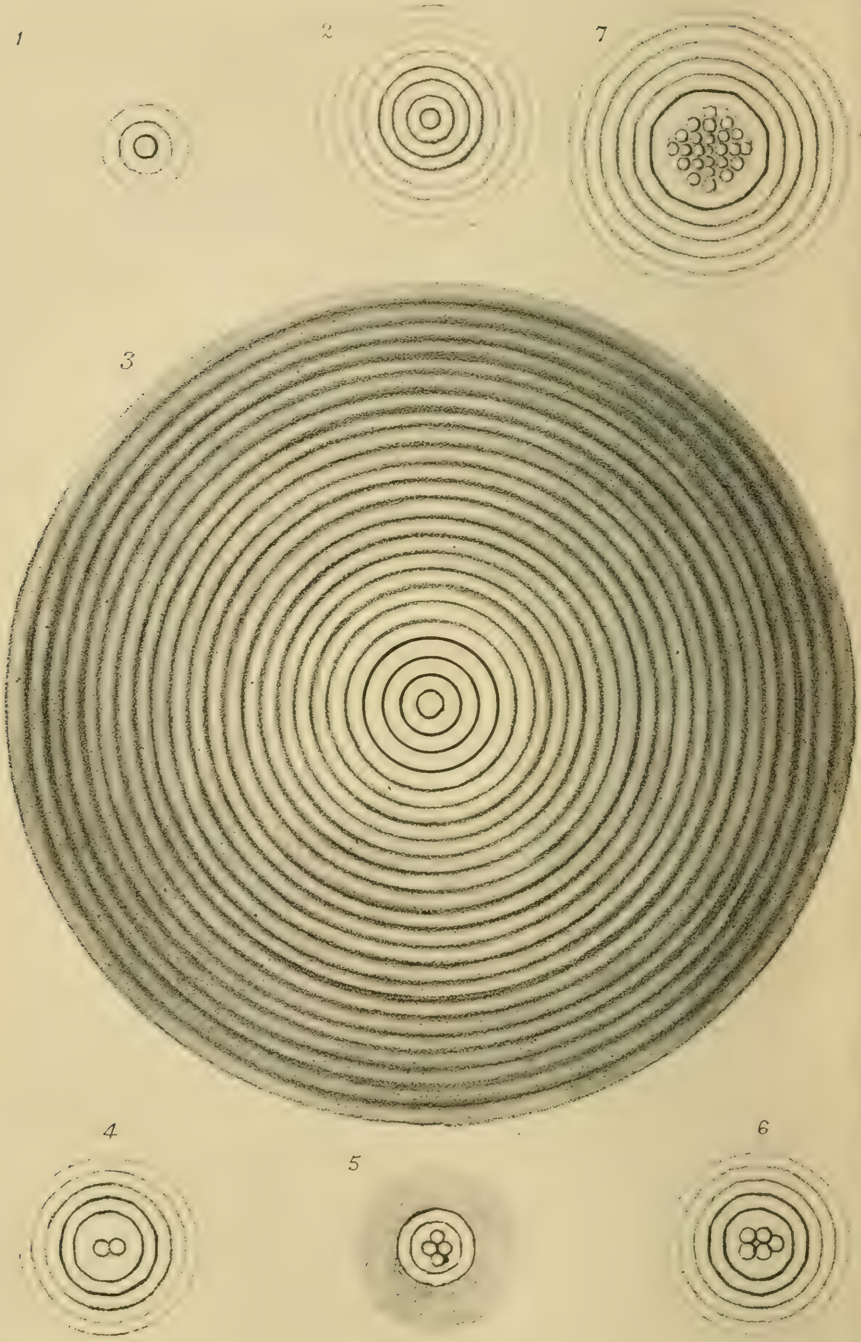
Turning to the human fluids, ova and minute larvæ of a nematoid worm were found, apparently as parasites, in the urine by Drs. Salisbury and Cobbold. Filariæ in the urine associated with chyluria and more or less hæmaturia, probably as cause from mechanical blocking of the capillaries and rupture of their walls, have been detected in Germany, and also in India by Dr. Lewis, A.M.D. The latter observer noted them in fifteen or twenty patients, in Europeans and East Indians or natives in about equal proportion. Dr. Lewis also discovered filariæ in the blood in four individuals, twice associated also with chyluria and the presence of the worms in the urine; one case was fatal from coexistent disease, but no clue as to the nature or cause of the worm infection was detected *post mortem*. Judging from the numbers present in a drop of blood, he calculated their presence throughout the circulatory system would amount in one individual host to 140,000. He ascertained that the blood filariæ were similar to those found in the urine. Their average size was,—length $\frac{1}{75}$ inch, breadth $\frac{1}{3500}$ inch, relative proportion of breadth to length 1 to 46, length of tail to total length 1 to 8. The body was filiform, head rounded, tail acutely pointed, texture of body translucent but becoming granular, marked by delicate faint transverse striæ, a foreshadowing of a differentiation of the body components into organs. A delicate faint membranous capsule, like the myolemma of muscular fibre, surrounded the worm during life, and in which the animal moved, but this feature was not constant after death. Their presence did not appear hurtful to the host beyond the blocking of the renal capillaries for a temporary period.

On the affinity of the dog filaria to congener forms, it may be observed that in the general outline of the body, the round oral aperture, the blind alimentary canal, and the large uterine cavity, the mature female canine worm approaches the *F. medinensis*, while the broods also present certain general features in common. Roughly speaking, the links between the mature worms and the ascarides and oxyurides on the one hand, and between the young and the trichinæ on the other hand, connect these as family groups in the animal world. It is, however, when we place the young canine worm side by side with the human blood filaria that the closest relationship is brought out. It is evident from what has been adduced as to the anatomy of the free young canine that its points of accord with the human filaria completely overbalance the points of discord. There are slight but insignificant divergencies in size, the only marked difference is the "delicate enveloping tube." As before said, though observed in the human worm during life, it was not constant after death, a feature clearly pointed out by Dr. Lewis; it received considerable modifications on the passing of the worms from life to death, and competent observers failed to detect it in a few preserved specimens sent to Netley. On the other hand, in the majority of the dead young canine worms there were no indications of its presence, although a halo of light thrown from the curved sides of the worm's body gave a phantom existence to it; as before remarked, also, the aspect of the tail end of the male at first strongly suggested it; yet in a few instances, especially after the staining of the textures by magenta, a faint outline could be traced along the worm for some distance which was not dissipated by focal alteration or varying the direction of the source of illumination of the microscope. Whether this unquestionable delicate line, locally bulging out occasionally, was due to the presence of an "enveloping tube," or a mere separation of the epidermis *en masse* from the cutis, I cannot state with certainty; the examination of the living young canine worm is necessary to determine the point; but meanwhile, the doubt thrown on its existence in the dead canine cannot, in the face of the death modifications observed in the delicate tube of the human, be considered as sufficient to differentiate the one worm from the other. That the human blood worm is the young brood of a filaria closely allied specifically to the filaria of the dog can hardly be a subject of controversy; the only point of doubt is the question of identity, and certainly the grounds for assuming it are strong.

Concerning the life history of the canine worm, it appears to me that the specimens, the subject-matter of this paper, tend to set one part of it at least at rest. It is quite clear that the mature worm can infect its host, and it seems equally deducible that the young may develop into mature helminths in the dog's blood-vessels. In this example we have mature males, females brood-containing, and a free young brood varying greatly in size and suggesting growth, in the same host. Whence and how came the mature worms? Considering their size and the absence of any boring apparatus as a means of locomotion through the tissues, we may put on one side the idea of their reaching the vascular canals in a mature state; the worm also is viviparous, and the question of the conveyance of soft, frangible, immature ova may be disposed of; the free active young remain. The faculty of migration of the white corpuscle of the blood through the tissues of the body has been demonstrated; the diameter of the body of the young filaria is considerably below that of the corpuscle; hence with the brisk, wriggling, movements of life, the possibility of their passage through a mucous membrane, especially through the soft granulations of an ulcer, is quite within the bounds of reality. Based upon the facts we know, we may in imagination follow them from a mucous tract (*e. g.* the intestine) to a lacteal or blood-vessel; they follow the course of the circulation, growing on the pabulum of the blood of the host, and easily passing with the corpuscles through the capillaries; soon their size unfits them to traverse every viscus, and the minute capillaries of the lungs act as a sieve to retain them in the venous circulation; they copulate and the females become fecund; a young brood arises to continue on the race, provided accidental causes, such as the mechanical blocking up of important blood-vessels by the parent worm, do not determine the death of the host. By this hypothesis the ingress of individuals capable of arriving at maturity is explained, while the countless hordes of young are rendered lucid only by the presence of one or more parent worms within the vascular walls. These parent worms after producing their progeny may possibly die and disintegrate, and so account for their absence, or non-discovery, in hosts teeming with the young brood.

The question, however,—whence come the young which, entering into the blood-vessels of the host, arrive at maturity? is not clear. Do they exist in the food or water which the dog feeds upon? Are they derived directly from the flesh of an infected animal fed upon, or can they pass an intermediate state in water subsequently lapped up by the animal? Take for instance an infected dog dying and disintegrating in a tank from which human beings and animals of all descriptions slaked their thirst (no uncommon condition one would think in Eastern countries), what would result to the imbibers? Assuming the possibility of the young retaining vitality in water, the impregnation of the water-drinkers by the young worms, and their subsequent life history in the host as above sketched, is highly suggestive, and supported by the experiments with the trichina. Or, on the other hand, take for example a portion of impregnated flesh taken in as food by human beings when badly cooked, or eaten raw by any of the carnivora, is it not within reason to assume the strong probability of infection? Considering the frequency with which the worms are found in dogs in China and Japan, it is to be hoped that these doubtful points will in the early future be cleared up by a few carefully-conducted experiments on non-infected animals, and so by this means the possibility of any human being carrying about within him swarms of loathsome microscopic worms be averted. The identity also of the human worm and canine embryo might be solved by the examination of the living dog worms.

It appears to me also that an accurate knowledge of the life history of the *Filaria immitis* may throw much light on doubtful points connected with the Guinea worm and congener forms.

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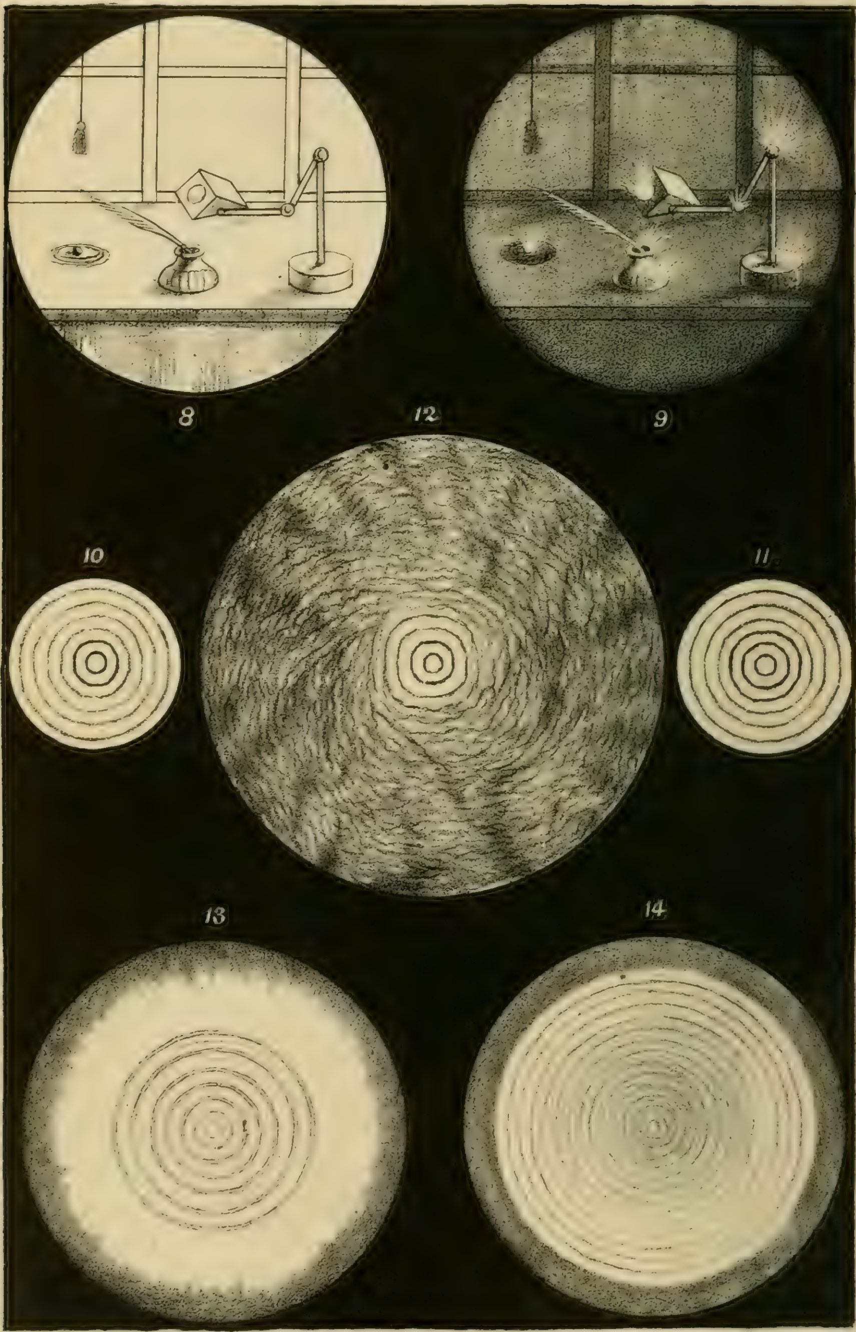
Circular Solar Spectra.

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# II.—*Researches in Circular Solar Spectra, applied to test Residuary Aberration in Microscopes and Telescopes;** *and the Construction of a Compensating Eye-piece, being a Sequel to the Paper on a Searcher for Aplanatic Images.*

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(*Communicated by* Prof. Stokes, Sec. R.S., *to the Royal Society. Received April* 24, 1873.)

Plates XXXIII., XXXIV., XXXV., and XXXVI.

The researches detailed in the present paper were commenced in May, 1871. The results arrived at were largely obtained from using the microscope. Similar but less brilliant and more scanty appearances can be obtained with the telescope; but the very high

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EXPLANATION OF PLATES XXXIII., XXXIV., XXXV., AND XXXVI.

*Circular Solar Spectrum.*

Plate XXXIII.

Fig. 1.—Primary rings finely defined at the first visible focal plane.

„ 2.—Secondary rings, at a deeper focus. The central disk should have been squared off.

„ 3.—Exhibition of the greatest display of rings, each annulus having its own breadth equal to that of the central disk, which is a brilliant white, the succeeding lavender-rose colour and red rings separated by dark rings, the first few of which are jet-black.

Figs. 4, 5, 6, 7.—Development of two disks instead of one; also of four and irregular disks showing the existence of displaced centres and irregular diffractions.

Plate XXXIV.

„ 8, 9.—The miniature prospect of the distant window is displayed sharply in Fig. 8. So soon as the sun began to shine, the blazing prism being quenched by turning it aside, every brilliant point became irradiated with an orange-red halo (Fig. 9). If the colour were corrected by change of the general adjustments so as to destroy halo, then the prospect in Fig. 8 became enveloped in a strong white mist of uncorrected residuary aberration.

„ 10, 11.—The slight deviations from the true circular form, owing to imperfect glasses, are here well represented.

Fig. 12.—Displays the very delicate engine-turned pattern and obscuration of the diffraction-rings by a badly-constructed glass (cheap German).

„ 13.—Shows a delicate set of rings between the coarser, expanding by a change of focus in a different manner.

The approaching halo and fog are well delineated.

Fig. 14.—Shows the blurred appearance of the rings when the spherical aberration is excessive; similar also to the evanishing spectrum occurring before the fog appears.

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* That part of the paper referring to the new correction for telescopes is omitted.

power and ready adaptability of the former confers some advantages not offered by the latter.

In both, however, the same principles are illustrated.

A cone of rays of small angular aperture having the object-glass for its base, in each case engages the eye-piece and emerges parallel, and the eye-pieces are similar in each.

Peculiar facilities for studying solar spectra and their indications of aberrations and mechanical errors also are afforded by the former. The focal plane of vision may be employed to examine

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Explanation of Plates—*continued.*

Plate XXXV.

Fig. 15.—In this case the heliostat was placed nearly 40 feet distant. The internal lenses of a fine $\frac{1}{4}$-objective being all removed, the thick front only was employed to form the miniature on the stage. A peculiar irregularity in the central jet-black rings is supplemented by extraordinary eccentric lines bordered by a new order of peripheral rings, obeying a different order of expansion. Viewed under Powell's best dry $\frac{1}{8}$th; a $\frac{1}{2}$-inch single plano-convex lens being used as eye-piece.

The next figures illustrate the effect of obliquity; the previous drawings exhibiting various effects during coincidence of the axis of the microscope with that of the miniature-forming lens.

Figs. 16, 17.—Represent a very beautiful variety of hyperbolic diffraction-lines seen when the axis of the solar ray is inclined about 6° to that of the microscope. Dist. of heliostat, 20 feet; magnifying power, 1000. Powell's best $\frac{1}{4}$ forms the miniature observed with best dry $\frac{1}{8}$th.

Fig. 17.—Miniature objective; a $\frac{1}{16}$th Gundlach immersion used dry. Microscope objective best $\frac{1}{8}$th *used dry*, eye-lens $\frac{1}{2}$-inch convexo-plane; parabolic curves and fine diffractions; obliquity 5°.

„ 18.—The appearance within the focus of the best and first resolution of the fog of under-correction.

„ 19.—Slight obliquity and under-correction.

Plate XXXVI.

Figs. 20, 21, 22.—Best $\frac{1}{8}$th and $\frac{1}{4}$ plano-convex stage-lens.

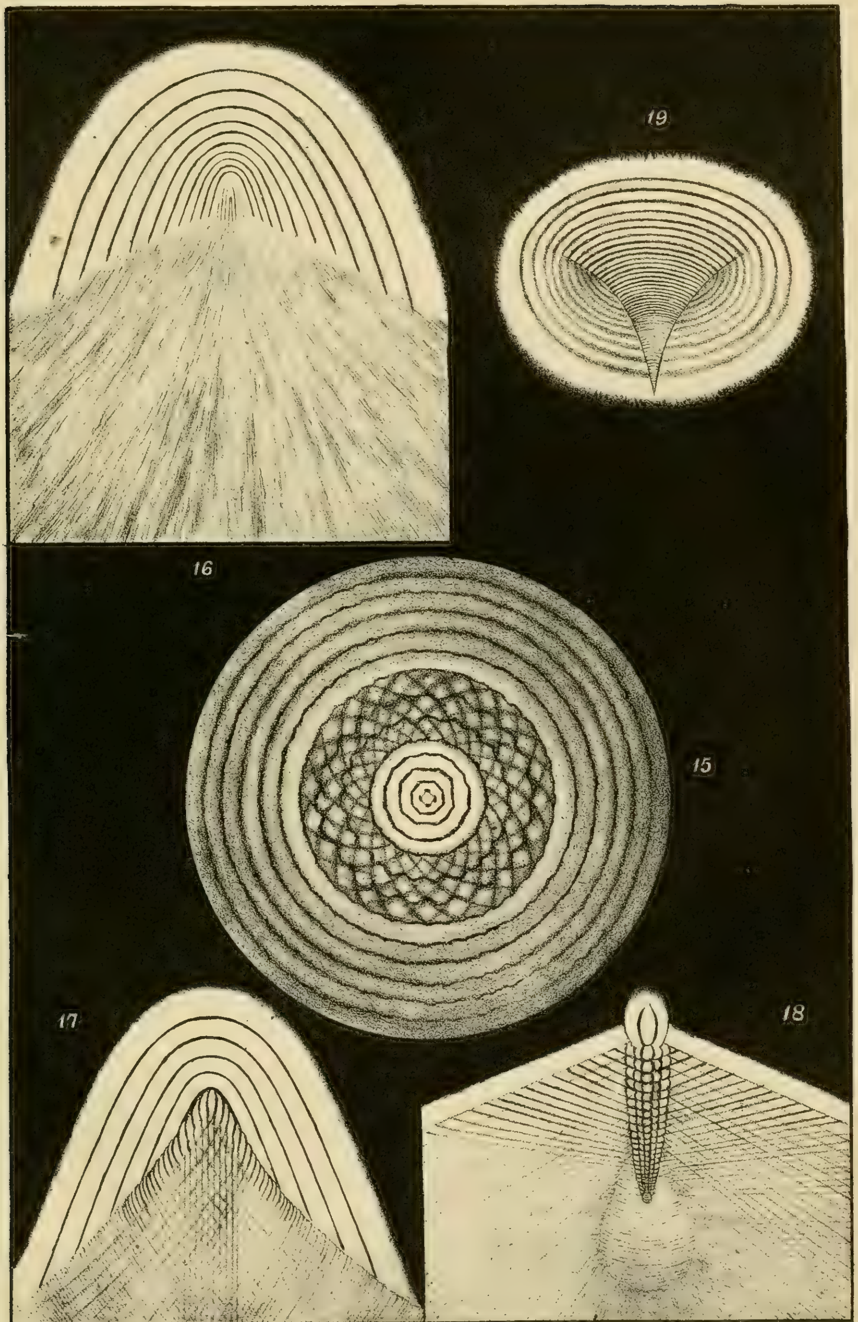
The circular spectra here delineated are produced by slight obliquity, and represent the appearances at different focal planes. The colours are extremely brilliant, and the lines perfectly sharp in their tracery.

Fig. 23.—Shows the lines formed by the circular solar spectrum viewed with the greatest obliquity attainable; the elliptic lines representing a plane cutting both sides of the cone of converging rays.

Figs. 24, 25.—Introduce a new order of figures formed by placing a mercurial globule 10 inches from the stage, and placing the $1\frac{1}{2}$-inch object condenser with its axis considerably inclined (an angle of 15°) to that of the microscope, as seen with a microscope armed with a good eye-piece (Kellner 1-inch) and a fine $\frac{1}{4}$-objective.

Very beautiful transformations of the circular solar spectra are seen by viewing the solar disk of the mercurial globule portrayed obliquely on the stage, which vary in their forms according as the glasses are under or over corrected, which it is needless here to describe in detail.

The figures displayed by the magnified artificial star for oblique reflexion render it probable that the obliquely illuminated mercury globule, viewed directly in close proximity to the front glass of the microscope upon the stage, is a very imperfect test, and the methods here described are submitted as possessing very superior delicacy and convenience.

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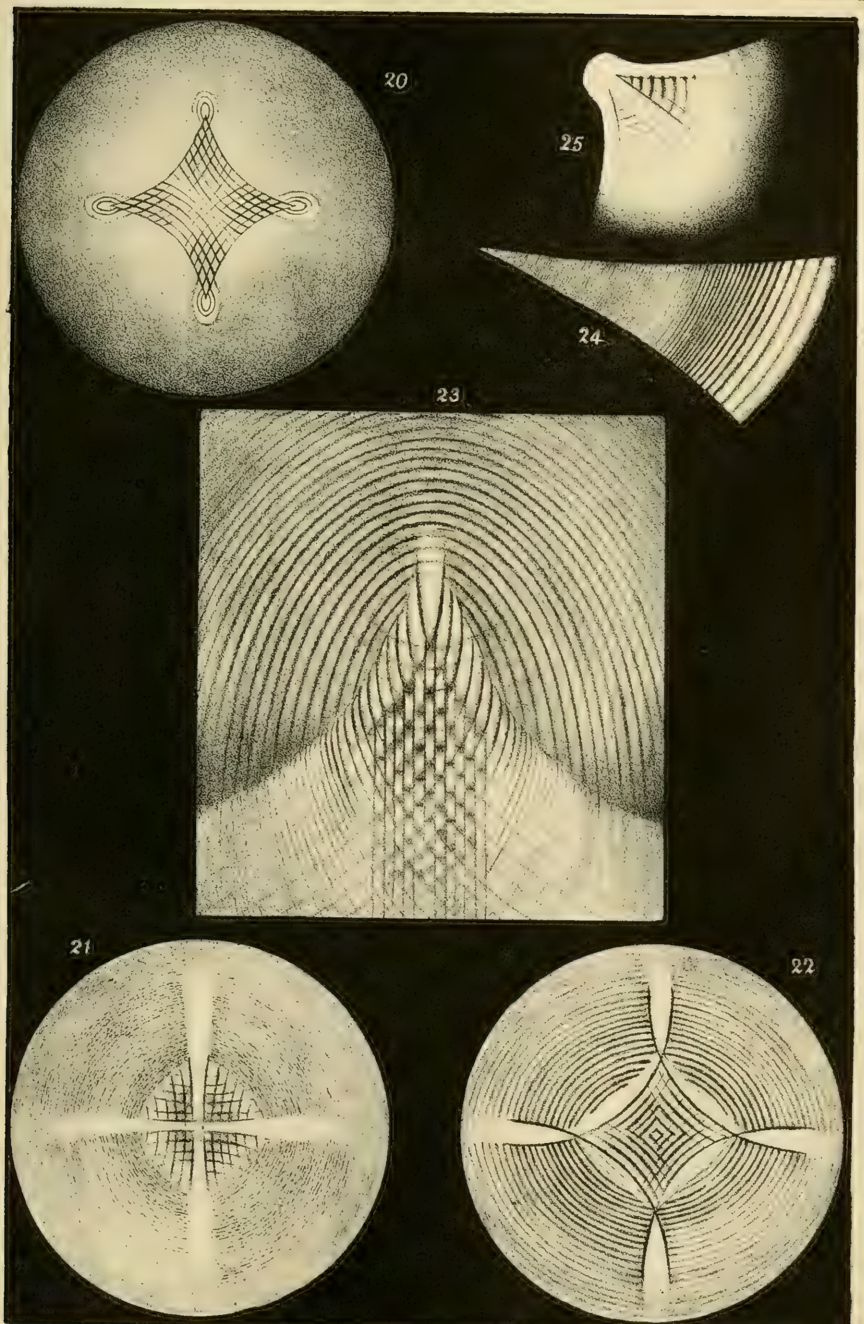


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Circular Solar Spectra.

the effects of the interference of complex cones of light of large angular aperture, at least twenty times larger than those observable by the telescope.

The subject of the optical contacts of Venus at the coming transit confers peculiar interest on the nature of accurate definition of the final image presented to the eye-glass, especially as the new parallax will be entirely dependent on the keen definition of the four contacts.

The discovery by the writer of an unsuspected residuary aberration in the best microscopes, described in the 'Philosophical Transactions' for 1870, renders it probable that some such a residuum still remains in telescopes; and this might impair the accuracy of such delicate observations as the apparent contacts between Venus and the solar limb. Eye-pieces, abounding generally with spherical aberration, require also particular attention. I have repeatedly observed a fine state of definition completely blurred merely by a change of eye-piece of the same power which no mere focussing ameliorated, and which could only be corrected by a change in the convergent pencil passing through the objective intrinsically affecting its aberration.

Another branch of such an inquiry would be the *nature of the definition of an organic particle under high powers*, as every such research, such as the detection of the characteristics of diseased and healthy cells, may be resolved into the power of the microscope to define a single organic particle. Such particles are generally brilliant and refracting, and the errors of observation are unfortunately at present of a numerous kind.*

## *On the Circular Solar Spectrum.*

If a lens † be placed within its axis coincident with that of the microscope; and if its principal focus, formed by the solar rays, be examined by an instrument of the highest quality, we shall find that minute slices, as it were, of the *solar cone* present phenomena of rare beauty and order dependent upon the quality of the examining instruments.

If two plano-convex lenses are placed with axes coincident, a good many coloured rings may be counted, but no black ones; so soon as their axes become oblique, the solar spectrum takes an intricate form, whilst the centre shows a brilliant cross (✠) very difficult to describe or represent by portraiture, but worthy of the highest photographic art are all the forms described.

This spectrum I venture to name the *circular solar spectrum*.

In my former research I had observed a flame-disk in a

* Appendix A.

† This term of course includes every form. Of concave lenses, however, only very small ones can be conveniently examined.

darkened room. This disk presented two or three diffraction-rings similar to those of telescopic stars, but much broader.

The same method was attempted with the sun. Various objectives were used to obtain a solar miniature of the sun's disk.

Plain mirrors of glass silvered at the back entirely failed.

In order to form a pure and brilliant solar spectrum under the microscope, it occurred to me to take advantage of the principle of total internal reflexion from a prism. I then constructed a prism-heliostat, which, acting in sunshine, presented an aërial miniature of the sun of great splendour (almost as dazzling as the sun itself) for half an hour, without further adjustment.

The prism-heliostat was furnished with a crown-glass double convex lens, itself being of flint, and other lenses of less focal length could be attached to diminish or increase the diameter of the primary image of the sun, which, as the focal length generally used was three inches, gave an image 3 sin 30′, in diameter, or one-fortieth of an inch nearly. In some cases object-glasses and eye-pieces were placed in the solar rays emanating from the prism.

Received by an inverted object-glass of the finest quality at a distance of 200 inches, the solar disk could be further miniatured to any desirable degree of minuteness. A theoretical diameter of sixteen millionths was found convenient. To moderate the overpowering brilliance of such a spectrum directly viewed, dark slides of graduated neutral tints were at first used, and smaller primary disks were obtained by using deeper lenses at the prism.

When this minute spectrum is viewed with a high power (800 or 1000), the phenomena attending residuary errors, whether of achromatism, spherical aberration, or mechanical construction, are demonstrated with so keen a severity upon the handiworks of man as to throw all other methods into the shade.

## *Phenomena Observed.*

With the flame-disk formerly used * only two or three diffraction-rings could (as already remarked) be descried; an extraordinary number of richly-coloured rings of dazzling brilliance was now exhibited at the instant of bringing the solar disk into the plane of focal vision.

The most striking feature, amid so much effulgence, was an intensely black (*jet-black*) diffraction-ring encircling the central disk at the clearest focal point. The appearance of the rings changed every instant with the slightest change of focus, and their tints indicated the nature of the "secondary spectrum."

Upon closer inspection, my curiosity was excited by observing

* 'Phil. Trans.,' part ii., 1870, p. 595.

the shape of the primary or central black ring deviating from a true circular form, somewhat squared off as though not consisting of one pure black ring. I then found, upon more careful examination, by change in the eye-pieces, length of body and "collar corrections," that it was composed of several eccentric rings. The research, as one depended upon the fitful gleams of a spring sunshine, though tedious, was at this point enlivened with the out-come of an important fact which will be more fully noticed farther on [*viz.* that achromatism and aplanatism,* in the best adjustable microscopes, at present were found to be altogether incongruous.]

The same result, the enlarged disk, as described in my former paper, was obtained.

The theoretical disk (exhibited on the stage of the microscope) appeared increased to nearly four times its proper size.†

Three general features were constantly observed. The rings were seen either wholly or chiefly on one side of the solar disk, *i. e.* either within or without the focus, or nearly similar, except in colour, on opposite sides of the focal point.

If the rings were on one side a nebulous brightness occupied the other, into which the solar disk suddenly resolved itself on a slight change of focus. But frequently this nebulosity assumed a fine-grained or "engine-turned pattern." Occasionally two primary disks, each with its own system of rings, struggled for the mastery; and on changing the focus, a chromatrope-effect was produced by the expanding rings, and their eccentric intersections, presenting an extraordinary loveliness of colours.

Another result somewhat startled me. In some of the best glasses the movement of the Ross collar adjustment for the position of the front lenses entirely decentred the solar disk, so that here two appeared occasionally instead of one.

This phenomenon compelled me to infer that in many cases the collar adjustment may become a greater source of error than the "thickness" of glass cover which it is intended to compensate; and that therefore excellent glasses, constructed with a permanent setting, are preferable, especially as a new compensation can be effected as described, p. 182.‡

* Freedom from Spherical aberration.

† Diameter of disk at prism 3 sin 30′ .. .. .. .. .. .. .. = ·026
Diameter of miniature at 200 inches distance, reduced by $\frac{1}{8}$-objective 1600 times .. .. .. .. .. .. .. .. .. = 0·000016

$$\frac{16}{1000000}.$$

Observed diameter of solar disk, exclusive of its jet-black diffraction-ring .. .. .. .. .. .. .. .. .. .. .. = 0·000061
Estimated breadth of black ring .. .. .. .. .. .. .. .. = 0·00002
And 0·000016 :: 0·000061 :: 4 : 1 nearly.

‡ By means of the compensating eye-piece.

The frequent appearance of several disks at once in the field of view caused me to suspect that the axes of the component lenses of the objectives were not always coincident.

An Andrew Ross "quarter" marked 1851, though of good quality, displayed several irregularly-placed central disks which formed so many different centres of diffraction-rings.

A Berlin glass of good quality showed a much finer primary black ring, and a splendid display of several coloured rings edged with black; in a deeper focus four false centres appeared.

It will be not out of place here to detail a few experiments conducted with the object of verifying the cause of irregularity in the primary black, and the particular signs of chromatic and spherical residuary aberration.

One of the finest "immersion" one-eighths of Powell and Lealand, made expressly for me in 1871, the aberrations of which were small compared with those of glasses of their old construction, was now used to form the miniature solar disk on the stage, as derived from the distant prism-heliostat.

I then examined the solar disk with a Powell and Lealand one-sixteenth immersion objective (1862 make) adapted to water in 1870, a water-film being introduced between the glasses whose axes had been carefully adjusted to coincidence.

*Two overlapping disks* were now seen. Each formed its own independent diffraction systems above the best focus, and evanished below it with a confused bright halo.

In order to determine the cause of this and to ensure one axis in the solar disk, I substituted for the miniature-giving objective a convexo-plane lens of half an inch focus; and as this gave too large a disk, the lens of the heliostat was reduced from three to an inch and a half focal length.

[Unless the solar disk is reduced, the splendid phenomena of the rings cannot be properly developed. Their number and colours change with the slightest change of the plane of focal vision, and a very fine and delicate focal adjustment screw, as well as great firmness in the apparatus, are essential to a successful display of the rings in all their wonderful beauty and complexity.]

The disk formed by the simple lens was now scrutinized with the Powell and Lealand celebrated immersion "eighth." Deeper eye-pieces and a lengthened tube were employed to subdue the intolerable brilliance of the coloured rings. They now exactly filled the whole field of view. At first, used dry (improperly), this objective displayed a crimson solar disk edged with an intensely black ring encircled with a much broader bright ring, resembling the planet Saturn viewed perpendicularly to the plane of his ring. (Pl. XXXIII., Figs. 1–3.)

Deepening the focus with exceeding lightness of touch, the central disk now became pearly white, set off prettily by its companion black ring and a number of pale lavender, rose colour, and then brilliant outer circles of bright green with intervals of orange red, and more outwardly, circles of red merging into ill-defined black.

But as the glass was constructed for vision through a film of water upon a thin glass "cover," I now attached (by moisture) a small fragment of cover, 0·003 inch thick to the eighth, and delicately focussed down upon the solar spectrum.

The solar disk then appeared single, circular, and bounded by a clear sharp black edge almost perfectly circular.

Upon examining the axis in different planes of vision, or different sections of the solar pencil, I counted no less than forty-eight magnificent rings (including the black rings and interspaces) displayed at one time in the same field of view. Derived directly from the sun, with the brilliance belonging to total internal reflexion, this rich assemblage of gorgeous rings, rivalling each other in prismatic splendour, set off by the sharp contrasts of jet-black well-defined borders, and shaded with the most delicate tints melting into one another with an exquisite softness, reminded me of the eloquent and glowing language of the late Sir John Herschel, when describing the phenomena of diffraction.

Doubtless, however, these appearances surpassed in intensity and brilliance those he described. (Pl. XXXIII., Figs. 2, 3.)

Careful measurements were made.

The diameter of the central disk, $\frac{1}{16333}$.

Breadth of its black ring, $\frac{1}{50000}$.

My surprise was further increased by observing that, by lengthening the tube to increase power, I was enabled to cause each of the ruled lines of a micrometric eye-piece (200 to inch) to coincide exactly with the inner edge of each black ring; so that the breadth of each complete ring was exactly the same as that of the central disk, *viz.* $\frac{1}{16333}$ of an inch. (Pl. XXXIII., Fig. 3.)

Slight changes in the colours of the rings were caused by the use of the Ross collar corrections.

In these researches a very near approximation to achromatism was signified by the whiteness of the central disk; the blackness of the fine rings contrasting finely with the intervening rings which were then of a lavender grey, or very pale and yet brilliant lavender.

Destruction of spherical aberration appeared imminent, when the rings, still coloured, were tolerably symmetrical on different sides of the finest focus with contrasting colours of the residuary spectrum. Mechanical errors were displayed by irregularity and complexity of form.

I shall now venture to give some particulars of the circular solar spectrum thus formed (as described) by a convexo-plane lens in a beam of sunshine examined by a high-quality immersion-objective armed with a small piece of broken cover attached by the cohesion of water.

Description of the Rings of the Circular Spectrum of a Convexo-plane Lens.

| Coloured Rings. | | Intervals. | |
|---|---|---|---|
| Solar disk | White | Primary ring | Jet-black. |
| Ring II. | Pale lavender | Secondary ring | Black. |
| „ IV. | Lavender | Third „ | „ |
| „ VI. | „ | Fourth „ | „ |
| „ VIII. | „ | Fifth „ | „ |
| „ X. | Pale rose | Sixth „ | Dark red. |
| „ XII. | Bright green | Seventh „ | „ |
| „ XIV. | „ | Eighth „ | „ |
| „ XVI. | „ | Ninth „ | „ |
| „ XVIII. | „ | Tenth „ | Black. |
| „ XX. | Dark orange | Eleventh „ | „ |
| „ XXII. | Deep „ | Twelfth „ | „ |
| „ XXIV. | „ „ | | |

Each of the rings, including the companion black or dark rings, appeared exactly of the same breadth, *viz.* 61-millionths of an inch, or nearly double the length of the wave of the extreme red ray, whilst the breadth of the primary black ring was nearly that of the wave-length for the line F in Fraunhöfer's spectrum, *viz.* 0·0004606 millimètres, or, since the French mètre is in English inches,

$$39 \cdot 37078984,$$

it corresponds to 52,256 waves to the English inch.

The delicate measurement of the primary black ring and disk was verified by a recording eye-piece micrometer. With this, and the objective used, it was found that one-thousandth of an inch on the stage measured 1138 divisions, *i. e.* eleven turns of the divided head and 38–100ths of a turn. One division therefore represented

$$\frac{0 \cdot 001}{1138} = \frac{1}{1,138,000} = \text{nine ten-millionths nearly.}$$

On estimating the breadth of the primary jet-black ring, and using a ruled glass micrometer, as I could detect no difference in the breadths of each ring, I felt justified in dividing the total diameter by the number of rings in order to obtain the breadth of one which gave $\frac{1}{16333}$. A much deeper point in the axis showed a very deep blue central haze, paling outwardly, and then melting into a final red fringe.

The precision of the mechanical construction of this fine objective was thus revealed by the use of a simple convex lens of crown glass. Any deviation from accuracy was at once detected by the converging pencil of the plano-convex lens, consisting of shells of rays of various refrangibilities, having their several foci arranged along the axis. As this axis was necessarily single and unique, the interference phenomena, especially the sharpness and intensity of the jet-black rings, could only be so superbly exhibited by the best glasses. Inferior glasses blurred them and dulled the rich beauty of the colours.

Contrasting with this the performance of a variety of glasses, both English and foreign, very peculiar appearances arose which doubtless indicated grave errors of construction. I will venture briefly to mention some of these:—

(I.) A variety of spurious disks oddly arranged were displayed.

(II.) The beauty of the rings was entirely marred; and

(III.) Very few rings could be developed, and sometimes no black rings whatever. (Pl. XXXIII., Fig. 7, and Pl. XXXIV., Fig. 14.)

(IV.) Notched, grained, and spotted; the rings were sometimes irregular in shape (Fig. 12).

(V.) A multitude of fine black eccentric rings, evidently arising from different centres, were seen upon a leaden-grey field surrounding the central disk, the confusion of the rings causing a bad achromatism.

(VI.) An "engine-turned pattern" was not unfrequent, degenerating into a peculiar grained and mottled appearance.

(VII.) A majority of the glasses were over-corrected spherically.

(VIII.) Achromatism and aplanatism in our best adjustable glasses were found to be altogether incongruous.

As this result was alluded to at page 175, I proceed to relate the circumstances of the observation. I constantly found in pursuing these researches that either achromatism was sacrificed to aplanatism, or that the attainment of achromatism destroyed the brightness and truth of aplanatism. I may relate the following experience.

Whenever in watching the heliostat the sun was clouded over, the microscopic miniature-perspective of the room and distant apparatus reappeared; and after various adjustments I obtained a perfect definition free from mist, and as clear and sharp as that of an opera glass. The prism and lens of the heliostat then gave a pretty picture of the passing clouds, as well as the small details of the distant objects; but the instant the sun began to shine, before the rings dazzled the sight, every shining point appeared haloed with a corona of orange and red. I now turned aside the prism: then every polished point in the full sunshine exhibited the same halo. Again in the shade, the picture resumed its sharp definition. Waiting again for the sun, forth shone the orange haloes. The corrections were diligently plied till the halo nearly disappeared. The sun passed behind a cloud. To my astonishment the former sharp clear prospect was now *bedimmed with a general white mist, obscuring all the details before so beautifully clear.* The appearance of this white mist, *above the best focal point,* whenever achromatism was attained by varying the adjustments of the screw collars, now convinced me that the modern English glasses, when rendered achromatic, beget a residuary spherical aberration, obscuring delicate structures (such as I propose to describe farther on), by a white mist corresponding to spherical over-correction—*viz.* the condition of the marginal rays for white light cutting the axis at points farther from the centre of the lens than the central rays, and that until this fact is acknowledged an insuperable bar to the finest definition will continue to exist. (Pl. XXXIV., Figs. 8, 9.)

Dr. Colonel Woodward, U.S.A., having taken up the research, declares he found it impossible to photograph the most difficult beaded objects unless, upon examining their image on a white screen, he represented the beads red upon a blue ground; then, using a solution of the ammonio-sulphate of copper to absorb the red rays, and then only, could he photograph the results I had described.* ('Monthly Mic. Journ.')

Residuary spherical aberration, it thus appears, is the chief cause of the difficulty experienced in defining organic particles—such as the molecules of physiological cells, blood-disks, mucous globules, and the discrimination of many forms of disease. It will probably remain uncorrected until opticians and observers abandon the false standards of definition still in vogue. If, then, it is at present impossible to avoid a residuary spherical aberration, whilst

* In confirmation of the same principle, the late Rev. J. B. Reade, F.R.S., wrote ('Popular Science Review,' p. 147, No. 35, 1870):—"Dr. Pigott has made also a very decided advance in the better correction of residuary aberration, a point which has, I believe, been almost completely ignored—nay even denied, until recently, by accurate observers as well as distinguished opticians. From my own experience in Dr. Pigott's studio, I have no doubt that his colour-test—a most interesting feature in his experiments—is the result of his finer balance of the aberrations . . . This new fact is one of the most striking phenomena in microscopical science of the present day." . . .

"Whether this colour-test is explained on the theory of vibrating wave-length corresponding to the infinitesimal thicknesses of films . . . or upon their radiation, refraction, and internal reflection of the spherical beads of which all scales and diatoms appear to be built up, are questions so recondite as to be worthy of the consideration of the most advanced physicists of the day."

As residuary aberration is still denied, I may be permitted to quote 'The Student' of February, 1870. It states that the writer "has told very plainly two startling and unwelcome *truths.* First, that observers have not seen their favourite test-object properly; and secondly, that their best object-glasses are afflicted with sufficient spherical aberration to render the structure which he describes invisible . . . and that all *difficult seeing* is in some suspense through these researches."

attaining very perfect achromatism, in the microscope, the finest definition will be obtained by stopping out the most obnoxious rays, either by using a monochromatic ray which suits the aplanatism, or using *bluish-green or blue glass* * to pale the red rays; for glasses may be aplanatic to one ray and not to another of a different refrangibility.

Before concluding this part of the paper I may be allowed to make a few practical conclusions for those who may wish to follow up this line of research:—

1. As stated in the paper "On a Searcher for Aplanatic Images," regarding a convex lens as under-corrected, under-correction is shown by the appearance of the rings below or beyond the focal point and evanishment into mist above it.

2. Similarity in the rings on both sides (with change of colour also) denotes a balance more or less delicate of the aberrations.

3. An eccentric position of the solar disk and a crowding of the rings more closely on one side than the other of the circular spectrum denotes parallelism, but non-coincidence of the axes of the convergent and divergent pencils.

4. Rare and beautiful forms resembling parachutes, vases or comets, made up of ellipsoid, parabolic or hyperbolic diffraction-lines, denote obliquity. (Plate VI., Proceedings R.S., June, 1873.)

5. Their form depends on the nature of the aberrations present, and the mode of arranging the axis of the cone of rays forming the solar disk.

6. Inaccurate centering of the component lenses, either at the heliostat or in the observing or miniature-making objectives, is shown by "eccentric turning" patterns and the appearance of two or several central disks at the smallest focal spectrum.

7. The apparatus necessary to display these brilliant phenomena must be exceptionally heavy and steady, and the fine adjustment should have a screw 100 threads to the inch; as the ten-thousandth of an inch in the axis of observation completely changes the aspect of the phenomena.

In none of these experiments did the supposed achromatism bear the severe ordeal of the circular solar spectrum. By no arrangements could colour be made to disappear. A white centre and exceedingly black rings, interspaced with a pale lavender and rose colour, were the nearest approaches to perfect achromatism which I could produce.

* Appendix B.

*On the Aberrations of Eye-pieces, with Suggestions for forming a Compensating Eye-piece for Microscopes and Telescopes; on the Principle of searching the Axis for Aplanatic Images.*

The Astronomer Royal has given an account of a trial of several kinds of eye-pieces, and some of their bad effects.*

The use of a solar disk formed by an eye-piece fixed close to the prism-heliostat deprived of its lens, and examined by the method already described by means of accurately corrected objectives, places the achromatism of the eye-piece under a severe scrutiny. Very rich and beautiful colours are developed in the solar rings (previously obtained as pale as possible), corresponding to the extent of the chromatic errors. A Huyghenian eye-piece was placed close to the heliostat so as to form a brilliant disk of the sun; the adjusted spectrum apparatus immediately flashed with brilliant coloured rings, before this, appearing pale lavender and white.

During the use of the searcher for aplanatic images, it occurred to me to investigate the effects of pushing the eye-piece gradually nearer the object-glass without a searcher.

I discovered that, when within 4 inches, the definition showed violent under-correction.

I now conceived the idea of substituting a traversing movement of the eye-piece, especially for glasses unprovided with a Ross collar, as a correction for thickness of cover.

A very firm sliding tube was constructed, and I now found I had substituted a range of several inches, as a correction, for that of a few hundredths of an inch used in the Ross adjustment.

*Experiment.*—Adjusting the apparatus and the screw collars of the objectives for severe testing, a bunch of small glass drops, of diameter 0·04 inch, was suspended in front of the heliostat so as to present a minute image of the sun. The searching eye-piece being placed at 10 inches distance from the stage of the microscope, in the plane of which the solar disk is formed, and the minute solar disk observed in a state of balanced corrections, it was found that as the eye-piece was traversed towards the disk it became gradually more and more under-corrected.

It now became evident that this movement was, upon a large scale, equivalent to the effect of the Ross collar movement upon a minute scale.

It now occurred to me, as a thick cover and a water film over-corrected an objective, that a dry objective might in many cases be transformed into an "immersion" simply by advancing the eye-piece; also that a sufficient variation of interval between the front lenses might in many cases enable a dry lens to act as an immersion.

[The immersion principle is valuable for the increased volume

* Astronomical notices.

of the cone of rays radiant from the illuminated particle mounted in balsam, a much larger pencil reaching the objective *viâ water* than can possibly be effected *viâ air*. The "critical angles" of total internal reflexion which determine the form of the caustic being so much larger in passing from glass into water than into air. I have shown elsewhere that the volume of the cone of rays transmitted from a radiant particle placed in balsam and surmounted with a thin glass cover, is about four times greater *viâ* water than *viâ* air; that result is explanatory of the greater brightness of the immersion lens.]

The question of the spherical and chromatic aberrations of eye-pieces has occupied the attention of the most distinguished mathematicians, and may theoretically be considered nearly exhausted, yet the practical detection of its existence is known to few, as it is liable to be mixed up with the objective aberration. The methods described are equally applicable to eye-pieces as to objectives.*

The construction of a compound compensating eye-piece which should almost be perfectly free from this residuum next engaged my attention.

From the discovery that the advance of an eye-piece towards the objective caused a violent *under-correction in the refocussed objective,* it became evident that a *shortened* microscope could be employed as a compound eye-piece nearly free from the usual aberrations, provided its object-glass were properly *over-corrected,* as compared with its performance at the usual standard distance of 10 inches.

The new eye-piece is finally corrected on the circular solar spectrum (herein described), being regarded and treated as a real microscope. Its object-glass, considering the exceedingly small pencil engaging it, may conveniently be formed of slightly over-corrected achromatic lenses, compensated by a variable interval. I have found an inch focal length sufficiently deep, mounted with a low eye-piece. The substitution of this compensating eye-piece for the ordinary deep Huyghenians afforded that degree of comfort in observation corresponding to enlarged pencils.

After adjustment it is quite as applicable to examine the performance of telescopes as microscopes. The adjustment is thus accomplished:—

1. The instrument, mounted as a complete microscope, was adjusted for the most perfect definition on an uncovered object; and supposing the glasses A, B (adjustable by a variable interval) defined perfectly with the usual length of tube, 10 inches, they require over-correction † or separation for a shorter tube of 6 inches.

* Appendix B.

† Considering the small angular aperture, a single set of achromatic lenses might be constructed and employed; but their correction is much more tediously attained than by separable sets of lenses.

2. Various eye-pieces were inserted, and the lenses A, B, separated or closed more or less till the most perfect definition was attained.

3. Now, regarding the instrument as a perfectly corrected compensating eye-piece, it is transferred to the tube of another microscope, the objective of which is again adjusted by the screw collar for the most distinct definition.

Tested on the principles glanced at in this paper, the corrected compensating eye-piece, free from the usual aberrations, may be confidently employed to test the circular solar spectrum of a minute distant solar heliostat disk, formed in its focal plane, by the objective also of a proposed telescope.

I trust observers with more convenient appliances and range of prospect will be induced to try a series of experiments on correcting residuary aberration in their object-glasses by the methods here laid down, by whom the writer will at any time be honoured by exhibiting some of the phenomena here described, as already done to Dr. Gladstone, F.R.S., and Mr. Beaumont, F.R.S.*

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## Appendix.

### A.—*Definition of Minute Organic Particles.*

A new fiftieth immersion lens, price thirty guineas, has been made for me by Messrs. Powell and Lealand; with this glass, without any obliquity, and using a tube 4 inches shorter than usual, and a B eye-piece, the upper continuous ribs of the *Podura* were resolved into strings of blue sapphire-like beads appearing perfectly circular. The interspaces between the markings at a lower focus showed lower strings of white beads.

At present nothing is more difficult of definition in the microscope than an assemblage of minute refracting organic particles. Virtually forming disks of light, these display the diffraction errors and phenomena more or less vividly. No English microscopist has yet succeeded, as far as is known to the writer, in displaying, in the apparently blank spaces, the beaded structure seen between the celebrated exclamation markings of the *Podura* test-object. Here a closely-packed mass of organic particles, highly refractive and transparent, obscure each other; brilliant points are swelled out exceedingly.

For a theoretical solar disk one-millionth of an inch † in diameter appeared as large as a disk the sixty thousandth. To define accurately bright organic particles, such as those of the smallest

* And more recently to our Secretary, Mr. Slack, F.G.S.

† Easily formed by placing a minute lens before the heliostat.

test *Podura*, the molecules of cancer cells and other diseased forms, monads (minute atoms of metal by reflected light might also be named), is at present impossible; when such delicate forms are in quest, all rays of an aberrating character must necessarily be extinguished.

B.—*Eye-pieces.*

The spherical aberration of positive eye-pieces may be examined as follows:—

The positive in an inverted position is placed under the stage, and forms an image of the solar disk for examination.—*Proceedings of the Royal Society*, Vol. XXI., No. 146, June, 1873.

[The fifth Plate belonging to this paper, representing the spectra exhibited by a globule of mercury, is at present omitted, but it may possibly reappear in a future article.]

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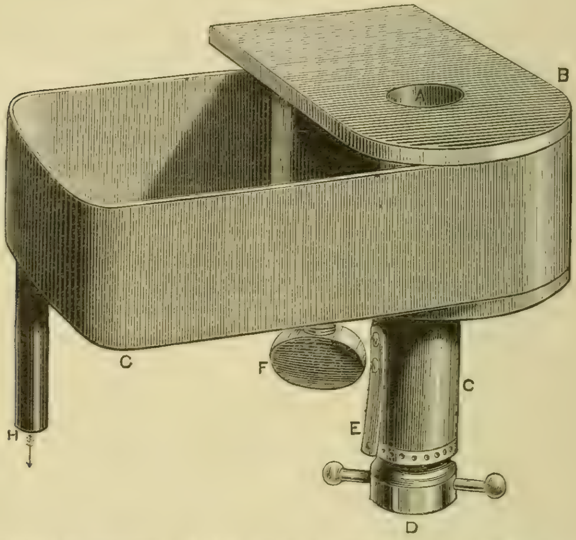
## III.—*A New Freezing Microtome.* By William Rutherford, M.D., Professor of Physiology, King's College, London.

All microscopists have experienced the difficulty which attends the cutting of soft tissues into thin slices. Valentin's knife is continually had recourse to for this purpose; but although by means of it excellent sections may be made of an organ having a density such as that of the kidney, it is by no means so successful in the case of mucous membrane, lung, or even in that of the skin; and for the brain, spinal cord, and the delicate tissues of the eye, spleen, thyroid and lymphatic glands it is useless. Such tissues require to be hardened ere they can be successfully sliced. They are usually hardened by means of chromic acid, potassium bichromate, Müller's fluid, alcohol, &c. In general, the hardening process requires considerable time, varying from several days to several weeks; and, after all, the tissues undergo alterations which, although advantageous in the case of some, are not so in the case of others. The vitreous humour is quite destroyed, lymph spaces are very often obliterated, soft tissues are sometimes shrivelled. The method of hardening the tissues by freezing has long been recognized as one of great importance in enabling the histologist to obtain sections of fresh tissues, but it has hitherto been little adopted owing to the inconvenience and clumsiness of the methods proposed.

In May, 1871, I published in the 'Journal of Anatomy and Physiology' an account of a microtome invented by me for the purpose of facilitating the process of freezing and of cutting frozen tissues. The apparatus there described and figured, although capable of doing much, is not quite so perfect as the following modification adopted by me some months ago. This new apparatus answers the purpose so satisfactorily, that it is now full time for me to publicly direct attention to it. The apparatus consists (Fig. 1) of a brass plate (B) with a hole in its centre (A). This

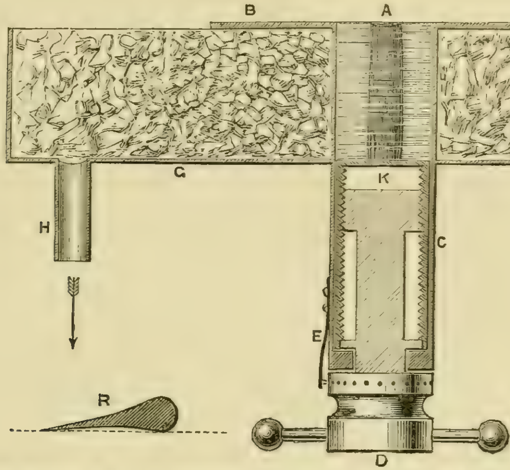
Fig. 1.




Rutherford's Microtome.—A, a Hole in the brass plate (B). C, Tube. D, Screw. E, Indicator. F, Screw for fixing the machine to a table. G, Box for holding a freezing mixture. H, Tube for permitting the water to flow from the melting ice.

hole leads into the interior of a vertical tube (E) with a movable bottom (K, Fig. 2), which may be raised or depressed by means of a screw (D). The tissue to be frozen and cut is placed in the tube (A). The section is made by means of a knife, which is glided horizontally through the tissue that projects above the level of the brass plate (B). The thickness of the section is regulated by an indicator (E). The machine is fixed to a table by means of a screw (F), and it may be employed for two objects—1st, for cutting tissues hardened in the ordinary way by chromic acid, &c.; and, 2nd, for cutting tissues hardened by freezing. The second method of using the machine will be more readily comprehended after a description of the first, which is simply this: Place a portion of hardened tissue, say a piece of spinal cord hardened by chromic acid, in the hole (A), and pour around it a mixture of paraffin (five parts) and hog's lard (one part) melted by the aid of a gentle heat. Or the paraffin mixture may be first poured into the hole, and the piece of tissue thereafter introduced and held in any desired position by means of forceps until the paraffin becomes sufficiently hard. In order that the paraffin may fairly support the tissue it is necessary

Fig. 2.




Vertical section of the Microtome.—The hole (A) is shown containing a piece of tissue, and the freezing box containing a freezing mixture. K, a movable bottom to the hole (A). R, Transverse section of the knife employed in making the sections. Other letters as in Fig. 1.

that the surface of the latter be dry. This is easily accomplished by leaving it exposed to the air for some time, either with or without previous immersion in spirit. A mixture of equal parts of beeswax and olive oil is much recommended for the same purpose as the paraffin in the above case. No doubt the wax and oil mixture is most excellent for imbedding tissues, and it can be cut with the greatest ease. But it melts at a higher temperature than paraffin, and, owing to the great thermal expansion, it retracts from the side of the tube of the machine, and so the wax cylinder becomes loose. The only way in which this can be prevented is by heating the machine to a like temperature before putting the wax into it. This is tedious; and inasmuch as it is unnecessary in the case of paraffin, this is to be preferred. Even with this, however, the paraffin cylinder is apt to become a little loose, and to turn round in the machine: hence it is important that there be an eccentric hole in the brass plug (K, Fig. 2), so that the paraffin may pass into the hole, and thereby be prevented from rotating. When the machine is used for the *second* object—that is, for freezing—the following directions are to be attended to:—Surround the freezing box with two or three layers of flannel, and screw the machine to a table. Unscrew the movable bottom or plug (K, Fig. 2), and pour methylated spirit into the tube (C); oil the side of the plug;

replace it; screw it down to any desirable extent, and there leave it. The object of this is to prevent the screw from becoming fixed by the freezing. The spirit which has come up above the plug (K) must be thoroughly removed by means of a towel, and a small slit at the margin of the plug carefully closed by means of hog's lard, which should also be spread in a thin layer around the entire margin of the plug to prevent the spirit from in any way reaching the cavity above the plug. The screw (D) must not be touched until the freezing is completed, in case this accident occur. The tissue to be frozen, together with an imbedding fluid, are placed in the hole. If we desire to be very careful of the protoplasts of the tissue, this fluid must be a so-called "neutral" fluid, such as blood-serum, albumen, or a three-quarter per cent. solution of common salt. It is, however, difficult to cut these fluids when frozen, owing to their becoming crystalline. For ordinary purposes it is preferable to employ a solution of gum arabic. This solution is made in the following manner:—Add to ten ounces of water two drachms of camphorated spirit and five ounces of picked gum arabic; when the gum has dissolved, strain the fluid through calico or tow, and preserve for use in a corked bottle. The gum, when frozen, can be cut with great facility; indeed, it can be sliced as easily as a piece of cheese. The gum or other fluid should be first placed in the hole in the machine, and when a film of ice has formed at the periphery the tissue should be introduced and held against the advancing ice until it becomes partially frozen. In this way a portion of tissue may be secured in any position for the process of section. Lay a piece of gutta-percha upon the brass plate (B) so as to cover the cavity containing the tissues, and prevent the entrance of heat and the accidental entrance of salt from the freezing mixture. Secure the gutta-percha by inverting, say, a small tumbler or beaker upon it, and place a weight thereon. Surround the box from side to side by two or three layers of flannel. These can be readily fixed by pushing them between the machine and the table to which it is fixed. Place in the freezing box (G) alternate quantities of finely-powdered ice and of salt, and take care that they are pushed round the tube of the machine, and also that the tube (H) is kept open in order to permit of the constant egress of the water from the melting ice. The freezing can be most rapidly effected by the addition, at short intervals, of small quantities of ice and salt, and by repeatedly stirring the mixture, in order that the escape of water may be facilitated. This is to be preferred to covering the whole machine up in a bag of the freezing mixture. This process is, when practised, really very simple, and can be fully carried out in from fifteen to twenty minutes. A number of tissues may be frozen and cut at the same time. It is possible, especially in winter, to have the tissue frozen too hard to permit of its being readily cut. It splinters when it is too hard. This is prevented by discontinuing the further addition of the freezing mixture, or by dropping water or a three-quarter per cent. salt solution at the ordinary temperature on the surface of the frozen tissue, or by heating the razor slightly. With regard to the process of section, it may be stated that a razor answers perfectly well for all ordinary purposes. The blade should always be *hollow* on both surfaces (R, Fig. 2). It is a mistake to employ a flat knife, for it is scarcely possible to keep the surface of the brass table of the machine smooth enough to permit of the knife lying quite flat. The knife should be *pushed* obliquely through the tissue, which should be cut at one sweep. This is not possible with a frozen tissue if the ice be too hard. For an unfrozen tissue imbedded in the paraffin the knife should be wetted with methylated spirit. This is dropped upon the knife from a funnel with an elastic tube depending from it, clamped by a Mohr's clip. The funnel is suspended at a convenient height above the machine. The spirit keeps the blade perfectly clean, and it forms a pool upon it, which enables the section to float over the surface with ease. In the case of freezing it is not necessary to wet the knife, for the melting ice readily does so.

Mr. McCarthy has modified my original machine by causing the ice box to project at the right as well as at the left side of the brass table (or the knife). I cannot at all approve of this modification, because it interferes very seriously with the movements of the right hand in the process of section. I have studiously avoided such an arrangement from the very first. The points in which my new differs from my old machine are these. The table for the knife (B) and the freezing box are larger. The escape tube (H) is larger, and the indicator is constructed upon a better principle. I advise those who may have obtained my old machine from Hawksley to get the freezing box made twice as large, and they will find it a very serviceable instrument, although, owing to the coarseness of the screw, necessitated by the nature of the indicator, that machine cannot give such a reliable result as that here described. The pathologist, the physiologist, the zoologist, and the botanist will find this machine of the greatest service. It supplies a desideratum long needed. But I must refer them to my forthcoming work on 'Practical Histology' for the special indication of the cases in which it is most applicable. In conclusion, I would express my gratification that so skilled a microscopist as Mr. Needham has publicly testified * to the advantages which my method of freezing tissues for the purposes of microscopy possesses over all others that have been hitherto proposed.

The new microtome is made by Baker, of High Holborn.—*The Lancet.*

* 'Monthly Microscopical Journal,' June, 1873.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*Microzymes productive of Gangrene.*—Some very curious and valuable experiments have been recently made by M. Chauveau, and are reported in the 'Medical Record' of May 28th, by Dr. Burdon Sanderson, F.R.S. At a recent meeting of the Biological Society of Paris, M. Chauveau related the results of experiments made by him for the purpose of determining the influence of the microzymes contained in certain purulent liquids in the production of gangrene. With this view, he used a method of experiment suggested by an operation commonly practised on rams in France as a substitute for castration. This operation is known as *bistournage.* It consists in reversing the testicle, and at the same time turning it round on its axis so as to give a double twist to the spermatic cord, close to the upper (in the reversed position the lower) border of the organ, which is then pushed upwards underneath the integument of the groin, where it remains. This operation, which is performed constantly by persons who make an occupation of it, is never attended with any bad consequences to the animal, either local or general. The envelopes of the testis become attached by vascular adhesions to the surrounding tissue ; but, as M. Chauveau has proved by careful injection, the organ itself is completely cut off from the circulation. The result is, that it dies and is eventually absorbed. If the parts be examined some time after the operation, the mass which represents the testicle is never offensive, but possesses a slight odour of rancid oil, indicating that it is undergoing fatty degeneration.

If, however, the operation be performed in an animal previously prepared by the injection of certain septic products into the circulation, the effects are different. To prove this, M. Chauveau obtains pus from a septic abscess in the horse, and deprives it of its corpuscles by subsidence and decantation. Having then ascertained by microscopical examination that it contains no formed elements excepting microzymes (rods and chains), he injects a quantity of the liquid, previously ascertained to be sufficient to produce a constitutional reaction of about twenty-four hours' duration. If within this period the operation of *bistournage* be performed, the organ becomes gangrenous, and the surrounding tissues undergo intense and rapidly progressing inflammation.

In this process, M. Chauveau believes that the microzymes are the active agents; and further, that the entrance of these organisms into the testis is an essential condition to its undergoing the gangrenous, *i. e.* putrefactive change.

To prove the first of these propositions, he varied the experiments by substituting for the septic purulent liquid first employed, the same liquid after depriving it of almost all its microzymes by a suitable method of filtration. It was then found that the injection produced no constitutional disturbance, and that the local process went on just as if no injection had taken place. To prove the second point, *viz.* the necessity of actual *penetration* of the septic products into the part, he performed *bistournage* on both sides at different times, *viz.* on the right side *before* injection of the microzyme liquid, on the left *after* the injection. On the left side there were gangrene and surrounding inflammation as in the former experiments, but on the right these were absent. Their absence appeared plainly to indicate that the entrance of blood containing septic products was a *sine quâ non* in the production of the gangrene. M. Chauveau would therefore refuse to attribute the result either to the increase of temperature (fever) or to the general functional disorder produced by the injection, but would refer it directly to the septic products, the presence of which in the blood is the determining cause of the fever itself. With reference to these important experiments, one of which Dr. Sanderson had recently the opportunity of witnessing, he thinks it is to be noticed, first, that although they appear clearly to show that in the present case the septic process was set up in the part by the agency of products carried into it by the blood-stream and of extrinsic origin, they are not contradictory to other facts which show that under other conditions an inflamed or injured part may become the seat of septic changes independently of any contamination from without; and secondly that they do not afford any answer to the question whether microzymes are the *generators* or merely the *carriers* of the poison resident in septic exudation-liquids. Their chief value consists in the light thrown by them on the mechanism by which septic impregnation of the blood *reacts* on local processes.

*Lamarck's 'Philosophie Zoologique.'*—We learn from a letter which a Dublin correspondent, whose name is not given, has sent to the 'Lancet' (July 12th), that the original edition of Lamarck's 'Philosophie Zoologique,' which appeared in 1809, has been reprinted with scrupulous fidelity by M. Charles Martins. The work marks the point of departure for all theories of evolution, and entitles France to claim a considerable share in the movement of which Darwin is the greatest representative, and which is now affecting so profoundly the philosophy of natural science. M. Martins prefaces it by an introduction treating of the biography of Lamarck, and bringing forward unknown facts relating to that *savant*—facts which were beyond the reach of contemporary appreciation, but which prove him to have anticipated at the commencement of the nineteenth century many of the most striking generalizations of living naturalists.

*Do Cryptogamic Plants influence the presence of Lead and Iron in Water?*—According to a Dutchman, M. W. Dammann, they do. In the 'Medical Record' (May 7) he says that during the years 1864–1867, a number of cases of lead-poisoning, both acute and chronic, came under his notice. On examining the water, negative results only were obtained. There were present, however, large numbers of cryptogamic plants, sometimes of a reddish-brown colour; and, believing it not impossible that lead might be taken up by these organisms in the same way as iron, sulphur, lime, &c., he filled two bottles with distilled water, and having placed some acetate of lead in one and some minium in the other, he introduced into both bottles cryptogams taken from water that had been kept in iron vessels. The following are the results at which he arrived from examination made after many months, the bottles having been in the meantime well closed. 1. Some cryptogamic plants are capable of taking up metallic oxides (lead, iron, &c.), and forming organic combinations. 2. For the formation of the organic structures, water is not sufficient; the presence of carbon, &c., appears to be necessary. While vegetations were developed in great numbers in the bottle in which acetate of lead had been placed, very few were formed in that containing minium. 3. For the testing of drinking water for lead, the ordinary reactions of sulphydric acid or sulphide of ammonium is insufficient. A large quantity of the water must be allowed to stand; after the upper part has been decanted off, the lower portion must be boiled down to a minimum and treated with strong hydrochloric acid. 4. The result of experiments made by treating the plants containing lead with a mixture of glycerine and pepsine renders it very probable that, when cryptogamic vegetations containing the metal are taken into the stomach, lead is separated from them by the gastric juice. The use of lead in water pipes is to be avoided; and if it be necessary to use water in which cryptogams containing lead are present, it should be allowed to stand, the lower portion should be rejected, and the water filtered before being used.

*Herr Huizinga on Abiogenesis.*—In the 'Centralblatt' (xv. 1873) Herr O. Huizinga suggests a mode of dealing with the abiogenesis question. He refers * to and repeats Bastian's and Sanderson's experiments on the development of low organisms in turnip infusions containing various salts, and finds that no bacteria are developed if the contents of the vessel be boiled, and whilst boiling the vessel be closed with a cap of filter paper. He remarks, however, that the paper might easily be the means of the introduction of germs, and that the only certain protection against such introduction is afforded by some material that can be strongly heated. He accordingly recommends certain plates named in Germany "Estricken," and which appear to be equivalent to our porous earthenware. Of these he makes stoppers that are luted into the mouths of the flasks, when their contents are boiling, with asphalte. Such stoppers permit the passage of air, but perfectly occlude the passage of germs. To prove this, he took two flasks containing solution of salt and ammonium tartrate. Into one he inserted a small quantity of dust collected in a room containing decomposing substances, as urine, whilst the other was closed when boiling and some of the dust distributed on the surface of the stopper. In twenty-four hours the first solution was already troubled, and soon after abundant bacteria appeared, whilst the other remained clear for more than a week, but was not further examined.

*What is and what should be the Work of Local Natural History and Microscopical Societies.*—Professor Gulliver gives his views on this matter in a contemporary, and as they are, in our opinion, of considerable importance, we quote them for our readers.† Talking of the

* 'Lancet,' July 12th. † See 'Nature,' May 22.

meetings, he says, whenever a rare plant or animal is exhibited at those meetings, we have always a wail about its having been "not long since often seen, though now fast disappearing." A chief cause of this is the deplorable rapacity of collectors of and traffickers in specimens; since the preposterous notion prevails that botany and entomology consist in a recognition of the mere physiognomy, without the least regard to the physiology, of species, and being able to call them by their scientific names.

And so it will be while local societies continue to encourage such errors, instead of promulgating the essential principles of botanical or entomological science, and obstructing the injurious operations of mere collectors or pretenders. And this desirable end, so far as regards taxonomy, might be easily attained without the least harm to rare species. Prizes for the best display, illustrated by microscopic drawings and preparations of the generic and specific characters of sections or the whole of many natural orders, would afford really good tests of the industry and attainments of the candidates. For example, why not try for this purpose the Willows, Grasses, or Sedges? Two of these orders have the further recommendation of being of great economic value. Again, as specific distinctions seem to be the ultimate aim of these societies, certain cells or tissues, such as the pollen, epidermis, hairs, and stomata, would afford good subjects for investigation in this point of view, as would also raphides and other plant-crystals, and very likely disclose valuable characters not yet recognized in the books of systematic botany.

I have been led to these remarks by the increasing frequency of the practice now deplored. As the West Kent Natural History, Microscopical, and Photographic Society is much and deservedly respected, and exercises justly considerable influence in its department, an extract from its last 'Council's Report,' p. 19, will suffice as a sample of the mischief:—"With a view to promote the study of entomology and botany among the members of the society and their families, the council, in the early part of the year, announced their intention of giving two prizes of 5*l.* 5*s.* each, one for the best botanical collection, the other for the best collection of Lepidopterous insects; all specimens to be gathered or taken within the West Kent district." This quotation is by no means intended for blame to any particular society, but merely as an example taken from one of the printed 'Reports' that has lately reached me of what is still being sown broadcast generally throughout the country.

*The Lymphatics of the Spleen.*—From an article in the 'Lancet' (July 12), we learn that E. B. Kyber describes the anatomy of the lymphatics of the horse as obtained from injections made with Prussian blue. Kyber agrees with Tomsa in admitting two systems of lymphatic vessels in the spleen; one belonging to the trabeculæ, which is in continuity with the lymphatics of the capsule; and a second accompanying the branches of the splenic artery, which are surrounded by its divisions as by a sheath. These two he names respectively the trabecular and the perivascular lymphatics. Occasionally the latter can be injected from the former. The perivascular lymphatics appear to arise in a delicate adenoid tissue enclosing the smaller arteries, partly from a plexus and partly from lymph cavities, the walls of which are formed of endothelial cells alone. The trabecular system of lymphatics arises in a plexus lying between the muscle-cell fasciculi. Kyber insists that a distinction must be made between the splenic pulp and the adenoid tissue surrounding the arteries, and points out the difference both microscopically and pathologically. The latter he regards as performing the usual functions of the lymphatic system; whilst the former, he conceives, may exercise that digestive action on the albuminates of the spleen which Schiff has demonstrated takes place.

*The Microscopy of the Delhi Sores.*—On this subject the address given by Dr. Parkes to the British Medical Association is of interest to the histologist.* He says, that in a class by itself, for the recognized cause of the disease cannot at present be referred to any plant, though it resembles perhaps no common animal cell, must be placed the small cell which, by its extraordinary powers of growth and attraction of food, causes the painful and obstinate sores known in India and Syria by so many names. The Delhi or Damascus sore, the Aleppo evil, and other names have been applied to a disease which is spread all over the East, affecting men and dogs, and which, though not fatal, is yet in the highest degree harassing and discomfiting. The discovery of the cause and its cure we owe to Dr. Fleming, of the Army Medical Service, and it is a good instance of the great use of the microscope in the hands of a competent man. Dr. Fleming found as a constant element in these rodent ulcers a small cell: its nature is quite doubtful; no kind of plant can be developed from it, and it is presumably of animal origin; it contains nuclei, and grows marvellously fast, though whether by cleavage or budding or exosmotic transit, so to speak, of small cells through its wall, has not been made out. By pressing on and absorbing the nutrition of the skin, it soon destroys portions of the surface, and forms most unsightly and painful ulcers. That this cell is the cause has been proved by repeated inoculations. It is very tenacious of life and resistant to chemical agents, hence the uselessness of the common plans of local treatment which have been so repeatedly tried without effect. The only cure is at once to destroy the cells with potassa fusa. In a few days a sore which has been open and extending for months is cured as by magic. The cure is infallible, and if this plan of Dr. Fleming is carried out, he will have the merit of having at once obliterated a disease which has been a plague for hundreds of years, and neither spared the great Aurungzebe in his hall of paradise nor the meanest pariah who was no more than useless dust beneath his feet.

*Russian Microscopic Specimens.*—We learn from the 'British Medical Journal,' that at a recent meeting of the Medico-Chirurgical Society of Edinburgh (June 18), Dr. Matthews Duncan gave a short account of some beautiful microscopic specimens kindly brought to

* 'Medical Times,' Aug. 9.

the Society by Dr. Slavjansky of St. Petersburgh, illustrating adenoma polyposum hæmorrhagicum uteri, and also epithelial cancer of the uterus. The differences in the arrangement of the nest of epithelial cells in the connective tissue was very well shown.

*Amphibians without Metamorphoses.*—The 'Medical Record' says that M. Davay has discovered in Guadaloupe a genus of frogs (*Hylodes Martinicensis*) which does not pass through the tadpole stage, but is completely developed in ovo.

*Helmholtz on the Membrana Tympani.*—In his recently-published book Herr Helmholtz enters into a minute description of the membrana tympani, which he shows to be, not, as hitherto supposed, highly elastic, but an absolutely inextensible membrane, chiefly composed of tendinous fibres; and shows that its curved form renders it essentially different from all other membranes hitherto studied in acoustics. The articulation between the incus and the malleus he regards as analogous to that racket construction well known in certain watch-keys which offer resistance in one direction but not in the other. The tensor tympani, in contracting, renders tense all the fibrous bands which give firmness to the position of the ossicles, except the ligamentum mallei superius, which runs in the same direction as the muscle.

*The Microscopy of Textile Fabrics.*—We learn from 'Nature' that Dr. Robert Schlesinger publishes (from the house of Orell, Füssli, and Co., Zurich) a small work on the microscopical examination of Textile Fabrics in the raw and coloured state, with a note on the mode of detecting "shoddy wool." It contains a complete account of the fabrics made from the various vegetable fibres in more or less common use, also from hair and silk, with their distinguishing characteristics, as exhibited under the microscope, when raw, spun or woven, and dyed, illustrated with twenty-seven woodcuts, and introduced by a preface by Dr. Emil Kopp.

*The Natural History and Microscopy of the Compositæ.*—These have been excellently given in the last number of the 'Journal of the Linnean Society,' in a most elaborate paper by Mr. Bentham, F.R.S., who occupies the entire number in the discussion of his subject. In accordance with the system proposed in the 'Genera Plantarum,' he divides the order into thirteen sub-orders, *viz.*: 1, Vernoniaceæ; 2, Eupatoriaceæ; 3, Asteroideæ; 4, Inuloideæ; 5, Helianthoideæ; 6, Helenioideæ; 7, Anthemideæ; 8, Senecionideæ; 9, Calendulaceæ; 10, Arctotideæ; 11, Cynaroideæ; 12, Mutisiaceæ; 13, Cichoriaceæ; the most important diagnostic characters depending on the structure of the pistil (in the hermaphrodite flowers), fruit, andrœcium, corolla, and calyx (pappus). A very exhaustive account is given of the geographical distribution of the sub-orders and principal families; and the first appearance of the order is traced with probability to Africa, Western America, and probably Australia.

*An Experiment in support of Pasteur* is reported by the 'Lancet,' which says that the experiment is based on the operation of castrating rams by twisting the cord. Such proceeding is very effectual, and does not interfere with the general health of the animal. Now M. Chauveau, of Lyons, began by injecting into the blood of a ram from four to five grains of the matter obtained from putrid abscesses. This dose is sufficient to impregnate the whole organism, but not strong enough to kill. The first symptoms of septicæmic fever, which generally last from twelve to forty-eight hours, were allowed to pass by, and M. Chauveau then performed the usual twisting operation, *viz.* within, and not disturbing the scrotum. But the testis, which, by the introduction of the irritating matter, received putrid fluids carried there by the circulation, instead of becoming simply atrophied, was seized with gangrene, turned sloughy, and the animal died. M. Chauveau (whose paper was presented to the Academy of Sciences of Paris by M. Pasteur) considers that the cause of the gangrene lies in the vibriones; for when the same experiment was performed with the *filtrated* septic fluid no gangrene supervened. It should not, however, be forgotten that Dr. Onimus has made experiments of a negative kind, and denies that any septic power is possessed by the vibriones.

*Experiments on Archebiosis: Dr. Bastian and Dr. Sanderson.* — The following two letters appeared in succeeding weeks in *July* last on this subject; and as the subject is of considerable interest we extract both from 'Nature.' Firstly, we give Dr. Sanderson's letter, which is in reply to an earlier one by Dr. Bastian. He says, from Dr. Bastian's letter in last week's 'Nature' I learn that my last communication has afforded him satisfaction. The gratification which I feel at this expression of his approval is mixed with some surprise; for however confirmatory my experiments may be of his, so far as relates to the bare fact that boiling is insufficient to destroy the germinating power of the turnip-cheese liquid, they certainly do not tell in favour of the inference which he is understood to draw from that fact.

The experiments which Dr. Bastian was kind enough to show me last December were regarded by him as unequivocal instances of spontaneous generation. He will remember that at that time I stated to him, both orally and in writing, that the significance of the results in their relation to the doctrine of heterogenesis, appeared to me to be doubtful, and that I thought it probable that they would be interpreted by different persons in opposite senses, according to their preconceived opinions. I expressed myself in a similar manner at a discussion which took place on the subject last winter at the Royal Society. It was for the purpose of clearing up this doubt that I made the experiments recorded in my last communication. I did not expect to prove that the production of bacteria in Dr. Bastian's experiments was *not* spontaneous, but merely to determine whether the fact afforded any support to the opposite conclusion.

Having first shown that living organisms increase and multiply in the liquid in question, when boiled at the ordinary temperature, under circumstances which absolutely preclude the introduction of living matter from without, I prove that under otherwise similar conditions this result is not obtained when the liquid is subjected to ebullition at a slightly higher temperature. I show further that the liquid even when heated to $102^{\circ}\cdot 5$ C. suffers no impairment of its power of supporting the life of bacteria, for by inoculating it with a drop of ordinary distilled water it at once becomes pregnant. Hence I conclude, not that spontaneous generation is impossible, but that the particular experiment in question is not an instance of it, and that no argument founded on it in favour of the doctrine is of the slightest value.

It is unnecessary for me to occupy your space by at any length adverting to the side questions raised by Dr. Bastian in the other paragraphs of his letter.

In examining the liquids within a few days after heating rather than later, I followed his own method.

I made no attempt to determine the temperature of ebullition in flasks with capillary orifices, because I know of no method by which it could be done accurately. Besides, it was not required for my purpose.

I employed the word "chance" in its ordinary sense. In the sentence to which Dr. Bastian refers I explained that, although there may be a limit of temperature at which a liquid, before possessing the power of breeding bacteria, is deprived of that power, experiments such as mine are insufficient to define that limit. As regards the turnip-cheese liquid it has been shown that between the temperatures of 100° and 102° C., the probability of pregnancy diminishes rapidly as the temperature increases. It is not as yet possible to say at what point the probability vanishes.

Then comes Dr. Bastian's reply, which says that "Dr. Sanderson expresses some surprise that I was gratified by the facts recorded in his previous letter. My reasons were these. Dr. Sanderson's experiments in the eight successive cases in which he employed the temperature of 100° C. for twenty minutes were entirely confirmatory of my own, and were, moreover, so conducted as to refute the objections which have been urged by Dr. Wm. Roberts and others."

As to the bearing of Dr. Sanderson's experiments with higher temperatures and more prolonged periods of exposure to heat upon the general question of the independent origin of living matter, I wholly dissent from his now expressed conclusions, for the following reasons:—

In the first place his fluids were not kept sufficiently long before they were submitted to microscopical examination. Dr. Sanderson is quite mistaken in supposing that in examining his liquids within 3–6 days after their preparation he was following my method—more especially in cases such as these where the fluids have been exposed to temperatures higher than usual, or to 100° C. for upwards of twenty minutes. Three to six weeks have often elapsed before I thought it judicious to open my flasks.* In opening all his flasks at the end of 3–6 days, Dr. Sanderson lost the opportunity of watching the changes which might have ensued later in many of his experimental fluids—and hence lost his right to draw any conclusions from these abortive trials.

Secondly, these experiments are open to another objection. Dr. Sanderson concludes from them that exposure to a temperature of 101° C. almost always arrests the tendency to fermentation in his

* See 'Beginnings of Life,' vol. i., p. 355, p. 441, and Append. C.

experimental fluids. This conclusion I believe to be erroneous, because in the former series of experiments which I performed in his presence, and of which he recorded the results in your pages,* fermentation occurred in the majority of cases in fluids which I have very good reasons for believing to have been raised to a temperature of 103°·33 C.† The method recently employed by Dr. Sanderson for superheating his flasks was needlessly complicated, and the exact temperature to which they had been exposed was known only by inference—never by direct thermometric observation.

Leaving now the discussion of the experimental facts, I come to the examination of Dr. Sanderson's inferences, which seem still more open to objection.

Dr. Sanderson, in common with most others, had up to the date of his witnessing my experiments, admitted that bacteria and their germs were killed in all fluids with which he had experimented at the temperature of 100° C.‡ It was, indeed, this conviction which inspired himself, and many others, with a strong disbelief in the results which I obtained with previously boiled infusions.

What remains, then, for Dr. Sanderson to do, prior to drawing inferences such as he now expresses, is to ascertain, by direct examination, whether the temperature of 100° C. is or is not fatal to the life of bacteria. It is upon this that the interpretation of my results can alone depend. I have already contributed my share to the inquiry by several long series of experiments, each of which has led me to the same conclusion, *viz.* that bacteria and their germs, when in the moist state, are killed at a temperature of 60° C.§ It is for Dr. Sanderson, or any competent observers who are sufficiently interested, to examine my experiments and results on this part of the subject, or else to devise others for themselves having a similar bearing.

If I am right in believing that 60° C. is the thermal death-point of bacteria in the moist state, the conclusion which must be drawn

* 'Nature,' vol. vii., p. 180.

† Dr. Sanderson was not aware of this fact, and says he does not know any means by which the temperature of a fluid boiling briskly in a vessel from which the steam escapes only through a capillary orifice, could be accurately estimated. The method which I adopted some months ago seems to possess this merit. I had a small maximum thermometer made for the purpose, 2½ in. in length, and graduated from 95°–115° C. Having straightened the neck of one of my retorts (capable of holding about two fluid ounces), it was filled with some hay infusion and the thermometer was introduced in such a way that its bulb remained in the midst of the fluid, about three-quarters of an inch away from the glass. The long neck of the retort having then been drawn out and broken off (so as to leave the usual capillary orifice), the fluid was boiled for five minutes before the vessel was sealed. The thermometer was found to stand at 103°·33 C. The retorts employed in my previous experiments with Dr. Sanderson were of the same size, and their contained fluids were boiled under precisely similar conditions. If larger flasks, containing more fluid, were employed, the temperature would doubtless rise to a still higher degree owing to a corresponding increase in internal pressure.

‡ See 'Thirteenth Report of Medical Officer of Privy Council, 1871.'

§ See 'Beginnings of Life,' vol. i., pp. 325–333; 'Proceedings of Royal Society,' No. 143, 1873; and another paper about to appear in the next number of the 'Proceedings.'

from the now admitted results occurring in fluids which have been heated to 100° C. suffices for my argument as to the reality of Archebiosis. The further investigation of the results of raising fluids to higher temperatures for protracted periods is of great interest, but does not at all affect the question of the reality of Archebiosis; and Dr. Sanderson's present experiments have therefore none of the significance in the argument which he strangely enough appears to claim for them.

Briefly, having admitted that bacteria arise in fluids which have been submitted to a temperature of 100° C., it is for Dr. Sanderson to show that they are not killed in fluids at 60° C., as I maintain that they are, before he can attempt with any effect to draw inferences of his own, or to criticize those which I have drawn on the subject of the independent origin of living matter.

## NOTES AND MEMORANDA.

**The Silicified Wood of Lough Neagh.**—A very good paper has been read on this subject before the Belfast Natural History Society, by the Rev. G. Macloskie, M.A., LL.D. He explains very fully, by reference to the geological nature of the surrounding country, how the idea arose. The numerous cuts which illustrate the paper give it an additional interest.

**The Development of Insects.**—On this subject we know of no work which can compare with those of A. S. Packard, jun., M.D. They are the fullest and most admirably illustrated memoirs we have seen for some time. We hope to notice them at length shortly. They are "On the Development of *Limulus polyphemus*," and "Embryological Studies on Hexapodous Insects." The plates accompanying these two essays more nearly resemble the French than the English style; they are really exquisite bits of drawing.

## CORRESPONDENCE.

### New Use for an Objective.

*To the Editor of the 'Monthly Microscopical Journal.'*

At Sea, *June* 27, 1873.

Sir,—May I presume to suggest to your readers a trial of the following experiment:—

With an A eye-piece and a 1-inch working objective: place a $\frac{1}{4}$-inch or $\frac{1}{2}$-inch objective *inverted over the eye-piece*, and examine any simple object, say a *Pulex*, they will find that they have a direct image, more amplification, and a greater working distance. I think it superior to any erecting glass or prism.

Yours very truly,

Spellowhane, Liverpool. John A. Perry.

## The relative Prices of English and American Objectives.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *August 15th*, 1873.

Sir,—In the article in your August number, by Dr. Pigott, in the following passage, "I have very little doubt that if anyone be willing to offer Messrs. Powell and Lealand double the price of their $\frac{1}{16}$th—the same as charged for Tolles' immersion $\frac{1}{18}$th, by Mr. Stodder, $175, or 34*l.* sterling—they would be able to produce a glass proportionately improved in some of the minor details," Dr. Pigott has—of course unintentionally—made a large mistake in the comparative prices of the two instruments—a mistake that "uncommercial" writers have too often made, in my experience, from not knowing the value of the United States' currency; and he has also made another mistake, in ignorance of what was sold to Dr. Woodward.

As these errors, from the wide territory in which your Journal is circulated and read, are calculated to do a serious pecuniary injury to Mr. Tolles, I will ask the privilege to make a definite statement of the comparative cost of the two objectives.

Dr. Pigott (evidently) values the pound sterling at $5 United States' currency. The actual value to-day is $5 60c. It was 10c. to 12c. more when Dr. P. was probably writing—(this change from the constantly fluctuating value of gold)—so that the price paid by Dr. W. was not = 34*l.*, but only 31*l.* 4*s.* nearly, a difference of more than 10 per cent. But the excess over the cost of the P. and L. $\frac{1}{16}$th was partly caused by the addition to Dr. W.'s $\frac{1}{18}$th of a "compound" dry front valued at $40; deducting this item, which is not included in the price of P. and L.'s $\frac{1}{16}$th, leaves the actual price (for comparison) of the $\frac{1}{18}$th immersion with a single front of a *new plan never before used in any objective*, $135 = 24*l.* 4*s.* nearly, instead of 34*l.* But the price that American instruments are sold at should not be compared with the price of English instruments in London, but with the price that they can be imported for and sold here. The price of P. and L.'s $\frac{1}{16}$th in London is 16 guineas; add the duty only, *no freight, insurance, or other charges*, and it costs here $131 71c. United States' currency—a difference from T.'s price of the $\frac{1}{18}$th $3 29c. only. The excess of American cost over the English may be fully accounted for by the different rate of wages of skilled workmen in Boston and London; for though Tolles does all the important optical work with his own hands—as I suppose that P. and L. do—yet he must employ some assistance for the brasswork, and the men capable of doing that work *to suit him* command wages double the income of many English clergymen. As it has been publicly charged that Tolles' prices are "enormous," and as Dr. Pigott's statements appear to confirm the charge, it is due to him (Mr. T.) that this detailed explanation (never before made) should be as widely published. I do not suppose that Dr. Pigott wrote by authority of Messrs. P. and L. in his suggestion that an offer of a higher price would produce a better lens; they probably will not deem it a compliment that it is suggested that the prices of their published list are not for the best lenses they know how to make. Yours, &c.,

Charles Stodder.

corpuscles, the largest a very great number. In many of the medium size, and most, if not all, of the larger ones, the general mass appeared to be vacuolated, often very irregularly, with the outlines of the vacuoles indistinct, or rather ill-defined. Upon long watching, the relation of these to each other might now and then be seen to alter, yet there was no appearance of pulsation. In only three examples were noticed any projections having the character of pseudopodal protrusions, and these were exceedingly delicate, short, and seemed ill fitted for progression of the masses in the ordinary manner of pseudopods. In several of the masses the general shape could be seen to change slightly, and in two was noticed some activity of the mass evinced by a restless kind of motion; an effort apparently to twist round on their axes, and this to the right hand, as seen in the field of the microscope, then returning to a less than their former position, towards the left or starting point. One was watched for a period of about half an hour, yet it never made more than one-quarter to one-third of a revolution. No motion could be detected amongst the little granular bodies *imbedded* in the ordinary masses. Their size was remarked to be very variable. In some of the masses they were enlarged and separated to a considerable distance from each other, though adherent by the viscid protoplasmic substance, and such masses appeared to have a general tendency to diffluence. In others, which seemed to have reached a particular stage or period, the muco-gelatinous substance was condensed into a distinct structureless cell-membrane or "cell-envelope," thus passing, in all probability, into a sessile condition. One such mass was removed along with several of the smaller viscid masses by a very fine camel-hair pencil on to a growing slide, having a tin-foil cell cemented to the centre, for the cover to rest on when over the aperture in the slide, thus to give freedom of motion for any of these small bodies; but unfortunately putting down the thin covering-glass ruptured the "cell-envelope." The small granules or corpuscles set free were watched under the microscope for some period. At first they moved somewhat slowly, but when at a little distance from the mass they jostled and jerked themselves about in a very active manner, much after the fashion of motile zoospores; yet with a power of 2000, Gundlach's immersion, No. vii. A, I failed to distinguish then or later any cilium. When two were adherent by the viscid substance between them, the motions were very violent, often as a sort of springing apart from each other to obtain freedom. Very many of these little bodies soon jerked themselves across and out of the field of the microscope. They soon ceased to exhibit activity, and after thirty hours they were motionless, and had not regained it or altered in any visible particular; after fourteen days in the growing slide they appeared rather less in size. It is very possible the "cell-envelope" was ruptured before these bodies had attained to their proper growth. Being desirous of preserving this particular mass for watching in the growing slide no reagent, as iodine, &c., was applied.

In some of the medium-size masses, and even in the smaller ones, without any distinct border to the edge of a denser character than the general mass, these corpuscular bodies were larger than those which were set free by the rupture of the enclosing membrane, but whether they differ *inter se* from the enveloped granular bodies is unknown.

On the sixth day in the growing slides amongst the little motionless corpuscles some Bacteria were present, as *Bacterium termo*, and *Bacillus subtilis*, Cohn.

Each of the corpuscles showed a well-defined central dark point on focussing up, which appeared bright and surrounded by a fine dark ring on focussing down; not exactly as if containing an ordinary nucleus, but rather as if it depended upon the pyramidal shape of a highly refracting central body; yet in some this small central mass under high powers appeared irregular in outline, and as if disposed for division, though without any corresponding difference in the outer portion of the corpuscle. *Vide* Fig. × 2040.

The masses when compressed indicated no distinct aperture, though in two uncompressed there appeared a small pale circular spot with a better-defined edge than the vacuoles, which possibly might represent an orifice or nucleus; yet it was only seen in two, so may have been accidental. When divided by force the masses remained separated, and were not drawn back, nor did there seem to be any tendency to divide, further than that many looked as if they had thrown off part of the mass which remained as a smaller globular or circular body adherent to it by the edge, sometimes two or three such being present.

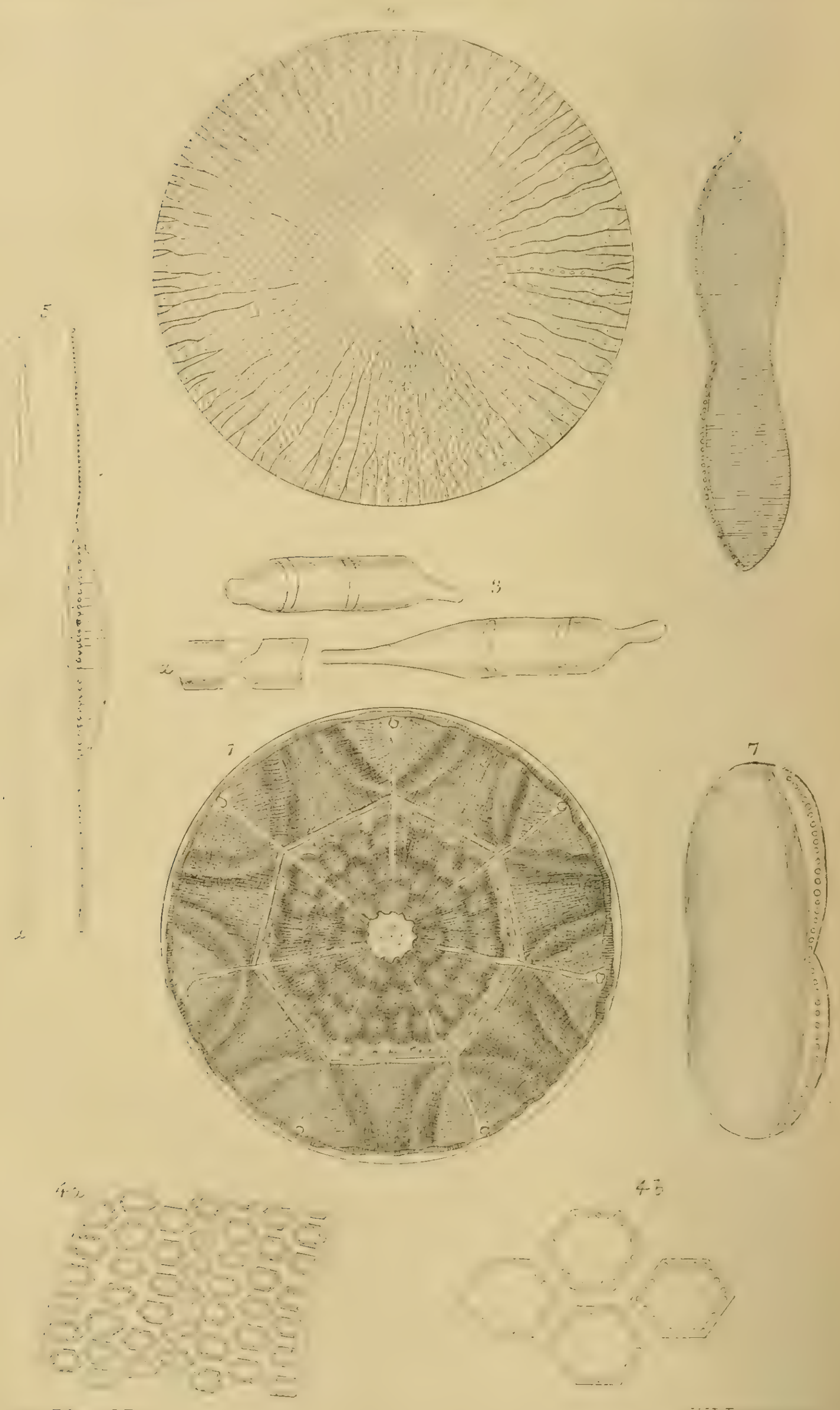
The term vacuole has been used to mean simply a more or less outlined differentiation of parts of the internal sarcode mass, and which appeared to alter in shape or position so slowly and indistinctly as to be only noticeable under long examination.

With the above characters I have found it somewhat difficult to relegate these bodies to any definite place amongst either the Phytozoa or Protozoa, though they fall, I think, more nearly to the naked Rhizopoda. No defined nucleus was certified as being present in any of the masses, nor was any act of fission noticed, and nothing of the character of ingesta was seen in the interior of any of them, so that the nourishment is probably drawn from the surrounding medium in a soluble state.

The only Amœbaform or true Rhizopodous bodies found in the same water were very minute sarcodic masses with comparatively long processes; some of these are figured, though they may not bear any relationship to the violet-coloured masses nor to the little corpuscles. Yet it is just probable that the motile zoospore-like bodies may, after losing the motile condition, pass into an Amœba-form state, still in the growing slide this has not been observed, the conditions possibly being at variance with the natural state.

As the distinguishing characters do not seem to correspond exactly with any of the recognized genuine naked or testaceous Rhizopoda, but to fall nearest to the genus Amœbæa, I propose, at least temporarily, to name it *Pseudo-amœba violacea.*

Since so much industry has been shown, especially by Häckel and others, in enlarging our knowledge of the Protista, it was judged better to bring the subject before your Society, though so imperfectly outlined, than to allow it to pass unheeded; trusting that the deficiency of the present details may be supplied hereafter by an extension of its life-history, if such be presented by future research.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## II.—*A Description of some New Species of Diatomaceæ.*

By F. Kitton, Norwich.

(*Read before the* Royal Microscopical Society, *Oct.* 1, 1873.)

Plate XXXVIII.

The genus Aulacodiscus is perhaps of all genera of Diatomaceæ the richest in beautiful forms, as the drawings by the late Dr. Greville, which have so often adorned the pages of the 'Transactions of the Royal Microscopical Society,' can testify.

A large proportion of these Diatomaceous gems were obtained from the Barbadoes deposits, and the species I have now the honour of describing was also procured from the same source. Unfortunately the quantity sent was almost infinitesimal, and yielded only two valves. The precise locality from whence this material was obtained is Clark's Cliff, Barbadoes.

*Aulacodiscus superbus*, n. sp., F. K.—Valve with a large central heptagonal depression, processes placed on the margins of the seven cuneate inflations. The heptagonal area marked with large hemispherical elevations, less conspicuous on the remainder of the valve, the surface of which (with the exception of a small central smooth space) is covered with distinct radiant moniliform striæ; striæ about 18 in ·001″; diameter of valve, ·0050″; length of sides of heptagon, ·0033″. Fig. 1.

Clark's Cliff, Barbadoes.

I am indebted to my friend A. Cole, Esq., of Liverpool, for the opportunity of figuring and describing this very beautiful form, who, risking the tender mercies of the Post Office smashers, kindly sent his specimen for my inspection. Under a $\frac{2}{3}$ objective the mulberry-like prominences are very distinct on the heptagonal area, but are less conspicuous as they approach the margin. The puncta are distinctly radiant from the centre to the circumference.

*Stictodiscus Crozierii*, n. sp., F. K.—Valve with numerous irregularly undulating costæ, which become very delicate as they approach the centre, within a short distance of the margin they divide, the spaces between the costæ distinctly punctate, central

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DESCRIPTION OF PLATE XXXVIII.

Fig. 1.—*Aulacodiscus superbus* × 500.
„ 2.—*Stictodiscus Crozierii* × 400.
„ 3.—*Isthmia vitrea* (frustules) × 400.
*a*, Ditto, ends of two frustules, dividing—
„ 4.—*a*, Hexagonal cells of *I. nervosa* × 600.
*b*, „ „ „ × 1500.
„ 5.—*Nitzschia ventricosa*, *a*. f. v. × 400.
„ 6.— „ *decora* × 400.
„ 7.—*Tryblionella conspicua* × 400.

puncta large, scattered, diameter of valve ·0064. Fig. 2. Plentiful in a gathering made by Captain Crozier in the Mauritius, very rare in some scrapings from a Haliotis shell, West Indies.

This very fine and distinct species is easily recognized by the numerous vein-like costæ, which become gradually more delicate as they approach the centre, within a short distance of which they disappear.

*Isthmia? vitrea*, n. sp., F. K.—Frustules trapezoidal, the opposite corners of the ends more or less produced, hyaline, valves oval or suborbicular. Shell scrapings, Sandwich Islands, R. M. Browne, Esq., Liverpool. Fig. 3.

I have placed this curious form in the genus Isthmia with some hesitation, as it wants the characteristic markings of the species belonging to this genus, but the contour of the frustule and mode of self-division resemble the forms belonging to Isthmia more than the species of any other genus with which I am acquainted. I have not been able to detect any markings with the highest power I have access to ($\frac{1}{10}$th immersion by Beck).

As the subject of secondary markings is of some interest, and as I do not remember seeing those on Isthmia noticed in any work on Diatoms, it will not perhaps be out of place here to give a figure of them. The drawing was made from a fine frustule of *I. nervosa* from Vancouver's Island. They are easily seen with a $\frac{1}{4}$ objective and oblique light; with higher powers they are very distinct. Fig. 4, *a*, × 600 diameters; *b*, × 1500 diameters.*

*Nitzschia ventricosa*, m. s., J. L. Palmer.—Frustules linear lanceolate, apices obtuse. Valve with ventral margin convex, dorsum straight or slightly convex, apices very much produced, awn-like, keel submarginal, punctate, puncta reaching to the extremities of the awn-like ends. Striæ faint, distant, about 13 in ·001″. Fig. 5.

Hong Kong, J. L. Palmer, Esq., F.R.C.S., &c., Liverpool. Rio de Janeiro and Bahia, Captain J. Perry, Liverpool.

This species was first observed by Mr. Palmer in his Hong Kong gatherings; and it afterwards occurred in great abundance in a sounding taken by Captain Perry off Rio de Janeiro; it appears to be nearly allied to *Nitzschia rostrata* of Smith. (The figure in the Synopsis is totally unlike the form for which it is intended.) I need scarcely say the median line and central nodule are wholly imaginary, as are also the longitudinal striæ; the latter has the keel nearly marginal and the striæ very fine, about 60 in ·001″.

*Nitzschia decora*, n. sp., F. K.—Valve linear elliptical, some-

* I think if the author of 'The Philosophy of Evolution' will carefully examine the markings on a diatom valve, he will admit that nothing is less probable than that they are produced by vibrations on the surface of the membranes of these shield-bearing organisms.

what deeply constricted at the centre, ends subacute, keel marginal, punctate, length ·0055′, striæ moniliform, distinct, about 36 in ·001″. Fig. 6.

Bahia, in a gathering made by Captain Perry. The species just described is not uncommon in the Bahia gathering. It differs from *N. plana* in the outline of the valve, and also in its much more distinct striæ; it bears some resemblance to *N. oblonga*, L. W. Bailey,* but is much more deeply constricted, the marginal punctæ are also very conspicuous in my species.

*Tryblionella conspicua*, n. sp., F. K.—Valve elliptical, with central constriction, ends broadly rounded; one of the margins punctate, puncta conspicuous, about 12 in ·001″; centre of valve with a longitudinal elevation gradually sloping towards the margins, striæ obsolete. Scrapings from Tredacua shells, West Indies. Fig. 7.

The genera Tryblionella and Nitzschia contain many forms which seem to combine the characteristics of both genera, *Nitzschia? panduriformis* and the species above described might fairly claim to belong to the same genus; both forms possess the longitudinal ridge-like elevation so conspicuous in *T. scutellum*, and which Smith, in the Synopsis, describes as a central depressed line. This error arose from the valve he examined lying with its inner surface exposed. The valve when seen with its outer surface uppermost may be likened to a miniature boat upset. Thanks to the binocular the actual form of most of the Diatomaceæ may now be easily understood.

* See 'Boston Journal of Natural History,' 1862.

## III.—*Nematophycus* or *Prototaxites?*

By William Carruthers, F.R.S.

A sea-weed or a conifer? Few vegetable structures, it has been hitherto thought, are better known than coniferous wood, and the last thing likely to be confounded with it would be the tissues of a sea-weed. The merest tyro in histological botany can have made little progress indeed if he cannot at once distinguish the tissues of a cellular cryptogam from the wood cells of a phanerogam. Yet this is the matter in dispute, as indicated by the title of this note, and it is certain that either Dr. Dawson or I have made so little progress in vegetable histology, as not to know such different structures when they are seen. The first stage in a child's literary education is the acquiring such a knowledge of the alphabet symbols as shall enable him at once to recognize them. The higher flights of reading, composition, and so on, are impossible till the letters are acquired. So in histology, generalizations as to affinities of organisms, made up of particular structures, can only be indulged in by those who know what the tissues are.

This, then, is the real point to be settled as between Dr. Dawson and myself. The mode of occurrence of the fossil has nothing whatever to do with the matter. It is only and entirely a histological question. Dr. Dawson, from the microscopic investigation of prepared sections of a fossil which he says "presents its structures in a perfection unsurpassed by any fossil wood known to me," has determined that it consists of "wood cells, showing spiral fibres and obscure pores." From the microscopic investigation of prepared specimens of the same singularly-preserved fossil I have determined that it consists of "elongated cylindrical cells of two sizes, interwoven irregularly into a felted mass." It is impossible to *reconcile* these two descriptions of the same fossil. The two kinds of tissues observed are as different as vegetable tissues can possibly be, as the following tabular contrast shows:—

| | | |
|---|---|---|
| Woodcells .. .. .. .. .. | = | Large tubes, or elongated cylindrical cells. |
| Fibres .. .. .. .. .. .. | = | Small tubes, or elongated cylindrical cells. |
| Spiral fibres lining the interior of the wood cells .. | = | Hollow tubes external to and fitting together the larger tubes. |
| Wood structure .. .. .. .. | = | Tela contexta. |
| *Prototaxites*, Dawson! .. | = | *Nematophycus*, Carruthers! |

These two very different views of the same object are shown in the drawings published by Dr. Dawson and myself. That I might accurately represent Dr. Dawson I included two facsimiles from his figures in my paper.* These may easily be contrasted with the plates accompanying that paper. Both drawings cannot possibly

* 'Mon. Micr. Journ.,' Oct. 1872, pp. 161 and 167.

be correct; and Dr. Dawson fairly meets the question when he says, "that Mr. Carruthers' figure (Plate XXXII.) is, in my judgment, to a great extent imaginary."

To prevent my opinion of the nature of the structure influencing the artist in his work, I departed from my usual custom of having the plates executed under my direction in the Museum, and gave the specimens to Mr. Blair that he might put on the stone just what he saw. In a letter from Mr. Blair, maintaining the accuracy of drawing and truthfulness of effect of his plates, he says, "As I did not make the drawings at the Museum under your direction, they were uninfluenced by any views you entertained on the subject." Before publication I carefully compared the plates, with the portions of the fossil drawn, and I am satisfied that Mr. Blair has rendered them with singular fidelity and accuracy.*

But the trustworthiness of Mr. Blair's drawings is put beyond doubt from the testimony of skilled and distinguished investigators who have examined this fossil, and to whom I referred at the close of my memoir. Mr. Archer, of Dublin, has again examined the specimens, and published at greater length than before his views regarding them, which are based on the existence of the two kinds of tissue. He thus sums up his estimate:—"If the large tubes showed septa at regular, in place of at very remote intervals, if at all, there would be much to call in mind a 'mass' of a *Cœnogonium*-like character—that is, large filaments running longitudinally, with an intervening hypha-like *tela contexta*, as it were, binding them together."† This and the similar testimonies of Prof. Dickie, of Aberdeen, and Prof. Agardh, of Lund, the first living algologist, who have also examined the specimens, may be nothing to Dr. Dawson, but they will carry conviction to all who are able to estimate evidence as to the accuracy of Mr. Blair's plates, and the correctness of my account of *Nematophycus*.

I regret that Dr. Dawson should feel so much "the tone and manner" of my paper. I endeavoured to avoid whatever would be unnecessarily objectionable to Dr. Dawson, but in a paper which was so completely destructive of his repeatedly published errors I could not expect to please him. Dr. Dawson compelled me to deal with the subject, as he had himself published in an imperfect and unsatisfactory manner opinions I had communicated to him in correspondence, and had then criticized these imperfectly-expressed views and set them aside. In stating at length my views of the structure and affinities of the fossil I used plain expressions, which the botanical reader

* If any reader is sufficiently interested in this matter to examine the specimens, they can at any time be seen, as they form part of the extensive collection of fossil plants prepared for microscopic investigation, belonging to the Botanical Department of the British Museum.

† 'Quart. Journ. Micr. Sc.,' July, 1873, p. 313.

would at once see were justified. I never used assumptions, fair or unfair, of Dr. Dawson's ignorance of the most familiar facts of structural botany to bolster up my arguments. That did not need to be assumed; and nothing is, I hope, more certain to the reader than that whatever were the familiar facts of structural botany known to Dr. Dawson while I was yet a school-boy, the nature of the wood cells of conifers, and the structure of the lower cryptogams were not amongst them, and that he yet remains in ignorance of these "familiar facts."

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## IV.—*On Immersion Objectives of greater Aperture than corresponds to the Maximum possible for Dry Objectives.*

By Assistant-Surgeon J. J. Woodward, U. S. Army.

I fear the readers of the 'Monthly Microscopical Journal' are by this time growing weary of the discussion of the angle of aperture of immersion objectives, which has been going on so long in its pages; but I must beg their indulgence for this second appearance, the question seeming too important to be left in its present unsettled condition.

My article published in the June number was in many respects incomplete, in fact, only touched in the briefest manner (on page 272) upon the optical considerations involved, for I sincerely hoped that with the hints there given, Mr. Wenham would have arrived at the same conclusions that I have done. I should have much preferred that this had been the case, and that a correct explanation of immersion objectives of greater aperture than corresponds to the maximum possible for dry ones should have come from him; for I have a great admiration for Mr. Wenham, whose large services in improvement of the microscope I fully recognize, and I regret to find myself in antagonism to him. I need hardly say that I have no sympathy with the personal aspersions of which he justly complains,* and am sorry that anyone in America should have thought proper to make accusations which I freely say I think as unjust, as they can appear to him. Nevertheless I am obliged to infer from Mr. Wenham's reply in the July number, that I have not made my views perfectly intelligible, and certainly as I understand his, they appear to me to be erroneous in several particulars, so that I feel called upon to explain myself further.

When I wrote my former paper, I understood Mr. Wenham to hold not merely that the angle of the extreme rays of the pencil transmitted through a given objective at a given position of the screw collar would be the same whether the medium in front was

* July number of this Journal, p. 10 and p. 40.

air, water, or balsam, but also to affirm distinctly that it was not possible to construct any objective which should transmit from water or balsam through the front of the objective a pencil of greater aperture than the maximum transmissible from air, which is limited by the refraction from air to crown glass to about 82°.* Notwithstanding the supposition of Mr. Brakey to the contrary,† I had not overlooked the ingenious experiments of Mr. Wenham, referred to in his articles in the January and July numbers, and it seemed strange to me that they did not suggest a broader view of the possibilities of the case than he seems yet inclined to take.

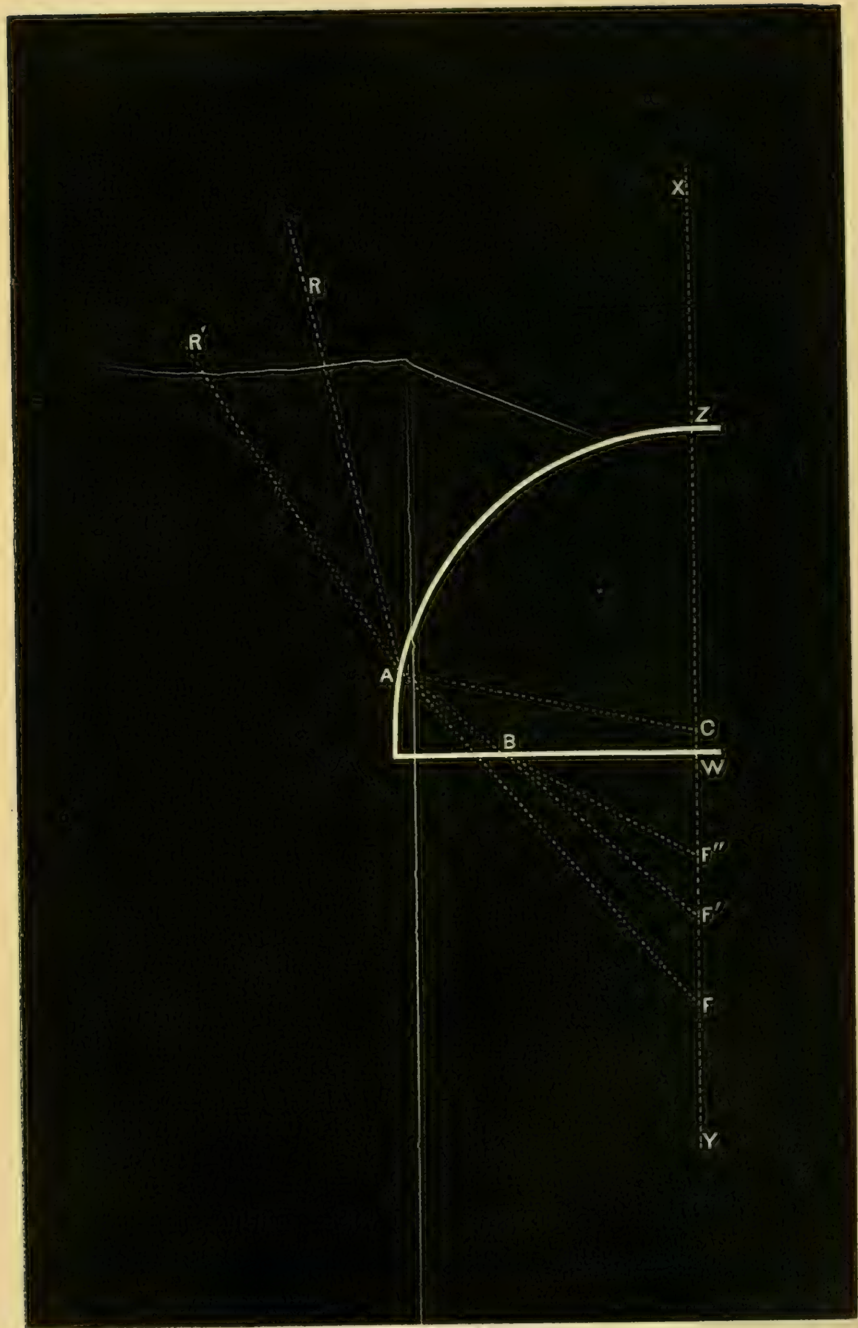
In the present paper I propose to show: first, that there is no theoretical difficulty in the way of the transmission from balsam through the front of an objective of a pencil of 100°; next, that it is practically possible to correct the aberrations of the pencil transmitted backward from a front of this aperture by two posterior combinations only; and, finally, I shall briefly reply to one or two remarks in Mr. Wenham's July paper.

I will begin by discussing the case of the front of an objective made of crown glass of 1·525 index, which shall have the same dimensions as the immersion front for a $\frac{1}{12}$th, figured by Mr. Wenham in this Journal, January, 1871, p. 23, *viz.* radius ·0315″, thickness, ·0340″. I represent one-half the section of such a lens in the figure (on next page) on the same scale as Mr. Wenham's figure, *viz.* fifty times the dimensions above given.

To simplify the discussion, I will suppose the medium in front of the lens to be balsam, with its index reduced by some admixture till (to make use of Mr. Wenham's favourite supposition) it shall have the same refractive power as the crown-glass front. If, then, a luminous pencil of 41° semi-aperture radiate from the point F situated in the optical axis X Y at a distance of ·0264″ from the front surface of the lens, the extreme ray will pass from the balsam into the glass in a straight line without any refraction, until it reaches the posterior hemispherical surface of the lens at a point A, 78° from the central point Z, and will then emerge into the air behind the front, suffering such refraction as to take the course A R, which will form an angle of rather more than 11° (11° 24′) with the optical axis. For if the arc A Z be assumed to be equal to 78°, and C be the centre of curvature, we shall have in the triangle A C F the angle C = 102° by construction, and the angle F = 41° by hypothesis, whence the angle A = 37°. Also in the same triangle, the side A C being the radius of curvature, is ·0315″ by hypothesis; so that we have one side and the angles of the triangle known, and by trigonometry compute the side C F = ·0289″ (very nearly).

* See his remarks on p. 118, vol. v., p. 30, vol. ix., and other places in this Journal.

† August number, p. 98.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Deducting from this the distance C W, which is the difference between the radius and thickness of the lens = ·0025″, we have W F, the distance of the radiant from the front of the lens = ·0264″, as given above.

Now in this case it is evident that the extreme ray F A forms at the point A an angle of 37° with the normal to that point A C, and therefore by the law of sines emerges into air at an angle of 66° 36′ from the normal, or which is the same, 11° 24′ + from the optical axis.

This is very nearly the course given to the extreme rays emerging from the posterior surface of the immersion lens in Mr. Wenham's figure. Not exactly, for I did not know the refractive index of his front, or the exact number of degrees from the central point of its posterior surface, at which the extreme rays were supposed to emerge. The case, however, is a strictly parallel one, and the result is accurate for the data given above. It is, however, easy to show that such a front can transmit a much wider pencil from balsam. In fact, it is only necessary to suppose the luminous point F to be moved nearer to W, so that its new position F′ shall be ·0168″ from the anterior surface of the lens, and it will be found that a pencil of 100° can be transmitted through the lens, the extreme ray on each side emerging into the air posteriorly at the point A as before, and, suffering refraction as it does so, taking the course A R′, which forms an angle of 32° 17′ with the optical axis.

For in the triangle A C F′ the angle C is still 102°, and if the angle F′ be assumed to be 50° (the semi-aperture of the pencil supposed to radiate from F′), the angle A will be 28°, and the side A C being known, C F′ is found trigonometrically to be ·0193″. Subtracting C W as before, we have W F′ = ·0168″, as stated above. Also the extreme ray F′ A forms an angle of 28° with the normal C A, and therefore, by the law of sines, emerges at an angle of 45° 43′ from the normal, or 32° 17′ from the optical axis. If now water be substituted for the balsam in front of the lens, and the radiant point be moved to F″, distant ·0111″ from the front of the lens, a pencil of 122°, radiating from that point to the surface of the lens, will there suffer refraction and pass into the glass, the extreme rays pursuing exactly the same course as in the last case, and emerging into air posteriorly at the same angle, *viz.* 32° 17′ with the optical axis.

Of course the same reasoning will apply to balsam angles between 82° and 100°. For example, if with balsam in front of the lens the radiant were ·0218″ distant, a pencil of 90° would be transmitted, and the extreme ray after emerging at A would form an angle of 21° 51′ with the optical axis; with water in front a pencil of 107° 38′ radiating from a point ·0159″ distant from the front of the lens would pursue the same course.

It could also be shown in the same manner that by moving the points F′ and F″ still nearer to the front, still greater pencils than 100° from balsam, and 122° from water, could be transmitted. I have selected the balsam angle of 100° for the demonstration because I shall presently describe an immersion $\frac{1}{10}$th made by Mr. Tolles, which has this balsam angle.

It seems almost unnecessary to add that the divergence of the extreme rays from the optical axis after their emergence into air behind the front might be diminished somewhat, and the aberrations of the front partly corrected, by substituting a properly constructed triple front for the single one above discussed. This is a mere matter of detail for the consideration of the maker, and does not affect the principle.

Next with regard to the possibility of correcting the aberrations of the divergent pencil transmitted backward from the front of the objective, and of forming a distinct image for the eye-piece when the balsam angle exceeds 82°. This question, I respectfully submit to my friend, Mr. Wenham, is not one which can be decided *a priori* by considerations of optical law. The devices of ingenious workmen are very numerous, and are generally kept secret. Hence mathematical analysis has not yet been applied to the discussion of the question in such a way as to enable us to predict all the possibilities of the case. Under these circumstances, all that we can do is to examine with candour any particular device submitted for our consideration, and to measure experimentally with suitable precautions the results attained.

In this spirit I have examined, since I wrote my first paper, two objectives constructed by Mr. Tolles, in which the balsam angle exceeds 82°, and in which the corrections are effected by two posterior combinations only.

The first is an immersion $\frac{1}{10}$th, specially constructed for the Army Medical Museum. This is an objective of three systems, the front being a single nearly hemispherical lens of crown glass, very similar in form to the one discussed above, and not very different in dimensions. It works as an immersion glass only. Its balsam angle measured by the method described in my last paper is 65° at the open point; at the point of maximum, which is reached before the screw collar is fully closed, it is 87°. I call attention to the fact that by my method the angle measured is that transmitted into *solid* balsam; into balsam "of the same index as crown glass," it would be a trifle greater. Now this objective performs charmingly at every position of the screw collar from the open point to the point of maximum angle, provided the thickness of the cover is exactly suited to the adjustment. Indeed I am compelled to give it the preference, for exquisite definition, over any objective of the same power that ever passed through my hands. At the point of maximum angle it works through covers rather thicker than the $\frac{1}{70}$th of an inch. This capability of working through very thick covers is a great advantage, permitting it to be used on many of the older preparations in the museum, with which no ordinary objective of similar power could be employed. I send herewith a photograph * of *Amphipleura pellucida,* intended to illustrate its defining power at the point of maximum angle. I purposely selected two frustules, which have served me to make many photographs with different objectives, some of which I have sent to London on former occasions. The slide has a cover about $\frac{1}{200}''$ thick, much too thin for the point of maximum angle with the present objective. I therefore laid on top of it, by water contact, a second cover, $\frac{1}{100}''$ thick. The photograph was taken through both covers, with all the resulting disadvantages of the additional surfaces; and the comparatively high magnifying power (1380 diameters very nearly) was obtained by distance only, no eye-piece being used. I beg you to show this photograph to any microscopists interested, and should like to ask those who examine it, whether they ever saw Amphipleura as well defined by a $\frac{1}{10}$th.

The second objective is an immersion $\frac{1}{10}$th, recently sent me for examination by Mr. Tolles. Its balsam angle, by my method, is 87° at the open point; over 100° when the screw collar is fully closed. This also is an objective of three systems only, but the front is a compound one. It works only through very thin covers, at the maximum rather less than $\frac{1}{200}''$ thick. I do not think its defining power equal to that of the $\frac{1}{10}$th last described; nevertheless, at the point of maximum angle it resolves *Amphipleura pellucida* quite as well as most of the $\frac{1}{10}$ths I have had the fortune to handle. I do not for a moment suppose that this objective is the best that can be made with a balsam angle of 100° and only two posterior combinations. It is, however, good enough to indicate the importance of further work in this direction, while the objective of 87° balsam angle appears to have gained superior defining power by its increased angle.

I have next to say a word with regard to a point in Mr. Wenham's article in the July number, to which I take exception. When he says that in measuring angles of aperture his "custom is (and will be) to set the objective in adjustment on a standard test of known average thickness of cover," I am obliged to think him quite in the wrong. However accurately the angle may be measured for the particular position of cover correction selected, the result can give no notion of the maximum angle with which the objective can be used, and such a method is especially inapplicable to those objectives which have a wide range of cover correction.

* The photographs sent are to be seen at the R. M. Society's Rooms.—Ed. 'M. M. J.'

Take, for example, the case of the single-front $\frac{1}{10}$th belonging to the museum, described above. When corrected for uncovered objects its balsam angle is 65°, and it magnifies 460 diameters at 50 inches distance; when closed to the point at which the Amphipleura picture was made, it magnifies 620 diameters at 50 inches, and its balsam angle is 87°. I hold that it would be as inaccurate to measure the angle at some intermediate point and to call it the maximum angle as to measure the magnifying power at an intermediate point, and call it the maximum magnifying power.

Lastly, with regard to the four-combination $\frac{1}{5}$th, described in my June article. Mr. Wenham asks me to "allow that this additional lens embodies a deviation from the original question, which was to the effect that there would be no loss of angle of aperture of ordinary objectives by the immersion of the front surface in fluids." I can only say that I so fully agree with Mr. Wenham's views on this part of the question, that had I understood nothing more to be in dispute at the time I wrote, I should never have written at all; and if Mr. Wenham will now say that this is all he meant seriously to maintain, and that he admits that a greater balsam angle than 82° can be given to immersion objectives, as I have endeavoured to demonstrate in this paper, it will give me great pleasure to make the admission he desires; but if he still adheres to the contrary opinion, I cannot but hold that the $\frac{1}{5}$th in question is a very excellent example of his error.

This four-combination $\frac{1}{5}$th, by the way, seems to have given great trouble to the Rev. S. Leslie Brakey. For him, indeed, the unfortunate glass is not an objective at all, but an "optical machine" (Query: Are not all modern objectives optical machines?), and after giving his notions of what a real objective is, he states, quite confidently, that "here the fourth 'system' is not a system in this sense at all. It is placed in front of an already perfect object-glass," &c.

I can only say to Mr. Brakey, that in this matter his thorough misconception of the bearings of optical law on the question in dispute has led him into the domain of the imagination (as well as into the world of Greek poetry, with which he seems even more familiar than with optics. This Journal, August, 1873, p. 99).

In point of fact the front of the $\frac{1}{5}$th in question is as much a "system" as the single front of any other modern objective with three systems. With the front at 12 inches distance from micrometer to screen the objective magnified without an eye-piece, 60 diameters at the open point, 75 diameters when fully closed. Without the front at the same distance it magnifies only 39 diameters at the open point, and 42 when fully closed. With the front it works wet and defines very well, without the front it works dry and does not define very well. I have thought it worth while to take two pictures of *Navicula Lyra* with the objective, prints of which I send you herewith. Both were taken with the same eye-piece and distance, simply the front was removed before making the second negative. The first is magnified 765 diameters, the second 450. I have sent copies of them to Mr. Brakey, who may perhaps learn from this lesson to be more cautious in describing objectives he has not seen.

I may say that since I first described this objective, Mr. Tolles has somewhat modified its front, so that now, with sensibly the same balsam angle, it nevertheless defines still better than before, and has a greater working distance. He has also sent me two immersion $\frac{1}{6}$ths, constructed on the same general plan, which deserves a moment's mention. In each of these objectives the front and second systems are fastened permanently together, and remain stationary while the two posterior systems are moved backward and forward by the screw collar. One of these glasses has a maximum balsam angle of 95°, the other of 92°; both define *Amphipleura pellucida* in a very satisfactory manner, and the latter has the capability, when properly adjusted, of working, with good definition through covers rather thicker than the $\frac{1}{75}$th of an inch.

I will conclude this paper by mentioning that I notice in a recent number of Max Schultze's 'Archiv für Mikroskopische Anatomie' (Bd. ix., Heft 3), a preliminary paper on the theory of the microscope, by Professor E. Abbe, of Jena, in which he also claims that it is possible in properly-constructed immersion lenses to correct the spherical aberration for greater angles of aperture than those corresponding to the geometrical maximum for dry objectives. He promises a more detailed paper, giving the grounds for his statements in the 'Jenaischen Zeitschrift für Medicin und Naturwissenschaft,' Bd. viii. This, however, I have not yet seen, but refer to it as of probable interest.

War Department, Surgeon-General's Office,
Washington, D.C., *September* 29, 1873.

## V.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S.

Plates XXXIX and XL.

### 9. Sphagnum rigidum Schimper.

Torfmoose, p. 65, Tab. XVIII. (1858).

Plate XXXIX.

Syn.—Schimper Synop. p. 678 (1860). Lindberg Torfmos. No. 8 (1862). Russow Torfm. p. 77 (1865). Milde Bryol. Siles. p. 390 (1869). *Sph. compactum β rigidum* Nees & Hnsch. Bry. Germ. I, p. 14, Tab. II, fig. 5* (1823). Bridel Bry. Univ. I, p. 17 (1826). C. Müller, Syn. I, p. 99 (1849). *Sph. ambiguum* Hübener Musc. Germ. p. 25 (1833). *Sph. tristichum* Schultz in Bot. Zeit. (1826). *Sph. immersum* Nees & Hornsch. Bry. Germ. I, p. 11, Tab. II, fig. 4. *Sph. strictum* Sulliv. Musc. Allegh. No. 201 (1845).

*Monoicous. Tufts dense, rigid when dry, glaucous-green above, whitish below;* stems erect, dark brown or blackish, 3–10 inch high, densely ramulose, usually 2, sometimes 3 or 4-partite; cortical cells small, non-porose, in 2–3 strata. Branches 3–4 in a fascicle, short, 1–2 erecto-patent, obtuse, the others deflexed, slender, flagelliform, lax-leaved, cortical cells longer, the porose scarcely distinct from the rest. *Cauline leaves minute, erect, inserted obliquely, broadly ovate, auricled, the apex rounded, erose;* areolation lax and rhomboidal in the middle, with a broad border of thin, narrow cells, without fibres or pores, or with a few fibres in the basal cells. *Leaves of the divergent branches quinquefarious,* from an ascending base, erecto-patent, *rigid when dry, ovate-oblong, very concave, and somewhat cucullate at apex, but when flattened out, more or less truncate and denticulate, the margin inflexed; hyaline cells confluent above and below, wide, reticulose-fibred, with many unequal pores, the marginal narrow, in two rows, the outermost of which has a longitudinal furrow; chlorophyll cells compressed nearly central.*

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### EXPLANATION OF PLATE XXXIX.

Sphagnum rigidum.

*a.*—Plant of the typical form.
1.—Part of stem with a branch fascicle.
3.—Fruit and perichætium. 4.—Bract from same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Leaves from middle of a divergent branch.
6 *a a.*—Areolation of apex of same expanded. 6 *x.*—Transverse section. 6 *p.*—Point of same. 6 *c.* — Cell from middle × 200. 6 *r.* — Reticulation at back × 400.
7.—Intermediate leaf from base of a divergent branch.
8.—Leaf from a pendent branch with an antheridium.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.
*β.*—Var. compactum.
*γ.*—Var. squarrosum.


Sph. rigidum

Leaves of the pendent branches elongated, narrower and more distant.

*Antheridia borne on the pendent branches, not numerous,* yellow when empty. Perichætia gemmiform, somewhat curved, not expanding, *bracts ovate and oblongo-lanceolate, subfalcate above, the cells with fibres and minute pores as in the branch leaves.* Capsule rather small, immersed or on a short peduncle. Spores ochraceous.

Var. $\beta$ *compactum.*

*Sph. compactum*, De Candolle, Fl. Franc. I, p. 443 (1805). Bridel Sp. Musc. p. 18 (1806).; Mantissa, p. 3 (1819). Bry. Un. I, p. 16 (1826). Schwaeg. Supp. I, P. 1, p. 12, t. 3 (1811). Funck Moostasch. p. 4, t. 2 (1821). Nees, Hornsch & St. Bry. Germ. I, p. 13, Tab. II, fig. 5 (1823). C. Müll. Synop. I, p. 98 (1849). Wilson Bry. Brit. p. 18, Tab. LXI (1855). Berkl. Handb. Br. Mosses, p. 306, Pl. II, fig. 2 (1863). *Sph. Helveticum* Schkuhr Deutsch. Moos, p. 12, t. 3 (1810). *Sph. obtusifolium β minus* Hook. & Tayl. Musc. Br. p. 3 (1818). *Sph. præmorsum* Zenker & Dietr. Musc. Thuring. Fasc. I, No. 18 (1821). *Sph. condensatum* Schleicher, Pl. Crypt. Helv. (1807). *Sph. cymbifolium β condensatum* Weber & Mohr, Bot. Tasch. p. 73 (1807). Roehling, Deutschl. Fl. III, p. 35 (1813).

Plants short, ½ to 2 inches high, in dense cushioned tufts; branches densely crowded, erect, short, thick and compressed. Colour pale rufescent, dirty white or pale green variegated with rufous; branch leaves rounded at apex. Capsules immersed.

Var. $\gamma$ *squarrosum.* Russow.

*Sph. humile* Schimper in Sullivt. Mosses of United St. p. 11 (1856).

Plants forming looser tufts, with more distant fascicles of branches, the divergent branches with loose squarrose leaves.

Hab.—Marshy heaths and moorlands. $\beta$, in drier places. $\gamma$, in south and central Europe. Fr. July.

Although the form *compactum* has usually been cited as the species, we must certainly regard as the type, the plant in its highest form of development, and this attains a height of nearly a foot, especially too since all the Sphagna have a corresponding compact form; and moreover Bridel describes his *S. compactum*, as having "filiform deflexed branches," which is doubtless the reason why Prof. Schimper regards it as partly including *S. cymbifolium.*

The Var. $\beta$ is most frequent with us, especially in the south, and is rarely found with fruit; in its smallest forms we sometimes find that the ordinary cauline leaves are absent, and in their place we have fibro-porose leaves like those of the branches.

The species is remarkable for the cell structure of the perichætial leaves being identical with that of the branch leaves, and also for the position of the antheridia, which are not as usual in amentula, but on the pendent branches, and thus were long overlooked.

Berggren, on Hunneborg mountain by Lindberg, and in various places in Westphalia, Silesia, and Holland.

In this country it was first found in 1853 by my friend Mr. Anderson at Darnholme near Whitby, Yorkshire, but not then determined, and the Rev. J. F. Crouch again collected it there in fine fruit in August, 1871. It has also been found on Ben Lawers by MacKinlay, and on Brickhill Heath, Bucks (Rev. J. F. Crouch), and probably occurs in other localities, but has been overlooked.

The two plants here brought together have been regarded by Müller, Schimper, and Hampe as distinct species, and Sullivant also describes *S. Mülleri* as monoicous, and *S. molle* as dioicous; Lindberg in 1863 published his opinion that they really belonged to one species, and of this there cannot be the slightest doubt, since they perfectly agree in structure as well as in external appearance. To Lindberg also we are indebted for the discovery of the male inflorescence, which I have not succeeded in finding. The name *molluscoïdes* though earliest in date is not retained for the variety, being ungrammatical in construction, and inapplicable since *molluscum* has ceased to appear as a species; it has, moreover, a very slight likeness to *S. tenellum*, but a considerable resemblance to some forms of *S. rigidum*, though very different in texture. It may be of interest to notice that *Sph. Austini* may now claim a place in the British Flora, Dr. Moore having collected it in the Island of Lewis in 1868, where it grows in great elevated hassocks. The plants are taller, and more densely clothed with branches than either the American or Scandinavian specimens.

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## EXPLANATION OF PLATE XL.

Sphagnum molle.

*a.*—Plant from Darnholme.
1.—Part of stem with branch fascicle and fruit.
2.—Male inflorescence after Sullivant's figure.
4′.—Perichætial bract after Sullivant's figure.
4.—Ditto from British plant. 4 *p.*—Point of same.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of basal wing.
6.—Leaf from middle of a divergent branch. 6 *p.*—Point of same. 6 *x.*—Transverse section. 6 *c.*—Cell from middle × 200.
7.—Basal intermediate leaf.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

## VI.—*On the Investigation of Microscopic Forms by means of the Images which they furnish of External Objects, with some Practical Applications.** By Prof. O. N. Rood, of Troy, N.Y.

[We have been requested by Professor Smith to publish the following letter, which, notwithstanding its date, we do with great pleasure, as it is of especial interest at the present moment.—Ed. 'M. M. J.']

It would hardly occur to a physicist, who was requested to determine whether a certain disk of glass was a convex or a concave lens of slight curvature, to attempt a solution of the question by glancing along the two sides; on the other hand, neglecting even to look at the glass, he would at once bestow his undivided attention upon the *images* of external objects formed by it, and thus with ease and certainty decide upon the nature, degree, and regularity of its curvature.

The simple idea here enunciated seems hardly to have been applied to the study of microscopic forms, though from some experiments lately made in this direction, I am firmly convinced that this method of determination is destined hereafter to play a most important part in microscopic observation. To the microscopist it will prove as powerful a means of investigation as it now is in the hands of the optician.

The most convenient and effective mode of proceeding in this case which has occurred to me is the following: the microscope is brought into a horizontal position, the mirror removed, and the illumination supplied by a candle or lamp placed in the axis of the compound body at a distance of not more than 3 inches from the stage. If now a small sphere of glass be properly supported on the stage, it forms behind itself a very minute inverted image of the flame of the candle; upon drawing back the compound body slightly, this image comes into focus, and is seen of course in an erect position. When a rod of $\frac{1}{10}$th of an inch in thickness is moved up and down between the flame and the globule, an image of it is seen in the microscope with great distinctness, and it is observed that the motion of this image follows in all respects the motion of the hand. Upon replacing the sphere by a minute concave lens, as an air-bubble in water, the reverse takes place; to gain distinct vision of the flame it becomes necessary to move the compound body within the focus, the image of the flame is seen to be inverted, and what is practically more important, the motion of the rod seems reversed. It will happen very generally in applying

* See some remarks by Mr. Charles Stewart, F.L.S., in vol. viii., p. 281, of 'M. M. J.'

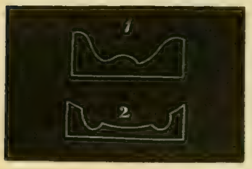
this method that the image of the flame is not sufficiently perfect to decide whether it is erect or inverted; the *motion of the rod* then furnishes us with a certain means of deciding this point; if its motion is natural the image is erect and the curvature convex, &c. After some practice it becomes easy to obtain the best focal adjustment for distinct vision of the rod, and in extreme cases, where the image is very badly defined, the focal adjustment is best made while the rod is in gentle motion. I now adduce one or two applications of this method.

### *Examination of the Nature of the Markings on the Coscinodiscus triceratium, &c.*

It is well known among microscopists that the controversy regarding the nature of the marking on these shells, after being carried on for several years with spirit, cannot even yet be considered as settled, one party contending that the areolæ are depressions, while their antagonists see them as elevations. Compare 'Carpenter on the Microscope,' page 280, American edition.

Fine specimens of these shells mounted in water were examined by a power of from 600 to 800 diameters; on moving the compound body within the focus, each hexagon was found to contain a small distinct image of the flame, the motion of the rod showed that the images were inverted, and consequently formed by *concave lenses.* As the index of the refraction of water is much less than that of silica, its effect is merely to diminish the action of the curved surfaces, but in no case to reverse it. These shells were now mounted in Canada balsam and observed. As the index of refraction of the balsam is somewhat greater than that of silica, it was to be expected that in the compound lenses of silica and balsam the latter would predominate and reverse the action, so as to present effects due to convex lenses. This was found to be the case, and in some of the valves the eye could readily follow in a hundred areolæ at a time, each flickering motion of the flame as it was stirred by the wind. The valves when mounted in balsam of tolu, which has a still higher index of refraction, gave like results. These experiments, which are not difficult to repeat, prove that the areolæ are well-formed *concave* lenses.

A similar mode of experimenting, which must be conducted on large valves and with some delicacy, shows that the border, or setting, so to speak, has the opposite curvature, *viz.* is convex; whether it is convex as a cylinder or beset with several convex markings I have not had leisure to determine, though in some large specimens the latter seemed to be the case. Indications also were observed in some large specimens, that would lead to the deduction of a form optically equivalent to that seen in Fig. 1; and certain allied forms readily furnished the curve seen in Fig. 2, the small depressions being pits.




This mode of experimenting often furnishes us the means of determining whether certain appearances are really due to *openings* or to some other cause; thus the small circles at the middle and ends of the *Pinnularia viridis* have been mistaken by some eminent observers for openings. Prof. Bailey proved by the action of hydrofluoric acid that they are in reality thicker portions of the shell, and examination by the method here described shows that they are convex lenses, giving often very well-defined images of the flame. The dots characterizing the Coniferæ furnish images of the flame indicating two or more curvatures; the ribs of the Pinnularia and the spaces between them have opposite curvatures, &c., but the examples already given may be sufficient to show the usefulness of the proposed method.

### *Index of Refraction of the Silica composing the Valves of the Diatoms.*

This point is closely connected with the foregoing, and it may not be amiss to detail a few experiments that were made to determine it.

Although Canada balsam has the same index of refraction as quartz, still the valves of the diatoms which are composed of silica are seen almost as distinctly in balsam as when mounted in water.

To ascertain the relation between the index of refraction of quartz and Canada balsam, independently of optical tables or laborious experiment, I combined a convex quartz lens of 1 inch focus, cut at right angles to the optic axis, with unheated fluid balsam placed on a glass slide; the two opposite refractions balanced with each other so perfectly that the combination acted like a plate of glass with plane parallel sides, and with ordinary means I was at a loss to discover any tendency to convexity or concavity. Balsam which had been heated was now combined with the quartz lens in the same manner; the balsam proved to have gained in refractive power, so that the combination now acted distinctly as a concave lens of weak curvature.

Diatoms were then mounted in this unheated fluid balsam, in which properly they should have been invisible, owing to the coincidence of refractive indices, but, as had been anticipated, they appeared beautifully, though perversely distinct. A casual remark from Alex. S. Johnson, Esq., concerning a certain chemical difference he had often noticed between ordinary silica and that composing the diatom valve again turned my attention to this point.

Experiments were made upon a sample of the Rappahannock infusorial earth, which had been given to me by Prof. Wm. B. Rogers, in its natural state. By immersing the valves in various liquids, I finally ascertained that in strong sulphuric acid they became either *invisible* or very nearly so, while the grains of sand on the slide retained their distinctness perfectly. It was curious to observe how by diluting the acid with water, the valves again became visible and distinct in outline markings. By igniting this earth I produced a slight change in the index of refraction of the silica composing the valves, so that afterwards they were visible with tolerable distinctness in the same sample of sulphuric acid.

| | | |
|---|---|---|
| Index of refraction of | water .. .. .. .. .. .. | 1·336 |
| ,, ,, | sulph. acid .. .. .. .. .. | 1·435 |
| ,, ,, | diatoms .. .. .. .. .. .. | 1·435 |
| ,, ,, | quartz .. .. .. .. .. .. | 1·548 |
| ,, ,, | Canada balsam .. .. .. | 1·548 |

This Table shows that the index of refraction of the diatoms is about half-way between that of water and Canada balsam, thus explaining the fact that they appear about equally distinct in both of these media.—*Silliman's American Journal*, vol. xxxiii., Jan., 1862.

TROY, *Nov.* 26*th*, 1861.

# NEW BOOKS, WITH SHORT NOTICES.

A Manual of Pathological Histology, to serve as an Introduction to the Study of Morbid Anatomy. By Dr. Eduard Rindfleisch, Professor of Pathological Anatomy in the University of Bonn. Vol. II. Translated by E. Buchanan Baxter, M.D., Lond. The New Sydenham Society, London, 1873.—We are very much indebted to Dr. Baxter for the extreme care he has taken in giving us this translation of Dr. Rindfleisch's two admirable volumes. We have not observed any part of this very difficult text whose rendering into English we can find fault with, and that is saying a great deal; but our praises are due to the editor also for the important alterations which he has had effected in certain portions of the work. Still of course it is to the author that our praises must be principally given, and we think he deserves them in an especial degree for the thorough absence of affectation which he exhibits in his preface, and for that true recognition of the fact that work is perpetually going on, and that his labours of to-day may be considered as old and out of date in a considerably short time. However, we have very great doubts on this latter part of his statement. If his work was merely a compilation, it might not prove so unlikely that a very few years would render it old and unworthy of scientific repute. But it being a book which contains especially the author's own observations and his own reflections (carefully made, most of them), it is, in our opinion, one calculated to live for a long period of time.

It may be said that the matter which these two volumes include might have been easily put into a single volume; and doubtless there are some who would look with approval on such a mode of alteration. For ourselves, however, we must express an opposite opinion. We think the author has done well in expanding his labours, and for this reason, that a book dealing with such a subject is best of an uncompressed style. It is much better to have a fact laid before you in two or three different ways than simply in the first form alone; this is so because you often grasp a thought when it is put before you in a different manner a second time; whereas if it were placed only in the one shape you would be far longer in appreciating the author's meaning. Besides, its style takes away from it the *vade-mecum* character which is now-a-days infinitely too common.

If one were to attempt to review this book at even a fair degree, it could not be done in less than a sheet of printed matter; for the contents are vast and the mode of treatment is, to a certain extent, original. We are sure, therefore, that both author and editor will excuse our very short notice on the plea that it has never been the plan of this Journal to give more than brief references to recent books. This essay covers more than 500 pages, so that readers will not find its study a fact of easy accomplishment, and it is divided into a general and special part. The general part is exceedingly interesting even to the mere scientific reader, and it contains chapters on the Retrograde metamorphosis and degeneration of tissues, and Morbid growths, including inflammation. Under these headings are discussed all the general principles of the science, and an attempt is made to explain the various morbid processes as to their physiological nature, the author being sufficiently honest to admit that many facts are entirely beyond explanation at present, while in other cases he gives explanatory ideas some of which must certainly be regarded as clever, even though they be simply hypotheses. Then comes the more purely pathological part. This occupies half the first volume and all the second one; it deals with the morbid state of the blood and the organs concerned in its renewal, more especially the spleen and lymphatics, the morbid anatomy of the circulatory apparatus, the morbid anatomy of the serous membranes, the morbid anatomy of the skin, the morbid anatomy of the mucous membrane, and similarly, under their different sections, the morbid anatomy of the Lungs, of the Liver, of the Kidneys, of the Ovaries, of the Testicle, of the Mammary gland, of the Prostate, of the Salivary glands, of the Thyroid body, of the Supra-renal capsules, of the Osseous system, of the Nervous system, and finally of the Muscular apparatus.

And under each of these divisions is the subject dealt with as fully and as minutely as possible, thus giving to the student a book which has no equal in the English language, and which—though in an inferior degree—will constitute an admirable companion to the three excellent volumes which have lately been completed of Stricker's 'Histology.' The following brief extract, which we give more for the purpose of showing the author's manner of dealing with the subject than for any especial value we attach to it, is still not without interest. Dr. Rindfleisch is explaining the mode of formation of bony growths, and he says, "I consider myself fully justified in expressing the view founded on the above data derived from normal histology, that peculiarities in the movement of the nutrient juices, and especially a certain retardation, or even stagnation, of their current, which may be assumed as likely in the said localities, owing to the absence of lymphatics, stand in some sort of causal relation to the process of calcification. Should this view be correct, we might conceive the precipitation of the earthy salts to occur in some way like this: the free carbonic acid, to which their solubility is due in consequence of its great diffusive power, forsakes the stagnant nutrient fluid, and escapes from the organism by other channels, while the calcareous salts, rendered insoluble by its removal, are forthwith deposited in a solid form." This quotation amply illustrates the author's tendency, while it is not a bad example of his style also.

The list of works referred to by the author is good, because most of the books are of comparatively recent publication, while—whether we are to thank Dr. Baxter for this we know not—it contains a more ample reference to English workers than is usual in similar German lists. This and the woodcuts—capitally drawn, and over 200 in number—unite with the text in rendering Dr. Baxter's translation an admirable and instructive volume, which every medical student who is worthy of the name should purchase and carefully study.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*The Structure and Regeneration of Nerves.*—We regret that the article on this subject which appeared some time since in the 'Medical Times and Gazette' has not been earlier reproduced in our columns; but assuredly in the case of so important a communication "better late than never" is perfectly excusable. Our knowledge of the minute structure of nerves has been considerably advanced by the recent elaborate researches of Ranvier, who has shown that the description of nerves hitherto given and accepted must now be modified in many particulars. Ranvier undertook three series of investigations—the first two upon the normal histology of the nerve-tubes and their sheaths; and the third in application of the discoveries he had already made, upon the changes which the nerves undergo after section. The results obtained will be given in the same order. The subject of Ranvier's first investigation * was the structure of the nerve-tubes, nerve-fibres, or primitive nerves, as they are variously named. An ordinary medullated peripheral nerve-fibre is composed, as is well known, of a protoplasmic axis cylinder, an insulating "white substance," or medullary sheath, in which the former is imbedded, and a nucleated membrane called the sheath of Schwann, which encloses the whole and gives the nerve the strength and resistance for which it is remarkable. We have hitherto believed that the nerve-tube is uniform in its entire length—no transverse section of it being different from another. The first important discovery made by Ranvier was that this description must be considerably modified; that a medullated nerve is not a uniform elongated structure, but that there occur upon it at regular intervals peculiar annular constrictions, due in part to a complete absence at these situations of the medullary sheath. This remarkable condition Ranvier was first enabled to appreciate by using some of the rarer histological reagents in preparing the specimens, such as picro-carminate of ammonia, perosmic acid, and nitrate of silver; but once the constrictions have been discovered and described, they may now be recognized without difficulty, even in fresh nerves. A medullated nerve-fibre must now be described as built up of segments exactly similar in every respect, arranged end to end, and separated (or united) by annular constrictions where their extremities come into contact with each other. Each segment of the nerve is composed of the three elements just enumerated—the axis cylinder, medullary sheath, and sheath of Schwann,—but here also Ranvier's description differs in some important respects from what was previously given. The Schwannian sheath of each segment is furnished with a single nucleus only, and this nucleus lies exactly in the middle—*i. e.* at an equal distance from the two ends—of the segment, and belongs rather to a delicate layer of protoplasm lining the interior of the Schwannian sheath than to the Schwannian sheath itself. The annular constrictions which the nerve presents, or, as it may be otherwise expressed, the planes by which the segments are united end to end, present the

* 'Archiv. de Phys. Norm. et Path.,' March, 1872.

appearance of clear, highly-refracting biconcave disks, seen in profile and placed across the long axis of the nerve. On careful examination each disk is found to be divided into two symmetrical halves by a transverse line of extreme fineness; either half of the disk belongs to the corresponding nerve-segment, and may be traced uninterruptedly into its Schwannian sheath and the protoplasm by which the same is lined. The septa thus formed between the individual segments are so far complete that, as has been already mentioned, they entirely separate the medullary sheath of neighbouring segments from each other, and make the medullary sheath of a nerve-tube not a continuous but a regularly interrupted covering. The axis cylinders of the segment, on the other hand, are all perfectly continuous; they pass uninterruptedly through a nearly central opening in the intersegmental disk, and thus there is a single unbroken conducting axis of nervous matter in each tube. The length of each segment while constant in a given nerve is decidedly less in a young than in an adult animal—that is, in a growing nerve than in a fully formed one; and Ranvier makes the important observation that newly-developed portions of nerves might thus be recognized in a healing wound.

The function of the annular constrictions in nerves is very evident. The fatty material of which the medullary sheath is composed is not permeable by the nutritive fluids; and it is only through these interruptions in the medullary sheath that the axis cylinder can possibly be nourished.

Ranvier next investigated the histology of the connective tissue around the nerves.* The most interesting points which he made out related to the structure of the sheaths immediately surrounding the primary bundles of nerve-fibres. These primary sheaths are composed of concentric lamellæ of a homogeneous elastic substance, in which bundles of connective tissues are disposed, the whole forming a covering of remarkable strength for the bundle of nerves which is enclosed. This explains the great resistance to suppuration and ulceration which nerves have always been known to possess. However, there is a limit even to this resistance. If the sciatic nerve of a living rabbit is laid bare, and water allowed to fall upon it drop by drop, paralysis of the corresponding muscles will follow in fifteen to eighteen minutes; and if an examination of the nerve be made at once, a remarkable alteration will be found to have taken place upon the fibres within the sheath, for the annular constrictions have disappeared and the whole nerve is swollen, especially the axis cylinder. In forty-eight hours the fibres have completely degenerated. From this observation Ranvier draws the practical conclusion that irrigation of a wound in which nerves are exposed may not be so harmless as is generally supposed.

In his third and last research, Ranvier made a practical application of the knowledge which he had acquired to the investigation of the changes undergone by a nerve after section.† The changes upon the central and peripheral ends of the cut nerve are remarkably different. While the central extremity presents merely a granular degeneration, and its axis cylinder remains uninterrupted, the peri-

* Ibid., July, 1872.

† 'Comptes Rendus,' December 30, 1872, No. 72.

pheral end exhibits inflammatory changes, and the functional elements suffer in a remarkable manner. The nuclei of the inter-annular segments of the surrounding protoplasm increase in size, press upon the parts within, and finally cut through the axis cylinder at the points opposite the nuclei. By careful observation Ranvier discovered that the axis cylinder is interrupted about the end of the third day after section; and it is exceedingly interesting that a complete anatomical explanation should thus be furnished of the fact observed by Longet, that the irritability of a divided nerve is lost from the third to the fourth day. The observation of Ranvier also furnishes an additional proof that the axis cylinder is the conducting element of the nerve. After the fourth day the inflammatory changes on the peripheral extremity of the divided nerve advance rapidly: the myeline of the medullary sheath is reduced to fragments, the nuclei multiply, and the vessels and fine connective tissue around the nerves participate in the change, which is the very opposite of a degenerative one, probably on account of the absence of all nervous control from the section of the nerve on the central side.

*M. Huizinga's Experiments on Abiogenesis.*—This gentleman writing from Gröningen, states that since a communication which was published in March last, a further investigation of the subject has shown him that the experiments then recorded do not yet fully prove the reality of abiogenesis. His argumentation based on those experiments is liable to the following objection:—

The principal experiment (water, potassium-nitrate, magnesium-sulphate, calcium-phosphate, glucose, and peptone) is conducted in a *neutral* solution. In the control-experiments neutral ammonium-tartrate is used as nutritious substance for the supposed germs. But this salt disassociates by boiling, loses ammonia, and the reaction becomes *acid*. When, therefore, bacteria appear in the principal experiment and not in the control-experiments, this result can be explained by admitting that the germs resist a temperature of 100° in a neutral liquid, but are killed by the same temperature in an acid solution. This explanation agrees very satisfactorily with the fact proved by Pasteur, that an acid reaction is much more deleterious to living germs than a neutral reaction at the same temperature.

This objection is very rational, but it does not throw over my conclusion respecting the reality of abiogenesis, for the following reasons:—

It is now obvious that in the control-experiments ammonium-tartrate cannot be used, a nitrogenous body must be sought, not too complex, that remains neutral by 100°. For this end I have found urea to answer well. Pure urea is perfectly fit to furnish nitrogen to the bacteria, but not to furnish them their carbon. Bacteria sown in a solution of urea and mineral salts do not develop themselves, but when sugar is added their growth goes forth rapidly. The following solution—100 c.c. water, 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, 0·04 grm. calcium-phosphate, 1 grm. glucose, 0·5 grm. urea, is eminently fit for the development of bacteria. Also a solution that contains instead of the sugar and the urea, 0·5 grm. peptone.

These solutions were now used in the control-experiments.

For instance :

*a.* Principal experiment. 100 c.c. salt solution,* 2 grms. glucose, 0·3 grm. peptone boiled and treated in the ordinary manner.† On the third day the liquid contains countless swarms of bacteria.

*b.* Control-experiment. 100 c.c. salt solution, 1 grm. glucose, 0·5 grm. urea, boiled exact. No bacteria appear; on the eighth day the liquid is perfectly clear.

*c.* Control-experiment. 100 c.c. salt solution, 0·5 grm. peptone, boiled, &c. On the eighth day complete absence of bacteria.

In each of these experiments the reaction is neutral. They are therefore fully comparable. The experiments *b* and *c* prove, moreover, that the closing tiles exclude completely the atmospheric germs, a fact that was also proved by direct experiments, wherein the solutions *b* and *c* were used and dust strewn on the closing tile in the manner formerly described.

But is it not possible to generate bacteria in a liquid which has been boiled when acid ?

To elucidate this point, the above-named solution *a* was rendered acid (2–4 c.c. of a 1 per cent. solution to 100 c.c.) and treated as usual. No bacteria appeared, whether the liquid was, after boiling, neutralized with soda or not.

But this negative result is easily conceivable ; for the acid alters essentially the calcium-phosphate, changes $CaHPO_4$ into $C_2H_4P_2O_8$. And that this alteration is not without influence, is rendered probable by the fact, which I have recorded in the 'Maandblad voor Natuurwetenshcappen,' No. 7 (April 23, 1873), namely, when in the principal experiment instead of $CaHPO_4$ is used a mixture of $Ca_3P_2O_8$, and $Ca_2H_4P_2O_8$ the result (the genesis of bacteria) is much less constant. The neutral calcium-phosphate by boiling with water breaks up in the basic and the acid salt, but this division must take place in the presence of sugar and peptone. On the other hand, the acid modifies the peptone. This is easily demonstrated by comparing, in the polariscope, the rotating power of a neutral peptone solution with the power of the same solution. After boiling with acid a notable difference is observed.

The acid can, nevertheless, be employed with the following modification :—In 100 c.c. water are dissolved 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, and 2 grms. glucose ; 2 c.c. of a 1 per cent. solution of tartaric acid are added, so that the liquid has a strong acid reaction. It is then boiled for ten minutes. Then with a red-hot platinum spatule a little soda is taken from a hot crucible and thrown in the flask. The quantity of soda required is approximately ascertained by a preliminary trial. Care should be taken not to render the liquid alkaline. Then 0·05 grm. calcium-phosphate and 0·3 grm. peptone are added together, and the boiling continued for ten minutes. The flask is closed as usual, and deposited in the hatching-bath. Three days after, it swarms with bacteria.

* Composed of 1 grm. potassium-nitrate, 1 grm. magnesium-sulphate, 0·2 grm. neutral calcium-phosphate in 500 c.c. water.

† See 'Nature,' vol. vii., p. 380.

When instead of calcium-phosphate and peptone, are added 0·05 grm. calcium-phosphate and 0·5 grm. urea, nothing appears; and the result is equally negative when the following solution is taken:—100 c.c. water, 0·2 grm. potassium-nitrate, 0·2 grm. magnesium-sulphate, 0·05 grm. calcium-phosphate, 1 grm. potassium-natrium-tartrate, 0·3 grm. peptone. In this latter case no acid is used. The addition of the tartrate is made to have a sufficient quantity of carbon in the liquid. These control-experiments prove that none of the employed materials, neither the glucose, nor the calcium-phosphate, nor the peptone, did introduce germs.

By these experiments the above-stated objection is, in my opinion, satisfactorily refuted.

In concluding these remarks, I must mention an important fact. For the above-described experiments, I employed mostly the ordinary glucose, an amorphous, yellowish white mass, not chemically pure. By crystallization from strong alcohol, I purified this sugar. In three different preparations I obtained thus three samples of perfectly white more or less pure glucose. One of these samples yielded, with peptone, bacteria; not so the other two. All three were prepared with the utmost caution respecting atmospheric dust, &c. That, moreover, the positive result could not be caused by an accidental admixture of germs was amply proved by the often-repeated control-experiments. It appears therefore that, besides the glucose and the peptone, a third substance is needed for generating bacteria, a body present in the ordinary glucose (starch sugar), but removed by purification. The nature of this body I have not yet been able to ascertain. But however important, this matter has no direct bearing upon the question of abiogenesis. For that this third unknown body cannot be (as some will probably presume) a germ, my control-experiments and also the above-described experiment, wherein the sugar was boiled with acid, do sufficiently prove.

*The Physiology of Menstruation.*—The 'Medical Times and Gazette' gives a very able leading article, which contains a full account of Herr Kundrat's researches on this point. It says, it is probably the general belief among physiologists and the profession in general that during menstruation one or more ova reach the uterus, and there either become attached to the surface of the mucous membrane or disappear, according as fecundation has occurred or not. If an embryo is developed from the ovum it will correspond with the menstruation immediately preceding—or, in other words, pregnancy will date from the menstruation which last occurred. Dr. Kundrat, of Vienna (Rokitansky's senior assistant), has just published an account of certain researches of his upon the anatomical condition of the uterine mucous membrane before, during, and after menstruation, which throws very grave doubts upon the correctness of this belief.* Kundrat's investigations are all the more worthy of attention that they were of a purely anatomical nature. He examined the mucous membrane of the human uterus in the intervals of menstruation, immediately before the hæmorrhage, during the hæmorrhage,

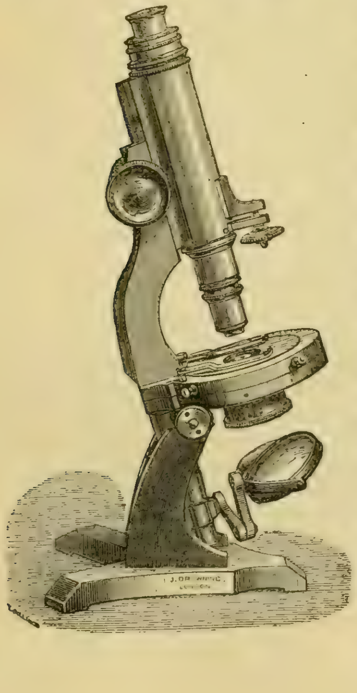
* 'Medizinische Jahrbücher,' 1873, vol. ii., p. 135.

and again after it had ceased, and the results which he obtained are certainly in favour of the considerable modifications which he would introduce into the physiology of ovulation and menstruation as presently received. The mucous membrane of the human uterus in the "state of rest" has certain peculiarities, as pointed out by the author. There is no submucous tissue, and the mucosa comes into immediate union with the muscular layer. Its matrix is peculiarly rich in round or spindle-shaped cells. The glands, which it is known to possess in great numbers, are lined, like the free mucous surface, with ciliated epithelium. This condition is markedly altered at the monthly period of uterine activity. The mucous membrane is swollen, thick, loose, and almost diffluent, covered with a whitish or bloody mucus, finely injected at spots, and in many cases uniformly coloured of a deep red. A microscopical examination reveals increased abundance of the cellular matrix, especially at the surface, with great elongation and dilatation of the glands. So far there is nothing specially original in the description given by Kundrat, but new and important facts remain to be enumerated. He discovered in the first place, that the condition of uterus just described probably precedes the occurrence of the discharge of the ovum and—what is perhaps more striking—the menstrual flow by "several days." The author considers that this observation goes far to prove that the uterus is prepared for the reception of the ovum a certain time before the rupture of the Graäfian vesicle. Again, while the rough characters remain as described during the menstrual flow, with the addition of the oozing from the surface, and for a short time after it has ceased, careful examination reveals a very remarkable change in the microscopic appearances. The cells of the stroma and the vessels, as well as of the epithelium of the glands and surface, are dull in appearance and filled with fat-granules. The question occurs, What is the relation of the hæmorrhage to this fatty degeneration of the cells and vessels? Kundrat replies by stating his belief that the hæmorrhage does not cause the fatty change, but is caused by it. He refers to the fatty change which is known to occur at the end of pregnancy, and would consider the two phenomena homologous. He also points out the improbability of the cause of the flow being found in congestion, as this occurs so frequently without hæmorrhage. One fact he has ascertained is that the fatty change is most abundant at the surface of the mucosa, where the bleeding takes place. The anatomical sequence of events therefore, according to Kundrat, at the monthly period of uterine activity is—swelling of the mucosa, fatty change in the cells and vessels, vascular rupture, and hæmorrhage. With the blood much altered epithelium is thrown off, but not the whole mucosa, as some believe. It is a short time after the cessation of the menses before the mucous membrane has returned to its "condition of rest."

In inquiring now into the physiological relations of the three processes—the swelling of the mucosa, the discharge of the ovum, and the flow of menstrual blood—Kundrat insists strongly upon the ascertained chronology of the events. The first mentioned of the three is the first in order of time, and it is almost certainly the preparation for the reception of the ovum. It is much more improbable that the uterus during the menstrual flow is in a condition suitable for this function—with a retrogressive process going on in the mucosa, its vessels ruptured, and its surface discharging blood. It is even more improbable that the mucosa in this state of degeneration will on the descent of an ovum take on a totally opposite process, and become highly developed. The type of the impregnated uterus is seen in the active uterus when the mucosa is swollen and menstruation has not yet commenced. If the bleeding does commence, it is a sign that the ovum has perished, and that the mucosa is returning to its state of rest. Thus we arrive at the highly important conclucion that a developing ovum, or growing embryo, belongs not to a menstrual period just past, but to one just prevented by fecundation. Löwenhorst has already expressed this opinion from a consideration of the clinical aspects of menstruation, and we believe that the method of calculating the duration of pregnancy suggested by the new facts is not altogether a new one among the gynæcologists and practitioners of this country.

## NOTES AND MEMORANDA.

**Browning's New Microscope.**—This instrument, which was exhibited by Mr. Browning, F.R.A.S., at the last soirée of the Royal Society, and which is illustrated in the adjacent cut, promises to afford many advantages to the scientific worker. It is really the adaptation of a well-known foreign plan, to the English microscope. Various schemes have been adopted for the purpose of getting a satisfactory rotating stage, but save in very expensive instruments they have failed through imperfection in the workmanship, so that, the centering not being perfect, objects sometimes almost travelled out of the field of view. With the contrivance adopted in the present instrument this absence of centering is impossible, for by a special adaptation, both body and stage being one piece, they revolve together. Hence, of course, the object is as central in one position as another. There are, to be sure, some slight disadvantages, as, for instance, the interference of the body at one point of the circle, and the disadvantage of a monocular as com-



pared with a binocular instrument, but these are very slight indeed, and we think that considerable credit is due to Mr. Mayall, who induced Mr. Browning to construct the instrument.

**What is the Cutisector?**—It is simply an instrument for taking sections of the living skin, and is likely to prove useful to those who are engaged in the study of skin diseases. It is thus described by Dr. H. S. Purdon, in a letter to the 'Lancet' (September 20th). He says, I think it may be useful to call attention to the little instrument known as the cutisector, the makers of which are Messrs. Tiediman and Co., New York. The cutisector is an extremely handy and convenient instrument for making *fresh* sections in various skin affections; indeed it was invented principally for this purpose by my friend Dr. Henry G. Piffard, of New York, and whose description of it appeared in the 'American Journal of Syphilography and Dermatology,' 1870. By using either spray the pain is very slight, and the ether hardens the tissue, and allows us to thus obtain a better section. Recently Dr. Piffard has further improved the cutisector, one of which he was kind enough to send me. With it I have made many sections from patients for microscopic examination, there being always a plentiful supply of material at the Belfast Hospital for Skin Diseases, especially (not to mention others) in one case of scleroderma, which is a rare disease, I obtained good sections of diseased skin. The cutisector is far over Valentin's knife or thin sections made with a razor, in which case the preparation to be examined with the microscope requires to be first immersed in some hardening solution, usually of chromic acid, or imbedded in wax, before a sufficiently thin section can be obtained. The little incision made by the cutisector heals at once, and if it is inclined to bleed I brush it over with some stypic colloid. Of course any thickness of skin may be obtained, as the blades of the instrument can be closed or separated by a screw as required.

## CORRESPONDENCE.

### Cause of Colour in P. formosum in using the Older Immersion Objective.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—Since I sent you the *audi alteram partem* letter (see last number, pp. 151, 152) complaining of the want of perfect achromatism in the immersion $\frac{1}{16}$th object-glass, I have received the enclosed letter from the makers, which so fully and lucidly explains the cause of it, that I think it would be a dereliction of duty to neglect its immediate publication: more especially as this is probably the only way in which your readers will obtain the information; as it is a plan of Messrs. Powell and Lealand (and a most excellent one it is) never to notice (publicly) any remarks upon their work; but to act on the principle of the proverb, "Do well, and let the world talk."

In fact, if they were to undertake to reply to all the attacks of cavillers, they might spend their whole time in writing; and with no effect whatever, except what is expressively called "raising the porcupine's quills," for microscopists, in general, appear to belong to the *genus irritabile.* But, here is the letter.

"No. 170, Euston Road, London, N.W., *Sept.* 30, 1873.

"Dear Sir,—We can answer the question contained in your letter in the last month's 'Monthly Microscopical Journal,' respecting the colour you see in the *P. formosum* when using the immersion arrangement. The cause is the 'want of achromatism in the object-glass,' and the reason is that the object-glass was not originally made as an immersion lens. Having to remove the *achromatic* combination and substitute a *single* lens, the posterior combinations are not sufficiently *over* corrected for colour to allow that, consequently you have the red rays predominate. In our *immersion* lenses now the formula is altered, and they are as achromatic as the dry. We recommended you to have it done for the reason, that you got a longer focus and enabled you to look at your old objects, which you could not do before. The only disadvantage over the present immersions is the little colour; it makes, as you say, 'a prettier object,' but the picture is not truthful.

"Apologizing for troubling you with this,

"We remain, yours respectfully,

"Powell and Lealand."

From this we learn—

1st. That if we wish to have a *perfect* immersion objective, we must have it regularly constructed, *throughout*, as an immersion one: but,

2nd. If we have one of our old *dry* ones modified into a *wet* one, we must submit to a (very slight) tint of what the poet Milton calls—

"Celestial rosy red, love's proper hue";

and which really makes, as I have said, "a prettier object."

After all, however, I hope to live to see the time when the highest attainable perfection of object-glasses shall be effected without the aid of water, or any other fluid; for though the mode may be ingenious, effective, and mathematical, &c., yet it is a very unpleasant one, to use a thing which will not act (properly) without first having its nose wetted!

In a future letter I purpose stating my own notions of a plan which, possibly, might be a step towards the attainment of this object: meanwhile

I remain, Sir, yours very respectfully,

H. U. Janson.

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## The Schleiden-Link Question.

*To the Editor of the 'Monthly Microscopical Journal.'*

78, King William Street, E.C., *Sept.* 11, 1873.

Sir,—Although I naturally feel reluctant to reply to an assailant, who thinks fit to conceal his name, yet it seems to me only right to call attention to his mode of attack.

The whole matter is made to turn on Schleiden's meaning, and then, by assuming that that distinguished botanist refers only to certain *omissions* in Link's plates, the question is begged most effectually.

In spite of the contrary opinion held by my nameless critic, I contend that Schleiden is only speaking of errors of interpretation either as omissions or commissions, and as such, I venture to think that the question possesses interest at the present time, from the similarity of some of Dr. Pigott's expressions to those made use of by Link.

I am, Sir, your obedient servant,

B. Daydon Jackson.

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## An Error.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *Sept.* 11, 1873.

Sir,—In copying the article from the 'Lens,' in September number, p. 148, you have of course copied the typographical errors. If you will in the next number ask your readers to substitute *lens* and *lenses* for the words "base" and "bases" where they are printed in the paper, you will oblige

Yours respectfully,

Charles Stodder.

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## Overcoming the Difficulty of Working with Immersion Lenses.

*To the Editor of the 'Monthly Microscopical Journal.'*

Liverpool, *Sept* 18, 1873.

Sir,—In this month's number of your magazine Mr. H. U. Janson describes what has been to him, and doubtless to many others, an important practical difficulty in the use of immersion objectives. He says:—"It is a decided objection that the interposed drop of water greatly prevents our judging of the actual distance of the outer lens from the covering glass" over the object. Having hit upon a very simple method of overcoming this difficulty, I will describe it. With a fine camel's-hair pencil put a drop of clean water over the lower lens of the object-glass, taking care to see that it adheres. Screw it gently on the microscope, and rack downwards until, on looking across between the slide and the object-glass, the drop of water appears sufficiently flattened out, and the focal distance of the lens approximated to. Then sit down to the instrument. Apply the left forefinger nail to the upper edge of the slide, immediately in front of the object-glass, and raise it until it is felt to touch. The distance will appear quite considerable. Then take off the finger from the slide, and, looking through the instrument, focus downwards with the fine adjustment, occasionally raising the slide as before with the left forefinger until the object is seen to come into view. In this way, the right and left forefingers acting together, a wonderful certainty is felt that is very pleasant, even when using the immersion $\frac{1}{20}$th. And if the covering glass be too thick to focus through, this is found out at once. I may add that this method answers best when the microscope is considerably inclined.

Yours truly,

JOHN NEWTON.

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## ASSISTANCE TO MICROSCOPISTS BY MR. WENHAM.

*To the Editor of the 'Monthly Microscopical Journal.'*

NEW YORK, U.S.A., *Sept.* 24, 1873.

SIR,—I desire, through your valuable Journal, to express my thanks to Mr. Wenham for the assistance so kindly given to microscopists in its pages. An amateur of humble pretensions, and no long experience in microscopic work, I have derived the greatest advantage from his instructions. I have recently finished a $\frac{1}{8}$th objective upon his formula, without which I should never have thought of attempting anything so difficult; and the result, due to no merit on my part but that of closely and carefully following Mr. Wenham's directions, is so good as to have greatly surprised my microscopic friends, and to repay me well for my labour. And when the fact is taken into consideration, that this is the first and only objective I have ever even tried to make, the value to an amateur of Mr. Wenham's instructions is sufficiently evident.

I trust I have said enough to prove to Mr. Wenham how well his articles in the Monthly are appreciated in America. Perhaps he will permit me to suggest to his consideration, that many of the amateurs here have not much money to spend upon their favourite study, and that in this country the price of objectives is very high. The powers above the $\frac{1}{8}$th are difficult of execution and of limited use; but formulas for one or more of the lower powers would be gladly received. And I should also like much to know how to make an immersion front to my $\frac{1}{8}$th.

In saying this I hope that Mr. Wenham will not be reminded of the famous definition of gratitude; but will believe in the sincerity of our thanks for what he has done for us, even if he, from any reason, declines to do any more.

V.

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## IN RE MR. WENHAM *v.* DR. PIGOTT.

*To the Editor of the 'Monthly Microscopical Journal.'*

LONDON, *Sept.* 16, 1873.

SIR,—The last effusion of Mr. Wenham, as a Vice-President of the Royal Microscopical Society, is one which excites a good deal of attention. It is quite unnecessary to say that it is either unfair or insulting. But a few temperate remarks may not now be unacceptable. We have seen a controversy about the angle of certain glasses extend over a very long period, in which, to say the least of it, after very strong and arrogant language Mr. Wenham has come off second best. I have no doubt the generality of your readers will think it wiser of Dr. Pigott to pursue his own researches, rather than to answer insinuations against his character, which might possibly be better settled elsewhere; at the same time, the peculiar way in which the subject has been treated will be best shown by placing some of Mr. W.'s statements within inverted commas for the reader's delectation:—

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"Dr. Pigott has devised no new method of any utility for deciding such errors, and that his inferences were drawn from an erroneous interpretation of the structure of known test-objects."

To this it may be replied:—

Dr. Pigott not only designed, but repeatedly described, tests not previously employed—the double star test, &c., &c.; and has supplied means of measuring with close approximation the actual amount of error produced in the apparent size of small beads by spherical aberration of the best glasses.

Mr. Wenham was invited to see the various apparatus employed, and to witness experiments therewith, but declined.

The inferences were not drawn, as stated, "from erroneous interpretations of known test-objects." If Mr. Wenham had chosen to see the experiments, he would have been aware that Dr. Pigott did not consider any of the known test-objects capable of affording sufficiently accurate information, and he therefore set to work to devise new ones about which there could be no doubt.

"The colour test is no new feature."

Dr. Pigott did not claim any novelty in the colour test as indicative of good performance. Dr. Goring nearly forty years ago preferred glasses rather under-corrected. But it remained for Dr. Pigott to discover that spherical aberration at present, in adjustable glasses, cannot be destroyed without disturbing the formerly so much valued achromatism. And in the August number of this Journal, he for the first time has shown the colour test may be employed for indicating minute changes in thickness or depth of focus so as to determine planes of position.

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"The mere assertion that there is a certain residuary aberration is unsatisfactory, and seems to have been raised from the region of phantoms, and its shadow-form is the result of a wrong interpretation of structure from illusory beadings."

Mr. Wenham again chooses here to ignore the fact that Dr. Pigott has indicated modes of measuring the amount of aberration, and that his tests were not confined to what Mr. Wenham calls "illusory beadings."

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"These (beadings) Dr. Pigott has great skill in displaying as a reality, enhanced by drawings made by persons who may be clever in ordinary use of the pencil, but clumsy and inaccurate in the delineation of microscopic subjects."

Anyone desirous of truth would surely not complain that when he exhibited "beads as a reality" they should be drawn as such. The beading has been photographed by Dr. Col. Woodward—does Mr. W. think his work clumsy and inaccurate?

Mr. W.'s reiteration that there are no beads in Podura scales is still unsupported by any satisfactory evidence. Dr. Pigott is prepared to demonstrate that the glasses that fail to show them are imperfectly corrected, and that in proportion as the residuary aberration is removed they come out more and more clearly. Mr. Beck's experiment proves the existence of furrows on the under-surface of the scales, but does not touch the question of whether or not there are beads between the two membranes of the scale.

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"*I know not one microscopist of any note who has investigated the subject that believes in him*" (Dr. Pigott).

Can Mr. Wenham mention a single microscopist who has seen Dr. Pigott's experiments and will endorse Mr. W.'s statements concerning them? If Mr. Wenham has in these attacks unalterably committed himself to the whole extent of his knowledge in these difficult researches, and pronounces thus authoritatively Dr. Pigott to be utterly wrong, so much more credit will be due to the latter when his views are finally established. His recent researches on circular solar spectra to test definition opens the whole question anew to those who are willing to search, rather than carp and cavil at what they will not or have not themselves investigated.

FAIR PLAY.

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## RE TURBERVILLE AND THE 'ENGLISH MECHANIC.'

*To the Editor of the 'Monthly Microscopical Journal.'*

SIR,—A large placard having been issued with a recent number of the 'English Mechanic,' in which my name occupies a prominent position, so that it would seem to appear that it was done with my own concurrence, and in which a particular motive is attached to you, *viz.* the refusal to print Mr. Turberville's letters, "because they were favourable to Messrs. Powell and Lealand," I hasten to repudiate all connection with, or responsibility for, this singular use of my name; and I herewith attach a copy of the letter addressed to the Editor of the 'English Mechanic.'

I am, yours sincerely,

G. W. ROYSTON-PIGOTT.

"RE TURBERVILLE.

"*To the Editor of the 'English Mechanic.'*

"SIR,—A large placard having been circulated with your Journal of September 5th, 1873, in which my name prominently figures, you will permit me, with your usual courtesy, to aver that Mr. Turberville has used my name on this placard without my knowledge or consent, and that I protest against such a liberty. I am under the necessity of thus writing, because it would appear at first sight that the placard was issued under my special cognizance and approval.

"I recommended Mr. Turberville to send his views to print; but if the Editor of the 'Microscopical Journal' has refused to publish them, that can be no reason for dragging my name before the public by a placard. There are, indeed, several very interesting points raised in his letter; but I cannot accord my unqualified approval to every opinion, nor in any way be responsible for them, especially as regards the differential merits of the opticians named in these remarkable letters."

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### Nitzschia curvula.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—Some confusion appears to exist respecting the above species. The form in the "Typen Platten" is the *Nitzschia sigma* of Smith, the *N. sigma* of Kützing is *Homeocladia sigmoidea* of Smith. The true *Nitzschia curvula* of the latter author is not a Nitzschia, but a Surirella *S. intermedia* of Professor Lewis.

Yours truly,

F. Kitton.

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## PROCEEDINGS OF SOCIETIES.

### Royal Microscopical Society.

King's College, *October* 1, 1873.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations received since June 4th was read, and the thanks of the meeting were voted to the donors.

The Secretary read a paper by Dr. Maddox, "On an Organism found in Fresh-pond Water." The paper was illustrated by drawings, and will be found printed at page 201.

The thanks of the Society were unanimously voted to Dr. Maddox for his paper.

The President said that on looking over the drawings which accompanied the paper, he thought that one of them seemed to present very strongly the characters of an amœba. He also understood Dr. Maddox to state in his paper that there was no sign of a nucleus in the specimens examined, but he thought that one or two of the drawings seemed to show what looked very like a nucleus. He hoped they should be favoured with some observations upon the subject from gentlemen present who might have given attention to the amœboid forms of life.

The President announced that they were favoured with another paper from Mr. F. Kitton, of Norwich, describing some new species of Diatomaceæ; but as the paper was merely descriptive, and had reference to objects figured, it would be taken as read, and would be printed in the next number of the Journal. He directed the attention of the Fellows to one species of great beauty, which had been named *Aulacodiscus superbus*. The paper will be found at page 205.

The thanks of the meeting were voted to Mr. Kitton for his communication.

The President called upon Mr. Wenham to say a few words to the meeting upon the microscopical effects produced upon glass by the sand-blast process, which was exhibited at the recent meeting of the British Association.

Mr. F. H. Wenham said that before doing so he should like to refer to a matter which was under discussion at the last meeting of the Society with reference to the question whether the "eye spot" in *Coscinodiscus* could really be the effect of a perforation. It would no doubt be remembered that the President was of opinion that no image would be seen unless formed upon something as a screen, and that he had himself undertaken to test the matter by experiment during the vacation. He had done so in a variety of ways, and had come to the conclusion that a real circular perforation, either in an opaque or semi-opaque body, was an unmistakable thing.

The President suggested that a dark spot might in some cases be produced by diffraction.

Mr. Wenham made the following remarks on the appearance of the American sand-blast process under the microscope:—

The pattern shown on this piece of glass was produced by the American "sand-blast" process, in a few seconds. As the appearance of the "greyed" surface under the microscope is quite distinct from that of ordinary ground glass, I bring it before the notice of this Society, as the microscope gives us some insight into the *modus operandi*. It was stated at the late meeting of the British Association, in the discussion that followed the description of the process, that a large crystal of corundum was speedily perforated with ordinary sea-sand and a blast pressure of 300 lbs. per square inch. Corundum is several degrees beyond emery in hardness, approaching near to that of diamond. But it was further stated that under the conditions named diamond itself speedily became worn away. At first sight it appears extraordinary that the hardest known material should quickly be destroyed by one infinitely softer. The microscope indicates that this is caused by the force and velocity of impact; it is not a grinding process at all, but a battering action, similar to that of leaden bullets against a block of granite.

A polished glass surface exposed for an instant to the sand-blast shows an aggregation of points of impact, from which scales of fractured glass have broken away in an irregular radial direction. It appears as if a pellet of glass had been driven in by the collision of the sand, and the wedge-like action thus set up had driven away the surrounding glass. All these spots or indentations, when tested by the polariscope, show a coloured halo round each, proving that the glass surface is under strain and ready to yield to further fracture.

The action, therefore, is not so much due to the hardness of the striking particles as the force and velocity of impact. This is sufficiently great to destroy the cohesion of the surface of the material operated upon. The external layer is carried against the under stratum, and the material is crushed and disintegrated by a portion of its own body.

No one would think of attempting to make any impression on granite with a piece of lead for a cutting tool, but leaden bullets fired from a rifle will speedily perforate a granite block, and the flattened bullets will have a hard coating of granite on the contact side, the débris of the surface that has been disintegrated. This is the action on a large scale; and the appearance and effect is the same on glass on such a minute one that it requires the microscope to demonstrate it.

The President said that there were several specimens of the action of the sand-blast upon glass placed upon the table for inspection; one of them had a hole entirely through it, and upon another there was the perfect pattern of a bit of lace. These effects were the result of well-known dynamical laws, all that was required being hard resistance on the part of the body to be affected, and a high velocity in the moving particles. A yielding substance would damp the force of the concussion, and would not be affected; just the same, in fact, as in catching a cricket ball its force is destroyed by drawing the hands back; and it was also well known that a suspended silk handkerchief would stop a bullet. Just in the same manner the lace, from its yielding nature, protected the glass, which was acted upon in all other parts by the impact of the rapidly-moving grains of sand.

Mr. Peter Gray thought it might be interesting to know that the process was to be seen in action every day still, at the International Exhibition.

Mr. Wenham said that a great deal depended on the pressure employed to cause the blast; with the steam blower the patterns were produced with great rapidity, but at the soirée, when only hand power was employed, hardly any effect was produced.

The President pointed out that the effect produced depended mainly upon the velocity of the impinging body; a tallow candle might be fired from a rifle with sufficient force to perforate a deal board.

Mr. Wenham said that the harder the substance the greater was the pressure required; corundum needed a pressure of 300 lbs. to the square inch.

The President said that this was of course a necessary consequence. If the force were diminished the effect would proportionately cease; if the candle were fired with the tenth part of a charge, instead of penetrating the board it would simply be squashed against it.

Mr. Charles Stewart exhibited under the microscope a beautifully-prepared specimen of a spermatophore of the common squid (*Loligo vulgaris*), and by means of black-board illustrations explained its structure and functions. He also described in the same manner the general arrangement of the generative organs of the male cuttle-fish.

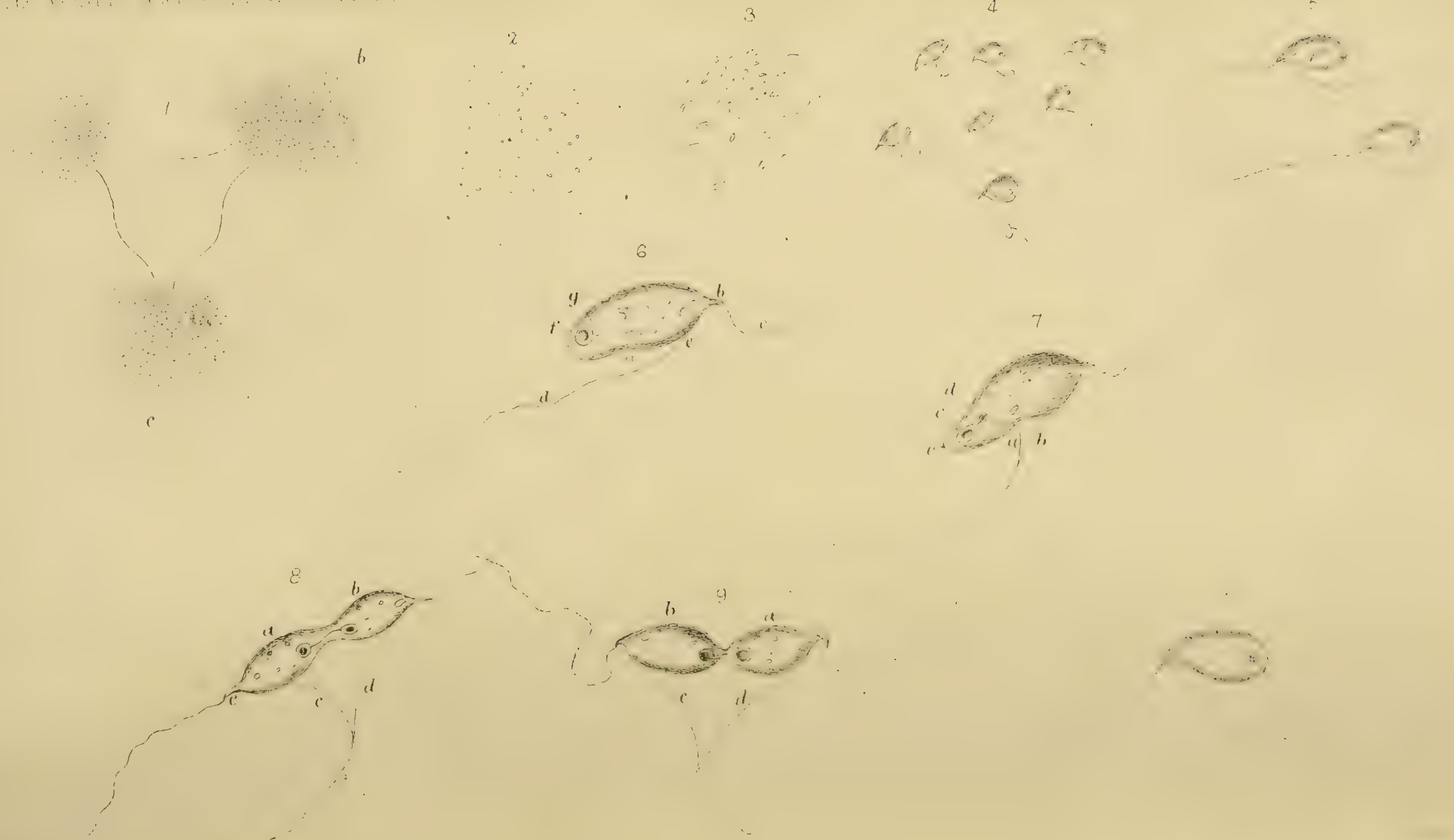
Votes of thanks having been passed to Mr. Stewart and to Mr. Wenham for their communications, the meeting was adjourned to November 5th.

Donations to the Library and Cabinet, from June 4 to October 1, 1873:—

| | From |
|---|---|
| Land and Water. Weekly .. .. .. .. .. .. .. | *The Editor.* |
| Nature. Weekly .. .. .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts' Journal. Weekly .. .. .. .. .. .. | *Society.* |
| Transactions of the Linnean Society, 2 parts .. .. .. .. | *Ditto.* |
| Transactions of the Royal Irish Academy, 6 parts .. .. | *Academy.* |
| Proceedings of the Royal Irish Academy, 8 parts .. .. .. | *Ditto.* |
| A Contribution to the History of the Fresh-water Algæ of North America. By H. C. Wood, jun., M.D. 1873 .. | *Author.* |
| Carcinologiske Bidrag til Norges Fauna. Af G. O. Sars. 1872 .. .. .. .. .. .. .. .. .. .. .. | *L'Université Royale de Christiania.* |
| On Some Remarkable Forms of Animal Life from the Great Deeps off the Norwegian Coast. By G. O. Sars. 1872. | *Ditto.* |
| On the Histology of the Test of the Class Palliobranchiata. By Prof. W. King .. .. .. .. .. .. .. .. .. | *Author.* |
| Quarterly Journal of the Geological Society, No. 115 .. .. | *Society.* |
| Journal of the Linnean Society .. .. .. .. .. .. .. .. | *Ditto.* |
| Bulletin de la Société Botanique de France, 4 parts .. .. | *Ditto.* |
| The Lichen Flora of Great Britain. By the Rev. W. A. Leighton. 2nd edition .. .. .. .. .. .. .. .. | *Author.* |
| Transactions of the Woolhope Field Club, 5 vols. .. .. .. | *Dr. Bull.* |
| Popular Science Review, No. 49 .. .. .. .. .. .. .. | *Editor.* |
| 42 Slides of Type Specimens of American Fresh-water Algæ from the collection of Dr. Wood, mounted for this Society by Mrs. Quimby, and presented by her and Dr. H. C. Wood. | |

Nicholas Henry Martin, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
*Assist. Secretary.*




Researches into the Life-history of monads.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1873.

## I.—*Further Researches into the Life History of the Monads.*

By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D.

(*Read before the* Royal Microscopical Society, *Nov.* 5, 1873.)

Plates XLI., XLII., and XLIII.

In the further pursuit of our inquiries we have succeeded in working out the morphological history of three forms which we believe have been hitherto undescribed.

The striking similarity of form and structure in all the extremely minute monads makes distinction of form almost impossible, whilst the tendency of individuals to vary from the type-form makes it unsafe. But physiologically and morphologically the recurrent cycle of sequence is unerring.

The form to which the following description applies, is found in vast numbers in the putrefying fluid resulting from the maceration of any of the *Gadidæ*; but it rarely appears until the maceration has proceeded for two or three months, and is always yielded most freely by the decomposition of the head.

Its average length is about the $\frac{1}{3000}$th of an inch. In form it is a long oval, inclined to an egg-shape. Its general form is seen in Fig. 6, Pl. XLI., and Fig. 1, Pl. XLII. At one end, generally the narrower, a sharp conical projection is found as in *b*, Fig. 6, Pl. XLI.; in the majority of cases it is curved, and from it a fine flagellum, from one and a half times to twice the length of the body, proceeds. Under this, and at a little distance from it, *e*, Fig. 6, another and longer flagellum arises, and with this the monad anchors itself to the covering glass, and constantly springs backwards and forwards by its recurrent coil and uncoil, reminding the observer of the vorticella, except that the uncoiling is as rapid as the coiling. Fig. 1, Pl. XLII., shows the coiled condition at *a*, the uncoiled state at *c*, while *b* is intermediate. It is possessed of a nucleus-like body, always at the end of the body opposite the proboscis, and a few vacuoles are scattered over the sarcode.

The commonest phenomenon exhibited by this form is its remarkable mode of fission. The first indication that it is about to proceed is given by a slight constriction as at *a*, Fig. 6, Pl. XLI., while an additional globule shows itself beside the nucleus as at *g*, Fig. 1. The constriction gradually becomes more marked, the whole body elongates, and the nucleus-like bodies separate from each other as in *c*, *d*, Fig. 7, while at the same time the flagellum *d*, Fig. 6, appears to split as in *a*, *b*, Fig. 7; and this continues until the lesser "nucleus" is on one side of the constriction and the primitive one is on the other, as seen in *a*, *b*, Fig. 8, meantime by means not clearly made out, a small cone of sarcode is pushed out at *e*, Fig. 7, which lengthens and shortly lashes as a flagellum *e*, Fig. 8, and the fission rapidly proceeds as in Fig. 9, where *a* and *b* have assumed the complete form. They now separate, the single anchoring flagellum *d*, Fig. 6, having completely divided as seen at *c*, *d*, Figs. 9, 10, and immediately on separation each flagellum is anchored, and the phenomenon figured at 1, Pl. XLII., commences; the same cycle speedily repeating itself. This, however, is not the only method, for less frequently the second beak makes its appearance at the front, as in *a*, *b*, Pl. XLII., Fig. 2, and the splitting is *longitudinal*; the anchoring flagellum dividing as in *g*, *h*, Fig. 2, and the whole proceeding gradually, as in Fig. 3, *c*, *d*, the flagellum splits in exact proportion to the sarcode of the body as seen at *i*, *j*; the process of fission completing itself in a manner indistinguishable from the former mode; *e* and *f*, Fig. 4, being complete, with perfect nucleus-like bodies, and the flagella *k*, *l*, dividing to the end, so that each monad is free. These modes of fission may continue for days without the slightest material change presenting itself to the most careful scrutiny with the highest powers; the process of fission occupying in each case six or seven minutes.

But persistent and continuous observation extending over many weeks enabled us to correlate apparently disconnected phenomena, and thus to complete the life cycle.

For years we had been familiar with a triangular form possessing four flagella swimming with tolerable freedom; this form now frequently occurred in the field; whilst the splitting of several apparently nucleated globular masses arrested our attention.

The aspect of one of the latter is seen at Fig. 1, Pl. XLIII.

This one was watched. The flagella *a*, *b*, were moving gracefully, but the body was fixed. After some time, when observing with $\frac{1}{50}$ and No. 3 eye-piece, a small cone of sarcode was pushed out at *c*. This was seen slowly to divide as at *d*, *c*, Fig. 2, and at the same time a diagonal line *c*, *a*, presented itself, and the two globules *d*, *e*, Fig. 1, had taken up a position on opposite sides of this line, as in *b*, *d*, Fig. 2. In thirty minutes this was almost complete, as seen in Fig. 3, beaks having formed apparently by extrusion of sarcode, and the two flagella *c*, *d*, Fig. 3, being nearly perfect, and in something less than four minutes they

separated, and soon became *free.* Thus they were precisely the forms we were working at, but they did not permanently attach themselves.

Four of these were on this occasion followed, and they swam freely until they came to a large group of the ordinary forms as drawn in Fig. 6, Pl. XLI., and in a very short time two of them had fastened themselves against two of those, so that the nuclei were towards each other. One of these was followed; and after several unsuccessful efforts, and considerable perseverance, it was found that the sarcode of the two bodies began to unite; the flagella *c* and *d*, Fig. 4, Pl. XLIII., working freely, and a slight oscillating movement, accompanied by an occasional jerk of the flagella *e*, *f*, continuing for some time. The sarcode began now to rapidly blend, and on the contact of the nucleus-like bodies *g*, *h*, the union was almost instantaneous, passing from two globules into one; while the flagella *e*, *f*, became detached and free, the whole body now swam with great ease as shown in Fig. 5. It began to be roughly triangular and rapidly increased in size, the nucleus stretching itself as the body became larger and more definitely equilateral as in Fig. 6.

This was the form we had so frequently met with but could not explain.

In two hours it had assumed a resting condition, although the flagella moved with a graceful but sluggish motion; but there was no trace of either nucleus or granulation; Fig. 7. In two hours more the flagella had disappeared and there seemed to be considerable lateral distension as in Fig. 8. The $\frac{1}{50}$ and No. 1 eye-piece had been employed throughout, but No. 3 eye-piece was now employed; and the watching continued for three hours more, during which time no changes ensued; but after this, sudden wave-like amœboid movements were seen, convincing us that the form was still living; and in twelve minutes afterwards the two upper apices of the triangle *burst* and there flowed out a dense yellowish glairy fluid which diffused itself rapidly; and was after repeated examinations found to be packed with the minutest dark granules. In a few seconds afterwards the apex *c* also opened in like manner, Fig. 1, Pl. XLI. In cases since observed, the whole of the apices have opened at once; and in one case only one opened, the other two remaining intact.

From these granules, whose minuteness we cannot express, the gradual growth to the parent forms was followed. Fig. 2 represents a field chosen an hour after emission. Fig. 3 shows the same field after three hours; and from this time the growth is more rapid, so that in two hours more the real forms, although small, present themselves as in Fig. 4, the anchoring flagellum being visible, but motionless, and in some cases coiled. The beak also was seen although its accompanying flagellum could not yet be made out. But in four hours from this time the field swarmed with active monads, springing like the parent forms as seen at Fig. 5.

The way in which the flagella first appear in these *germinal* forms we have not discovered: but the proboscis flagellum appears to lengthen by motion up to a certain point; and we have satisfied ourselves that the proboscis is extruded sarcode, being the earliest differentiation of the granule. The nucleus first appears as a black speck and then slowly enlarges.

It remained then for us to discover the relation between the form 1, Pl. XLIII., and the ordinary form, Fig. 6, Pl. XLI.

We did this by constantly watching the behaviour of the forms we had seen germinally develop.

In the vast majority of cases nothing but the fission first described and figured in Pl. XLI. was seen, and it would appear that in the "growing cell" at least, that this process is exhaustive, continuing only in vigour for a certain time, and then becoming weaker, and at last ending in death.

But amongst the mass of anchored forms some few were seen (much larger than the others) which occasionally *detached* themselves and swam slowly, the trailing flagellum flowing gracefully behind; as shown in Fig. 5, Pl. XLII. It was watched with $\frac{1}{50}$ and No. 2 eye-piece. In the course of an hour it became still; but both its flagella were free. An amœboid condition supervened, causing the whole substance of the sarcode to be pushed out to *a* and *b*, Fig. 6, while a large disk *c* is constantly present in this stage, and exhibits an opening and shutting motion like that of the eye-lid, opening at either hand from a median line and *snapping* with great force. In the course of three hours it had passed from an oblong into a rough lozenge-shape, and from that to a disk, the flagella being still attached and waving. Fig. 7. The vacuoles *d*, *e*, gradually condensed into a dark globule, and a small cone of sarcode was pushed out at *c*, while a line from *a* to *b* became shortly visible. What followed this was simply a repetition of what is recorded on a preceding page, and is drawn in Figs. 1 to 8, Pl. XLIII., and Fig. 1, Pl. XLI.

We had thus gathered up the threads and completed the life history. The usual method of multiplication is by fission, which goes on apparently to exhaustion. Amongst enormous numbers there are a few distinguished from the others by a slight increase in size and the power to swim freely. These become still;—for a time amœboid—then round; a small cone of sarcode shoots out, dividing and increasing into another pair of flagella The disk splits—each side becomes possessed of a nuclear body, and two well-formed monads are set free. These swim freely until they attach themselves to an ordinary form that has just completed fission, so that the nuclei are approximate. Sarcode and nuclei melt into each other; the form becomes free-swimming and triangular in shape—rests—loses its flagella; becomes clear and distended: then bursts at the angles, pouring out indescribably minute granules, from which myriads of new forms arise and repeat the cycle.

We have not burdened the reader with our failures and disappointments, but have simply tabulated results.

We have made careful researches on the effects of temperature on the adults and the germs respectively; we think the results of considerable interest: but we can best give them when we have described the other forms, giving the results together.

## II.—*Some Remarks on the Art of Photographing Microscopic Objects.* By Alfred Sanders, M.R.C.S., F.L.S., and F.R.M.S., Lecturer on Comparative Anatomy at the London Hospital Medical College.

(*Read before the* Royal Microscopical Society, *Nov.* 5, 1873.)

It has often struck me as a curious fact that the process of taking microscopic photographs has received so little attention from working anatomists. I think the solution of this enigma is to be found in the immense amount of apparatus which is supposed to be required; to look at Moitessier's * book, or, worse still, at the paper by Dr. Berthold Benecke, in Max Schultze's 'Archiv' †—to contemplate the paraphernalia there set forth, the condensers, achromatic and non-achromatic, the plate of ground glass, and the long array of apparatus, is enough to deter anyone whose time is fully occupied from attempting the art. Other writers seem to require the whole force of a government establishment, a large darkened room, and a heliostat; they speak of employing a practical photographer one or two evenings a month to help them to reproduce all the more interesting of the month's observations, forgetting apparently that it might be necessary to copy fresh objects which would not keep until the photographer happened to be disengaged. I have found it possible to dispense with most of this apparatus, and to do the work with a microscope, an ordinary camera, and a deal or mahogany board.

In the succeeding remarks I do not think that I have anything absolutely new to give; yet there are many little processes, and if I may use the term "wrinkles," which would have saved me a world of trouble if I had been acquainted with them formerly, and which I hope will be of corresponding service to others who may be desirous of acquiring skill in the art; they are not to be found in books, and I have had to learn them by sheer experience. My apparatus is very simple; it consists of a mahogany board four feet in length and ten inches in width, which is made to double up in the centre for convenience in travelling; there is a slit running longitudinally from near one end to within three inches of the other; at the extremity three screws are arranged so as to fix down the microscope square to the board; taking an ordinary bellows camera, I have had the frame which carries the lens separated from that which carries the focussing glass, and fitted to a foot which can be fastened at any part of the board by means of a screw passing through the slit; the focussing frame has been treated in the same manner; the two parts were then connected by

* La photographie appliquée aux recherches micrographiques.
† Dritter Band erstes Heft, 1867.

a treble fold of black calico long enough to reach from one end of the board to the other; this calico bag is kept apart by two rows of rings which run along a couple of brass rods attached one to each upper angle of the focussing frame; the whole is so arranged that the picture from the object under the microscope falls on the centre of the focussing glass, which is made by pouring a very thin solution of starch over a piece of patent plate, and allowing it to dry spontaneously in a horizontal position. When the apparatus is required for use it is placed on a table, the microscope is fixed in its proper position; the body being arranged horizontally is pushed through the opening for the lens in the front frame, and is surrounded by black velvet, so as to make the aperture impervious to light. The focussing frame is fixed at any point on the board according to the magnifying power required; the fine adjustment is moved by means of a rod attached to the side of the board, the further extremity of which carries a small grooved wheel which moves the fine adjustment by means of an elastic band. The only other piece of apparatus required is a small glass cell filled with a solution of alum, which cuts off the heat rays of the sun without in the least diminishing the light. The eye-piece is always taken away from the microscope, as its presence diminishes the light and the definition, the increase in size of the image being obtained by a method of enlargement to be mentioned presently. With the above arrangement the $\frac{1}{8}$th object-glass gives a magnifying power of 350 diameters. The advantage of employing the microscope itself instead of having the object-glass fixed to a special frame, as some recommend, is obvious, for if anything occurs in one's researches a copy of which it would be advantageous to keep, it can be photographed at once (provided the sun shines), with less difficulty than by using the camera lucida. There is a good deal of trouble attendant on getting the focus properly; with the $\frac{1}{5}$th or $\frac{1}{8}$th and higher powers the image looks scarcely more defined on the focussing glass when it is in focus than when it is just out of it; a magnifying glass must be used, a watchmaker's lens, or an ordinary doublet does very well. When the object-glass is just within focus there is to be observed round the external edge of the subject to be photographed a border of white light; as the object-glass is being moved away, this border diminishes in width, and just as it gets out of focus the bright border suddenly changes to a dark one; the moment must be seized when this bright border is on the point of disappearing and before the dark edge is seen; at this point the object is exactly in focus. To get the best effect the adjustment for covered objects must be screwed down, and the thinnest possible covering glass (0·005 inch and less in thickness) must be employed. If this is not done, concentric lines, called interference lines, are apt to surround the subject, spoiling the effect and damaging the negative. I have found that no other light answers so well as sunlight for microscopic photographs; artificial light is a delusion, with perhaps the exception of the electric light, but the trouble and expense of this precludes its employment in a private house, for at least fifty cells would be required. Magnesium ribbon gives an impression, but I have always found it impossible to get a good focus; perhaps if it could be arranged so as to give a steadier light it might answer; Dr. Woodward appears to have succeeded with it. As before mentioned, condensers, ground glass, &c., are unnecessary, at least for the ⅛-inch and lower powers; the ordinary concave mirror attached to every microscope being all that is requisite; but even with the lowest power this mirror should be used, as with the flat one the image of the spots of dust and other extraneous objects comes out with painful distinctness. If the object to be copied is an ordinary microscopic preparation, no especial precautions are necessary; but in cases where fresh tissue examined in fluid is the subject, it is better to paint the edge of the thin glass cover temporarily with gold size to prevent evaporation; this is easily rubbed off after use. If the subject is not very pervious to light, a good plan is to paint the surface of the slide round it with Indian ink, in fact, to stop out all light except that which passes through the object. Hitherto I have spoken only of taking the negative; I now come to the consideration of the best way of printing. It is generally remarked that the former may have all the finest definition that can be desired, but that in the latter the greater part of this distinctness is lost. Now by the process which I am about to describe, prints can be obtained absolutely equal in point of definition with the negatives, and three or four times their size; for instance, if a negative has been taken by the ⅛th objective, doubling it will show all that is seen (being in focus) by that glass with the A eye-piece; trebling its size will show the same as with B eye-piece, and so on; but if the negative has been taken by the 1½-inch objective, magnifying it six times will not make it show what is to be seen by the ⅔rds objective, so that by this process one cannot substitute a lower for a higher objective, but simply compensate for the absence of the different eye-pieces in taking the negative. The method consists simply in printing on a collodion film instead of on paper. Moitessier is the only writer on microscopic photography, that I am aware of, who mentions it. The same apparatus is used for printing in this manner as for the preceding process; the microscope being removed, a short focus photographic lens is screwed into its place; the front frame is then fixed at such a distance from the focussing frame as to give a magnifying power of say three diameters. Now another piece of apparatus comes into use; this is a wooden frame to carry the negative; it works in a groove in a block of wood of such a size as to make the central point of the negative coincide with the central point of the lens; the frame for the negative is kept in place by a spring, and the block can be screwed down at any point of the slit before mentioned. The space between the negative and front of the camera should be covered with a focussing cloth, so that no light should enter the lens except through the negative. To prevent the print being reversed, it is necessary to take the impression through the back of the plate. The apparatus being properly arranged, the whole is turned at an angle towards the sky so as to be clear of trees or other obstructions near the horizon; direct sunlight is not required, and indeed is detrimental to this part of the process, although Moitessier recommends a complicated system of condensers; but these are superfluous when the enlargement required is so small. There are several precautions to be used in preparing the plate to receive the image; in the first place, the collodion must not be too thick, for, if so, it has two disadvantages,—the whites of the image are sure to have a yellowish tinge, and the film is apt to slip off either in the nitrate bath or during the subsequent operations, so that it is better to add a small quantity of ether (ʒ j to ℥ j). In the next place, previously to pouring on the collodion, the plate must be rubbed over by means of a bit of rag, with wax dissolved in ether; care must be taken not to apply too much, for in that case it forms reticulated markings on the film; nor too little, or else the collodion will not come off the glass in the succeeding parts of the operation. The glass plate must be coated as thickly as possible with the thinned collodion, as it will then come off more easily. Having taken the image of the negative and developed it in the usual manner (I find the gelatino iron developer answers extremely well for this process), the next step is the toning; this is best accomplished by means of chloride of gold, which gives a good black; platinum is, I think, not quite so good; other substances may be used, but they do not answer so well; uranium gives an ugly reddish-brown colour; bichloride of mercury, with the subsequent addition of very weak solution of hyposulphite of soda (gr. 1 to ℥ j water), gives a good colour, but is excessively troublesome to use, as the mercury makes the film very rotten; so gold, although expensive, is the best; gr. 1 to ʒ j of water is poured over the collodion positive until the black colour is seen through the back of the plate when held over a dark material, such as velvet. It does not do to hold it up to the light, for then the print may look toned when it is not so. When the above quantity will not tone any longer some more gold must be added, but the remainder need not be thrown away, as it keeps well and will do again another time. The effect of this procesd may be varied according to the subject; if the plate is exposes only just long enough to get an image so that a prolonged development is required, the resulting print will be of a fine black colour; but if a very long exposure is given, and the development correspondingly shortened, the print is softer and has the colour of a lead-pencil drawing, which is better for microscopic objects. The positive having been washed and toned, the next step is to apply to its surface a piece of paper which has previously been coated with a layer of gelatine, about gr. xxv. gelatine to ℥j of water, to which about five drops of glycerine and a trace of chrome alum has been added. The best way to effect this transference of the film is to lay the plate in a dish of clean water, not necessarily distilled; a piece of the prepared paper is soaked in the water until thoroughly wet and then applied to the face of the positive beneath the surface; the latter is then lifted out of the water with the paper on it; this prevents all air-bubbles getting between the two, but if any should chance to find an entrance they must be gently pressed out. The plate having been allowed to get dry, is again soaked in water for a few hours, when the paper may be lifted off with the collodion film attached. Very often the film will come off without previous drying, but it is safer to do so; if it comes off when it is dry, as it sometimes will, we have a print with a highly polished surface, which is no doubt very pretty but not so good in an artistic point of view. If the above directions should be faithfully followed, a print giving all the details of the original negative, and magnified three diameters, will be the result. I will conclude this paper with an account of a mode of transferring negatives whereby they can be carried about by dozens as easily as so many sheets of tissue-paper, and by which means the glass plates can be used over and over again for an indefinite length of time, until they get so much scratched as to be worthless. They can be cleaned between each time of being used, by a strong solution of washing-soda; they should be allowed to soak in this for at least a week. The fluid which forms the tissue, which I believe was first recommended by Mr. Walter Woodbury, is made as follows:—12 gr. of pyroxyline is dissolved in each ounce of a mixture of equal parts of ether and alcohol and 25 drops of castor oil subsequently added; the proportions must be properly arranged within certain limits; if too much castor oil is added the resulting film will be sticky and soft, if too little it will easily break. To use this fluid it is necessary to prepare the negative beforehand, by pouring over it, after it has been sufficiently washed, a very weak solution of gum, one part of ordinary office gum to five or six parts of water; this must be done twice. If too strong a solution is applied the film will crack in all directions; if not enough the image will be dissolved. When the negative is dry it is carefully placed in a horizontal position, by means of a levelling stand, and the liquid is poured over it; this must be done with great circumspection or otherwise air-bubbles are apt to form in the tissue. It is best to use a long-necked bottle and to hold the mouth close to the negative so that none of it shall drop out but shall flow gradually. If a tendency to air-bubbles is shown, the solution had better be thinned with ether; a very small quantity of Canada balsam added to it diminishes the tendency to this defect, but causes reticulations in the film, which, however, do not show in the print. At first the film becomes of an opaque white colour, but it clears after a time and becomes quite transparent; the plate is then put into water and after a few hours the tissue will come off easily, bringing the negative with it. It is not necessary in this part of the process to rub the plate with wax, previously to taking the negative. The convenience of this tissue beyond the facility of carriage is, that if one has two or three negatives of different examples of the same kind of cells for instance, they can be printed together by simply cutting them out of each and sticking them on to a sheet of the tissue by means of gum, when a print of the whole can be taken, and if the negatives are of different densities, the weaker ones can be covered over for a part of the time of exposure. It is often necessary to paint out the background of a negative for this purpose; nothing is better than Bate's black varnish; this works very well with turpentine, and should be applied under a simple microscope of considerable power, or by the compound microscope with the erector, so as to get the edge thoroughly smooth. I hope in the preceding remarks that I have done something towards simplifying the process under consideration, a process which although I imagine not destined to supersede the pencil, yet has such great advantage that it ought to be encouraged. Its advantage may be summed up in one word of great importance to scientific men, who are or ought to be searchers after truth,—that one word is accuracy; whatever is in focus on the slide will reappear in the negative. On the other hand its disadvantages are twofold; one which appears insuperable is that it only shows objects in one plane, the other is that sunshine is necessary. This in a climate like ours is very serious, but it may be overcome by patience and waiting for a fine day; nevertheless it is sufficiently provoking to have one's work interrupted by a sudden overclouding of the sky. Whoever will invent a steady light of great actinic power, which shall be inexpensive, not requiring quarts of acid or a small steam-engine, will confer a benefit on the science of anatomy.

# III.—*Immersed Apertures.*

(*A Reply to Col. Dr. Woodward.*)

By F. H. Wenham, Vice-President R.M.S.

My best thanks are due to Col. Woodward for the handsome way in which he acknowledges in the Journal of last month any services of mine for the improvement of the microscope. I can assure him that it is a real pleasure to discuss a subject with one who writes with such ability and candour, even when we must necessarily differ in our views. I desire the result sought for without prejudice, and will test it by practice as soon as the conditions are satisfactorily determined.

This interminable aperture question commenced *three years* ago, and anyone taking the trouble to track back (I cannot now find time) will see that it began by my controverting a statement that there was *no* loss of aperture by immersing the front of the objective in fluid media. It is needless to review the particulars of my position, which is now practically acknowledged in *that* respect.

After a period of quiescence the controversy has now opened again, and, Col. Woodward's letter requiring notice, I have again to reply. He first *naïvely* says that after some hints given, he sincerely hoped that I would have come to the same conclusion that he has done. For this I do not blame him, having a weakness this way myself.

I cannot think that I have anywhere stated distinctly that it was not possible to construct an object-glass with an immersed angle exceeding 82°; for I wrote on this subject near twenty years ago, demonstrating the loss, and *actually constructed* and described a combination that gave the full aperture with improved definition; it is therefore futile to bring this against me, as I at once concede the full aperture on an immersion system specially designed for the purpose, as that was. The front lens may act either positively or negatively, or be neutral, according to its position.

Col. Woodward now proposes to show that there is "no theoretical difficulty" in obtaining a balsam aperture of 100°, and kindly tenders for enlightenment a diagram to make the way clear, for which I thank him, and having taken it into consideration, let me first apply a sentence that he has made handy for me, thus: "This question I respectfully submit to my friend *Col. Woodward* is not one that can be decided *a priori* by considerations of optical law. The devices of ingenious workmen are generally *kept secret*." The truth of the first part of this statement I will take upon Col. Woodward's own diagram. In the last particular he labours under a disadvantage, perhaps, not shared by myself; for should I venture to demonstrate a principle, I will give the exact dimensions, with the optical conditions upon which it has been carried out practically; for unless this is done, discussion on the points involved is excluded. The facts observed may do credit to the ingenuity of the optician who professes to have accomplished the feat; but the rest is utter darkness, as far as any contribution to the science of optics is concerned.

In speaking of Col. Woodward's diagram, I need not weary my readers with indices of refraction, and angles innumerable, but will take the outside focal point F to be a correct one, that will eventually cause the slight convergence from the back of the series requisite to bring all the rays to the long conjugate focus at the eye-piece, in order to form an image there. To prove his position, Col. Woodward *assumes* another nearer focus F′ with the same objective, and with the ray actually emergent from the same point at the posterior of the front lens, in order to show how the greater angle *can* be obtained. But in this case, if F is considered right, F′ must be wrong. The first position forms a posterior focus—*the second does not,* for the rays will be so divergent that they will be dispersed, and not collected at all, so no image can be formed.

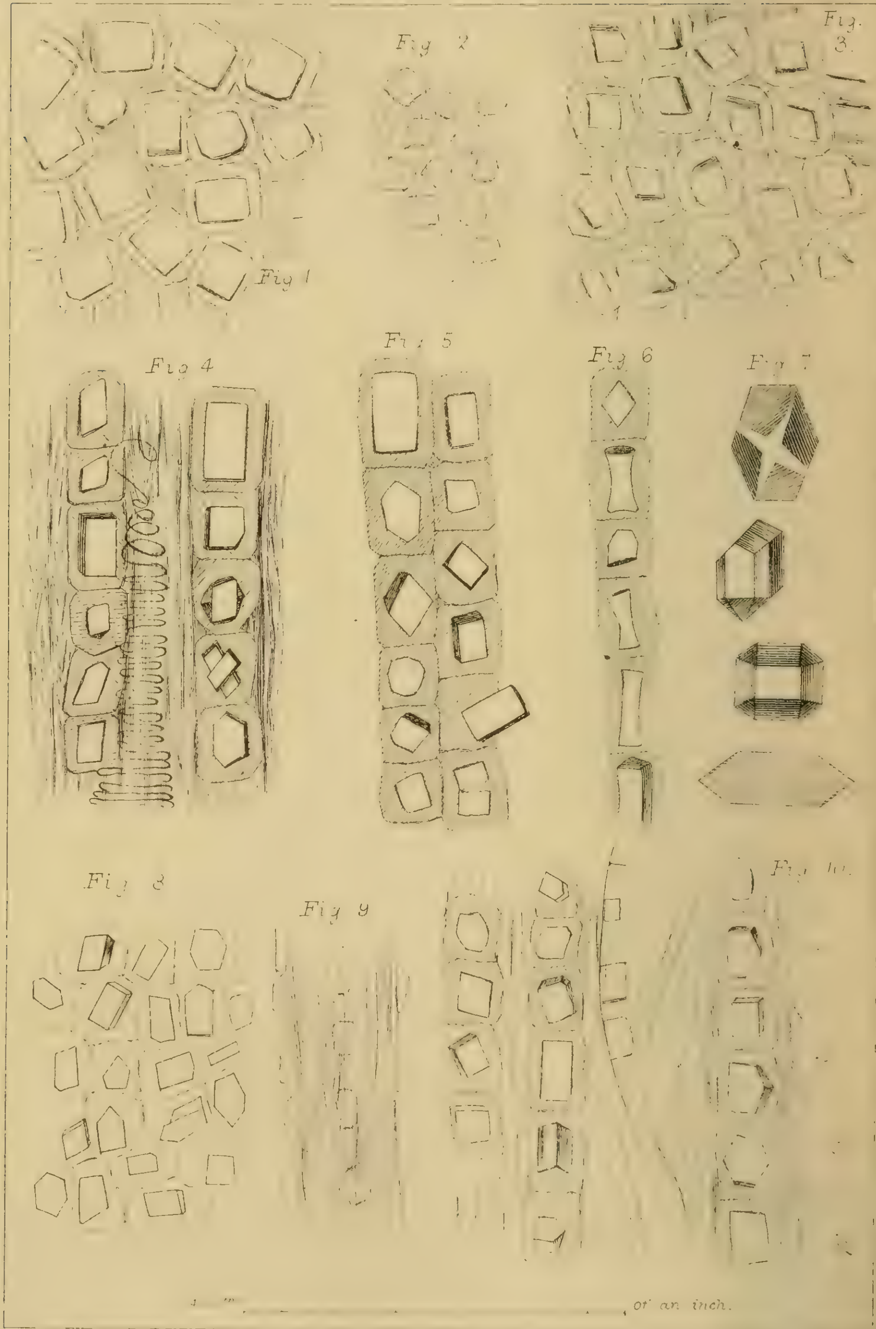
Let us extend Col. Woodward's demonstration, and consider all points from F up to the surface of front lens when in balsam. As you approach nearer, a ray is still transmitted (up to 180° if you please). Where they will all go to scarcely requires consideration. The practicable limit Col. Woodward has yet to demonstrate, for the present diagram is inadequate to prove his position. Can he show us the passage of the rays through one of the object-glasses such as he advocates in a diagram of correctly enlarged dimensions? I shall then have tangible material before me, and will enter upon the consideration with enthusiasm. If this is not in his power, I may, perhaps, help the inquiry by another question. In the external angle that he has given from outer focus F the incidence on front surface is within 41°, or the limits of the total internal reflexion that *must* confine the emergent or internal angle to within 82°. That this inner angle in the body of the front of a *dry* lens *cannot be exceeded* will, I think, not be disputed by anyone at all conversant with optics. Now, rays making the inner angle that he has shown from focus F′, having a more oblique incidence in the material of a dry lens, will be totally reflected back into the body of the glass, rendering such an angle impossible in this case. Therefore, as total reflexion must limit the internal angle to within 82°, like a circular stop, I will ask Col. Woodward if in any of the extra object-glasses he speaks of for obtaining larger immersion apertures he has observed any such limit, *when dry*, to be exceeded when the front is immersed? The capability of taking in a few extra rays may depend upon the form and size of the back lenses. With an additional front, the utmost limit may be secured, as by myself proved a score of years ago. But after all, what admissions am I now expected to make? I first controverted the preposterous assertion, that more aperture was gained by immersion, as the fluid was alleged to admit extra rays, erroneously taken from an assumed radiant point, quite regardless of their ultimate destination; and the further statement, that there was no loss of aperture by using an ordinary object-glass as an immersion.

Suggestions and possibilities of doing this by peculiar means (such as I had already used) never entered into the early phase of the question, or they would then have been met by me.

Concerning the five or more degrees to be obtained in excess of the 82° of an ordinary object-glass, I will say nothing till I have witnessed some actual facts, forewarned by Dr. Woodward's sentence, to be "cautious in describing objectives he has not seen." Extra rays might possibly be accounted for in several ways. At present evidence is required in order to demonstrate their position and value in the micro-objectives under question; till this is laid down I am unable to make any concession on theoretical grounds.

I must finally protest, in consequence of the allusion to the glass forwarded to me for the measurements of the relative air and immersed apertures. Across the Atlantic I have been universally condemned for this, and *without exception* held to be "quite in the wrong." I really cannot see how I am to blame in the matter. The object-glass was not professedly an immersion one, and I had no pretence for altering the adjustment to suit this unnatural condition. Had I tampered with the adjusting collar during the trial, no end of insinuations could have been brought against me for "trickery." I did what I considered right for proving a question of relative loss of aperture in balsam. I should have been quite content to try it, if the adjusting collar had been pinned fast by the senders in any position that they thought gave the desired definition, or to have tried it with one of the Continental $\frac{1}{10}$ths, such as are set to an average thickness of cover, without any adjustment at all; then this unhappy adjustment imputation could not have been raised. I am at length told that the object-glass defines best with a cover $\frac{1}{70}$th thick. Had I been previously informed of this, I certainly would have got a Podura specially mounted with this thickness for the occasion, as nothing of the kind is prepared for sale in this country.

To all these points I have before replied to the same effect. The controversy has been so long and tedious, that it is not a matter of surprise that they should be forgotten.




Professor Gulliver on Plant Crystals

# IV.—*On the Crystals in the Testa and Pericarp of several Orders of Plants, and in other parts of the order Leguminosæ.*

By George Gulliver, F.R.S.

Plate XLIV.

## Sect. I.—Crystals in the Testa and Pericarp.*

*Interest of these Crystals.*—Microscopists have of late been so much interested by the markings on the surface of seeds, that for specimens of them we see many advertisements; and, indeed, these pretty and attractive objects are now familiarly known and much prized for the microscopic cabinet. But their value might be much increased were the examination of them carried a little deeper into the texture of the seed-coat and extended to its immediate coverings; and the present notice is intended to show that the crystals which constantly abound in one or other of these parts in many plants, and are as constantly absent from the same parts in numerous other plants, afford really beautiful microscopic objects, which may prove good characters in systematic botany. Some of them, too, have the advantage of being easily prepared and preserved, as anyone may learn in the gooseberry, elm, black bryony, and the geraniums.

The inquiry concerning the distribution of the crystals may afford, also, additional means of illustrating the life history of plants, still so miserably defective in our books of descriptive botany; and no doubt when these crystals have been sufficiently studied they will supply instructive characters. We may expect that those botanists who will not undertake the inquiry may contemn it by the general and true remark that such crystals occur in numberless plants; but this is no answer to the particular and rational question as to the orders or species which are or are not characterized by certain saline crystals in the fruit or other parts of

EXPLANATION OF PLATE XLIV.

All the objects are drawn to the scale of which each division represents $\frac{1}{4000}$th of an English inch.

Figs. 1, 2, 3, and 7.—Crystals in the pericarp and testa: Fig. 1, in the pericarp of *Geranium Robertianum;* Fig. 2, in the testa of the same; Fig. 3, in the pericarp of *Geranium phæum;* Fig. 7, four crystals from the testa of *Tamus communis.*

Fig. 4.—Crystals in the sutural margin of the pod of *Lathyrus odoratus.*

,, 5.—Crystalline fibres from the leaf of *Mimosa pudica.*

,, 6.—Crystalline fibre from the leaf of *Phaseolus multiflorus.*

,, 8.—Crystalline tissue in the membranous part between the nerves of the calyx of *Trifolium pratense.*

,, 9.—Crystals in the nerve of the same calyx.

,, 10.—Four chains of crystals in the liber of *Mimosa pudica* with a row of five parenchyma-cells to the right.

* Read to the last meeting of the British Association at Bradford.

the plant. Many of the raphides and different forms are figured, after my old and extensive researches, in 'Science Gossip' for May, 1873. In the 'Quarterly Journal of Microscopical Science,' July, 1873, I have given an engraving of the crystals in the testa of the elm; and now is to be added a notice of similar crystals in the same part or its covering of other plants.

*How to find the Crystals.*—These crystals are most easily found in the seed-coat or pericarp, while it is yet somewhat soft and transparent before it acquires hardness and opacity by perfect ripeness. The thinnest possible sections are to be placed in a drop of water or glycerine on the object-plate, and firmly pressed down by the glass cover. Thus they may be examined first with an objective of half an inch focal length, and afterwards with deeper powers from $\frac{1}{8}$th to $\frac{1}{16}$th. This last will probably not work through glass covers of common thinness, but it may act satisfactorily when the focus is lengthened and the power increased by the immersion front. Another and easier plan, often very successful and always useful, is to mash up or comminute minute and thin fragments, by the point of a penknife, in a drop of fluid on the object-plate, by which means, and the aid of needles, some very suitable bits may be so divided and flattened as to show the crystals admirably.

*Distribution and Size of the Crystals.*—They occur regularly and constantly in the testa or pericarp of many plants, as I have witnessed, for example, in the orders Tiliaceæ, Aceraceæ (*Acer campestre* and *A. pseudo-platanus*), Geraniaceæ (Pelargonium, *Geranium phœum*, *G. pyreniacum*, *G. dissectum*, and *G. Robertianum*), Grossulariaceæ (*Ribes grossularia* and *R. rubrum*), Primulaceæ (*Anagallis arvensis*), Ulmaceæ (*Ulmus suberosa*), Dioscoreaceæ (*Tamus communis*), and some others. No doubt numerous additions to this list will be made by future observers. But though in the testa or pericarp of certain species of divers orders these crystals are constantly present, they are as regularly absent from other orders. Thus I have not yet found the like crystals in the testa of Umbelliferæ, Leguminosæ, and many other sections of the British flora; and yet similar crystals abound in the pod and other parts of leguminous plants. The crystals in the testa or ovary of Compositæ I have figured in the number of 'Science Gossip' above cited.

Though the crystals are often plainly seen, they are not always easily found. In the horned poppy they are obscure and only about $\frac{1}{8000}$th of an inch in diameter; and in the maple and sycamore the crystals often occur in isolated patches, or so scantily as to escape notice. The crystals in the gooseberry and the elm are about $\frac{1}{3000}$th of an inch in diameter, and so very distinctly and regularly studded, each within a plain cell, throughout the testa as to present an appearance of crystalline tissues, forming very pretty microscopic spectacles; while in the red currant the crystals are scarcely half the size, and by no means readily distinguishable. In the black bryony (Plate XLIV., Fig. 7) they are beautiful and large, about $\frac{1}{1500}$th of an inch in diameter, thickly set at regular distances throughout the testa. And as this plant, like other Dioscoreaceæ, abounds in true raphides, it affords a good instance of their occurrence with other saline crystals in the very same species. So, too, raphides and long crystal prisms may occur in single plants of certain Pontederaceæ and Liliaceæ; and the short prismatic crystals (Plate XLIV., Figs. 1, 2, and 3) in the fruit of Geraniaceæ are very different forms from the sphæraphides which are so common in the calyx of the same order. Many similar examples to the same effect are recorded in my former memoirs on plant-crystals.

It is hoped that this communication may induce microscopical observers, both neophytes and experts, to pay some attention to a branch of phytotomy which has been too much neglected. The pursuit might prove pleasing and instructive, as well to those who are so frequently inquiring for "good microscopic materials," as to botanists with the higher aim of expounding the life-history and natural characters of the manifold members of the vegetable kingdom.

### Sect. II.—Crystals in Leguminosæ.*

*Name of the Crystals.*—As they mostly belong to one or more of the prismatic systems, and are seldom twice the length of their breadth, we may provisionally call them *short prismatic crystals*, some specific term being needful to distinguish them from raphides, and from the other long or acicular forms which I have always called crystal prisms. It should be borne in mind that raphides, regularly having rounded shafts and tips, have not the figure of prisms; that the long crystal prisms, on the contrary, though often as thin as raphides, have distinct faces and angles; and that the objects now to be described under the name of *short* prismatic crystals are very different in shape from either raphides, long crystal prisms, or sphæraphides.

*How to find the Short Prismatic Crystals.*—This may be done after the manner recommended in Sect. I. Of Leguminous plants the novice may commence his examinations in the leaves of the common white or Dutch clover, or of *Mimosa pudica*, and the young pods of the garden pea (*Pisum sativum*); taking care to look especially at the fibro-vascular bundles, alongside of which the crystals occur abundantly in strings of cells. To facilitate their exposure those bundles may be dissected from the surrounding parts, and then cut or scraped into thin shavings, or mashed into fragments, in a drop

* The substance of this section was orally communicated, with drawings, and extemporaneous demonstrations in the fresh plants, to the East Kent Natural History Society, Oct. 2, 1873.

of water or glycerine on the object-plate. Thus the crystals will be quickly and easily found; and in the fibrous bundles of the sutures of the green pea-pod, they may often be detached by comminution of the part from their seat, so as to be made to roll over and display their forms in the microscopic field of vision. Not so in the tough inner skin of the pod-valve of this plant, among the fibres of which the crystals are thickly studded, and so firmly fixed in and hidden by this dense tissue, as not to be easily seen therein or detached therefrom. But by drying it, and then scraping it with a knife in a drop of turpentine, the texture is made more transparent, and some of the crystals may be found floating freely and separately around; and indeed plant-crystals, often but dimly seen through thin fragments of the tissue in water, occasionally in the dry state become plainly visible when treated with turpentine or oil of cloves. Other means of detaching the crystals from, or exposing them in, their seat will, of course, be tried by the practical phytotomist; and to this end boiling in water or in a strong solution of caustic potass will frequently prove more or less efficacious. The alkali sometimes facilitates the separation or isolation of the tissues or cells, so as to show them very advantageously. For example, by this treatment two layers may be plainly demonstrated in the seed-coat of Tamus; one layer composed of parallel fibres about $\frac{1}{2000}$th of an inch in diameter; and another layer of roundish or polygonal cells, each containing one of the crystals (Plate XLIV., Fig. 7). And of these may be made and easily preserved novel and beautiful microscopic preparations.

*Composition of the Crystals.*—They appear to consist chiefly of oxalate of lime. In none of many trials did the crystals dissolve with effervescence in acids; though the carbonate of that earth is not uncommon as the main constituent of plant-crystals, as I infer from the experiments I have made on the rhombohedral or some such forms in Cactaceæ, and on the sphæraphides which are often either abundant or deficient in the leaves of Urtica. It is well known that the leaves of Bryonia are studded with scabrous tubercles, and to be regretted that of the intimate nature of these no information is given in the books of descriptive botany. Each of these "asperities" or "callous points" is about $\frac{1}{114}$th of an inch in diameter, and composed of many smooth, hyaline, round, or oval granules, the mean size of which is about $\frac{1}{6000}$th of an inch; and they are soluble with brisk effervescence in dilute acids.

*Form, Size, and Situation of the Crystals.*—In the leaves and other parts of Leguminosæ the crystals are much of the same size and shape as already described in the testa of many other orders. But the figure and size are so variable, even in one plant, as to defy precise and intelligible definition. The mean diameter of the crystals is about $\frac{1}{3000}$th of an inch; and in the garden pea they run much larger. As to form, they are both simple and compound; they generally belong to one or other of the prismatic systems, and among them may be seen rhombs, cubes, tetrahedrons, lozenge-shapes, parallelopipeds, and hexagonal prisms. The crystals are often hemiedral or unsymmetrical, and indeed as frequently so irregular in outline as to present it curved or broken, quite unlike that of a regular saline crystal, and resembling a starch granule, or contracted at the sides like a dice-box, or, more rarely, bulging there like a rolling-pin or skittle. Such forms, by the tests of acids and iodine, are easily distinguishable from starch; and none of them are ever so elongated as the objects which, under the name of crystal prisms, I have long since distinguished from raphides. In Leguminosæ the crystals are commonly in strings of cells, with one crystal, rarely two or more, in the centre of each cell; and thus is formed a system of crystalline fibres, running parallel, as already mentioned, to the fibro-vascular bundles (Plate XLIV., Figs. 4, 5, and 6). But not always thus; for in many instances the crystals are regularly dotted throughout a membranous part, as may be well seen, for example, between the nerves of the calyx of Trifolium (Plate XLIV., Fig. 8), and so presenting a pretty form of a crystalline tissue.

*Confusion of Terms and Vagueness of Knowledge.*—So common are minute crystals of one form or other in flowering plants, as to have arrested the attention of the earlier observers; but the knowledge we at present possess of the distribution in the vegetable kingdom of the crystals depicted in Plate XLIV., is still but little in advance of what it was at the time of Schleiden's 'Scientific Botany.' This frequent presence of such crystals in one or other part of numerous widely different orders of plants, and the still further confusion arising from the misuse of terms, has made more difficult the discovery of any rule concerning the occurrence of any special form of crystals in particular parts of the frame of the species of the manifold vegetable genera or orders. All microscopic crystals in them, of what form soever, were confused together, under the name of raphides, up to the advent of my researches; and are still too often so confounded, to the obstruction of botanical science, even by some of the most eminent botanists. Thus, in the latest edition of Henfrey's 'Course of Botany,' the subject is perfunctorily and erroneously treated; and in the recent and much-esteemed 'Treasury of Botany' there is no notice whatever of either crystal prisms or sphæraphides, and only the word raphides occurs, with this definition: "Crystals of various salts formed in the interior of plants by the combination of vegetable acids with alkaline bases." Thus we still have sad work in books of high pretensions; and the more so as there is to be found in older and popular dictionaries, making no point of botany, shorter and more accurate definitions, as may be seen, for example, in Ogilvie's edition, published in 1859, of 'Webster's English Dictionary.' Perhaps some knowledge of the subject may become popular, now that it has been illustrated by figures in 'Science Gossip' for May, 1873. But very little seems to be known about the short prismatic crystals; for, in the last July 'Quarterly Journal of Microscopical Science,' we find Professor McNab, of Dublin, announcing the discovery, in Germany, of "numerous crystals of calcium oxalate in the bracts of Medicago, Trigonella, and Pocockia."

*The Short Prismatic Crystals in Leguminosæ.*—But these crystals, so far from being confined to those plants, and still less to their bracts, commonly occur abundantly in the calyces, leaves, bracts, pods, and other parts of numerous species of the order. I have found the crystals thus in Medicago, Melilotus, Trifolium, Lathyrus, Pisum, Vicia, Onobrychis, Phaseolus, Mimosa, Chorozema, Robinia, and many other members of the same order. In these the crystals were always present; but not so in a few examinations made of some species of a few more genera, including Ulex, Genista, Lotus, and Acacia, in which the crystals were either very scanty or wanting. In the leaves of Wistaria I found sphæraphides with the short prismatic crystals. Thus, so far as these researches have gone, it appears that these last-named crystals are very beautiful, and common, but not universal, in leguminous plants.

*Multitude of these Crystals.*—In the course of these examinations, a remarkable abundance of starch in the trefoils, and in the other leguminous plants which are most relished by ruminant and other animals, was so apparent as to arrest the attention. But the quantity of the short prismatic crystals was a much greater novelty and surprise. In a bit, only $\frac{1}{70}$th of an inch in length, of the midrib of a leaflet of clover, I have counted ten chains, each containing twenty-five of the crystals; and thus, there being 250 of them in view in that $\frac{1}{70}$th of an inch of the midrib, an inch thereof would contain no less than 17,500 of the crystals, without reckoning the number in its branches and in the two other leaflets, or elsewhere. And, by a like observation, no less than 21,000 of the crystals were reckoned in one inch of the sutural margin of a single valve of one pea-pod; so that, multiplying this number by 12, the average length of each of the four separate sutural margins of the full-grown pod being three inches, we have in those sutures alone the amazing number of 252,000 of the crystals!

*Significance of these Crystals.*—Professor Rollestone has somewhere made a remark to the effect that structures which, from their minuteness, or obscurity of function, appear insignificant or useless, may in reality rise in connection with this fact into the more importance. Here we have crystals in cells, organized structures of great beauty, regularity and constancy, and moreover most marvellously numerous in the plant. And will any physiologist now maintain, as often has been maintained, that such structures are mere freaks of nature, of no relation to or value in the life and use of the species? Though we cannot at present see the full meaning, some partial gleams of it may appear, and prove good suggestions for future researches. Probably the earthy salts, stored as we have seen in various parts of the plant, may be needful for the preservation of the fertility of the earth, by being regularly restored to it in the fallen leaves. And when we consider the importance of lime in the economy of animals, we may well admire this one of several sources by which, as we now see, nature has so abundantly provided this earth in that very provender on which many animals greedily feed. Has any chemist ever determined the percentage of lime, and starch and its derivatives, in the leguminous plants used as fodder for ruminant and other animals, and the relation of such constituents to the value of such food? What are their absolute and relative quantities in a truss of clover or saintfoin? Surely questions of this rational sort will have to be solved, sooner or later, in the interest of scientific agriculture. Though the present is but a very fragmentary contribution to the life history of the vast order of leguminous plants, it is novel, and may, when further extended, lead to curious and useful results. We can now perceive some of the significance of these crystals. But why they should be constantly present in certain parts of the structure of one plant or group of plants, and as regularly absent from the same parts of others; why, instead of the form of shapeless precipitates, the lime should occur in crystals within beautifully-organized cells, arranged with exquisite regularity, we can nowise understand. Here science is still in complete darkness, utterly unable to see the cause of these phenomena. And if so as regards such lowly objects, we may derive from them—and their number is legion—lessons of humility, which should not be without use to those philosophers who believe themselves able to unveil, by mere physical inquiries, the mysteries of the highest creation.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*Microscopic Anatomy: the Necessity of its Study.*—In his recent able address to the department of Anatomy and Physiology at the Bradford meeting of the British Association, Dr. Rutherford made some important observations and offered some sound advice to intending microscopists. He considers that it is in a different position from ordinary anatomy. Requiring, as it does, the microscope for its pursuit, it could not make satisfactory progress until this instrument had been brought to some degree of perfection. Doubtless much advantage is still to be derived from improvements in the construction of this instrument; but probably most of the future advances in our knowledge of the structure of the tissues and organs of the body may be expected to result from the application of new methods of preparing the tissues for examination with such microscopes as we now have at our disposal. This expectation naturally arises from what has been accomplished in this direction during the last fifteen years. For example, what valuable information has been gained regarding the structure of such soft tissues as the brain and spinal cord by hardening them with such an agent as chromic acid, in order that these tissues may be cut into thin slices for microscopical study. How greatly has the employment of such pigments as carmine and the aniline dyes facilitated the microscopical recognition of certain elements of the tissues. What a deal we have learned regarding the structure of the capillaries, and the origin of lymphatics, by the effect which nitrate of silver has of rendering distinctly visible the outlines of endothelial cells. What signal service chloride of gold has rendered in tracing the distribution of nerves by the property which it possesses of staining nerve fibrils, and thereby greatly facilitating their recognition amidst the textures. Moreover, of what value osmic acid has been in enabling us to study the structure of the retina. In the hands of Lockhart Clarke, Beale, Recklinghausen, Cohnheim, Stultz, and others, these agents have furnished us with information of infinite value, and those who would advance microscopical anatomy may do so most rapidly by working in the directions indicated by these investigators. In human microscopical anatomy, indeed, there only remain for investigation things which are profoundly difficult, such as, for example, the structure of the brain, the peripheral terminations of nerves, the development of nerve tissue, and other subjects equally recondite. But in the field of comparative anatomy there is far greater scope for the histological investigator. He has only to avail himself of those reagents and methods which have recently proved so useful in the microscopical anatomy of the vertebrates; he has only to apply those more fully than has yet been done to the invertebrates, and he will scarcely fail to make discoveries. For the lover of microscopical research, there is, moreover, a wide field of inquiry in the study of comparative embryology; that is to say, in the study of the development of the lower animals. Since it has become clear that a knowledge of the precise relations of living things one to another can only be arrived at by watching the changes through which they pass in the course of their development, research has been vigorously turned in this direction, and although an immense mass of facts has long since been accumulated regarding this question, Parker's brilliant researches on the development of the skull give an indication of the great things we may yet anticipate from this kind of research. "Speaking of microscopical study before this audience, I cannot but remember that in this country more than in any other we have a number of learned gentlemen who, as amateurs, eagerly pursue investigations in this department. I confess that I am always sorry to witness the enthusiastic perseverance with which they apply themselves to the prolonged study of markings upon diatoms, seeing that they might direct their efforts to subjects which would repay them for their labours far more gratefully. I would venture to suggest to such workers that it is now more than ever necessary to abandon all aims at haphazard discoveries, and to approach microscopy by the only legitimate method, of undergoing a thorough preliminary training in the various methods of microscopical investigation by competent teachers, of whom there are now plenty throughout the country."

*Palæontology and Embryology united by Evolution.*—Professor Allman, F.R.S., in perhaps the ablest address that has been given for many years to the British Association, afforded an admirable illustration of the importance of evolution in bringing two branches of science to bear on each other. He said that through the hypothesis of evolution, palæontology and embryology are brought into mutual bearing one on another. Let us take an example in which these two principles seem to be illustrated. In rocks of the Silurian age there exist in great profusion the remarkable fossils known as graptolites. These consist of a series of little cups or cells arranged along the sides of a common tube, and the whole fossil presents so close a resemblance to one of the Sertularian hydroids which inhabit the waters of our present seas as to justify the suspicion that the graptolites constitute an ancient and long since extinct group of the Hydroida. It is not, however, with the proper cells or hydrothecæ of the Sertularians that the cells of the graptolite most closely agree, but rather with the little receptacles which in certain Sertularinæ belonging to the family of the Plumularida we find associated with the hydrothecæ, and which are known as "Nematophores"; a comparison of structure then shows that the graptolites may with considerable probability be regarded as representing a Plumularia in which the hydrothecæ had never been developed, and in which their place had been taken by the nematophores. Now, it can be shown that the nematophores of the living Plumularida are filled with masses of protoplasm which have the power of throwing out pseudopodia, or long processes of their substance, and that they thus resemble the Rhizopoda, whose soft parts consist entirely of a similar protoplasm, and which stand among the Protozoa, or lowest group of the animal kingdom. If we suppose the hydrotheca suppressed in a plumularian, we should thus nearly convert it into a colony of Rhizopoda, from which it would differ only in the somewhat higher morphological differentiation of its cœnosarc, or common living bond by which the individuals of the colony are organically connected. And just such a colony would, under this view, a graptolite be, waiting only for the development of hydrotheca to raise it into the condition of a plumularian. Bringing now the evolution hypothesis to bear upon the question, it would follow that the graptolite may be viewed as an ancestral form of the Sertularian hydroids, a form having the most intimate relations with the Rhizopoda; that hydranths and hydrothecæ became developed in its descendants; and that the rhizopodal graptolite became thus converted in the lapse of ages into the hydroidal Sertularian. This hypothesis would be strengthened if we found it agreeing with the phenomena of individual development. Now such Plumularida as have been followed in their development from the egg to the adult state do actually present well-developed nematophores before they show a trace of hydrothecæ, thus passing in the course of their embryological development through the condition of a graptolite, and recapitulating within a few days stages which it took incalculable ages to bring about in the palæontological development of the tribe. I have thus dwelt at some length on the doctrine of evolution, because it has given a new direction to biological study, and must powerfully influence all future researches. Evolution is the highest expression of the fundamental principles established by Mr. Darwin, and depends on the two admitted faculties of living beings—*heredity*, or the transmission of characters from the parent to the offspring; and *adaptivity*, or the capacity of having these characters more or less modified in the offspring by external agencies, or it may be by spontaneous tendency to variation.

*Coal under the Microscope.*—Those who are interested in microscopical palæontology will remember with what pleasure they read Professor Huxley's lecture published on this subject some years since. Since that time, of course, many changes in palæontology have taken place, and in none more than in our views of the coal structures. Hence Professor Williamson, F.R.S., was quite justified in again going over the ground of Professor Huxley, as he did at the Bradford meeting of the British Association. In his evening lecture Professor Williamson said that Professor Huxley, in referring to the numerous small bodies met in some coal mines, spoke of these bodies under the name of sporangia, or spore cases. Now he (Professor Williamson) had come to the conclusion that they were all spores of two classes—the larger ones called macro-spores, and the smaller ones micro-spores. A large number of the plants, if not all, found in the coal-measures belonged to the cryptogamic plants, in which was found no trace of seeds or flowers. The reproductive bodies that took the place of seeds were little bud-like structures, to which the name of spores was given. In a certain class of those plants, the club-mosses for instance, were two kinds of these spores. The sporangia of club-mosses and similar plants never became detached from their parent stem. They burst and liberated multitudes of contained spores, which were objects like those so abundant in many coals. But these spores did not play so important a part in the formation of coal as Professor Huxley supposed. On examining these objects it was found that each of the little rounded disks exhibited three ridges that radiated in a triangular manner from a common centre. These disks were originally masses of protoplasm, lodged within a mother-cell. By-and-by each of these masses broke up into three or four parts; and it was found that to accommodate one another in the interior of their circular chamber, they mutually pressed one another. To illustrate the mutual compression, Professor Williamson produced a turnip, which he had cut into four parts, that corresponded exactly, he said, in their arrangement with the arrangement of the four spores in the interior of the mother-cell.

Then Professor Huxley held that coal consisted of two elements. Professor Williamson, exhibiting again a piece of coal, said the dirty blackening surface was a thin layer of little fragments of woody structures, vegetable tissues of various kinds, known by the name of mineral charcoal. These layers of mineral charcoal were exceedingly numerous. Professor Huxley, recognizing the abundance and significance of these little spore-like bodies, thought that mineral charcoal formed only a portion, and a limited portion, while the great bulk of black coaly matter was really a mass of carbon derived from chemically-altered spores. He thought that on this point they would be obliged somewhat to differ from Professor Huxley.

The bed which had been most widely quoted as containing most beautiful spores was found in the district of Bradford. If everything decayed, and Bradford was by an exceedingly improbable combination of circumstances to pass out of memory, it would be remembered in scientific history as the locality in which the "better bed" was found. The fragment he held in his hand was a fragment of the better bed. On examining it for a moment through a magnifying glass he saw that it was a solid mass of mineral charcoal, yet the microscope revealed in it no trace whatever of organic structure. Therefore, while Professor Huxley divided coal into two elements—mineral charcoal and coal proper, including in the latter term altered spores—he would say that coal consisted of three elements—mineral charcoal, black coal derived from mineral charcoal, and spores.

This outline of the history of coal led them to the independent conclusion that two elements were mingled in coal; the vegetable débris, or broken-up fragments, of the plants of the carboniferous age were intermingled with the peculiar spores to which Professor Huxley had so properly called attention. In proceeding to deal further with the plants of which coal was formed, the lecturer took occasion to acknowledge with thanks the loan of certain valuable specimens, to illustrate his discourse, from the Bradford Museum. One of these specimens was a most rare and valuable specimen, which he would be glad to take away with him to Owen's College, if he had the chance; but he was afraid the Bradford people were too conservative to stand that.

After giving a number of botanical and other details with regard to the plants of which coal was formed, he said our knowledge of this subject resolved itself into two divisions, *viz.* that of the outward forms of plants, and that of their inward organization. These two lines of inquiry did not always run parallel, and the one great object of recent research had been to make them do so. Specimens throwing light on the subject had been found at Arran, Burntisland, Oldham, Halifax, Autun in France, and elsewhere, and upon these a host of observers had been and still were working. It had long been known that most, if not all, the coal plants belonged to two classes, known as the Cryptogamia, or flowerless plants, and the gymnospermous exogens, represented by the pines and firs. All recent inquiries added fresh strength to this conclusion. One of the most important of these groups was that of the Equiseta or horse-tails, and which were represented in the coal by the Calamites. The long cylindrical stems, with their transverse joints and longitudinal grooves, were shown to be casts of mud or sand occupying the hollows in the piths of the living plants. Each of these piths was surrounded by a thick zone of wood, which again was invested by an equally thick layer of bark. Specimens were shown in which, though the pith was only an inch in diameter, the wood and bark combined formed a cylinder 4 inches thick, giving a circumference of at least 27 inches to the living stem. But there exist examples of the pith casts alone, which are between 2 and 3 feet in diameter. It was evident, therefore, he concluded, that the Calamites became true forest trees, very different from their living representatives—the horse-tails of our ponds and marshes.

After describing the organization of these plants, the Professor proceeded to describe the Lycopods of the coal-measures as represented by the Lepidodendra, Sigillariæ, and a host of other well-known plants. The living Lycopods, whether seen at home or in tropical forests, are dwarf herbaceous plants, but in the carboniferous age they became lofty forest trees, 100 feet high, and 10 or 12 feet in circumference. To enable such lofty stems, with their dense mass of serial branches and foliage, to obtain nutrition, an organization was given to them approaching more nearly to that of our living forest trees than to that of any recent cryptogams. A succession of woody layers was added to the exterior of those previously existing; so that as the plant rose into the air the stem became strengthened by these successive additions to the vascular tissue. As this process advanced it was accompanied by other changes, producing a large central pith, and two independent vascular rings immediately surrounding the pith, and the relations of these various parts to the roots, and leaves, as well as to the nutrition of the plants, were pointed out. The fruits of these Lycopods were then examined. The existence of two classes of spores corresponding in functions to the stamens and pistils of flowering plants, was dwelt upon. and one of these classes (the macrospores) was shown to be so similar to the small objects found in coal, as to leave no doubt that those objects were derived from the lepidodendroid and sigillarian trees which constituted the large portion of the forest vegetation.

Certain plants known as Asterophyllites were next examined. The ferns were also reviewed, and shown to be as remarkable for the absence of exogenous growth from their stems as the Calamites and Lycopods were for its conspicuous presence. The structure of some stems supposed to represent palms was shown to be that of a fern, there being no true evidence that palms existed in that age. The plants known as coniferous plants, allied to pines and firs, were described, and their peculiar fruits, so common at Peel, in Lancashire, were explained, and some plants of unknown affinities, but beautiful organization, were referred to. The physiological differences between these extinct ferns, and other plants, especially in their marvellous *quasi*-exogenous organization, were pointed out, and the lecturer concluded by showing how unvarying must have been the green hue of the carboniferous forests, owing to the entire absence from them of all the gay colours of the flowering plants which form so conspicuous a feature in the modern landscape, especially in the temperate and colder regions. The antiquity of the mummy, he added, was as nothing compared with the countless ages that had rolled by since these plants lived, and yet they must not forget that every one of those plants, living in ages so incalculably remote, had a history, an individuality, as distinct and definite as our own.

*Structure of the Lung in Pneumonia.*—A paper on this important subject has been published by Herr Friedländer, and also upon the pathological processes occurring in pneumonia established by section of the pneumogastric nerve in the rabbit. His inquiries into the latter point were chiefly undertaken with a view of determining whether the pneumonic inflammation was due to the entrance of fluids and solids into the respiratory passages consequent upon the paralysis of the glottis, or whether it was a neuro-paralytic phenomenon. He finds * that numerous dark-red spots, not containing any air, make their appearance within six hours after section of the vagi in the vicinity of which the tissue, though still capable of being inflated, is infiltrated with bloody serum. Sections of these parts which, after having been inflated, have been preserved in alcohol, show that at such points the alveoli are filled with a fibro-granular mass, red blood-corpuscles, and a considerable number of very large coarsely-granulated elements. These last-named large cells, which are for the most part more or less spherical in form, though sometimes elliptical or polygonal, are either firmly adherent to the wall of the alveoli, or lie free in their cavity. Their protoplasm occasionally contains, besides the ordinary coarse granules, brown and black pigment-granules, as well as red blood-corpuscles, and when examined on a warmed microscope stage, exhibits distinct changes of form, but none of locomotion. These cells have already been observed by Colberg in the catarrhal pneumonia of man, and have been described by him as "swollen epithelial cells." In this view Friedländer appears disposed to coincide, regarding them as forms which are the direct consequence of the swelling of the normal epithelial cells in the serous fluid poured out and surrounding them. In favour of this view he advances the additional argument that similar forms of cells may be met with wherever, as in simple hypostasis or in multiple capillary embolia, a sanguinolent serous fluid is poured out into the alveoli.

* Vide 'Lancet,' Oct. 4.

Similar forms may also be encountered in the lungs of rabbits rendered œdematous by a clip placed on the aorta ascendens, and even in pieces of perfectly fresh lung immersed either in serum or in some other indifferent fluid. On the other hand, such cells are never met with in healthy lungs, if immediately after removal from the body they are immersed in alcohol, or if the bronchiæ are injected with glycerine-gum. In such cases the cells lining the alveoli present their normal aspect. Hence it may be fairly concluded that these large cell-elements represent the normal cells lining the alveoli swollen by the imbibition of a watery fluid, and as their formation is due to purely passive conditions, he is not disposed to believe that they play any important or active *rôle* in the inflammatory processes as some have maintained. In the later stages of the pneumonia caused by section of the vagus—that is to say, about the twelfth hour—Friedländer found, in addition to the above, a large number of lymph-like corpuscles, which for the most part possessed many nuclei, not only in the cavity of the alveoli, but in the connective tissue surrounding the vessels and bronchiæ, whilst the tissue of the septa was tolerably free from them. At the same time white blood-corpuscles accumulated in the layer of blood lying next to the wall of the small arteries and veins, and these, or the lymph-like cells, escaping into the cavity of the alveoli, converted the whole lung-tissue into a compact, dense mass, from which sections could be readily made. After a little while the lymphoid elements and swollen epithelial cells underwent fatty degeneration, a sufficient proof that the latter do not actively participate in the inflammatory process.

*Papers on the Structure of the Internal Ear.*—Some papers of great interest are briefly recorded in a late number of the 'Medical Record.' They seem to be taken from the same number (No. 3, 1873) of the 'Monatshrift für Ohrenheilkunde.' The first is by Gustav Brunnen, who says that on examining the articulations of the incus alike with the malleus and the stapes, he finds that they are scarcely to be regarded as joints properly so called; but that they have a peculiar construction, a fibro-cartilaginous substance being interposed, to which the cartilage-covered surfaces of the bones on each side are more or less continuously attached. The second is by E. Tuckerkandl, who finds a small arteria stapedia constant in man; it is a branch of the stylo-mastoid, and passes through a triangular opening in the Fallopian canal, where it runs above the fenestra ovalis, penetrates the membrana obturatoria of the stapes, and is distributed on the promontory, often anastomosing with the artery that accompanies Jacobson's nerve. It is injected from the external carotid, while the other vessels of the tympanum are not. The third is by Professor Rüdinger, who brings his own experience to prove that the Eustachian tube is habitually closed. On swallowing during a lecture, he felt the usual sensation in the ears, followed on the right side by a peculiar cramp-like sensation. His own voice sounded louder and of a different timbre, and even painfully loud, so that, though interested in watching the condition, he was compelled at last to perform another act of swallowing, when the whole condition ceased. He ascribes it to a cramp of the dilator of the tube, and holds it proof that a closure is needed to exclude sounds from within. In the fourth, Herr Weber describes the structure and attachments of the tensor tympani, with some new points. He thinks the muscle consists in nearly its whole length of three separate strands. Owing to the narrowness of the canal, the edges are rolled spirally around each other, and this gives it the appearance of a spindle-formed muscle. In the last, Dr. Rueddinger distinguishes in the membranous canals of the labyrinth four layers—(1) the connective-tissue layer, (2) the hyaline tunica propria, (3) the papilla-formed projections, (4) the epithelium.

## NOTES AND MEMORANDA.

**The Position of the Brachiopoda.**—One of the finest papers that we have ever seen is that which Professor Edward Morse has kindly sent us, and which we shall notice more fully in a succeeding number. It is upon the systematic position of the Brachiopoda, and extends over sixty pages of small type, and has seven capital illustrations. The author aims at showing that the true position of these animals is among the Vermes, not with the Mollusca. And we may as well confess that he has done a deal towards convincing us of the accuracy of his view.

**Who First Examined the Diatomaceæ?**—This question has been recently answered by Professor H. L. Smith, the well-known American Diatomist. He says, in the last number of the 'Lens,' that for the first discovery of forms belonging here, which are in some measure given with certainty, we have to thank O. F. Müller, who described and figured a *Gomphonema* in 1773 as *Vorticella pyraria*, and in 1783 a *Fragilaria* as *Conferva pectinalis*, also a *Melosira* as *Conferva armillaris*. A much greater sensation was made by the discovery of the so-called staff animalcules (*Vibrio paxillifer*) by Müller, and which the discoverer, at first, did not know where to classify, but later embodied it in the genus *Vibrio*, in his large work on Infusoriæ.

**Professor Onimus on Septicæmia.**—In the 'Medical Record' for November 19th Professor Onimus is severely taken to task by the editor of that journal for "his complete ignorance of the work already done in the same direction by others." The article is worthy of attention.

**Experiments on Potato Blight.** — These have been conducted recently by Mr. T. Taylor, of Washington, and are of some importance. He says that in four glass jars he placed a pint of water. In No. 1 were placed a portion of fungus, *Peronospora infestans*, and the half of an Ohio potato remarkable for its healthy appearance. In No. 2 were placed a diseased potato, containing *Peronospora infestans*, and the half of a potato received from Santa Fé, New Mexico. In No. 3 was placed the second half of the Ohio potato alluded to, and in

No. 4 the second half of the Santa Fé specimen. In Nos. 3 and 4 was also put ½ oz. of pure sugar, to assist fermentation. These specimens were subject, during the experiments, to a temperature of about 75° Fahr. The respective jars were examined from day to day. On the sixth day the Ohio specimen in No. 1 was found to be rotting rapidly, while the Santa Fé specimen in No. 2 was apparently uninjured. Specimens Nos. 3 and 4 were undergoing slow fermentation. At first the water containing the New Mexican specimen became more milky in colour than did that of the Ohio specimen, but the deterioration on the third day was greater in No. 3 than it was in No. 4. On the twentieth day the Ohio specimen was perfectly dissolved, forming a pulp, while the Santa Fé specimen retained its perfect consistency throughout. On examining the pulp of No. 4 under the microscope, he found that the starch granules were arranged in cellulose cells, no liberated granules appearing on the field of view. Bundles of mycelium and budding spores appeared in profusion between the cells. Few infusorials appeared in view. The odour was slightly sour. The appearance of No. 4, as seen under the microscope, of about 80 diameters, was remarkable as contrasted with No. 3. The latter specimens presented a mass of infusorial life, mycelium, and budding spores. He made many examinations of the pulp to detect starch cells if present, but found none. The fermentation had completely destroyed them. The odour was very bad. The Ohio specimen in No. 1 rotted much quicker under the influence of *Peronospora infestans* than it did under the *Torula* fungus favoured by the action of sugar in No. 4 solution. The Santa Fé specimen in No. 2 resisted the *Peronospora infestans* fungus better than it did the *Torula* fungus in No. 4; but, by the use of either fungus, the tendency of any variety of the potato to resist fungus action may, by this mode, be easily decided. Since the preceding experiments were made, other northern and eastern varieties have been tested by fungoid solutions in contrast with some of the New Mexican varieties, giving like results, clearly demonstrating the superiority of the Santa Fé potatoes over all others thus far examined in respect to their powers of resisting fungoid and infusorial action.

# CORRESPONDENCE.

## "Fair Play" and his "Temperate Remarks."

*To the Editor of the 'Monthly Microscopical Journal.'*

Padnal Hall, Chadwell Heath, Essex, *Nov.* 3, 1873.

Sir,—In the last Journal I find myself involved in a cross-fire. The aspersions of "Fair Play" require a reply to his "few temperate remarks." When a man modestly communicates useful facts anonymously, the practice is laudable, indicating a pure spirit of philanthropy; but if this is done in a *controversy* as a cover for personal matters, a very different opinion must be formed of a writer who has neither the candour nor courage to disclose his name. He must not expect much influence, or attract respectful notice; and "Fair Play" coming forward in an optical question, should bring some little knowledge of the science, in order at all to be recognized as a judge. He may take this as "strong and arrogant," or, according to his idea, "*insulting*" language; it may be hurled at some one with whom I am intimately acquainted, but as he is firing from behind a bush, I cannot recognize him. Perhaps I ought to thank him for the extraneous information he has volunteered—expressed not without a tinge of malignity—that I have "come off second best" in the controversy concerning angles of aperture. I am not conscious of this; but, on the contrary, have some hope that the readers of this Journal will consider that it has been the means of eliciting some useful practical matter of a character not usually disclosed, and shown some optical errors which it may be better to point out than see permanently recorded. I can assure "Fair Play" that the moral of his letter does not incline me to kiss the rod that he would hold up to me as a warning in future; for I will take the liberty of controverting anything that appears to me to be an optical error, come from what source it may. The style of the reply must be influenced by that of the articles to which it refers; if these are brought forward pompously, and assumed as new discoveries, not to be questioned or discussed, then I submit that a strong tone is allowable.

The readers of this Journal can judge from my articles how far I have made any "insinuations against Dr. Pigott's character," for therein is all that I have written concerning him. "Fair Play" makes this, to me, most unwarrantable assertion, "Mr. Wenham was invited to see the various apparatus employed, and witness experiments therewith, but declined." This statement is simply not true; I never received a letter from Dr. Pigott, or request to be present at any specific time or place. I have a right to take up these questions as I find them *in print*, and to argue them on this evidence alone; I turn over p. 23 (containing matter that I have already commented upon), and refer to hand-drawings as evidence by which it is sometimes impossible to convey a true idea of difficult structure. "Fair Play" sneeringly asks if I "think Dr. Woodward's photographs clumsy and inaccurate?" I reply, certainly not; through Col. Woodward's kindness I possess them all, and prize them highly. I have been collecting peculiar forms of Podura scales and fragmentary pieces, in order to photograph them for the purpose of showing that the "note" structure is not "*an optical illusion caused by the oblique crossing of rouleaus of beads on the opposite sides of the scale*," as Dr. Pigott repeatedly states them to be, but that it arises from the peculiar form of *longitudinal ribs*, which may be clearly isolated and seen alone, and will produce these photographs when a contrary opinion of any weight should call for them. I have not denied a structure between these ribs. This was not the question at all, as many Podura scales show unmistakable indications (analogous to those in other Lepidoptera) of crossbars, which can also easily be developed as beads by myself.

Dr. Pigott's unsupported assertions, that the Podura note markings, called by him "shillelahs," and recently "tadpoles," are optical illusions, have been thrust before us *ad nauseam* for years past. If I have disputed this in strong terms, and as I have already controverted this repeatedly, it may perhaps be unnecessary to go over the same ground.

I repeat the sentence quoted in italics by "Fair Play." "I know not one microscopist of any note who has investigated the subject that believes in him" (Dr. Pigott), and will discuss the subject with any observer of note holding his opinion, and giving a *bonâ fide* name, with freedom from that acrimony which "Fair Play" so readily attributes to another from his own example.

As "Fair Play" supposes that Dr. Pigott's views are to be finally established, and says "his recent researches in circular solar spectra and test definition opens the whole question anew, to those who are willing to search rather than carp and cavil at what they will not or *have not themselves investigated*" (the italics are mine), he compels me to notice that which I should not otherwise have thought worth while.

At page 18 of the 'Monthly Microscopical Journal' for July, 1873, Dr. Pigott announces, in huge capitals (as I find it) his forthcoming CIRCULAR SOLAR SPECTRUM; I consequently anticipated some new fact, or that something original had been observed in the properties of light. Much to my surprise, however, under this imposing title, did I find merely the colour rings, caustic curves, &c., derived from a highly luminous point, such as that from a sunlit mercury globule, perfectly well known *a quarter of a century ago* to those engaged in the correction of object-glasses, and employed to discover errors of centering, figure, or oblique pencils, adjustment, distance of lenses, &c., having the engine-turned patterns, blurred disks, and curious chequered forms, some of them in regularity, with almost diatom-like markings, associated with bright colouring. These familiar appearances, which anyone can produce, indeed "opens the question anew"—to himself at least—and with a very proper beginning, I readily admit.

As there is nothing in his letter that requires scientific consideration, I now take leave of "Fair Play" and his "few temperate remarks," asking your readers to consider how far the inconsistent title he has assumed has exalted the cause that he advocates. I have felt as if replying to a nonentity, where a name might perhaps be entitled to some respect.

F. H. Wenham.

P.S.—As I have already stated, I repudiate making any "insinuations against Dr. Pigott's character;" but I take this opportunity of explaining, as certain remarks appear in a letter of mine (published at page 17, No. 443 of the 'English Mechanic' for September last), written in the belief that the publication of certain correspondence had been at his request (as stated therein), and which I considered not to his credit. Seeing he has come forward candidly to disclaim all responsibility for the appearance of these letters (see last Journal), I now willingly withdraw any expressions that his allusions to myself gave rise to.

It is scarcely necessary to add that I should not have taken any notice of the correspondence referred to, had it not appeared under the distinct patronage of Dr. Pigott, in an important public journal.

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## Dr. Woodward's Reference to Mr. Brakey.

*To the Editor of the 'Monthly Microscopical Journal.'*

*November 6th*, 1873.

Sir,—In the concluding part of Dr. Woodward's paper in your present number there is a special reference to myself, on which I have some observations to make.

In a former letter I had called attention to the fact that he made an oversight in supposing he had something new to tell on the question he was discussing. I observed that the plan of getting a wider aperture by the insertion of an extra hemisphere between objective and object lay outside the limits of the present controversy, being on all sides acknowledged; and that it was, moreover, already old and well known, having been long since worked out in theory and practice. The language in which I pointed this out was no doubt plain language, inasmuch as such was necessary to keep the real issue from confusion. That it was not without personal courtesy to the writer will, I believe, be equally plain to anyone who may choose to refer back to my letter (p. 98).

To this Col. Woodward has replied in a style of language which I was scarcely prepared for—from him. It seems I thoroughly misconceive optical law; and know as much about Greek as I know about optics; but he will teach me a lesson, &c., &c.

This is not exactly an example of the cold dry light of science—that *siccum lumen* which Bacon always commends for investigating truth. The meaning of it is, of course, that Dr. Woodward, proud (and justly proud) of his reputation, did not like to find that he had brought before the world, with ceremony of diagrams and array of witnesses, news which was no news; and in a not unnatural irritation at having it told he forgot himself—a little.

To come to the substance of his defence; this needs but a short answer. I had pointed out, as already said, that the structure now brought forward for the purpose of this controversy was not in the common acceptation of the term an object-glass, because it was an object-glass and something more; having, that is, an added convex lens in front of it. His reply is, that having used it without the added lens, it "does not define very well." Possibly. I can only say I am sorry for it, but it is not my affair. With the special merits of this or any other individual glass I have nothing to do. What I called attention to was the *structure*, not of one glass in particular, but of every combination put together on such a principle. Now an objective, having a front of the ordinary kind, with the usual posterior combinations, and which will show objects not only when mounted in balsam or fluids, but also in air or *in vacuo*, is already perfect or complete, having all its working parts. If, in front of this, we fix an extra "system," then we form a new compound instrument or structure. Whether such a compound structure is to be called an object-glass, is, of course, only a question of words, about which it would be frivolous to dispute. Anything may be called anything. But what is not a question of words, but of things, is, that the new structure is not an object-glass *in the same sense* in which the word had been used in this discussion; inasmuch as it possesses different properties, and works under different conditions. What this difference consists in I pointed out specifically in my former communication. And that the assertion about the limitation of the angle had reference to object-glasses, as commonly understood, is seen very plainly from the original of the discussion; for such are the object-glasses described and figured in his large diagram by Dr. Pigott, who first introduced the question and first fell into the error.

The other part of Dr. Woodward's paper does not immediately concern myself. I therefore only mention in passing that it contains a similar oversight, which I have no doubt will be pointed out.

Neither in this nor in his former paper has Dr. W. thrown any new light upon this subject. And it is, I think, to be regretted, that he has complicated a question in itself elementary, and of no difficulty whatever, by coming forward without having first taken the trouble to ascertain what exactly it was that had been asserted and was denied.

Your obedient servant,

S. Leslie Brakey.

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## "Fair Play" on Dr. Pigott.

*To the Editor of the 'Monthly Microscopical Journal.'*

224, Regent Street, *Nov.* 10.

Sir,—Your correspondent, "Fair Play," is an ardent believer in and defender of Dr. Pigott's claims to be a discoverer in the difficult subject of high-power definition.

I take some interest in the subject, and would spare no pains to inform myself of any real advance made. But because in many of Dr. Pigott's contributions to the 'Monthly Microscopical Journal,' I see but a sprinkling of valuable original matter embedded in a mass of irrelevancy that suggests error when not defying comprehension, I am led to a feeling of doubt as to the justice of his claims to be considered the inventor of a really successful Aplanatic Searcher. Will "Fair Play," who is so well informed of Dr. Pigott's case, or still better, will Messrs. Powell and Lealand tell us what share Dr. Pigott really had in the invention of the "Aplanatic Searcher"? Whether the basis of it was perchance some previous attempt of these renowned opticians to construct a successful Amplifier—Dr. Pigott's share in the invention not being the discovery of any new principle, but limited to the suggestion of minor details?

The question of the practical utility of the Aplanatic Searcher is probably decided in the negative, because, though Dr. Pigott has over and over again vaunted its supreme necessity for obtaining the best results with high powers, yet Messrs. Powell and Lealand, who constructed it, did not exhibit it in December last in connection with their new $\frac{1}{50}$th and $\frac{1}{80}$th immersions. And though, on the other hand, Dr. Pigott says, "It increases magnifying power and penetration, and enables us to use a low objective instead of a much higher one giving as much power"—Messrs. Powell and Lealand did not use it at their recent exhibition of their new and remarkably fine $\frac{1}{4}$th immersion—when surely if they had believed in it they would have used it.

I think, sir, many of us do not join in the chorus of jubilation in honour of Dr. Pigott, because we feel a sort of *je ne sais quoi*, which a little frankness in answering these inquiries may possibly remove.

I remain, Sir,

Your obedient servant,

JOHN MAYALL, jun.

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## HOW TO PUT AN END TO MR. WENHAM'S AND DR. PIGOTT'S CONTROVERSY.

*To the Editor of the 'Monthly Microscopical Journal.'*

SIR,—Instead of carrying on perpetual literary battles about Dr. Pigott's "bead theory," why, may I respectfully ask, don't the parties for and against, fight at once with their own object-glasses? —fitting umpires being chosen to determine which side is victorious. As Dr. Pigott and Mr. Wenham happen to be foremost in the fray, let their finest object-glasses, their $\frac{1}{5}$th, and $\frac{1}{8}$th, and $\frac{1}{10}$th, and $\frac{1}{16}$th, and $\frac{1}{25}$th, be fairly tested one against the other, not necessarily on objects that "ought" to show the beads, but on ordinary standard tests, such as *P. angulatum*, *P. Spencerii*, *N. rhomboides*, *Acus*, &c. Experienced microscopists need not be told the points of a good object-glass, and can trust their own eyesight when bringing object-glasses to bear on the above tests; and they may rest assured that those which will show them best, will also show the beads on Podura, *if* there are any to be seen there. Thus there might be a fair chance of ending this paper warfare; a result much to be desired, as it is scarcely generous or in good taste to write what involves constant aspersions on Mr. Wenham's character and judgment—who, as an amateur, has done more for the microscope than any man living, and in respect of whom scientific antagonists might surely forget their differences in gratitude for all that he has accomplished to further their favourite pursuit. As I have no reason to be ashamed of my views, and indeed feel convinced that a large body of microscopists will uphold them, I shall not scruple to give my name and residence.

Believe me, yours faithfully,

J. J. PLUMER.

THE HAYES, STROUD, GLOUCESTERSHIRE,
*Nov.* 12, 1873.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *November 5th*, 1873.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society received since October 1st was read, and the thanks of the meeting were voted to the donors.

The Secretary called the attention of the meeting to three photographs by Dr. J. J. Woodward, of the Army Medical Department, Washington, and which had been brought there that evening by Dr. Lawson. Two of them were of *Navicula lyra*, and the third was of *Amphipleura pellucida*, magnified to 1380 diameters with a Tolles' $\frac{1}{10}$th immersion objective.

The Secretary read a paper, by the Rev. W. H. Dallinger, "On some further Researches into the Life History of the Monads," in which the author minutely detailed a number of interesting observations conducted by himself and Dr. Drysdale. The paper, which was in continuation of one already published in the Journal, was accompanied by some very fine drawings as illustrations. It will be found printed *in extenso* at p. 245.

A vote of thanks was unanimously passed to Mr. Dallinger for his communication.

Mr. H. J. Slack thought that this paper was very remarkable on account of the extreme minuteness of the bodies which it described; it was also of much importance as bearing upon what was injudiciously called "spontaneous generation"; for if so high a power as a $\frac{1}{50}$ inch was required to see the granules or germs from which the monads were developed, not only many of their changes, but their existence, would be overlooked by those observers who had worked only with lower powers. It would also be easily seen that a person using a lower power might assert that a fluid contained no germs whatever, whereas in reality it might contain millions. This was, he believed, the first time that observations had been recorded as to anything like a true sexual process in such excessively minute bodies. The paper was also of much interest as showing how little stress must be placed on external form in determining species, for here were many quite dissimilar forms, such as round, oval, and triangular, yet all belonged to the same species in different stages. They were so unlike each other that anyone might have called them different species, and have proceeded forthwith to give to each of them a terrible long-tailed Greek name.

The President thought the question of so-called spontaneous generation was so important that whatever threw any additional light upon the subject could hardly fail to be of great interest.

Mr. Alfred Sanders read an interesting paper "On the Art of Photographing Microscopic Objects," in which he showed that the costly apparatus and extensive arrangements frequently regarded as essential to success might readily be dispensed with, and the most satisfactory results obtained with appliances of the simplest kind. The paper, which fully described the entire process of manipulation, will be found printed at p. 250.

A vote of thanks to Mr. Sanders for his paper was moved by the President and carried unanimously.

Mr. Wenham said he was glad to see this very important question revived, and hoped that other persons might have their attention drawn to it. When it was first brought out it was scarcely thought worthy of consideration, and was altogether ignored as being useless. He quite agreed with Mr. Sanders that there was nothing like sunlight for the purpose. Those who practised the process would do well to have their objectives supplied with a regular series of stops, which would often be found advantageous; by using the lowest stop they would find that they would get much greater distinctness. He also quite agreed with Mr. Sanders, that the apparatus employed might be very simple; he was often called upon to practise the process, and found it so little trouble that he could arrange everything he wanted for the purpose in half an hour. He enjoyed the luxury of a dark room for it, but this might be done without, as Mr. Sanders had shown; he had a heliostat to direct the ray of sunlight through the shutter into the room, and a piece of yellow glass up above would let in light enough to work by. All that he required for taking the pictures was within easy reach, and he was able to take them with the greatest rapidity. He worked, in fact, inside his camera. The heliostat which he used was an ordinary one, not one of the self-moving ones, he would not be bothered by anything so troublesome; he placed it outside the window, and when its position required altering, he simply shifted it with his hand.

The President said it was not strictly speaking a heliostat at all, but merely a solar reflector to direct the beam into the room.

Dr. Matthews inquired of Mr. Sanders how he managed to procure collodion for the purpose without "structure."

Mr. Sanders said he found that the "structure" of the collodion did not make any difference as to the results.

Mr. Wenham noticed that in his paper Mr. Sanders mentioned that he used very thin collodion, and in practice it was found that the thinner the collodion the less "structure" it showed.

The President asked for information as to the conditions on which this reticulated "structure" depended.

Mr. Wenham said he could not very well account for it, unless it might possibly be some sort of form of internal crystallization. It was found to be extremely regular. Like many other colloid substances, it had this tendency to break up into regular forms, like artificial diatoms, or like Mr. Slack's silica films.

Mr. S. J. McIntire read a paper entitled "Notes on Acarellus," in which he described certain insects found parasitic upon *Obisium*, and closely resembling the *Hypopus* of the 'Micrographic Dictionary,' and the insect mounted by Mr. Topping under the name of "Parasite of House Fly." The paper was illustrated by drawings and by specimens, both alive and mounted, exhibited under microscopes in the room. It will appear in the next number of this Journal. Specimens of apparently similar insects described by Mr. Tatem as *Acarellus Muscæ*, to which allusion was made in Mr. McIntire's paper, were sent to the meeting for comparison by Mr. Tatem, and were exhibited under the microscope by Mr. T. Curties.

The thanks of the meeting were unanimously voted to Mr. McIntire for his paper.

The President, after remarking that the paper seemed to open up questions as to the relationship of supposed distinct species of *Acarelli*, announced that the Society would hold a scientific evening on December 10th.

The meeting was then adjourned to December 3rd.

Donations to the Library, November 5, 1873:—

| | From |
|---|---|
| Land and Water. Weekly | *The Editor.* |
| Nature. Weekly | *Ditto.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts' Journal | *Society.* |
| Journal of the Quekett Club, No. 24 | *Club.* |
| The Lens. Vol. 2. No. 3 | *Editors.* |
| Report of the United States' Patent Office for 1869, 70, and 71, in seven vols. | *The United States' Patent Office.* |
| Three Photographs | *Dr. J. J. Woodward.* |

WALTER W. REEVES,
*Assist.-Secretary.*

## READING MICROSCOPICAL SOCIETY.*

*October* 7, 1873.

Captain Lang exhibited the so-called *Gomphonema coquedense*, which appears to be merely the sporangial form of *G. geminatum*, being sparsely scattered amongst a gathering of that species. He also exhibited a ten-angled *Triceratium favus*, a recent *Omphalopelta versicolor*, and an *Actinoptychus trilingulata*. All these had been kindly sent by Mr. Kitton, who states that the last has never (he thinks) been found except by himself and Mr. Brightwell in some West Indian material. Captain Lang, however, showed valves from Singapore shell-cleanings which Mr. Kitton, to whom they were sent, thinks identical. In the opinion of Captain Lang, *Omphalopelta versicolor* must certainly belong to the same genus as *Actinoptychus trilingulata;* and now that the absence, presence, or number of the processes is allowed to be immaterial, he would suggest that the genus *Omphalopelta* should be abolished, as it has been by Professor H. L. Smith in his new Conspectus of the Diatomaceæ.

Mr. Tatem exhibited gizzards of insects mounted as opaque objects on white-spot ground, *viz.* those of the cricket, cockroach, *Carabus nemoralis*, *Carabus violaceus*, *Hydrophilus*, *Goerius olens*.

Mr. Austin exhibited small fungi, *Didymium cinereum*, *Diderma vernicosum*, *Helminthosporium Tiliæ*, *Helminthosporium Smithii*, section of *Peziza leporina*, and growing tip of root of *Poa annua*, stained with carmine.

* Report supplied by Mr. B. J. Austin.

# INDEX TO VOLUME X.

END OF VOLUME X.

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