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AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

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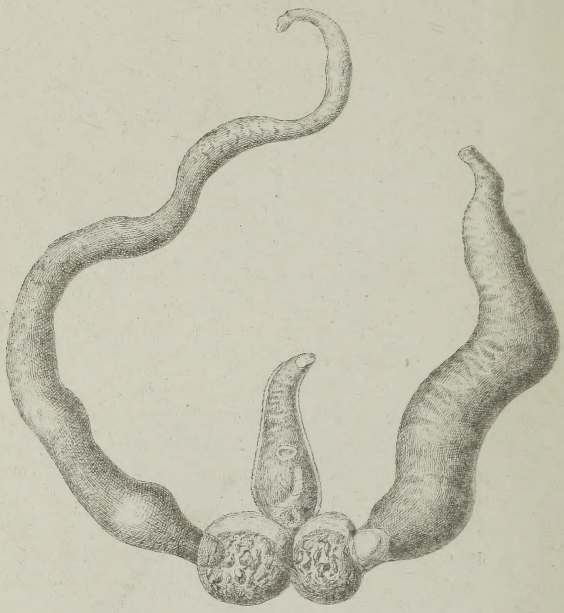

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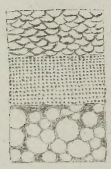

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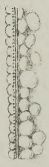

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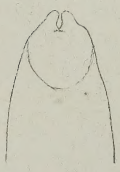

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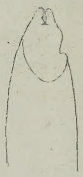

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W. West & Co lith.

Bucephalus polymorphus.

THE

MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1875.

I.—*Notes on Bucephalus polymorphus.*

By Chas. Stewart, F.L.S., Hon. Sec. R.M.S., &c.

(*Read before the* Royal Microscopical Society, *June* 2, 1875.)

Plate CVII.

Through the kindness of Mr. Badcock I have had an opportunity of examining that curious animal *Bucephalus polymorphus*, which has lately been exhibited at this and other Societies. I regret that I have only some slight notes to offer on its structure, and can add nothing to our knowledge of its life history.

The Bucephalus consists of three parts, viz. two spheres, two appendages borne on these, and a central main body. The spheres are hollow and contain granules which freely move from one to the other. In shape each resembles a peach, with the part corresponding with the cleft of the fruit and point of insertion of the stem, represented by a smooth area facing the central body of the animal, and extending to their outer sides where the appendages are attached; the remaining part of the spheres presenting a rough warty aspect.

The appendages are densely filled with minute granules, most abundant near their surface, but in their interior mixed with numerous transparent spherules. The appendages are in a constant state of contraction and expansion, their extremities often suddenly curling round and forming a spiral of from two to three turns. The contraction of the appendage in length is accompanied by a synchronous contraction of the sphere at its base, which forces its contents for a short distance into the appendage, although a thin membrane, which prolapses, appears to separate their cavities. As might be expected, when the appendage elongates the sphere dilates and the contents of the appendage prolapse into it.

The movements of the central body are not so energetic as those of other parts, and resemble those of a leech when it is fixed by its

EXPLANATION OF PLATE CVII.

Fig. 1.—*Bucephalus polymorphus.*
„ 2.—Three layers of central body.
„ 3.—Optical section of ditto.
„ 4.—Front of body.
„ 5.—Side view of ditto.

posterior sucker. For its examination a power of five or six hundred is desirable, although its general features may be studied with a $\frac{4}{10}$ths objective. Its surface is studded with considerable regularity by minute, upstanding, firm elevations resembling cells, but apparently quite structureless. Beneath this layer and looking like a changed condition of its deepest part, are granules of uniform size and symmetrical arrangement, in rows corresponding with and transverse to the longitudinal axis of the body; the latter being most marked, gave a striated appearance, reminding one of striped muscle; at other times by a slight alteration the resemblance to the markings of a Pleurosigma was most striking. Beneath this layer is a thin structureless membrane, by a reflexion of which a space near the mouth is cut off from the main body; this space, together with the general interior of the creature, is occupied by a granular protoplasmic substance mixed with clear droplets. The mouth has three lips, and leads into a slight depression or pharynx, often deepened by retraction into the space above mentioned. On the middle of the ventral surface is an oval sucker, in size about a third the diameter of the body: transverse striation is very distinct in its central depressed part. Behind the sucker a clear, elongated, curved space was frequently seen in the interior of the body, which space, after remaining some time, appeared suddenly to collapse. On the dorsal surface a little in front of the sucker was situated a somewhat stellate space, over which the surface of the body was depressed. These spaces perhaps represent a rudimentary water-vascular system. Near the sucker, in all specimens examined, were three minute spherical concretions of unequal size, moving freely in the general substance; they were the only parts that showed double refraction under polarized light, under which they were very conspicuous. On the under surface near the mouth was an elevation with a notch at its posterior margin; at the attached end close to the spheres were two diffused masses of greater opacity and granularity.

SCALE FOR THE MEASUREMENT OF THE ANGULAR APERTURE OF OBJECT GLASSES.

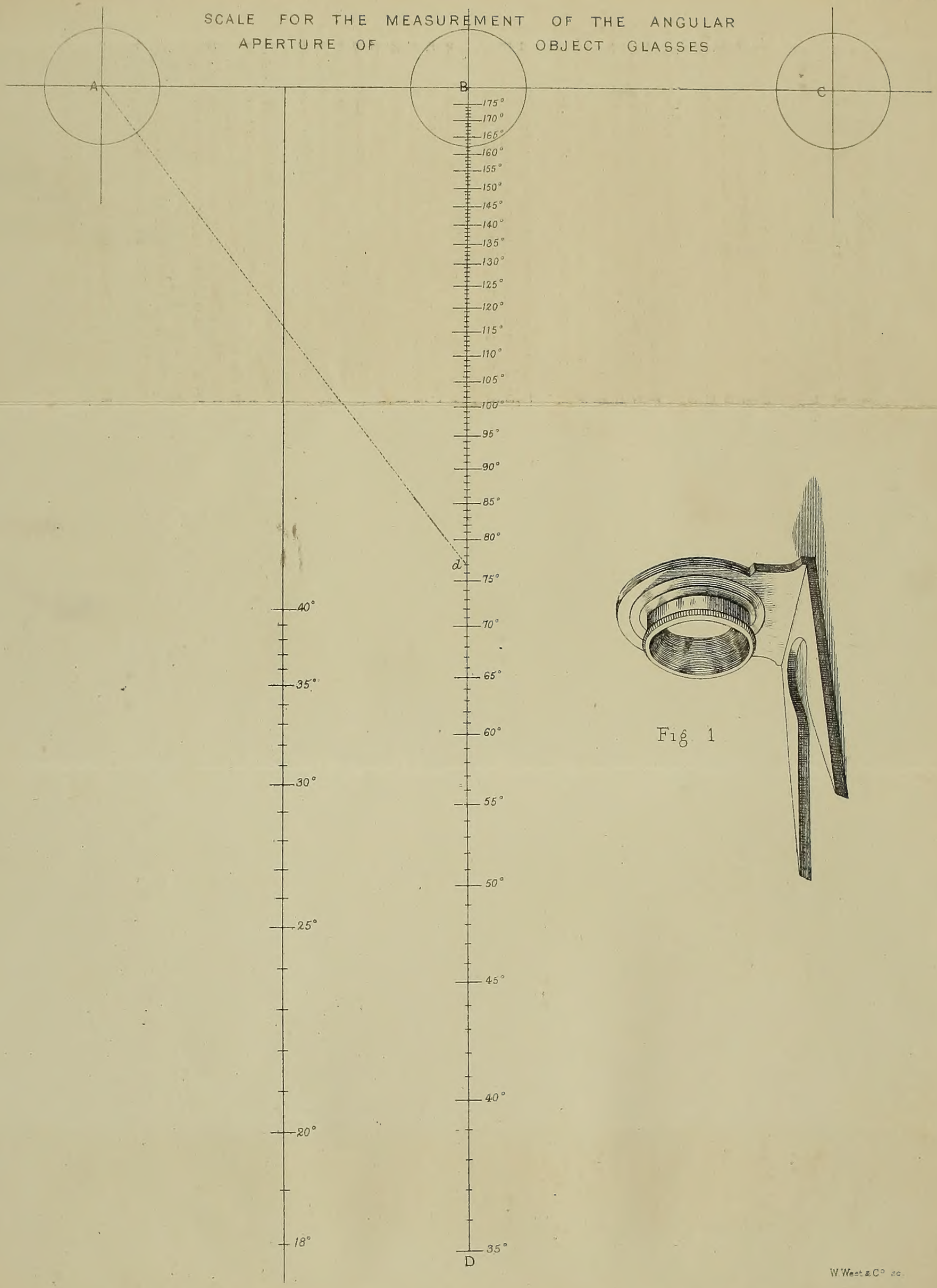

Fig 1

W West & Co sc.

II.—*Measurement of Angular Aperture.*

By J. W. Stephenson, F.R.A.S., Treasurer R.M S.

(*Read before the* Royal Microscopical Society, *June* 2, 1875.)

Plate CVIII.

The necessity of an easy method of determining the angles of aperture of object-glasses has long been apparent, and the want has recently been more than usually felt, when the relative angles of different constructions have been more prominently brought before the Society.

The object of the scale for the measurement of angular aperture which I have placed on the table (with some lithographic copies) is to supply this want, and its merits, if any, are the simplicity of its construction and its strict adherence to the principle of estimating angles which has hitherto been usually adopted.

Being merely a scale of the cotangents of half the angles indicated, it may be laid down from any ordinary trigonometrical tables.

It is hardly necessary to point out that the distance A B being constant, the line A *d* is always the cotan. of the angle A *d* B; but as the angle to be measured is twice the angle A *d* B, it is so marked on the scale.

The method of using the scale is this: some small and narrow body, readily visible, such as a flame or a strip of white paper on a black ground, is accurately placed at each of the points A and C, and viewed through the back of the object-glass to be measured, which is placed above, with its axis parallel to the line A D. By gradually advancing the objective from D towards B, the images of the flames (or any other objects employed) separate, until at a certain point each is seen on the opposite margins of the posterior lens of the combination. At this point the angle of the glass is seen immediately beneath the front lens.

It will be observed that for the lower angles a side scale has been introduced. This is necessary in consequence of the very rapid increase in the length of the cotangents of small angles. In using this reduced scale, which is only half that of the principal scale, the objects to be viewed are placed on the points A and B, instead of the points A and C.

With high powers it is desirable to view the images with an ordinary hand magnifier, but with low powers this is quite unnecessary.

A convenient, but by no means necessary, form of object-glass holder is shown in Fig. 1, in which the Society's screw is fixed at right angles to a brass plate, cut out to facilitate the reading off with objectives of different lengths.

In the examination of two objectives of equal power without some means of determining their relative apertures, it is quite possible that an erroneous judgment may be formed. It may be that one glass will resolve a given diatom with ease which the other will not touch, and a hasty conclusion may be arrived at that the former is the better instrument; but let each be examined for aperture, and the cause of the supposed superiority may at once be disclosed. The greater resolving power may be merely excess of aperture, and it may turn out that the better glass, both in its manufacture and for all practical purposes, may really be that which has failed on surface striation or dotted tests.

It may be said generally that if two objectives differ widely in aperture they are incapable of comparison; each may be excellent in its way, one in resolving power on surface markings, the other in what is *now* called "penetration," or depth of focus. But they cannot be compared. In emphasizing the word "penetration," attention is drawn to the change which has been made in the meaning attached to the word, which by some writers is used as the equivalent of angular aperture.

In absolute observations of every kind the element of "personal equation" exists, but it will be evident to all that in determining the difference of aperture of two objectives that element is necessarily eliminated.

III.—*Notes on the Use of Mr. Wenham's Reflex Illuminator.*

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

(*Read before the* Royal Microscopical Society, *June* 2, 1875.)

If Mr. Wenham's Reflex Illuminator for High Powers is used under the circumstances for which he especially contrived it, little difficulty will be found with suitable objects. The light, as he explained, penetrates only where the object makes a new surface on the slide, and "acts," to use one of his familiar phrases, "like a hole in a dark lantern."

The effect is so admirable upon many objects, such as scales of insects, certain micro-fungi, minute algæ, desmids, diatoms, &c., that everyone who has successfully tried it must wish to add to its range of utility, and this may be easily done.

It will be found that most balsamed objects, and many in which the covering glass lies very close to the slide, give with it so much false light when ordinary objectives are employed, that the result is very unsatisfactory. This false light will be found in many cases so oblique that it can be got rid of by using an objective with a small angle, or temporarily reducing the angle of an ordinary high power by a movable stop.

For example, a slide of *Surirella gemma* and this illuminator exhibited no false light with a glass of about 70°; some, but not much, with a fine $\frac{1}{4}$th made on Mr. Wenham's new formula, and having an angle of 150°, too much to be endurable with Powell and Lealand's immersion $\frac{1}{8}$th full aperture; and none with the same glass and with a stop limiting the rays admitted to about 90°.

Many slides of butterfly and other scales taken at random from a cabinet become manageable with reduced apertures, and the effects, when the plan succeeds, are very curious, beautiful and instructive. Mr. Wenham has alluded to the changed aspects obtained by rotating the apparatus when employed upon the so-called Podura scale, *Lepidocyrtus curvicollis,* and similar observations may be made with regard to Lepisma scales, and those of various insects allied to Podura. Indeed it is not prudent to pronounce an opinion upon any scale of difficulty until this method has been tried, and all the aspects it produces considered in their mutual relations.

It is by no means intended to advise microscopists against the use of this apparatus with large-angled glasses upon objects mounted so as to be fit for it; but when slides fail, the observer is recommended not to abandon the plan, but to reduce the angle of the glass and try again, and with good chances of success. The apparatus has a remarkable power of increasing both the penetration and the resolution of good objectives.

IV.—*On Dr. Schumann's Formulæ for Diatom-lines.*

By W. J. Hickie, M.A., S. John's College, Cambridge.

Plate CIX.

In compliance with a general and strongly expressed wish to that effect, we have here attempted to give the readers of the 'M. M. J.' some sort of an idea of Dr. Schumann's famous work, though we fear that, for so doing, some may liken us to the simple-minded man in Hierocles, who exhibited a brick as a specimen of the house

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EXPLANATION OF PLATE CIX.

Fig. 1.—*Amphipleura pellucida.* "One specimen gave 45 transverse lines, and 45 longitudinal lines, and 61 inclined lines in $\frac{1}{100}'''$. Another specimen exhibited 38 transverse lines and 55 inclined lines. If we add these, we get $a = b = 41\frac{1}{2}$, $c = 58$. As $41\frac{1}{2}.\sqrt{2} = 58\frac{2}{3}$, there is no doubt that we have here *corresponding* series of the simplest sort."

By *inclined lines* Dr. Schumann means imaginary lines drawn at a certain angle from a top row of transverse dots to a lower row of transverse dots. See his own explanation on pp. 8 and 13 of this number.

Fig. 2.—*Navicula lata* (= *Pinnularia pachyptera* Ehrenb.). "Four specimens from the *Siebenseethal* exhibited 7 canals, and one from the *Kohlbachthal* gave 12 canals in $\frac{1}{100}'''$. The closeness of the series of puncta traversing the whole frustule is defined by the formula $\alpha = 4a$."

Fig. 3.—*Navicula borealis.* "The closeness of its series of puncta is defined by the well-founded formula $\alpha = 4a$."

Fig. 4.—*Mastogloia antiqua* Schum. "Whilst specimens found in Prussia gave 27 transverse lines in $\frac{1}{100}'''$, those from the High Tatra exhibited 44, 45, and 48; to which estimates the formula

$$a = 19 + \frac{h}{600} \cdot \frac{11}{4}$$

corresponds."

Fig. 5.—*Navicula crassinervis* var. Compare the figure of this in Smith's Synopsis. Dr. Schumann's figure gives the transverse lines alone. Pritchard (Infusoria) says, "Striæ wanting or indistinct."

Fig. 6.—*Navicula rhomboides*? (Ehrenb. Amer. III. 1, 15). "Three frustules from the *Mengsdorfthal* with 78 transverse lines in $\frac{1}{100}'''$ appear to belong to Ehrenberg's form, though their length is only 8–9 T." His drawing gives *no longitudinal* lines. He seems therefore either to have overlooked them, or not to have known of their existence.

Fig. 7.—*Nav. rhynchocephala.* "In one part of the High Tatra with 34, in another with 36 punctated transverse lines in $\frac{1}{100}'''$."

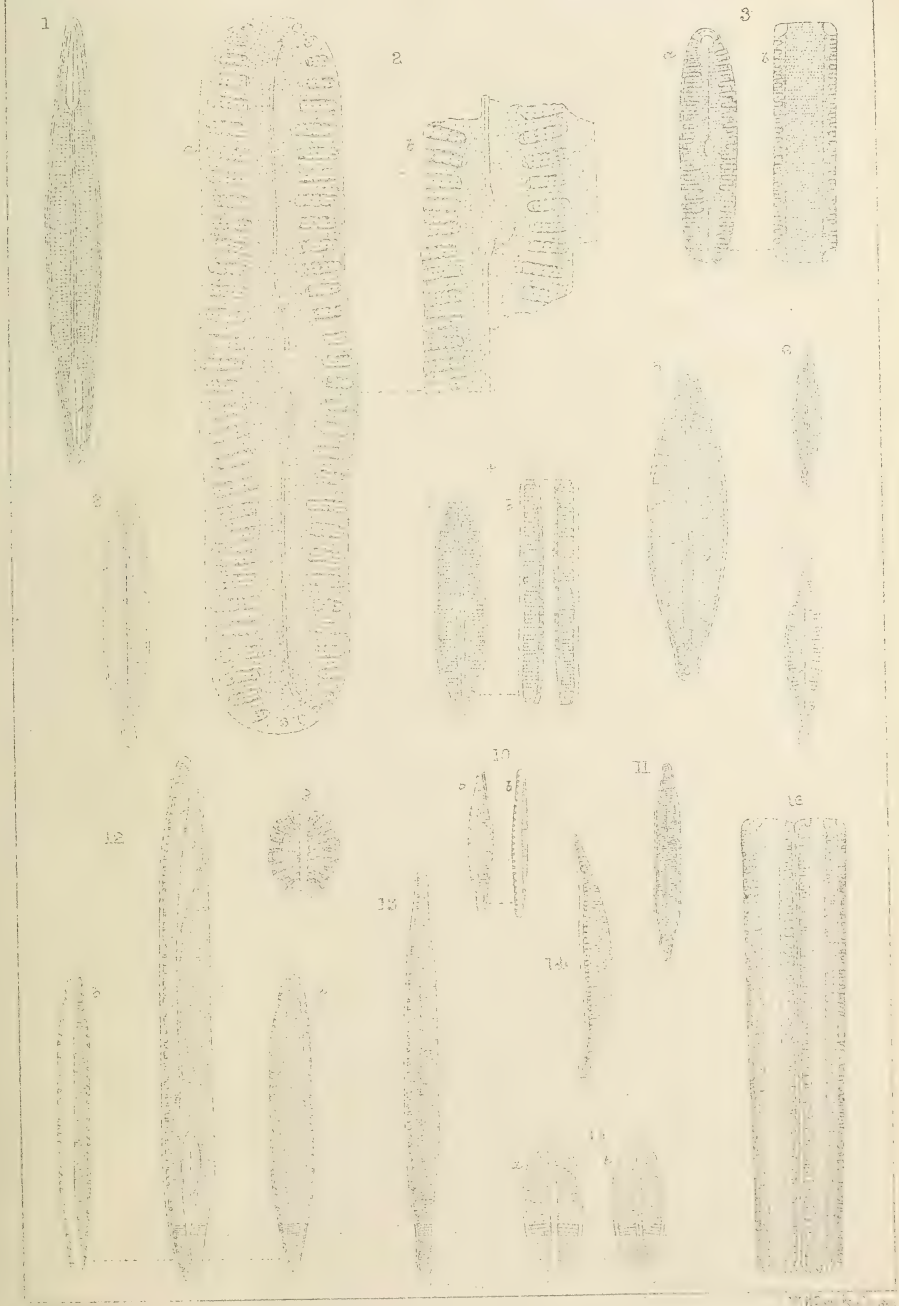
Fig. 8.—*Nav. angustata.* "This diatom has in Prussia 40, and in my highest station on the Tatra 68 transverse lines in $\frac{1}{100}'''$."

Fig. 9.—*Campylodiscus nanus* m. "On the margin appears a series of fine puncta, which, at an elevation of 5500 feet, numbered 53 in $\frac{1}{100}'''$."

Fig. 10.—*Nitzschia minutissima.* "On combining my estimates of ten specimens found in Prussia with my estimates of the sixteen found on the Tatra, I obtained the following formula for the number of marginal puncta:

$$A = 24 + \frac{h}{600} \cdot \frac{5}{9}.$$

If this formula were well established, we should have an excellent means of determining the height of a place by the help of microscopical observations, as this diatom is probably frequent on other mountain ranges also."



Structure of the Diatomaceæ.

he had to let. Light reading we certainly cannot promise them; but, such as it is, we commend it to the perusal of those "qui volunt et possunt."

The learned author begins by observing that, though several microscopists have published valuable remarks on the diatom-frustule and its striæ, yet one essential property has been left uninvestigated, namely, the dependence of the different striæ-systems on one another—in other words, the relation in which the diatom-lines stand to one another.

He proposes therefore at the outset to investigate certain principles which regulate the *position* and *closeness* of the diatom-lines, without dwelling on the *form* of the material puncta or the dots. He then adds, "I shall first discuss two simple cases of frequent occurrence, and then subjoin some data, which apply to every position of rectilinear diatom-lines.

If we examine a *Navicula*, we observe on each side of the median line striæ running to the marginal edge in a direction perpendicular to the median line; these transverse lines, in some species, it would seem, consist of canals, in others of material puncta, more or less distinct, whose centres are mostly equidistant from one another.

But the canals also constantly form equidistant spaces; perhaps in consequence of constrictions or intrusive ridges. Frequently they look like strings of pearls communicating with one another by wide openings. If we examine one of the two adjacent series, we find in most species of *Navicula* that this series is a repetition of the fundamental series; that therefore the connecting line of adjacent puncta of both series runs parallel to the longitudinal axis of the *Navicula*. Thus the transverse and longitudinal lines present the appearance of a chess-board.

Yet the longitudinal lines generally have a width apart different

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FIG. 11.—*Nitzschia thermalis.* "One specimen exhibited 16 eye-like puncta and 80 transverse lines in $\frac{1}{100}'''$, and another 18 such puncta in $\frac{1}{100}'''$."

FIG. 12.—*Nitzschia media.* "Three frustules from the *Mengsdorfthal* showed 26 puncta and 74 transverse lines in $\frac{1}{100}'''$."

FIG. 13.—*Nitzschia communis* (= *Synedra notata* Ktz.). "Seven specimens gave 28 puncta and 80 transverse lines in $\frac{1}{100}'''$."

FIG. 14.—*Nitzschiella closterioides* (Grunow, Wien, 1862, p. 582, XII. 19). "Two frustules exhibited 31 puncta and 90 transverse lines in $\frac{1}{100}'''$, whilst another from the *Kohlbachthal*, though only half the length, showed 29 puncta and about 100 transverse lines in $\frac{1}{100}'''$."

FIG. 15.—*Achnanthes elliptica* m. "The number of the coarse lines in the lower, middle, and upper stations of the Tatra amounts to 35, 38, and 41 respectively in $\frac{1}{100}'''$."

FIG. 16.—*Gomphonema montanum.* "The transverse markings of the shell are canal-formed. In two specimens with 25 canals the shell exhibited 62 fine transverse lines in $\frac{1}{100}'''$."

The author adds that the entire number of species found on the Tatra amounts to 235 at least.

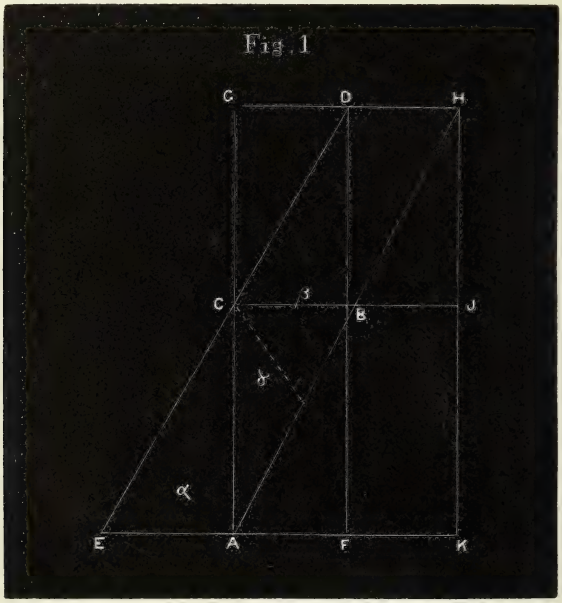
from that of the transverse lines. Consequently the entire structure consists of numerous rectangles, standing upon and near one another.

On the contrary, in other species of *Navicula* the puncta of the next succeeding series are, as it were, put out of place, and not till the third series do we find a repetition of the first.

In this case the structure is divided into oblique-angled parallelograms.

The former case I call the *corresponding*, the latter the *alternating* series.

In Figure 1 let G A be the longitudinal axis of the frustule (or a line parallel to it), and the horizontal lines G H, C J, A K, the so-called transverse striæ, and the lines standing perpendicular to them, the longitudinal striæ.



The structure then consists of rectangles, one of which is A C B F. This I divide by a diagonal into two right-angled triangles, and take the triangle A B C as the basis of the structure.

Let α denote the width apart of the horizontal striæ, and β the width apart of the vertical striæ, and γ the shortest distance apart of the inclined striæ E D, A H, &c.

Ehrenberg more than thirty years ago drew attention to the value of the magnitude α, as he found it, in the same species, pretty nearly a constant. He ranked it therefore as a characteristic

mark of the species, and determined the number of such striæ there should be in a Paris line.

For example, if there be 30 such transverse striæ in $\frac{1}{100}'''$, then

$$30\,\alpha = \tfrac{1}{100}'''; \text{ therefore } \alpha = \tfrac{1}{3000}'''.$$

If we let a denote the number of these transverse striæ in a certain length, taking either $\frac{1}{100}$ of a Paris line, or $\frac{1}{1000}$ of an English inch, or any other standard of measurement, and denote the standard itself by E, then

$$a.\alpha = \mathrm{E}, \qquad \alpha = \frac{\mathrm{E}}{a}, \qquad \frac{1}{\alpha} = \frac{a}{\mathrm{E}}.$$

If we call the corresponding numbers of the vertical and oblique striæ b and c respectively, then

$$\frac{1}{\beta} = \frac{b}{\mathrm{E}}, \text{ and } \frac{1}{\gamma} = \frac{c}{\mathrm{E}}.$$

It follows now simply from Figure 1 that

$$\gamma.\mathrm{AB} = \alpha.\beta, \qquad \gamma = \frac{\alpha.\beta}{\sqrt{\alpha^2 + \beta^2}},$$

$$\frac{1}{\gamma} = \sqrt{\frac{\alpha^2 + \beta^2}{\alpha^2.\beta^2}} = \sqrt{\frac{1}{\beta^2} + \frac{1}{\alpha^2}},$$

or

$$\frac{c}{\mathrm{E}} = \sqrt{\frac{b^2}{\mathrm{E}^2} + \frac{a^2}{\mathrm{E}^2}}; \qquad \therefore\ c = \sqrt{a^2 + b^2}.$$

We see now that the line-number, c, is more directly dependent on the line-numbers, a and b, than γ on α and β. And herein lies the value of the above given definition of the line-number, independently of the fact that the magnitudes a, b and c are to be directly considered, whilst α, β, γ have to be deduced from them afterwards.

We may remark also that the numbers of the lines stand in the same relation to one another as the corresponding sides of the triangle, inasmuch as both are inversely proportional to the heights of the triangles.

Yet (setting aside the three primary series) others also appear, which I call secondary, namely, the series A D, B E, C F.

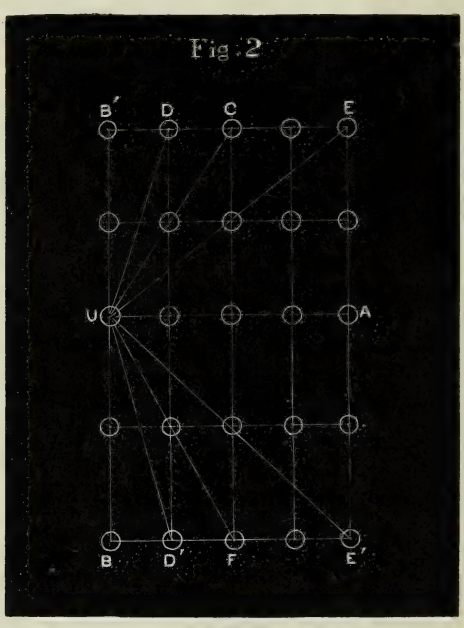
Now, if we call the numbers of the lines corresponding to them d, e, f, we easily find

$$d = \sqrt{a^2 + 4b^2}, \qquad e = \sqrt{4a^2 + b^2}, \qquad f = c.$$

In Figure 2 the direction of the above-mentioned series is denoted by the lines U A, U B, ... U F.

To these, as the figure shows, two others are added, namely, U D′ and U E′.

The two latter we get from the triangle B C G. Moreover the number of the secondary series is not limited to these; for we may divide the structure in many other directions by straight lines drawn through fixed points. Their common line-number is $\sqrt{p^2 a^2 + q^2 b^2}$, where p and q are any whole numbers.



But it is of no practical utility to concern oneself with these, since it is difficulty enough to find out the lines U D, U E, and those corresponding to them, even in coarsely marked shells.

The form also of the dots in this case plays an important part.

Lastly, if the inquiry is after the smallest angles which the lines U C, U D, U E make with the longitudinal axis, the question is one of easy answer. The trigonometrical tangents of these angles are, respectively,

$$\frac{a}{b}, \qquad \frac{a}{2b}, \qquad \frac{2a}{b}.$$

The following genera and species, so far as my observation goes, exhibit *corresponding* series:

Epithemia, *Eunotia*, *Himantidium*, *Meridion*, *Podosphenia*, *Rhipidophora*, *Odontidium*, *Diatoma vulgare*, *Fragilaria virescens* and *elliptica*, *Synedra*, *Tabellaria*, *Gomphogramma*, all species of *Surirella* known to me, in their fine systems of puncta, *Amphi-*

pleura pellucida, Denticula, Nitzschia amphioxys (with less distinct longitudinal lines), almost all species of *Cocconeis*, perhaps all species of *Achnanthidium* and *Achnanthes; Rhoicosphenia curvata, marina, fracta* and *baltica; Cymbella, Cocconema, Encyonema, Amphora*, all species of *Ceratoneis* known to me, *Gomphonema*, almost all species of *Navicula* (and amongst them perhaps *N. firma*), *Amphigomphus dilatata, Scoliopleura Jennerii* (with irregular longitudinal lines), *Scol. convexa*, almost all species of *Stauroneis*, as *Staur. punctata, truncata, Eichhornii, Phœnicenteron* and *amphicephala; Pleurostaurum acutum, Frustulia Saxonica, Doryphora Böckii*, those species of *Pleurosigma* treated of by Smith and Rabenhorst as Section 2, several cylindrical forms, on their front sides, as *Melosira distans, nivalis, salina.*

The formula $c = \sqrt{a^2 + b^2}$ is excellently well adapted for determining the amount of error of estimate.

For example, I found in one specimen of *Pleurosigma attenuatum*,

$$a = 30, \qquad b = 22, \qquad c = 36.$$

If the proportionably easy estimates of a and b be assumed to be exactly correct, then must $c = \sqrt{900 + 484} = 37 \cdot 2$; consequently the relative error of the last estimate would be

$$\frac{1 \cdot 2}{37 \cdot 2} = \frac{1}{31}.$$

Again, three specimens of *Pleurosigma strigilis* on an English slide gave me,

(1)	$a = 25$,	$b = 35$,	$c = 44$, *instead of*	43.
(2)	$a = 26$,	$b = 36\frac{3}{4}$,	$c = 46$, ,,	45.
(3)	$a = 26\frac{1}{3}$,	$b = 38$,	$c = 47\frac{1}{2}$, ,,	$46\frac{1}{5}$.

Smith's estimate is, if we turn his figures into our measurement,

$$a = 32, \qquad b = 35\tfrac{1}{2},$$

and therefore the number of the transverse striæ perceptibly greater. To be sure, I measured the width of the lines in all three instances close to the central knot, and this may possibly have given rise to the divergence.

In *Pleurosigma fasciola* I found $a = 54$, $b = 50$. The oblique striæ in this case I was unable to see.*

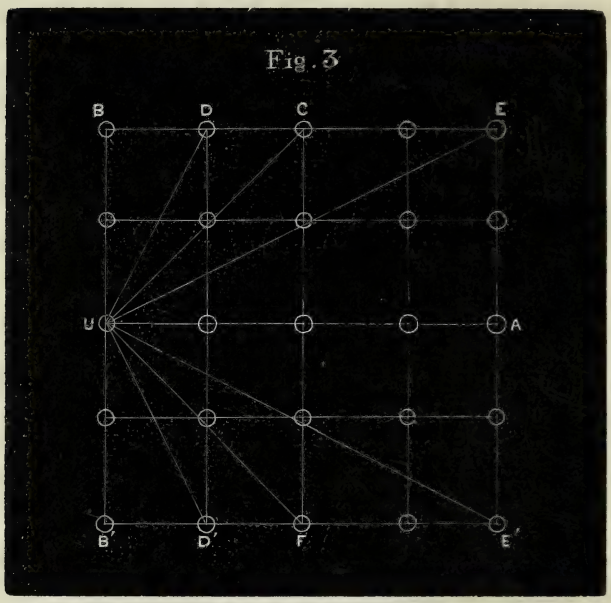
* And no wonder; for there are no oblique striæ on it. In resolution, to know beforehand what to look for is half the battle; therefore drawings of the Diatomaceæ without the striæ are almost worthless.

Special Case.

Not unfrequently the width apart of the vertical series, or lines, is about equal to that of the horizontal lines, $d.h.b = a$.

In this case (for which see Fig. 3),

$$c = f = a\sqrt{2}, \text{ about } \tfrac{7}{5}a.$$
$$d = e = a\sqrt{5}, \quad ,, \quad \tfrac{9}{4}a.$$



The two most distinct of the oblique systems of lines U C and U F form with the axis angles of 45°, and consequently stand perpendicular to each other.

Of the secondary systems, U D and U E′, as also U E and U D′, stand perpendicular to each other, as they form with the axis angles of about $26\frac{1}{2}$ and $63\frac{1}{2}$ degrees.

To this class belong *Epithemia proboscidea, Westermanni, granulata; Himantidium pectinale, Fragilaria virescens, Denticula elegans, Amphipleura pellucida, Stauroneis Eichhornii* and *amphicephala, Pleurostaurum acutum, Pleurosigma lacustre, Melosira distans* and *salina.*

Yet this special case is seldom sharply marked.

I found, for example, in one specimen of *Fragilaria virescens,*

$$a = 38, \qquad b = 36, \qquad c = 54.$$

In both cases the relative error of the third not easy estimate, if we assume a and b to be exactly correct, or $a = b = 37$, is, suppose, $\frac{1}{32}$.

I find the average values to be

For *Tabellaria fenestratata*, $a = 33$, $b = 32$,
„ *Gomphogramma rupestre*, $a = 33$, $b = 29$.

The triangle A B C, which I take as the basis of the structure, is in this case isosceles.*

The directions of the three primary systems of lines are determined by the sides of the triangle, and the transverse lines by C B, and the inclined lines by C D (or A B) and C A.

The three secondary systems of lines have the directions A D, B E and C F. The longitudinal lines D A, P Q, H F, &c., consequently appear here as secondary.

For this structure the following formulæ hold good:

$$d = \sqrt{4b^2 - a^2}, \quad e = f = \sqrt{2a^2 + b^2}, \quad b = c = \tfrac{1}{2}\sqrt{a^2 + d^2},$$

where a denotes the number of the transverse lines, and d the number of the longitudinal lines which appear in a given space,—say $\frac{1}{100}$ of a Paris line.

In Case 1 we found the following rules: If we square the numbers of the transverse lines and of the longitudinal lines, and add these squares, and then extract the square root, we get the line-number for the primary oblique system. Here in Case 2 we must further divide the number so found by 2. This is the essential difference between the corresponding and alternating series and their structures.

If we be in doubt to which class the species of diatom belongs, we may employ this means to decide the matter, when the single puncta are indistinct.

For example, if, in any species, we have found 30 as the number of the transverse lines, and 40 as the number of the longitudinal lines, and lastly, 50 as the number of the most distinctly appearing oblique lines, then we are certain that the series of puncta are corresponding series.

If, in another species, we find 30, 40, and 25 as the corresponding line-numbers, the structure consists of alternating series of puncta.

If we take $\phi_1, \phi_2, \ldots \phi_6$ to denote the smallest angles which the six systems treated of form with the axis, then we get

$$\phi_1 = 90^\circ, \quad \phi_2 = \phi_3 = \frac{A}{2}, \quad \sin.\,\phi_2 = \sin.\,\phi_3 = \frac{a}{2b}, \quad \phi_4 = 0^\circ,$$

$$\sin.\,\phi_5 = \sin.\,\phi_6 = \frac{3a}{2\sqrt{2a^2 + b^2}}, \quad \tan.\,\phi_5 = \tan.\,\phi_6 = 3\tan.\frac{A}{2},$$

* The diagrams pertaining to this part have been unavoidably omitted for typographical reasons.

where A denotes the angle B A C, which one may easily get from the estimated line-numbers, since $\sin. \frac{A}{2} = \frac{a}{2b}$.

Whilst diatoms of an extended form mostly exhibit corresponding series of puncta, those whose obverse sides are developed about a point not unfrequently exhibit alternating series.

First Special Case.

When $A = 60°$, then the triangle A B C is equilateral (see Fig. 5).*

In this case,

$$a = b = c, \qquad e = f = g = a\sqrt{3}, \text{ about } \tfrac{7}{4}a,$$
$$\phi_1 = 90°, \qquad \phi_2 = \phi_3 = 30°,$$
$$\phi_4 = 0°, \qquad \phi_5 = \phi_6 = 60°.$$

The three primary systems cut one another at an angle of 60°; the secondary likewise. The latter stand perpendicular to the former, and may be easily found in consequence.

To this class belong most of those species of *Pleurosigma* which Smith and Rabenhorst treat of as Section 1.

Further:

Biddulphia turgida and *radiata* (Syn. LXII. 384, 385).
Triceratium favus (Syn. XXX. 44).
Podosira Montagnei (Syn. XLIX. 326).
Melosira subflexilis, *orichalcea*.

Yet even here some notable deviations from the uniformity of the numbers of the lines occur. I found, for instance, in a specimen of

Pleurosigma angulatum, $a = 44$, $b = 46$, $c = 46$,

and in

Pleurosigma strigosum, $a = 46$, $b = 46$, $c = 39$.

In the latter case, according to my estimate, the angles of inclination also of the two oblique systems were sensibly different.

Second Special Case.

When $A = 90°$ (for which case see Fig. 6),* then

$$b = c = \frac{a}{2}\sqrt{2}, \text{ about } \tfrac{7}{10}a, \qquad d = a,$$

$$e = f = \frac{a}{2}\sqrt{5}, \text{ about } \tfrac{9}{8}a, \qquad \phi_1 = 90°,$$

$$\phi_2 = \phi_3 = 45°, \qquad \phi_4 = 0°,$$
$$\tan. \phi_5 = \tan. \phi_6 = 3, \qquad \phi_5 = \phi_6, \text{ about } 71\tfrac{1}{2}°.$$

* See footnote on preceding page.

If we compare the special case of 1 with this, we find there $c = a\sqrt{2}$, and here $c = \frac{a}{2}\sqrt{2}$, a ratio which furnishes us with an excellent means of distinguishing the structure of different diatom shells.

To this class belong *Navicula tumens* and *Nav. sphærophora*, Wien, 1860, II, 34; *Cocconeis decussata*, Ehrenb. Amer. II. vi, 13; *Cocc. placentula* and *oceanica*.

Many species of *Pleurosigma* at least approximate to this case, as the inclination of their oblique systems of lines to the middle line does not amount to 60°, but sinks towards 45°.

As a case in point, I mention *Pleur. elongatum*.

One specimen of *Stauroneis nobilis* m. gave

	$a = 30$,	$b = c = 21$;
Another gave	$a = 27$,	$b = c = 19$;
A third gave	$a = 31\frac{1}{2}$,	$b = c = 21\frac{1}{2}$.

As $21 . \sqrt{2} = 29 \cdot 7$, and $19 . \sqrt{2} = 26 \cdot 9$, and $22\frac{1}{2}\sqrt{2} = 31 \cdot 8$, the measurements agree right well.

Let this much suffice as a *specimen* of Schumann's *modus operandi*. Those who are desirous of a further acquaintance are referred to the original work.

PROGRESS OF MICROSCOPICAL SCIENCE.

Minute Structure of the Brain of the Insane.—This is a subject of considerable importance, the more so as some of our best authorities deny that the brain of the insane is materially different from that of the perfectly healthy man. The 'Medical Record' for April 14 gives an account of a paper on this subject by an American physician, which is of interest. It is as follows:—Dr. Walter Kempster, of the Northern Asylum for the Insane, Oshkosh, Wisconsin, presented to the Chicago Society of Physicians and Surgeons some "Notes on the Microscopical Appearances of the Brain of the Insane," based on the examination of forty-nine cases. For a number of years Dr. Kempster has been making systematic microscopical study of the brain, and has examined the lesions of all forms of insanity, from acute mania to dementia, including puerperal and epileptic insanity. In each and all forms he has found a marked lesion—so that certain lesions may be grouped together as common to certain forms of insanity, to which lesions any particular type of insanity is palpably due. There is a wide difference between the lesions of acute and of chronic mania.

1. In certain forms of insanity, and notably in dementia, the finer capillaries show marked indications of disease. The perivascular sheath surrounding the vessel is distended; so much so, that sometimes the vessel itself appears to lie in a tunnel, its calibre being much less than the sheath, doubtless due to repeated capillary congestions of the vessels—often diseased—irregular in calibre, suggesting the idea of aneurismal dilatations, but entirely distinct from the miliary aneurisms ably described by Charcot.

2. Next there is a degeneration, best studied in cases of dementia of syphilitic origin, and in the medulla oblongata, in the wall of the capillary, presenting dark-red patches at various points outside its walls, which gradually thicken, and appear to be due to a fatty metamorphosis or atheroma. The description by Meynert, though accurate, is by no means so complete as could be desired.

3. In 1871, while examining a section taken from the grey and white matter of the third left anterior convolution, there was found a peculiar appearance of the tissue. Situated in the white substance, but very closely to the grey matter, there were a number of small white spots, some round, some ovoid, clearly defined, in sharp contrast with the nerve-tissue, varying in size, from $\frac{1}{50}$ to $\frac{1}{200}$ of an inch in diameter. These appeared to be of a granular consistence, and much more dense in structure than the surrounding brain-substance; each was disconnected from the others, and normal white matter intervened. They did not absorb carmine, and were not connected with the capillaries. On the surface of some of the spots are fibres of connective tissue and crystals of margarine. To determine the true character of these spots and the degeneration, very elaborate and extensive microchemical manipulations were made. On allowing a section to dry, either with or without the nitric acid treatment, these spots appear to

project above the surface of the section. By teasing, they may with difficulty be removed. None of these spots have been observed in the grey matter. They are most numerous in the medulla oblongata, and may be found in the white matter of the spinal cord.

4. There is another form of degeneracy, one which was found in the cases of acute mania. The spots are less in size; are far more numerous than in the other variety (3); resist carmine staining; do not possess the granular characteristic; there are no spindle-shaped fibres of connective tissue about them; they behave very differently under the micro-chemical tests applied to the other variety of spots. The points of resemblance are mainly in colour and apparent density. Neither of them have any investing membrane.

5. A fifth variety, as large in size as the third, possesses a dense investing membrane, which resists carmine staining and is less granular than the third and fourth. It exists in the same brain with the fourth variety. These spots or masses of the fifth variety are called "colloid," because of their resemblance to such growth, and are found in the medulla oblongata and pons Varolii. The last three varieties of degenerated masses, or spots, have one feature in common, a well-defined edge, a clean-cut margin, easily made out.

6. A sixth variety, common in cases of dementia, and where the atheromatous capillary is found, is one in which the mass passes insensibly into the surrounding normal tissue. This form is larger and less distinct than the others. It more nearly resembles normal brain-tissue. Sometimes these masses are lobulated. They are granular and dense, less numerous than in the other varieties, and do not appear in clusters. They appear to destroy or transform the tissues, and if surrounding a capillary, destroy its walls. A point of resemblance in common with the third variety is, that connective-tissue fibre appears in both.

The condition of the cellular structures of the brain, of the nerve-fibres and so-called lymph-spaces, are all fields rich in results not here spoken of.

The Blood-globules of a Nemertian Worm.—The 'Academy' (May 8) says that M. Marion, in a communication to the French Academy, describes a nemertian worm, *Drepanophorus spectabilis*, as possessing "a vascular apparatus which exhibits the surprising peculiarity of containing elliptical, slightly flattened red globules, like those of human blood. Their largest diameter is 0·01 mm. . . . When a portion of the body of the worm is pressed, these corpuscles accumulate in certain parts of the circulatory system, and form a mass of an intense red colour. The movements of the globules can be followed by viewing the animal as a transparent object. They are put in motion by a colourless liquid, in which they float in a constant direction. The animal possesses a median dorsal vessel, and two lateral ones, situated on the ventral side. Below the nervous ganglions the dorsal vessel bifurcates, and anastomoses with the two lateral trunks, which follow the posterior margin of the superior ganglions, and are prolonged into a cephalic ansa. The dorsal canal gives rise to transverse ansæ, regularly spaced. Each of these branches continues to

the flank of the creature, then curves back towards the ventral surface and opens into the lateral vessel. There are, consequently, numerous capillary ramifications, exceptional amongst the nemertians, but recalling the arrangement described by M. Blanchard in *Cerebratulus Liguricus*." "The structure of the highly developed proboscis necessitates the establishment of a special genus for these nemertians;" and M. Marion adopts the name "Drephanophorus" proposed by Mr. Hubrecht.

The Character of the Starch-granule.—Professor Harrington, in a paper in the 'American Naturalist" for April, says that if we take a little of the starch from the potato and dry it, without the addition of water, at a temperature of perhaps 150°, we shall see a dark point appearing at one end—usually the smaller. This is the nucleus, and around it are arranged concentric rings. It has been described as a little pedicle or stem by which the starch-grain is attached to the cell-wall. This was when it was still thought that the grains budded out from the wall, a theory completely disproven now, by what is known of the development and functions of the wall, as well as by specific observations on the formation of the grains themselves. The nuclei have been described too as holes, passing into the interior from the outside, and admitting the materials from which the successive layers were formed from without inwards. If the development of the starch-grain were endogenous, there might be some ground for this hole-theory of the nucleus, but it is now well proven that their formation is from within out, or exogenous. There is no easily accessible specimen at this season of the year to illustrate this, but writers generally refer to ripening corn, where all the stages can sometimes be seen in a single grain. However, we can easily prove with the specimens under examination that the nucleus is neither a little stem nor a canal. If it were either, it would appear, as we roll the grains, sometimes elongated. As we roll the grains over, by inclination of the stage, or pressure from one side, as before, we see no difference in the shape of the nucleus. It is the same round or angular black spot, occupying the same position from whatever point it is viewed. If we are lucky, we may get a grain up on end, and examine it in the direction of its long diameter. The position of the nucleus and arrangement of the rings remain the same.

What can we conclude concerning the nature of the nucleus from this? It was indistinctly or not at all visible in the fresh grain; it becomes visible on drying, and looks like an air space. It is in the structural centre of the grain. If the drying is carried far enough, cracks may be seen extending from the nucleus. They generally radiate, looking something like a star. Sometimes one long crack runs the greater part of the length of the grain. The cracks may and may not reach the surface. Taking all these facts together, we can draw the fair conclusion: that the layers differ in density; that the inner layers are softer than the outer, because they contain successively more water; that the water is driven off by the heat and the consequent vacuity appears, forming the "nucleus," where there is most water, that is, in the innermost layers; that farther drying

causes cracks to appear in the harder layers, the longer the drying the more extensive the cracks.

The Minute Structure of the Ovule.—Three or four of the last numbers of the 'Botanische Zeitung' have been almost wholly occupied with an article of Celakovsky's, entitled "Vergrünungsgeschichte der Eichen von Alliaria officinalis," or history of the development of phyllody in the ovules of the plant named. The morphological dignity or nature of the ovule is still a moot point with physiologists, who are by no means agreed as to the significance of the monstrous conditions and transformations of this organ observed occasionally in different plants. The most logical view seems to be that it is of the same nature in all plants, though the explanations offered for different teratological phenomena exhibited by the ovule would point to a diversity of origin and dignity. Celakovsky seeks to throw some light on this subject by a careful and minute study of the different phases of transformation or malformation observed in the ovules of a proliferous inflorescence of *Alliaria officinalis.* He objects to the assumption that because the ovule sometimes develops as a shoot, the nucleus is a bud. Through this long paper he describes and figures the various modifications he has found of the leafy transformations of the ovular coats and a "funicular appendage" and the presence of a bud. He endeavours to show that the ovular shoot is not a metamorphosed state of the nucleus, and says here we have an indisputable proof that the ovular shoot is not a transformation of the nucleus, and every explanation that the latter is anything more than an outgrowth or metablast must fail. In his investigations he believes he has found the nucleus present in the same ovule in which the bud is developed, and quite independent of it.—*The Academy.*

The Microscopic Appearances in Inflammation of Connective Tissue.—Dr. G. Thin has communicated a valuable paper on this subject to the Royal Society. The following abstract of his views is taken from the 'Proceedings of the Royal Society' (No. 160). The author, referring to observations recorded in his previous papers, distinguishes in the cornea primary bundles of fibrillary tissue, which are covered by elongated flat cells, layers of quadrangular flat cells (which are analogous in appearance and relative position to the layers of cells described by him as investing the secondary and tertiary bundles of tendon), and the stellate cells. To these he now adds a description of parallel chains of spindle-cells, each cell having two processes, one at each end of the spindle, by which it is joined to its fellows on either side. These cells are coextensive with the cornea substance, and are present in every interspace of the primary bundles, and consequently layers in different planes cross each other at an angle.

They can be occasionally seen in thin vertical sections of the fresh frog's cornea, treated in osmic acid; and from such preparations a cell with its terminal processes can be sometimes isolated. They are more easily seen in similar sections which have been 15–30 minutes in half per cent. solution of chloride of gold, and then sealed up in concentrated acetic acid and examined 24–48 hours afterwards.

They have no anatomical continuity with the stellate cells.

In the fresh frog's cornea examined entire in serum, the structure, looked at through the anterior epithelium, can be seen to be broken up by clefts, the borders of which have a double contour. These clefts extend from the epithelium to a varying depth into the fibrillary tissue. They are arranged sometimes concentrically, and sometimes in waving lines, which give off branches which are narrower as they approach the centre of the cornea. The double-contoured borders are not parallel to the median plane of the cornea, and can be traced only by changing the focus.

From the existence of these clefts the author infers a division of the cornea substance into compartments equivalent to the secondary and tertiary bundles of tendon.

In inflammation the clefts are much widened, and their finer ramifications become visible. In preparations of inflamed cornea different tracts of cornea substance bounded by the clefts are coloured of different shades by chloride of gold, the difference affecting the fibrillary tissue, and more markedly the spindle-cells.

The serous contents of the interspaces of the inflamed cornea differ in character from those of the healthy cornea, inasmuch as the former show, more abundantly, the dark granular substance which results from the reduction of the chloride of gold.

In a very early stage of inflammation (after a few hours) the distension of the narrow spaces between the primary bundles and of the wider and more yielding spaces between the lamellæ, corresponding to the larger bundles, favours the action of chloride of gold; and preparations can thus be obtained by this reagent which show that the two kinds of flat cells which cover the respective surfaces are arranged after the manner of an epithelium. The cells thus seen can be identified by their size, contour, and arrangement, as those which are isolable from the healthy cornea by warm saturated solution of caustic potash, and which can be seen in preparations sealed up in aqueous humour.

A similar distension occasionally permits the demonstration of the layers covering the secondary bundles of tendon.

That the successful gold reaction in such cases is probably due solely to the distension of the interspaces, is inferred from the fact that in the tendo Achillis of frogs which have died from disease, and have been some hours in water after death, the author has obtained gold preparations showing not only the cells of the secondary bundles (Ranvier's cells), but also small groups of the long narrow cells which cover the primary bundles.

In the cauterized frog's cornea, examined in blood-serum after twelve hours' inflammation, portions of the primary bundles are found lying loose on the surface. These detached portions have a nearly constant length, a uniform breadth, sharply-defined even borders, are sometimes puckered transversely, occasionally show a faint appearance of longitudinal fibrillation, and are sometimes cut transversely, at one or more points, by straight hyaline lines. They resemble accurately the primary bundles of the neurilemma of the sciatic nerve and the rods of the retina of the healthy frog.

They stain deeply in gold preparations, and are then always puckered transversely.

In gold preparations of the inflamed frog's tongue, isolated primary bundles, identical in appearance and breadth with those of the inflamed cornea, are to be found.

The depth of staining by gold shows that the constituent elements of the primary bundles undergo a chemical change in inflammation.

The author has studied, by means of chloride of gold, the effects of inflammation in the quadrangular and in the long flat cells which cover the bundles in the interior of the cornea, but chiefly in frog corneæ sealed up in blood-serum, the latter method being found more certain to give available preparations.

The only appearance observed, anterior to a complete destruction of the cell, was a division of the nucleus into two or more parts. In serum preparations the products of the division assumed the form of circles of highly refractive particles. Similar particles were sparsely scattered in the substance of the cell.

The area of any one circular product of this division was always much smaller than that of the undivided nucleus.

In regard to the stellate cells, the author questions the correctness of the accepted theory, which implies an identity of the cell and its processes with the visible protoplasm. He considers that the refractive particles, which constitute what is visible in the cellular protoplasm, are suspended in a fluid, similarly to the pigment-granules in the pigment-cells as described by Mr. Lister. The phenomenon described by German investigators as "Zusammenballen" of the cell-processes, he attributes to a collection of the protoplasmic particles in the centre of the cell, similar to that which takes place in concentration of pigment. This opinion is borne out by a comparison of gold and osmic acid preparations. In conditions in which, by the former process, an isolated globular body is seen, osmic acid preparations show that the anastomosis of the thread-like processes remains complete. Reasoning analogically from the results obtained by gold in other tissues, he infers that it is what may be described as the contents of the cell and processes which stain by that method.

Treatment by osmic acid is the only reliable method by which he has obtained satisfactory preparations showing the stellate cells in the inflamed cornea. The advantages of this mode of treatment are much enhanced by subsequent staining with red aniline, which especially differentiates the protoplasm and processes. Subsequent staining by hæmatoxylin renders the nuclei visible.

The only change, except that of destructive disintegration, observed by the author as a consequence of inflammation in the stellate cells, consists in the anastomosing processes being, in gold preparations, occasionally represented by fine darkly-stained lines, on which are a series of small globular swellings placed at short regular intervals, giving any one process an appearance identical with that presented by an ultimate nerve-fibrilla in a gold preparation. The same appearance is also to be seen in osmic acid preparations, and is suggestive of points of communication between the lumen of the process and the

interfibrillary space. (This is the only form in which the author has seen the processes of the stellate cells in inflamed corneæ in gold preparations. They are usually invisible by that process.)

Appearances indicative of a dividing nucleus were rarely seen, and their interpretation is doubtful. Both in respect to the nucleus and the processes the stellate cells are the most stable of all the cellular elements of the cornea.

Between the layers of the superficial corneal epithelium a network of stellate cells can be seen in serum preparations of inflamed cornea. Indications of similar cells can be seen in gold and hæmatoxylin preparations of the healthy cornea.

In inflammation the cells of this network show a very great increase in size as compared with their appearance in health.

The changes produced by inflammation in the spindle-cells may be divided into three stages:

(*a*) Preparations examined in serum show that the cell-protoplasm has become increased in amount, and that the cell-processes can be distinctly traced. This stage can be observed after twelve hours' inflammation, resulting from slight cauterization in a winter frog. The swelling of the protoplasm is often confined to one or more tracts of the cornea, one of the above-mentioned clefts separating the area of this appearance from that of the normal cornea. The area extends from the neighbourhood of the cauterized part towards the limbus.

(*b*) The swelling of the protoplasm extends along the processes from one cell to the other, a chain of spindle-cells being often represented by a long column of protoplasm on which there are very slight constrictions. This description applies to osmic acid preparations. Deep staining with red aniline and subsequent treatment with acetic acid renders the nuclei visible in this protoplasmic column. This stage is well seen in osmic acid preparations of a rabbit's cornea which has been twenty-four hours inflamed by the passing of a thread.

(*c*) With more or less increase in the amount of protoplasm, and with or without its presence in the processes in a granular form, nuclear bodies (resulting from a division of the nucleus) are seen in osmic acid preparations to be contained in, or partly expelled from, the cell, which are identical in appearance with the red blood-corpuscles seen in the new vessels in the same preparations. This identity in appearance is further maintained by staining osmic acid preparations with red aniline, in which the nuclear products and red blood-corpuscles are stained a like tint and deeper than the other elements. The author infers from these appearances that in inflammation the nuclei become free bodies, which are equivalent to red blood-corpuscles.

The appearances described by Key and Wallis, Cohnheim, and others as white corpuscles in "Spindelform," are seen in osmic acid preparations to be spindle-cells made more prominent by inflammation.

The "spiessartige Figuren" seen in gold preparations are produced by the protoplasm which immediately surrounds the nuclei of the spindle-cells being visible, whilst from the mode of preparation the connecting processes are invisible.

White blood-cells in the inflamed cornea can be identified with

most certainty in osmic acid preparations. They are found in groups in the wider spaces, in rows in the nerve-channels and between the primary bundles (corneal tubes of Bowman), and in large numbers in the tracts between the larger bundles. They are mostly round, sometimes club-shaped, never pointed at two extremities as an elongated shuttle-shaped mass (that is, never *spindelformig*, *spiessartig*). A small minority consist of a double body formed by two rounded globular masses joined by a smooth isthmus. When stained by hæmatoxylin, nuclei are found in either end, but not in the isthmus. The author infers that we have here a corpuscle in process of division.

In rabbit corneæ, in which inflammation has lasted about a week, some white corpuscles are seen with uneven contour; and bulging outwards from, or lying close beside, them are bodies evidently nuclear, and which are affected by osmic acid and subsequent staining with red aniline, in a manner identical with the red blood-corpuscles seen in blood-vessels in the same preparation. The identity of the escaped nuclei with red blood-corpuscles is shown by a comparison of their respective size, evenness, colour, and contour.

The author infers a production of red blood-corpuscles in inflammation from the nuclei of the white blood-cells.

In observations on human blood, and that of the mouse, by staining with hæmatoxylin, he has found that while the great majority of the red corpuscles do not quickly stain in a weak solution, there are some which at once stain a deep blue, and that there are white corpuscles in which a narrow protoplasmic margin encloses a deep blue nucleus similar in contour and size to the stained red corpuscles. Amongst the red corpuscles of the frog are a minority which are recognized as being red corpuscles by their size, smooth contour, and absence of granulation, but in which there is no hæmoglobin, and the nucleus quickly stains blue in solution of hæmatoxylin, like that of the white cells.

Transitions occur in which a less and less capacity of staining on the part of the nucleus takes place, *pari passu*, with an increase in the colour characteristic of hæmoglobin in the body of the cell. In the fully developed red corpuscle, the nucleus stains only after it has been for some time in contact with a weak solution of hæmatoxylin.

The author has observed in the blood of the mouse fœtus the nuclei of the nucleated red blood-cell escape from the larger cell, and then become indistinguishable in form and appearance from the small red corpuscles of the mature animal present in the blood under examination.

These observations, taken in connection with the bodies that are formed in the spindle-cells and white corpuscles in inflammation, support, as the author believes, the doctrine of Wharton Jones, in regard to the formation of the red blood-corpuscles.

The mode of formation of capillary blood-vessels he believes to be identical in inflamed and in fœtal tissue. In studying this subject he has found special advantages from the use of osmic acid, with or without subsequent staining in hæmatoxylin. The stages in this formation are as follows.

(*a*) The spindle-cells enlarge and contain several nuclei which can be identified, whilst within the cell, as being of a similar nature to red blood-corpuscles. A current of blood-plasma from the nearest vessels passes, at the same time, into the interfibrillary space in which the spindle-cells lie.

(*b*) The nuclei escape from the spindle-cells into this space, where they are indistinguishable in appearance from the ordinary red blood-corpuscles.

(*c*) By a process of diapedesis the formed elements of the nearest blood-vessels pass into this space and the circulation is established.

Various appearances lead the author to suppose that the fibrine of the plasma solidifies on the outer surface of the current and forms the substratum of the new vessel, and on this substratum the white blood-corpuscles fix themselves and spread out as an epithelium.

From interfibrillary spaces in the inflamed cornea, in which formation of blood-vessels was actively taking place, the author has isolated white corpuscles in various transition stages towards the appearance and shape of epithelium; and, from rapidly enlarging vessels, cells which, from their form, he believes to be transitionary to that of smooth muscular fibre.

As the new capillary forms, the enlarged spindle-cells decrease to their ordinary size.

In preparations of blood-serum of the frog sealed up, after a few days, the hæmoglobin may be observed to assume special forms inside the corpuscle, or to disappear from it, and so produce changes in the appearance of the corpuscle identical with those described by Arnold as taking place in the tongue of the living animal after diapedesis.

The above observations were made chiefly on the cornea of the frog and rabbit; and the inflammation was mostly produced by solid nitrate of silver, the passing of a thread, and the application of methylated alcohol.

In the winter frog (*Rana esculenta*), cauterized in the centre of the cornea, the first entry of white corpuscles attributable to inflammation was observed, after forty-eight hours, in the wider spaces near the limbus. After four days they could be observed in considerable numbers, and 2–6 could be seen in one so-called space (*lacuna*).

The Heart of the Snail.—Dr. M. Foster and Mr. Dew-Smith, who have been conducting some interesting physiological experiments on the snail's heart, give the following account of its structure.*

While the auricle is a sac, with quite thin and smooth walls, the bundles of fibres in the ventricular walls bulge out largely into the cavity, and are so arranged that the ventricle has the same spongy structure as that of the frog and many other animals. Neither in the auricular nor ventricular wall can the presence of any nerve-cells, or collection of nerve-cells in the form of ganglia, be detected, whether in fresh specimens or in those treated with various reagents. The interlacing intricately-arranged bundles of fibres are composed of a granular protoplasmic-looking tissue, quite unlike the ordinary mus-

* Vide 'Proceedings of the Royal Society,' No. 160.

cular tissue of the body, and in many ways resembling the cardiac tissue of vertebrates. The walls are thickly studded with nuclei, some of which possibly belong to an external tesselated epithelium. Other nuclei are undoubtedly the proper nuclei of the contractile elements, and the remainder seem to be of the nature of connective tissue. Of none of them can it safely be said that they are the nuclei of nerve-cells. Molluscan nerve-fibres might undoubtedly, unlike vertebrate medullated nerve-fibres, easily escape detection; but Mr. A. S. Lea, of Trinity College, carefully examined for us the whole of both the auricle and ventricle without discovering any distinct nervous structures. He also went systematically over the margins of both the aortic and pulmonary orifices, but could find no nerves running into or out of the heart. In no other way could nerves become connected with the heart. And, opposed as it may seem to general experience, and still more to recognized opinions, we are led to the conclusion that the heart of the snail has no nervous connection with the rest of the body; nay, more, that it has within itself no distinctly specialized nervous mechanism, but that its contractile elements are composed of protoplasm, arranged, it is true, more or less in fibres, yet otherwise but slightly advanced in differentiation.

Structure and Motion of the Spermatozoa.—These have been thoroughly investigated by Dr. T. H. Einer, who has published an important paper on the subject. He says:—The spermatozoa of dwarf flying-mice show peculiarities which have also been observed in other mammalia. The head and centre of these spermatozoa are unusually broad, and are not continuous, but are interrupted by a very fine thread, which is sometimes quite long, so that this thread can for convenience be called the throat. In the night-flying mouse this thread is ·0007 of a millimeter in length. In those cases where there is no throat to be seen, there is at least a division in the body, and we can easily suppose one to exist, but our lenses are not powerful enough to reveal it. A small line is seen in the middle of the centre, continuing through the throat into the head and anterior extremity. Sometimes this fine line can be seen as a separate line as it passes through the throat-piece.

On the spermatozoa of *Vesperugo pepestrellus*, cross-lines are often seen upon the throat-piece, dividing it into three or four square pieces, held together by the fine central thread. On others of the same individual, this appearance is not seen. The addition of improper fluids arrests the motion of the spermatozoa, and then these peculiar markings are no longer seen. In other species of mammalia, something of the above structure could be found, viz. in horses, mice, and guinea-pigs, oftener in cattle and *Erminea mustela;* also sometimes in the dog and cat.

In man he has often failed to find the line in the middle piece, but the fine joints were frequently made out in the throat-piece.

The author has not seen the central thread in the head or centre-piece of the spermatozoa of any mammalian in perfectly natural semen.

But some observations on not quite fresh semen seemed to show that the central thread in these parts was masked in the fresh state

by something opaque; and in every case of dog's spermatozoa and in a single case of man's, where no central thread was seen, it could be rendered visible. On the spermatozoa of the cat and hare there is a bound-up appearance at the situation of the middle piece.

These general observations, together with the conduct of the spermatic elements of the flying mouse, show that the central thread must be a peculiar element of the spermatozoa of mammalia; and that the middle piece consists of a central thread and of a protoplasma cloak covering the same, which cloak is very often divided into little cubes. This cloak is a remnant of the original formative cell of the spermatozoa. This appearance is common also on normal spermatozoa, and is often crowded in rumples on different parts of the middle piece, especially on the anterior end.

The tail also of mammalian spermatozoa consists of a fine thread and protoplasma covering which is wanting in the extreme end of the tail. We can follow the centre thread in some flying mice quite to the end of the tail.

In the night-flying mouse, cat, and hare, the extreme end of the tail is thicker than the central thread, and is always, in all mammals, thicker than the very thin throat.

Secondly, the author speaks at length of the motion of these bodies, their kind and cause.

In the triton and salamander the spermatozoa have a finny edge running along the whole length on both sides. These edges move in screw-like windings commencing at the back of the head and extending to the tail in quick succession, and a steady and uniform motion is imparted to the body.

In spermatozoa which have no finny edge the forward motion is produced by rapid turning upon the long axis. The extreme end of the tail goes round in circles, and in this way turns the whole body. The rate of propulsion is in proportion to the rapidity of tail-rotation. The tail does not always turn in the same direction. When turning very slow the rotations can be counted.

The author finally says:—The motion of the spermatozoa of mammals and other vertebrates is on the principle of a screw. The streams in the protoplasma rush to the end and cause the end to rotate, whence the whole body is turned and the forward motion produced.—*Virch.* '*Physik. Med. Ges.*' in Wurtzburg, vi., 3, p. 93.

Observations on some Marine Rhizopods.—At the meeting of the Academy of Natural Sciences of Philadelphia, on March 16, Professor Leidy remarked that he had spent a short time last August at Noank, on the coast of Connecticut, where Professor Baird was then engaged in pursuing his inquiries and investigations as United States Commissioner of Fisheries. Through the kindness of Professor Baird he had been enabled to make a few observations on some marine Rhizopods.

Some years ago, on the beach at Newport, R.I., he had noticed that the ripple marks of the sand were crested with white particles, which could be scraped up by the handful, and which he at first viewed as the pulverized débris of various calcareous shells. On closer exami-

nation the material was found in large proportion to consist of the dead shells of Foraminifera. The immense quantities of these remains, extending in innumerable ridges over the broad expanse of the beach, had led him to suspect that he would find them living in the greatest profusion in the dredgings off the coast of Noank. In this view he had been disappointed, though many living individuals were obtained in dredging, adhering to hydroids, sponges, and the roots of fuci. The number of species observed was small, though the individuals of several of them were numerous. In the best condition, and especially abundant, were two Foraminifers, a Miliola, and a Rotalia, exhibiting some variety of form.

The Miliola resembles the *Quinqueoculina meredionalis* of Dorbigny, and is probably the same species. The shell, from $\frac{1}{8}$ to $\frac{1}{6}$ of a line in breadth, is white and more or less translucent, or is colourless and transparent. It exhibits five compartments or cells, in the mouth of the last and largest of which there is a blunt, conical tooth. The interior soft structure was yellowish brown, or pinkish brown, darkest in the smallest cell, successively lighter in the others, and sometimes nearly colourless in the last or largest cell. In the last cell, and less frequently in the second cell, the soft matter exhibited many globules of transparent, colourless liquid. In the active condition the animal protruded a multitude of exceedingly delicate pseudopods, which, radiating from the mouth, ramified and frequently anastomosed in the most intricate manner, as usual among Foraminifers.

The Rotalia is a beautiful, spiral, many-chambered shell, from the $\frac{1}{10}$ to the $\frac{1}{6}$ of a line in breadth, and strongly resembles the *Rosalina varians*, as represented in fig. 8, plate iii., of Schultze's Polythalamien. The shell is white and more or less translucent, and is composed of from twelve to eighteen cells. The soft structure within is dark reddish or yellowish brown in the smallest cells, light brown or yellowish in the larger cells, and faintly yellowish or colourless in the largest cells. Pseudopods radiated everywhere from the minute pores of the shell.

A few Polythalamous shells were observed, which appeared to be composed of particles of sand cemented in the same manner as in the fresh-water Difflugians. One of them was a spiral shell, like a Rotalia, composed of eighteen cells, and measuring about $\frac{1}{7}$ of a line in breadth. The soft structure within the smallest cells appeared to be amber-brown.

Another of these arenaceous shells resembled in its shape and the alternation of the cells the *Textilaria agglutinans* of Dorbigny, of the West Indies. A specimen of thirteen cells was about the $\frac{1}{10}$ of a line long by $\frac{1}{16}$ of a line at the broad end. The soft structure was reddish brown within the smallest cells, becoming successively lighter in the larger cells, until in the last or largest it was colourless, or nearly so.

A third form consisted of a straight or slightly bent series of cells, for the most part oblate spheroidal, and successively increasing in size. The first cell is globular and larger than the few succeeding ones. The last or largest cell is more of a conical form. The interior

structure was faintly yellowish or nearly colourless. A specimen of eighteen cells was $\frac{1}{4}$ of a line long, with the last cell about $\frac{1}{20}$ of a line in diameter.

An interesting Rhizopod, not pertaining to the Polythalamous foraminifers, to which my attention was directed by Professor Verrill, frequently occurred in the mud dredged off the Connecticut coast.

The same creature is referred to by Professor Verrill in the Report of the Commissioner of Fish and Fisheries for 1871 and 1872, page 503, as being extremely abundant in the clear silicious sand dredged from Vineyard Sound.

The creature was discovered by Dr. Sandahl in the Bohnslaüs Archipelago, and is described in the Ofvers. K. Vetensk. Ak. Forh., Stockholm, 1857, 301, under the name of *Astrorhiza limicola.* It is also referred to in Thomson's 'Depths of the Sea,' p. 75, as occurring in the Atlantic ooze off the Faroe Isles.

The case of this Rhizopod is constructed of angular particles of quartz sand, cemented by tenacious matter mingled with the finest dark-coloured mud. The body of the case is discoid or lenticular, with a number of short cylindroid processes radiant from the margin, giving the case altogether an irregular stellate form.

Sandahl describes the shell as exhibiting scattered yellowish-brown spots, unequal, irregular, and somewhat shining. These spots, in the specimens examined by me, are due to the translucent quartz particles through which the yellowish colour of the interior soft structure of the animal is seen. Sandahl gives the number of radii from 10 to 15, and the size of the case from 3 to 4 lines. Our specimens measured from $2\frac{1}{2}$ to 4 lines, and exhibited radii from 6 to 13 in number.

The interior soft substance of the little mud stars is a viscid, mucoid matter. The ectosarc is colourless. The entosarc was granular and yellowish, sometimes containing ova-like bodies, with darker yellow or orange-coloured contents. Besides these the entosarc contained clear globules and a multitude of diatoms, principally a species of Coscinodiscus.

He failed to see the Astrorhiza in a very active condition, probably from the hot summer weather too quickly giving rise to decomposition in the material collected. Only in two instances did he discover the animal with a number of delicate filamentous pseudopods projected from the processes of the disk. The pseudopods as seen, and as represented by Sandahl, are like those of the Foraminifera.

In the single-chambered character and structure of the case, Astrorhiza resembles the fresh-water Difflugia, but differs in having many orifices, to protrude the pseudopods, instead of a single one.

American Observations on Stephanoceros.—This animal, which has been so well worked out by Mr. Cubitt in these pages, has recently had American attention devoted to it. In the 'Proceedings of the Philadelphia Academy of Sciences' for April, 1875, Mr. C. Newlin Peirce exhibited drawings of a specimen of an aquatic animal, belonging to the genus Stephanoceros, which had been recently

observed by him. In doing so, he said he was induced to bring it before the Academy because it was, he believed, rarely met with in this country, and had not been previously here described.

In its main characteristics, such as spiral carapace or case, five tentacle-like lobes armed with cilia, or, more properly, setæ, eye-spots, jaws, stomach, &c., it corresponds with the description given by Mr. C. Cubitt in his paper entitled "Observations on the Economy of Stephanoceros," in the 'Monthly Microscopical Journal,' vol. iii., 1870, p. 242; but in addition to that very full sketch of this interesting object, there were some points of interest not there recorded. First was the great length of setæ or bristles projecting from the ends of the tentacles (only to be seen by especial care in focalizing with the lens), these overlapping each other formed a network in which were entrapped Paramœcia of various sizes, as many as forty having been observed there at one time. And by virtue of these long setæ the animal's facility for procuring prey was greatly enhanced.

These minute objects which served as food were by a spasmodic effort of the bristles gradually brought within the arms, and from there, with this continued spasmodic movement which has been described by Mr. Cubitt, were brought within the vortex induced by an arrangement of cilia around the mouth, which, unlike the setæ on the tentacles, were, while the animal was feeding, kept in a whorl.

The action of the setæ on the lobes of the Stephanoceros is spasmodic; it creates no vortex, and it is only by actual contact with these setæ that floating particles are whipped within the area enclosed by the lobes, where, by the same whipping action, they are twitched from point to point, irregularly downwards, until they come within the range of a vortex, which is due not to any action of the setæ, but to a range of minute *cilia* in the funnel, distinct from the foraging appliances.

For two weeks the animal under observation fed voraciously. The last few days of this time granular layers were rapidly deposited on each side of the body just within the case, until the upper part of the carapace was distended with this accumulation. For twenty-four hours following this condition but little or no food passed into the digestive cavity; any infusoria or other foreign substance accidentally coming within the tentacles being immediately expelled by a sudden constriction of these organs at their base.

It was evident from appearances that some change was about to take place. The animal, at first very sensitive, withdrawing into its cell on the slightest jar of the table on which the instrument was placed, now but seldom contracted its retractile muscle even though the zoophyte trough, in which it was examined, was quite violently tapped.

On the sixteenth day of observation it was unavoidably left for a few hours; on returning to it the tentacles, with the above-described accumulated dark mass, were found to have left the original case and were attached to a portion of the plant beneath the branch to which it (the original case) adhered. It now presented somewhat the appear-

ance of an animal figured and described by Pritchard as a young Stephanoceros, a dark globular mass with five spreading or divergent tentacles, and at the distal extremity a very slight prolongation by which it was attached to a plant-stem by an almost invisible thread, devoid entirely of any cell or carapace. Not long, however, was it destined to remain in this nude condition, for in twenty-four hours appearances of a cell were visible, and within three days it was domiciled in as beautiful a spiral case as the one it had left. Its contractile muscle developing rapidly with the length of the cell, in a few days it presented to the observer all the peculiarities of the parent, and within two weeks was again ready for another change such as is above described, and which was accomplished with a similar result. The Stephanoceros being too high in the scale of animal life to propagate by gemmation or division, the process above portrayed can have but a remote influence upon reproduction, as there was no multiplication by this change.

The original cell with its retracted body within, though remaining for weeks in an apparently perfect condition, was not seen to increase or in the least to change—the growth-force seemingly being confined to the detached head and its accompanying organs.

Dr. Leidy stated that he had never seen specimens of Stephanoceros until they were shown to him by Mr. Peirce.

NOTES AND MEMORANDA.

Wenham's Reflex Illuminator.—Mr. Samuel Wells writes as follows to the 'Boston Journal of Chemistry' (June, 1875):—Last September I received this ingenious illuminator from London, and examined several slides with its aid. The beautiful effect of a bright and clear illumination of the object, shown on a dark background, as described by the inventor and by Mr. Slack, was very interesting and instructive. I had then but few slides on which I could use it, and laid it aside for further investigation at a future time.

Mr. Wenham describes it in the 'Monthly Microscopical Journal,' vol. vii., p. 236. It was designed for the illumination of such objects, mounted dry, as adhere to the surface of the slide, by rays of light of such obliquity that they cannot be transmitted beyond that point. The illuminator being connected with the slide by a film of water, the light passes through to the upper surface of the slide, and, if there is no object in contact with the slide at that point, is totally reflected; if, however, a diatom or other object rests on the slide at the point where the light strikes, the rays enter the object and are diffused by it so that it becomes in effect self-luminous.

This appears to be the only use for the illuminator described by the inventor. I have, however, lately used it for a different purpose,

and with results quite surprising. While studying high-power objectives of large angular aperture, I experimented with various methods of illumination, and among others applied the reflex illuminator. I find that some immersion objectives are capable of transmitting the extremely oblique rays that pass through the illuminator, so as to give a bright field when used on balsam slides. In dry mounts the light cannot be transmitted beyond the upper surface of the slide, but in balsam-mounted slides the light passes to the upper surface of the cover and is there totally reflected. If an immersion objective is adjusted and connected with the cover by a film of water, the total reflexion will be destroyed, and the light pass through the cover and water into the front of the objective. The ultimate direction of the ray of light after passing through the illuminator is not changed by the introduction of the different media (balsam, glass, and water), and the angle at which it enters the objective must therefore be greater than 41°. In examining Möller's Probe-Platte, a balsam mount, under these conditions, with light from a kerosene hand-lamp, I easily resolved the *Amphipleura pellucida;* so clear and decided were the lines that with a power of 8000 they were still visible. I first obtained this unexpected result with my Powell and Lealand $\frac{1}{16}$th, and the light was sufficiently bright to render it possible to use a $\frac{1}{2}$-inch eye-piece and concave amplifier. As the $\frac{1}{16}$th has the power of a $\frac{1}{20}$th, I obtained, by estimation, a power of 8000 diameters.

The resolution of this difficult diatom, as well as the *Frustulia Saxonica* and *Nitzschia curvula* (Nos. 18 and 19 on the Probe-Platte), far surpasses any that I have ever seen by artificial light, and rivals the beautiful resolution obtained by monochromatic sunlight. With this illuminator it is much easier to resolve the *Amphipleura* in balsam than to resolve it dry with any other artificial illumination.

I find, however, as yet but few objectives capable of transmitting light of such extreme obliquity through the back systems. The Powell and Lealand $\frac{1}{16}$th, as I have above stated, succeeds admirably. My Tolles' $\frac{1}{18}$th immersion gave only a dark field.

Of several others I have succeeded with only three: a $\frac{1}{10}$th made several years ago, a four-system $\frac{1}{6}$th, and a four-system $\frac{1}{10}$th, the last two of recent construction, and all three made by Tolles, and all of course immersion. I have not had access to Continental objectives of wide angle. The three objectives of Tolles last named resolve the whole of the frustule of the *Amphipleura* at once, while the Powell and Lealand $\frac{1}{16}$th resolves only a part at one view, even when the whole frustule is in the field. The advantages of the reflex illuminator in thus furnishing light of greater obliquity than has been obtained by other methods, seem to me worth considering by those interested in testing the resolving power of objectives.

I find it advantageous to connect the illuminator with the slide by glycerine, instead of water, as it does not evaporate. The higher refractive power of glycerine makes no difference in the ultimate direction of the light.

With high amplification the lines of the *Amphipleura* become decidedly beaded, but do not separate into dots.

Powell and Lealand's $\frac{1}{8}$th new Immersion Objective.—The 'Academy' (May 8) says of this glass: "We have had an opportunity of trying one made for Mr. Lettsom, and it is certainly a very remarkable production, able to show very minute structures for which much higher objectives have hitherto been employed. It has a considerable working distance in proportion to the magnification it affords with deep eye-pieces, and gives a wonderful view of diatoms flat enough for its angle of aperture and contiguity to the object. It has also sufficient penetration for small live objects, and has plenty of light with D and E eye-pieces, which cause no noticeable deterioration of its performance when, as should always be the case with high powers, an achromatic condenser is employed."

CORRESPONDENCE.

What are the Characteristics of Frustulia Saxonica?

To the Editor of the 'Monthly Microscopical Journal.'

Denstone, *May* 18, 1875.

Sir,—I am afraid the 'Monthly Microscopical Journal' (vol. ix., p. 86) must have made some mistake, and have misrepresented Dr. Woodward in that paragraph headed "Frustulia Saxonica as a Definition Test." What is there said is by no means very clear; but it certainly does make him assert one of two things: either (1) that *Frustulia Saxonica* is a one-lined object (i. e. has transverse, but no longitudinal lines); or (2) that, though it undoubtedly has transverse, and may possibly have longitudinal lines as well, no one as yet has succeeded in seeing the latter, but that those who fancied they saw them, as Dippel and others, have been deceived by "diffraction phenomena."

If either of these interpretations represents his real views, I would request Dr. Woodward of his patience and courtesy to permit me to make a few remarks.

In the summer of 1872 I called upon Herr Seibert at Charlottenburg for the purpose of procuring one of his high-power immersion lenses. On this occasion, after letting me see what his manipulative skill could accomplish in resolving the tests I had brought with me—and amongst the first was an extremely fine-lined specimen of *Frustulia Saxonica*—he next showed me a couple of very beautiful photographs of that diatom, one of which exhibited the transverse, and the other the longitudinal lines, with far more clearness, sharpness, and distinctness than the printer will be able to reproduce the words I have here written.

Some ten days later I called upon him again between eight and nine in the evening, and before he would let me go he insisted on showing me what the glass I had selected could do on his own private

slide of *Frustulia Saxonica.* This time, instead of bright sunlight, his means of illumination were about as bad as bad could be—in fact, nothing but his wife's drawing-room lamp, with a ground-glass top.* Nevertheless he succeeded in showing me with my newly purchased objective the longitudinal lines as plainly and visibly as any of us are ever likely to see our own faces in our looking-glasses.

Again, in the summer of 1873, when a resident of Dresden, I had a visit from Dr. L. Rabenhorst, the well-known author of the 'Süsswasser-Diatomaceen,' and showed him, at his own request, a variety of test-diatoms, such as *Navicula crassinervis, Surirella gemma,* &c., &c., and amongst these one special slide of *Frustulia Saxonica,* which exhibited *both* lines so clearly and beautifully as to draw from him the usual German exclamation of delight and admiration, "Wunderschön!" With *Navicula crassinervis* he was less satisfied.

Now, bearing in mind that Dr. Rabenhorst is both the discoverer and namer of the diatom in question, I will put it to Dr. Woodward, even supposing that Herr Seibert could twice impose upon me for *Frustulia Saxonica* something that was not, whether I could have any chance of imposing upon Dr. Rabenhorst after the like manner, or upon Herr Alex. Lindig, late Optician to the Court of Saxony, to whom I showed it three nights later, who may be credited with the ability to recognize the markings of a well-known Saxon diatom when he saw them on a slide bearing his own label.

On turning to my own MS. note-book on test-diatoms, no portion of which was written later than the year 1872, I find the longitudinal lines of *Frustulia Saxonica* there described as "slightly wavy, but considerably less wavy and less apparent than in *Rhomboides.*" The plain truth is, I never knew there was any special difficulty about these longitudinal lines till I saw it so stated in the (supposed) extract from Dr. Woodward's paper. I will add further, that on the evening of the 14th of this month, when it first occurred to me to write on this subject, I put upon the stage of the microscope the same slide I had shown to Dr. Rabenhorst and Herr Lindig; and it so happened that the very first shell that came in front of the objective exhibited *precisely the longitudinal lines,* and as plainly as I could ever wish to see them. *Of course* I used no condenser. I have always regarded that article, *when employed on high-power delicate tests,* as a mere optician's booby-trap. Its only use there is to disguise the optician's faulty workmanship and to make a bad glass pass muster for a good one.

The reader would do well to see what Dr. Schumann † has to say on this point, when about to attack that intricate customer *Navicula lata.* Advanced microscopy under such circumstances is open to the gravest suspicions.

I had also no difficulty in bringing into view those wide-spaced,

* I intend to forward a copy of this number to Herr Seibert, at his present address, so that my statements may come before him for correction, if I have said the thing that is not. I am sure he will be not a little surprised to learn that *Frustulia Saxonica* has no visible longitudinal lines.

† 'Die Diatomeen der hohen Tatra,' p. 73.

spurious lines alluded to by Dr. Woodward, which some persons have nicknamed "ghost lines." As the frustule was already lying vertical, I had merely to move it about an inch and a half to the (apparently) left side of the objective.

Fortunately, the dispute—if there be any dispute—is one that can be easily settled. Either Dr. Woodward may forward a letter to Herr Seibert, "Optisches Institut, Wetzlar,"—and from my knowledge of Herr Seibert's obliging disposition I am sure he will readily send him a copy of the photographs shown to me,—or, if Dr. Woodward prefer it, I will forward to Mr. Lealand (of the firm of "Powell and Lealand") the slide I showed to Dr. Rabenhorst; and then, if Mr. Lealand cannot see the longitudinal lines, the case is ended.

I may as well mention at once that my collection of Frustulia Saxonicas, which is a pretty extensive one, ranges from specimens having lines closer and finer than those in *Amphipleura pellucida* to such as exhibit lines as coarse and strongly marked as those in Mr. Norman's *Nitzschia sigmoidea.* Thanks to Dr. Schumann's admirable pamphlet, we now know that this great diversity in the same species depends on differences of elevation of the diatom's habitat, that is, on differences of temperature, inasmuch as there is a fall of one degree of Reaumur for every 600 feet of elevation above the sea level. He further tells us (pp. 7, 17) that he found *Frustulia Saxonica* at various elevations, from 4000 to 6454 feet above the sea level. See also pp. 85, 93.

One word more about slides. Many a man fancies he has got a bad glass when he has really got a bad slide. There are slides and slides; and, unfortunately, the bad ones are in a terrible majority, as bad things usually are. Something of this sort has been at the bottom of Dr. Woodward's (reported) failure. If he is rightly reported as having failed, my explanation is, that he failed where everyone else would have failed, and has merely been wasting his time on a bad slide.

I am the more inclined to adopt this view from Dr. Woodward's mention of Möller; for I hardly need say that Möller, in the matter of test-diatoms, is the very worst guide anyone could possibly take. He seems indeed not to have the slightest idea that any test-diatom can require for its resolution any position other than horizontal or vertical. It is also notorious that, if a customer applies to him for *Navicula crassinervis*, he sends *Frustulia Saxonica* labelled "Navicula crassinervis"; and if the customer wants *Frustulia Saxonica* he sends it labelled "Frustulia Saxonica." Wrong naming seems to be his speciality.

It is quite true that Dr. Schumann * says (in which point I do not agree with him): "*Frustulia Saxonica* Rabenh. Alg. p. 227. It appears in the following forms:—

"1. *Nav. crassinervis* Breb. Syn. I. p. 47. xxi. 271. Beitr. p. 10, II, 13 *c.*

"2. *Nav. cuspidata* Ktz. Syn. xvi. 131. Beitr. II, 16 *a.*"

But to say that *Frustulia Saxonica* has as its sub-species *Nav. crassi-*

* 'Die Diatomeen der hohen Tatra,' p. 79.

nervis and *Nav. cuspidata* is something very different from saying that the two things are *identical*, as Möller says in his printed explanation accompanying his Diatomeen-Probe-Platte :—

"18. Navicula crassinervis Breb. = Frustulia Saxonica Rabh."

Let the reader compare Dr. Rabenhorst's drawing of *Frustulia Saxonica** with the drawing of *Nav. crassinervis* in Smith's Synopsis. I may here remark that Dr. Schumann's own drawing of *Nav. crassinervis* (plate iv. fig. 57) is markedly different from *Fr. Saxonica.* It is also very strange that Dr. Rabenhorst himself, when he saw them both in quick succession at my rooms in Dresden, had no suspicion that they were *identical.* Surely he ought to know. And now to revert to what I have said about *position.*

So far as my experience goes *Navicula crassinervis* is not readily resolved unless it lies perfectly horizontal.

Nav. rhomboides is perfectly resolved only when the (apparently) right-hand apex has an elevation of about 9°.

With *Frustulia Saxonica* it is exactly the reverse. It is fully resolved only when the (apparently) left-hand apex is elevated about 15°. And this will serve to distinguish them. In *Rhomboides* also the transverse lines are much closer than the longitudinal, whereas in *Frustulia Saxonica* it is just the reverse. Their general contour, again, is different. In *Rhomboides* the marginal edge runs pretty straight from the apex to the centre, where the frustule exhibits an obtuse angle. In *Frustulia Saxonica* the marginal edge forms a fairly continuous curve, and the ends are considerably sharper. There are also some characteristic differences in their central knot, which, however, are less apparent.

I have entered into these minute particulars lest anyone should suspect that Messrs. Seibert, Rabenhorst, Lindig, and myself have been confounding *Frustulia Saxonica* with *Rhomboides.*

In conclusion, I will beg Dr. Woodward not to interpret any word or sentence I have here used as intended to detract from his well-earned reputation as one of our greatest living microscopists, or as provocative of controversy, which I cordially detest. If he finds any expression that can bear such an interpretation I shall wish it unsaid. For my own part, I see no reason why matters microscopical may not be talked over as amicably as the state of the weather or the prospects of a good harvest.

Yours faithfully,

W. J. Hickie.

Zeiss's and English small-angled Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

May 18, 1875.

Sir,—At the last meeting of the Royal Microscopical Society, one of the Secretaries, Mr. Slack, read a very interesting paper on the above subject, and I have no doubt that all microscopists will be thankful for such a subject having been brought forward afresh.

* 'Die Süsswasser-Diatomaceen,' plate vii. fig. 1.

Dr. Carpenter has advocated, for many years, small-angled objectives "for the ordinary purposes of scientific investigation," and " . . . . for almost every purpose *except* the resolution of diatom-tests, objectives of *moderate* angular aperture are to be decidedly preferred." In his fifth edition of 'The Microscope' (published with the assistance of Mr. Slack), he says, page 208, "several opticians now make objectives of these limited apertures ($\frac{1}{2}$ inches of 40° and $\frac{1}{4}$ths of 75°), of excellent quality, and very moderate price," and page 819—"the excellent small-angled $\frac{1}{5}$th and $\frac{1}{6}$th made by Mr. Swift expressly with this view."*

Messrs. R. and J. Beck supply their $\frac{4}{10}$ths and $\frac{1}{5}$ths with both small and large angular apertures, besides the objectives of their "Popular" and "Universal" series, in both of which the angular apertures are small.

Messrs. Powell and Lealand supply $\frac{1}{2}$ inches with angular apertures of 40° or 70°; $\frac{1}{4}$ths of 95°, 130°, or 145°; $\frac{1}{8}$ths of 140°; but I am informed they will supply $\frac{1}{8}$ths with an angular aperture of 100° if desired.

Messrs. Ross and Co. supply their patent $\frac{1}{2}$ inches, $\frac{3}{10}$ths, and $\frac{1}{5}$ths, and will supply most likely, "later on," their $\frac{1}{7}$ths, with both large and small angular apertures.

This clearly proves what Mr. Crisp said at this month's meeting of the Royal Microscopical Society: "Opticians are not to be blamed for making their objectives with large angles. The blame entirely rests with microscopists, who want to be educated. . . . Objectives are made with large angles to meet the demand."

If the above facts are taken into consideration, and also that the leading English opticians will, no doubt, produce objectives of any focal length and angular aperture desired, I do not see the reason why foreign objectives should be brought forward in such a *prominent* manner to the detriment of English ones. The cheapness of *some* of the foreign objectives weighs, certainly, very much in their favour; but to balance this there is the great uncertainty as to *what* quality of objectives one may get when ordering them, if the opportunity of selection is not afforded. The *best* foreign objectives are *only* obtainable under the *most favourable circumstances.*

I have received some very good objectives from *one* Continental optician, but have also been very glad to dispose of objectives received from *two* other Continental opticians as soon as I had examined their performances, *which were certainly much less than* "moderately bad"! !

I do not believe that the most strenuous advocate of foreign objectives is prepared to maintain their superiority, or even their equality, to the objectives produced by the three leading firms of London opticians.

I am, Sir, your obedient servant,

A. de Souza Guimaraens.

* Vide also Mr. Slack's letter, 'M. M. J.,' vol. xi., p. 264, June, 1874, &c.

The Mode of Recording Absorption Spectra.

To the Editor of the 'Monthly Microscopical Journal.'

New York, *May* 26, 1875.

Sir,—I write in advocacy of Mr. Sorby's proposal that the absorption spectra should be recorded in wave-lengths, having employed this plan for nearly two years. I have, however, abandoned the prism, and observe all spectra with the diffraction grating, and obtain the wave-length directly by the well-known formula $\lambda = \delta \times \sin. \theta$. I have tried a number of gratings both transmission and reflecting, and find that a transmission grating containing between four and five thousand lines to the inch, is best adapted to absorption work. The one I now use contains 4320 lines and was ruled upon Mr. L. M. Rutherford's machine. With this grating the value of θ is $5° 45''$ for the D line.

I enclose a diagram which I use in recording spectra. Will you have the kindness to forward it to Mr. Sorby? and oblige

Your obedient servant,

Henry G. Piffard, M.D.

[On this letter being forwarded to Mr. Sorby, the following reply was transmitted.]

2, Cambridge Villas, Buxton, *June* 10, 1875.

Dear Sir,—I am much obliged by your having sent to me Dr. Piffard's letter. I have long known the special advantages of a diffraction grating, but of course prisms have other very important advantages, and are in many respects made more convenient to use with a microscope. What led me to propose the method described in my late paper was the belief that by observing with a prism and then so easily reducing the observations to wave-lengths, we should to a great extent unite the merits of both systems, and avoid their disadvantages.

Yours very truly,

H. C. Sorby.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *June* 2, 1875.

Charles Brooke, Esq., F.R.S., Vice-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 Society were voted to the donors.

Mr. J. W. Stephenson said he had placed upon the table a number of copies of a scale to be used for the measurement of the angular aperture of objectives. He had caused them to be printed, thinking that they

might be useful to many persons who were interested in the subject, and he had therefore much pleasure in offering them for distribution amongst the gentlemen present. The scale (which was enlarged upon the board by Mr. Stephenson for the purpose of explanation) was printed upon a sheet of paper about $14\frac{1}{2}$ in. $\times$ 10 in., across which, at about 1 inch from one end, a straight line was drawn; and this line was bisected by another straight line, which was extended to the opposite end of the paper, and along which a scale of cotangents was marked from 175° to 35°. (See p. 3.)

Mr. Slack suggested that if the paper was mounted upon a smooth board, and two knife edges of ivory were substituted for the night-lights used by Mr. Stephenson, the scale could be used in daylight, and he thought with advantage over the lights.

Mr. Crisp was quite sure that by the time they met again in October, Mr. Stephenson would have invented something much better for the purpose than night-lights.

Mr. Wenham thought this had supplied a want: in principle it in no way differed from the ordinary way, but was carried out rather in a different manner. He was inclined to doubt whether this plan would give perfectly accurate results, but he thought it would be correct within a degree.

The President thought as there was a little difficulty in seeing the very small images of the lights, it might be better to convert the object-glass into a telescope by placing a lens behind it.

Mr. Wenham said that in using the ordinary sector for the purpose, when a glass was properly stopped the angle could be determined very accurately; but with one not properly stopped there was no such sharp demarcation, but the light would vanish away gradually almost up to 180°.

The thanks of the meeting were voted to Mr. Stephenson for his communication.

Mr. Charles Stewart said that through the kindness of Mr. Badcock he had lately been enabled to make an examination of that curious creature known to them as *Bucephalus polymorphus*. (Drawings of the above, together with his remarks, will be found at p. 1.)

Mr. Badcock thought there was one little point which Mr. Stewart had omitted to mention, and this was a dark spot which he had observed midway between the central tip of the creature and the ventral orifice.

Mr. Stewart said he was under the impression that this dark spot might be the three little concretions before alluded to. The only other thing which had not been mentioned was a problematical thing, which he wanted to examine further before pronouncing an opinion upon. Immediately in front, and dorsal to the sucker, there was a considerable sized depression, having underneath it a space apparently identical with the elongated space representing the water-vascular system. As the creature was dying, he hardly liked to speak with certainty about it, but thought it might be the anterior extremity of this curved space.

Mr. Badcock said that another point of interest was the periodical

times at which these creatures came forth; he got a batch of them now about once in every ten days: this was from the same mussel as he had last summer.

Mr. Loy asked if Mr. Badcock had ever dissected a fresh-water mussel and found any of these creatures in it, or whether he knew of anybody who had ever done so. He had himself dissected a great many, but had never yet been able to find any of them.

Mr. Badcock said he had never found any in a mussel, and never found any near a mussel except once.

Mr. Slack asked if Mr. Loy had looked for any sporocysts; he thought all one could expect to find would be some minute white threads in the liver and genital organs. From the translations and drawings given in the 'M. M. J.' for April, it would seem that previous observers had left much to be elucidated, and Von Baer had used too low a power to enable him to make out the anatomical details which Mr. Stewart had described.

Mr. Loy said he had got mussels of various kinds, and had obtained them from different sources, but could neither find the creature nor anything like it—not even the threads mentioned by Mr. Slack.

Mr. Badcock suggested that perhaps Mr. Loy had looked for the creatures at the wrong time of year; he found they disappeared altogether during the winter months.

Mr. Loy said he looked for them in January, February, and March: he had tried all sorts of mussels from all sources, and had never been able to find a trace of anything resembling them.

The President thought it would be of much interest to determine if possible the alimentary system.

Mr. Slack thought the sporocysts seemed the most interesting things connected with these creatures; it appeared that they branched out and germinated, and cast off germs which resembled the early stages of the parent creatures.

Mr. Badcock was afraid it would be premature to say that he had traced them to a second stage; but he might say that he had kept some of them for some time in a bottle, and had found in place of them a creature somewhat resembling the other, with the central body surrounded by cilia, and having a rudimentary central tail. He had on a former occasion attempted to bring one in this condition to the Society for exhibition, but unfortunately it got crushed in coming.

Mr. Slack read a paper "On the Use of Mr. Wenham's Reflex Illuminator." (See p. 5.)

Mr. McIntire said he could speak to the difficulties attending the use of the apparatus when used with an objective of more than 100°. The suggestion about stopping down the angles was a good one; some very beautiful effects might then be produced.

Mr. Crisp thought the most convenient apparatus for stopping down was the iris diaphragm.

Mr. Wenham inquired whether Mr. Crisp had done this in the case of very high powers, because he thought in those cases it would be apt to cut off the field in consequence of the diaphragm being so much behind the conjugate focus.

Mr. Crisp said he had tried it with a $\frac{1}{8}$th inch having the back cut down very flat so that it could come very close to the back lens.

Mr. Wenham hoped some ingenious member would invent some form of diaphragm to suit it.

The President thought it might be possible to construct an iris diaphragm of a number of thin strips of metal in such a way that it could be expanded or contracted.

Mr. Wenham had thought of this plan, and also of making an aluminium tube with a collar to run up and down the case, but the difficulty then was to get at it to work it when in position.

Mr. Slack asked if it would be difficult to make a kind of rotating diaphragm for the purpose.

Mr. Wenham said that if it became a question of cutting through the object-glass, he would rather use a slide than anything to rotate.

Mr. Slack said that in the case of *Eupodiscus Argus* the surface was too much curved for any high-angled glass to show the object properly, but with a low angle the piece that came into focus was shown perfectly. Without penetration it was not possible to get a proper idea of the structure.

Dr. Matthews said that he had very little to report concerning the diatomaceous earth committed to him at the last meeting except a series of failures. He had boiled some down in nitric acid, but still it retained its shape, and when he crushed it the diatoms were also broken. He gave some to Mr. Topping, and he seemed to be able to do it perfectly, and had mounted a specimen. However, he was unable himself to make out its species, and had therefore handed it to Mr. Hailes, who in turn being also puzzled had sent it to Mr. Kitton, who was himself uncertain as to what species of *Orthosira* or *Melosira* was present. The specimen was exhibited under a microscope in the room.

Mr. Slack had also looked through the collection sent by Mr. Hanks, and found amongst it many interesting additions to their slides of minerals. There was also a very fine polychroic object—sesquioxide of chromium.

Mr. Wenham asked Dr. Matthews if his difficulty arose from the silicious character of the earth, as in that case he thought if it were boiled in a weak solution of soda the silicious material would give way.

Dr. Matthews said he had tried soda and potash three times, and also repeated boilings in nitric acid, and had boiled it to dryness, but still unable to succeed.

The proceedings were then adjourned until October.

Donations to the Library since May 5, 1875 :

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Journal of the Linnean Society. No. 59	*Ditto.*
Journal of the Geological Society. No. 122	*Ditto.*
Monthly Notices, &c., of the Royal Society of Tasmania	*Ditto.*

WALTER W. REEVES,
Assist.-Secretary.

Medical Microscopical Society.

Friday, May 21, 1875.—Dr. F. Payne, President, in the chair.

Fatty Degeneration of Muscle.—Notes on this subject were read by Dr. Kesteven. He observed that the expression "fatty degeneration," as usually employed, was a misnomer; that the change is one in which, as a matter of fact, fat plays no part at all. Instead of fatty matter, the degenerated muscles exhibit a disintegration of the contents of the muscle fibrils, obliterating their striation. Along the centre of these are to be seen dark-brown or black granules of a pigmentary character, not removable by æther or alkalies. The change begins and ends in the fibrils, and is very distinct from the fatty condition, in which the fibrils retaining their striation are separated by deposition of adipose tissue. The latter form of disease is more or less associated with changes in the nervous centres, while in the former it is not so. Preparations and illustrative drawings were exhibited by the author, of muscular atrophy, pseudo-muscular hypertrophy, rupture of the left ventricle, infantile paralysis, and the so-called fatty degeneration.

The President and several of the members took part in the discussion that followed.

Myelitis.—A paper on this subject, by Mr. D. J. Hamilton, was read by the President. The author pointed out that much uncertainty existed as to the real character of the various lesions of the spinal cord, known as hardening, softening, sclerosis, disintegration, &c., and more especially as to their connection with the secondary changes depending on loss of the trophic influence by nerve-cells on the fibres to which they are attached; and as to their connection with inflammation.

In order to elucidate the latter point, it was necessary to know definitely in the first place what changes inflammation does produce in the spinal cord; and with this end the author made the experiments recorded in his paper. His method consisted in first producing a lesion known to be inflammatory, and then examining the morbid appearances, all preconceived ideas on the subject being purposely laid aside. All appearances were verified by abundant repetition, and only those which were found to be invariable were recorded. The experiments were conducted in Professor Stricker's laboratory at Vienna. The method of experimenting was: A small animal, as a cat, having been narcotized, the spinal cord was cut down upon at the junction of the dorsal and lumbar portions, and a thread passed through for about an inch in a longitudinal direction. The wound was now closed, and the animal killed after forty-eight hours, by which time inflammation was abundantly set up. The cord was then cut up into pieces an inch long, hardened in chromic acid and spirit, and kept at a low temperature. Sections were made in a microtome, stained with carmine and mounted in dammar.

When examined, the tissue at the seat of lesion was found broken down, and showed numerous extravasations of blood; but the true inflammatory area was seen at a little distance from this, being gene-

rally most marked in the deepest portions of the anterior columns and in the commissure. Changes were seen in the nerve-tubes, the nerve-cells, and the neuroglia.

In the *nerve-tubes* the most remarkable changes were seen in the axis cylinder, this being dilated at irregular intervals into large oval swellings, from five to ten times its normal diameter, still contained in the distended and attenuated nerve-sheath, and composed of a transparent substance, deeply stained by carmine. These oval swellings were sometimes connected together by the unaltered axis cylinder, sometimes detached. In many of them evidence was seen of fissiparous division, groups of smaller though similar transparent bodies being produced, which appeared to spread beyond the original but now empty nerve-sheath, and to invade the surrounding tissues, being then identical with the so-called "colloid bodies" abundantly met with in chronic affections of the spinal cord.

In the seat of most intense inflammation the bodies underwent a further and more remarkable development, becoming granular, and several nuclei being formed in their interior. The corpuscles then presented the appearance of mother cells, and in some cases these mother cells were broken down, and the young cells in their interior were set free as pus-corpuscles.

The changes in the *nerve-cells* were less important, consisting chiefly of swelling and molecular transformation, a condition often described as an œdematous affection. In no case was division of the nerve-cells observed.

The *neuroglia* was less altered than might have been expected, but in many instances abundant nuclei were seen, the origin of which was obscure.

Abundant evidence was seen of the emigration of leucocytes from the blood-vessels, the adventitious sheaths being filled with them, as in inflammation of other parts. Similar appearances were seen in a case of syphilitic disease of the nerve centres.

After a brief discussion on the paper, Mr. Groves described and exhibited Crouch's improved fine adjustment for microscope stands, made on the Continental model, in which great stability was combined with easy and regular motion, the points of friction being reduced to a minimum. He also described a centering sub-stage arrangement for the same stand.

Quekett Microscopical Club.

Ordinary Meeting, April 23.—Dr. Matthews, F.R.M.S., President, in the chair.

The President announced the death of Mr. Robert Hardwicke, which had occurred since the last ordinary meeting, and passed a high eulogium upon him. Mr. Hardwicke was one of the founders of the club, and its Treasurer from the commencement, and allowed his house of business to be used as its office. He also published the Journal, was an active and useful member of committee, and greatly advanced the interests of the club. A resolution expressive of the

loss sustained by the club was moved by Dr. Braithwaite, and duly carried.

Mr. W. W. Jones described an instrument for cleaning very thin covering glass without danger of fracture.

Mr. T. C. White made a further communication respecting the salivary glands of the cockroach, and described an instance in which the sac when dissected out was found filled with fluid containing salivary corpuscles. He exhibited one of these sacs still attached to the thorax, the whole being immersed in alcohol.

Dr. D. Moore read a paper, giving "The results of some observations on the *Bucephalus Haimeanus* of M. Lacaze-Duthiers, and another allied organism not yet named." Soon after reading his last paper, he found that the organism he had figured as the young of the cockle had been fully described by M. Lacaze-Duthiers, in the 'French Annals of Natural History,' under the name of *Bucephalus Haimeanus*, the other allied organism obtained from the mussel not having been, so far as he knew, either described or named. The admirable *résumé* of the subject by one of the Hon. Secretaries of the Royal Microscopical Society, which appeared in the 'Monthly Microscopical Journal' for April, rendered the history which he had intended to give of previous observations and conclusions unnecessary. He then gave a short sketch of the life history of a Cercarian, *Cercaria ephemera*, described by Siebold, which went through all its changes in certain water-snails, whose skin it penetrated, in which it became encysted, and developed into a perfect sexual *Distoma*, whose eggs in the course of time produced the sporocysts or nurses, which were found to contain *Cercariæ* in various stages of development, which being ultimately set free from the sporocysts and snail in a way not clearly traced, might then be found as freely-moving *Cercariæ* in the water, ready to start on the same round again, thus forming a complete illustration of alternation of generations. This life history, with modifications, was supposed to apply to *B. Haimeanus*, the ramified tubular structure in which it was found being considered a sporocyst. After comparing his own observations with those of M. Lacaze-Duthiers, showing that the facts observed were much the same, although the interpretation he had been led to put on them was different, he pointed out that the branched character and apparent connection with egg-sacs observed in this structure might be accounted for, supposing it an anomalous form of sporocyst, by such sporocyst being contained in the genital ducts. The presence of blind extremities, and an appearance of budding, he had thought indicated the points where egg-sacs had emptied their contents into the tubular structure. If it were a sporocyst, the blind extremities were doubtless the ends which M. Duthiers had found it almost impossible to examine. Other appearances described by M. Duthiers he briefly alluded to, the interpretation he had put upon them being influenced by his different experience. He had never found *B. Haimeanus* in oysters, but only in cockles containing fully formed eggs, although he had examined large numbers procured from Hayling, where from their juxtaposition it would appear probable that this organism might be found in both if parasitic. He thought

M. Duthiers' figure was drawn from a somewhat immature specimen, and mentioned that a difference in structure visible at the lamellar extremity under polarized light, both in *B. Haimeanus* and the other allied organism, had induced him to think that here might be the beginning of rudimentary shells. If the structure containing *B. Haimeanus* could be found in oysters, it would disprove his conjecture as to the nature of these organisms. He briefly alluded to the recorded encystment of *B. Haimeanus*, which, if confirmed, would introduce us to a life history of remarkable character and great interest. How the ova of the supposed sexual fluke entered the cockles so abundantly on our coasts had not as yet been indicated; and if the structure in which *B. Haimeanus* was found was a sporocyst, it was an anomalous one, and furnished a subject for good work in tracing its parentage and development.

Mr. W. Fell Woods read a paper "On the relation of *Bucephalus* to the Cockle." Having stated that his investigations began as far back as June, 1872, he traced, as the result, verified by his notes up to May, 1873, the growth of his impression that *Bucephalus*, instead of being parasitic, was the larval form of the cockle. Having detailed the reasons, he pointed to the independent examinations of Dr. Moore, commenced in May, 1873, as issuing in the like conclusion. These facts were his warrant for still advancing the theory at the meeting of the Royal Microscopical Society on the 4th of November last. He then adduced more recent observations, furnishing new confirmatory facts, which he considered of great interest; and after indicating some points of difference between Dr. Moore and himself, and referring to the memoir by M. Lacaze-Duthiers on *Bucephalus Haimeanus*—to whose description of the earliest stage he took exception, he pointed to his own discovery of the same eggs contemporarily in the ovisacs of the cockle and in the supposed sporocysts; and whilst admitting the parasitic character of *Bucephalus*, if its observation in the oyster were reliable (his own large experience had never afforded an instance), he argued that either, 1st, *Bucephalus* is the larva of the cockle (and if not, it remains an interesting question for solution, what is?), or, 2nd, *Bucephalus* is a parasite, but if so it does not render the cockle sterile, as asserted by M. Lacaze-Duthiers; and, 3rd, the connection of the tube with the ovisacs, as established by the presence of the eggs in both, proves it is not an independent sporocyst, as asserted, but an organ of the cockle; whilst, 4th, if this connection be denied, though the case quoted seemed to render it certain, *Bucephalus* must still be developed from eggs seen in the tube, in contradiction of a third statement of M. Lacaze-Duthiers.

Some illustrative drawings were exhibited.

THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1875.

I.—*Number of Striæ on the Diatoms on Möller's Probe-Platte.*

By F. Kitton, Norwich.

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

In the table of measurements of the striæ of the various diatoms on Möller's Probe-Platte appended to Mr. J. E. Smith's paper published in the last number of this Journal, *Nitzschia curvula* is stated to have 84·7 in ·001″. As I am well acquainted with this form, not only from specimens of Professor Smith's own gathering, but also from many other sources, I felt certain that some error had been made. I have therefore examined one of Möller's preparations of the form he calls by this name, and, as I suspected, it is not *Nitzschia curvula Sm.* but his *Nitzschia sigma.** The figure in the Synopsis is very characteristic so far as the outline of the valve is concerned, but the striation (very imperfectly shown) represents the striæ far too distant. The Synopsis states the striæ to be 56 in ·001″. I have never seen any British specimen of this species with striæ so close as this, and in a gathering from Felixstow they are about as easily resolved as those on *Pleurosigma quadratum:* about 40 in ·001″ I consider to be a nearer approach to accuracy. The species sold by Möller certainly has them very fine, and not easily resolved with $\frac{1}{8}$th objective and B ocular, but in no respect is there any difference of specific value to distinguish it from the British form.

N. curvula of Smith (= *N. sigmatella* Gregory, 'Quar. Jour. of Mic. Sci.,' vol. iii.). The specific characters in the Synopsis are not only obscure but misleading. The description is as follows:

"F. V. linear, tapering towards the truncated extremities. V. linear acute, striæ obscure. Almost identical with *N. sigma*, but distinguished from that species by its more delicate striæ and fresh-water habitat." It will be seen from the above description that no definite idea of this species is obtainable; no allusion is made to the sigmoid form of the valve nor to the pseudo-punctate appearance of the margins of the valves; the remark that it is almost identical with *N. sigma* is also misleading, it really bears no resemblance to that form, and the striæ are at least as distinct, and perhaps more

* I have forwarded specimens of both forms, which can be seen at the Society's rooms.

so. It is only from the examination of authentic specimens that it is possible to ascertain the form to which Smith gave the specific name of *curvula*. Gregory gives a good figure, but no description.

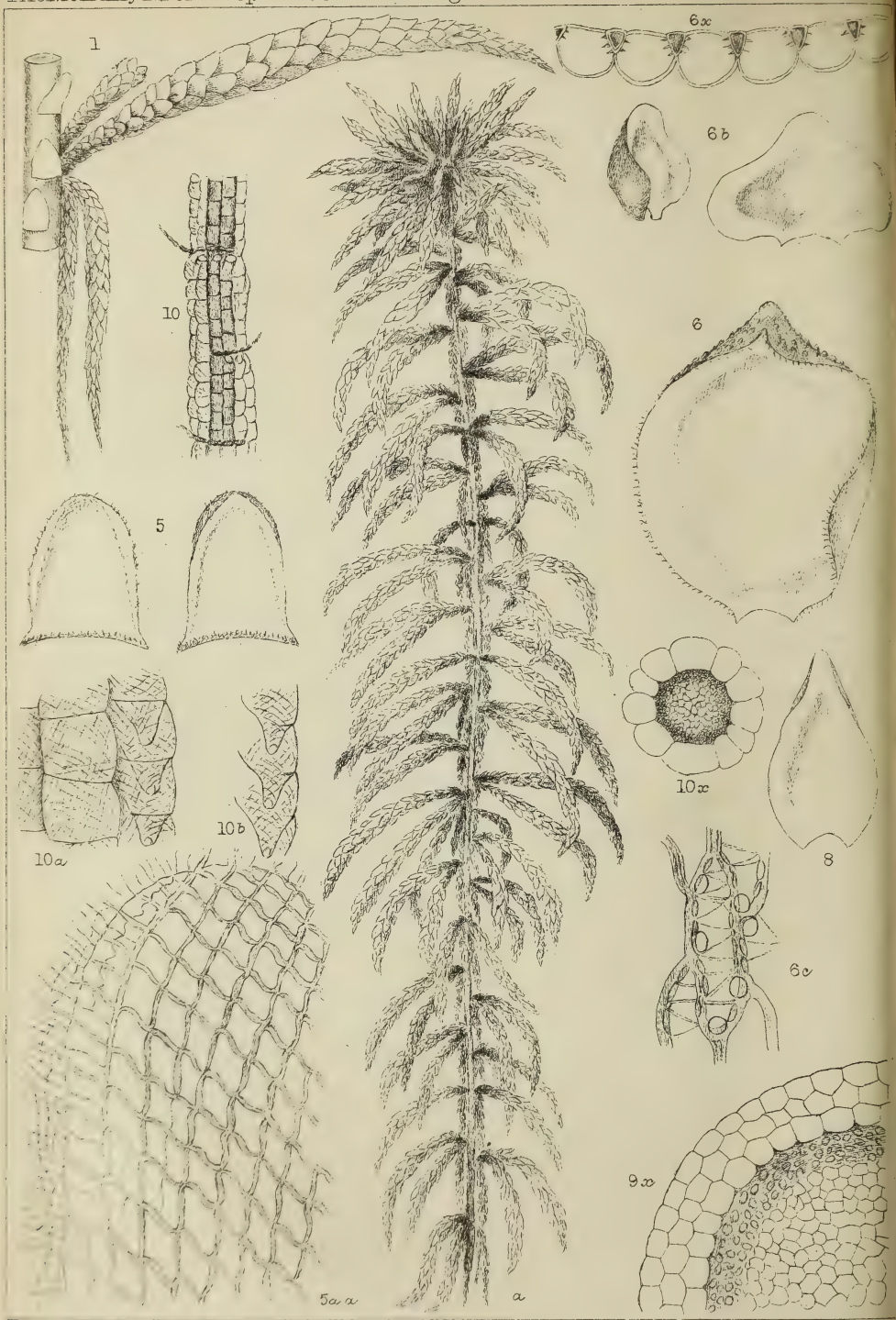
"Dr. Lewis, U.S.A., in his paper on 'Some new and singular intermediate forms of Diatomaceæ,' has properly removed this form from the genus Nitzschia and placed it with the Surirellæ. He gives the following specific characters:

"*Surirella intermedia*, n. sp. Frustules free. Valve linear, strongly sigmoid, with attenuated rounded apices. F. V. straight or slightly sigmoid, expanding at the sub-truncate extremities. *Alæ* usually distinct, twisted near the ends of the valves, giving rise to a spathulate appearance. *Canaliculi* numerous, inconspicuous, reaching the narrow central blank line. Striæ distinct, variable as to number and fineness."

The value of the distance of the striæ on the Diatomaceæ either as a test for the resolving powers of objectives or as specific distinctions is but little; in the latter they may be said to be valueless, and in the former case they are only of value when the actual distance of the striæ is known. Möller's slide of *N. curvula* (= *N. sigma*) is mounted from a brackish water gathering from Schleswig and contains the same forms as my own Felixstow gathering, viz. *N. sigma* very plentiful, *Pleurosigma fasciola* common, *P. angulatum*, *N. minutula*, &c.; but in my gathering, the striæ on the two first-named forms are resolvable with a $\frac{1}{4}$th objective of 75° angular aperture made twenty-two years ago by A. Ross. On *P. fasciola* I can resolve both sets of striæ, and with Beneché's No. 7 and C ocular I can see both sets at once (that is to say, the dots are shown in squares). The striæ on the same forms on Möller's slide I can only resolve with my $\frac{1}{8}$th, *N. sigma* with difficulty, and only one set of striæ at a time on *P. fasciola*.

In Möller's beautiful preparation of *P. angulatum* the striæ on that form are easily resolved with my Ross $\frac{1}{4}$th, whilst those from many British localities are quite invisible under the same conditions. *Nitzschia sigmoidea* (*type*) is not an easy test for a good $\frac{1}{8}$th, but its *var.* β is easily resolved by a good $\frac{1}{2}$ inch.

This variation in the distance of the striæ on the same species of diatom probably arises from a more vigorous growth in the frustule, and it will be generally found that the more robust the frustule is, the more distinct are the striæ. An unscrupulous dealer in objectives has only to take advantage of this fact, and he can palm off inferior objectives by guaranteeing their being able to resolve certain so-called tests; for example, a very inferior $\frac{1}{4}$th would show the striæ on *N. sigmoidea* β, or an $\frac{1}{8}$th of moderate quality would probably show the markings on the coarsely striated *P. fasciola*, which would be perfectly incapable of resolving those on Möller's slide.



H. Bruckmann del. ad nat. W. West & Co. i

Sph. Portoricense.

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

Plates CX. and CXI.

21. Sphagnum Portoricense Hampe.

Linnœa 1852, p. 359.

Plate CX.

Syn.—Sullivant Icon. Musc. p. 3, Tab. 2 (1864).—Austin Musc. Appalach. No. 1 (1870).
Sph. Sullivantianum Austin in Amer. Journ. Sci. 1863, p. 252.

Dioicous? in large soft tufts, pale fuscous below, pale glaucous green above. *Stems* 8–14 *in. high, stout,* simple or bipartite, firm, pale brown; *cortical cells in* 2–3 *layers, containing spiral fibres but few pores. Stem leaves auricled,* erect or deflexed, *subquadrate-ovate, fringed round the entire margin,* upper cells rhomboidal, lower elongated, all without fibres or pores.

Ramuli 4–5 *in a fascicle,* 2–3 *divergent, arcuate-patent, subclavate-fusiform, attenuated at base, the leaves julaceously imbricated;* pendent branches more slender, lax. *Cuticular cells spirally fibrillose with few pores, the transverse walls geniculate downward into the adjacent cell, usually having a pore at the apex of the bend.*

Leaves of the divergent branches small below and widely cordate or semicircular, becoming larger above, narrowed at base, the median orbiculate-ovate, squamoso-scabrous at back of the strongly cucullate apex, very narrowly margined, all minutely fimbriate throughout, the fibrils of the fringe formed of the commissural walls of destroyed hyaline cells; *lower hyaline cells elongato-rhomboidal, upper rhombic, with numerous strong papillæ internally on the wall combined with the chlorophyll cells,* all fibrillose, with several large pores at the margin; *chlorophyll cells triangular in section, interposed between the hyaline on the concave surface of leaf.* Fruit unknown.

Hab.—Swamps in mountain districts. First found in Porto Rico by Schwanecke. Manchester ponds, Ocean County, New Jersey (Austin).

EXPLANATION OF PLATE CX.

Sphagnum Portoricense.

a.—Plant from Austin's collection.
1.—Part of stem and branch-fascicle.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Leaf from middle of a divergent branch. 6 *x.*—Section of same. 6 *c.*—Cell from middle × 200. 6 *b.*—Leaves from base of the same branch. 8.—Leaf from a pendent branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves. 10 *a.*—Cuticular cells of same. 10 *b.*—The same seen laterally. 10 *x.*—Transverse section of a branch.

This fine and rare species stands at the head of the *Cymbifolia* group, and naturally arranges itself with *Sph. Austini* and *papillosum.* It may at once be distinguished by the beautifully fringed margin of the branch leaves, and by the curious downward prolongation of the transverse wall of the cortical cells of the branches, which may be readily observed in the series of cells at each lateral margin.

Subgenus Isocladus. Lindb.

Plants lurid-whitish-green, glossy. Ramuli 2–4 in a fascicle, all uniform and divergent, with very large, narrow, loosely spreading leaves, their cells very narrow, without fibres and with a central longitudinal row of pores.

22. Sphagnum macrophyllum Bernhardi.

Bridel, Bryol. Univ. I. p. 10 (1826).

Plate CXI.

Syn.—Drummond Musc. Amer. Coll. 2, No. 18 (1841). Sulliv. Musc. Alleghan. No. 207 (1845). Mosses of Un. St. p. 12 (1856). Ic. Musc. p. 1 t. 1 (1864). C. Müll. Synops. I. p. 91 (1849). Sull. Lesq. Musc. bor. Amer. No. 1 (1856). Austin Musc. Appal. No. 41 (1870). *Isocladus macrophyllus* Lindb. Ofv. af K. Vet. Ak. Förh. XIX, p. 123 (1862).

Dioicous, pale olive-green, fuscescent below, when dry glossy and shining. Stems 6–10 *in. high, rather rigid, very fragile,* fuscous, simple or dichotomous by innovation, *with* 2–3 *layers of cortical cells.*

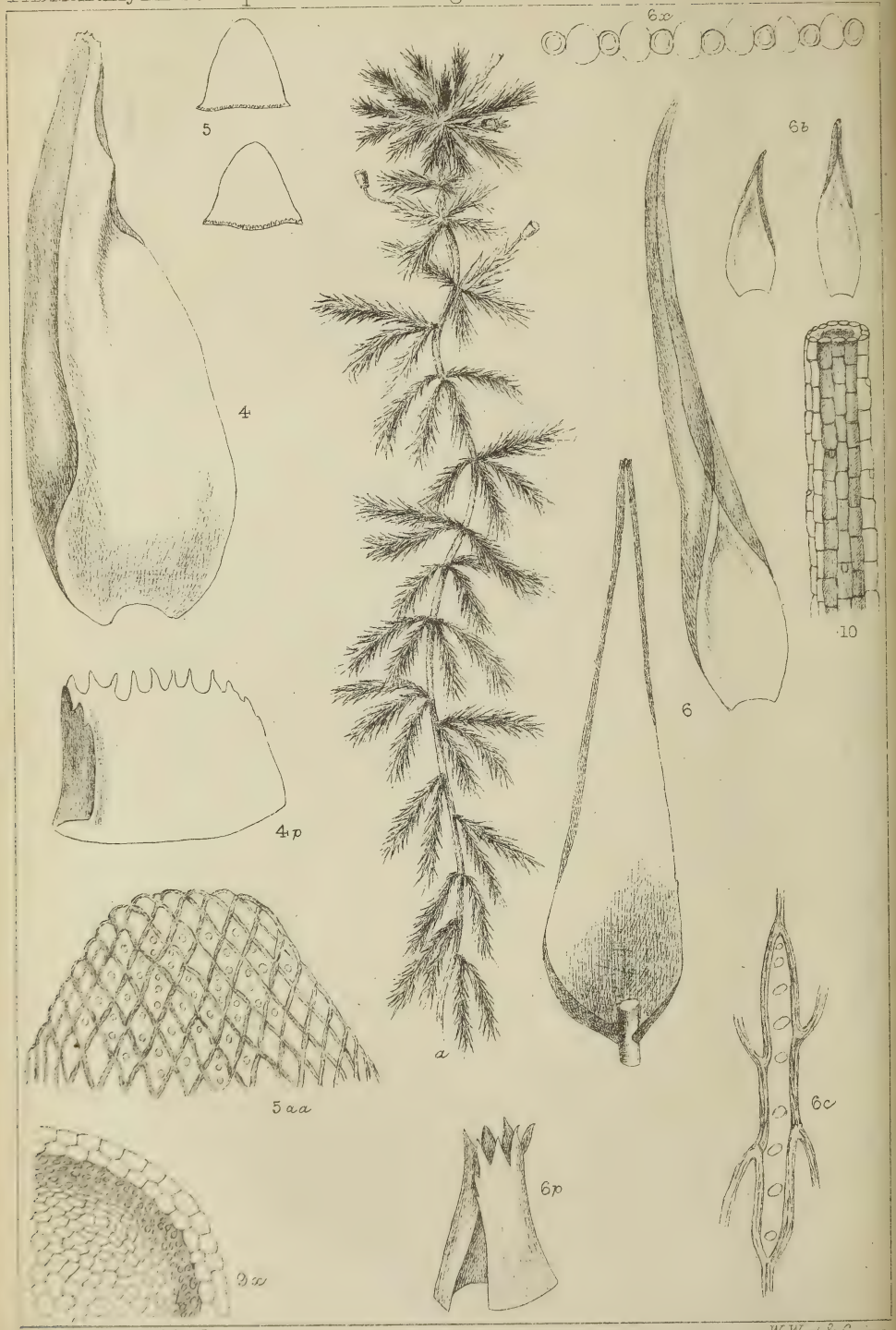
Branches crowded in a spinose capitulum 3–4 *in a fascicle, uniform and similar, divergent, dependent, straight, subflabellate, lax-leaved,* the cortical cells short, uniform, with few pores. *Stem leaves minute, very broad at base, ovate-oblong, obtuse, entire,* the hyaline cells rhomboid, without fibres, but with 1–3 central pores.

Branch leaves rather rigid, subdistichous, small at base of branch, soon becoming elongated, narrowly lanceolate, and lanceolate-subulate, involute-concave, bordered by 1–2 rows of extremely narrow cells, *apex somewhat truncate with* 7–8 *teeth. Hyaline cells elongate flexuoso-fusiform, with* 6–10 *pores in a longitudinal median line; free from fibres. Chlorophyll cells circular in section, separating the hyaline both in front and back.*

Fruit in the upper fascicles or in the coma, divergent; *perichætial bracts* 6–9, *lax, oblong-ovate, uppermost convolute, truncate, and toothed at apex, the areolation resembling that of the branch leaves.* Capsule small on a shortish slender peduncle; spores sulphur coloured.

Male plant and prothallium unknown.

Hab. — North America. Near Philadelphia (Bernhardi). Swamps in Louisiana (Drummond). Raccoon Mountains, Alabama



R. Braithwaite del ad nat. W. West & Co. imp.

Sph. macrophyllum.

(Lesquereux). Green County, Mississippi (Tice). New Jersey (Austin).

Quite peculiar among the Sphagna, by the uniform branches in the fascicles, the slender pendent branches found in most of the species being wanting, and also by the central position of the pores and total absence of fibrils. The convolute leaves and pointed branches give it somewhat the aspect of *Hypnum cuspidatum*, while if we overlook the pores, the areolation is not unlike that of *H. riparium* or *fluitans*, and with this approximation to the true mosses we conclude our series of Sphagna.

EXPLANATION OF PLATE CXI.

Sphagnum macrophyllum.

a.—Fertile plant from Drummond's collection.
4.—Perichætial bract. 4 *p.*—Point of same.
5.—Stem leaves. 5 *a a.*—Areolation of same.
6.—Leaves from middle of a branch. 6 *p.*—Point of same. 6 *x.*—Section. 6 *c.*—Cell from middle × 200. 6 *b.*—Leaves from base of the same branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

III.—*On the Unit of Linear Measurement.*

By Rev. D. Edwardes, M.A., St. Chad's College, Denstone.

Reading Schumann's 'Diatomeen der Hohen Tatra' a short time ago, I came across the following passage: "Auch die Engländer werden auf dem Felde der Diatomeen den englischen Zoll, wenngleich er durch die experimentalen Arbeiten Newton's eine besondere Weihe erhalten hat, aufgeben und mit dem Maase Ehrenberg's messen." This was written in 1867. I immediately set to examine a few of the last volumes of the Microscopical Journal, as being the best test I could think of as to what extent the event of things has proved Schumann's prophecy true. I found, as I expected, scarcely an instance of an English microscopist making use of the Paris line, which was Ehrenberg's standard unit.

Although English microscopists are thus still sturdy Englishmen, yet there is a large number of English men of science who seem to think that wisdom is found only on the other side of the Channel, and are endeavouring to further in this country the arbitrary introduction of the French system of measurement.

I have no wish to predict what will be the system of measurement fifty years hence, but it does seem to me that both parties—the English and the French—are content to put up with a more im-

perfect and unsatisfactory state of things in this respect than the occasion requires. I am probably right in conjecturing that there are not many thinking men who are absolutely satisfied with our own system, in spite of its many excellent points. Those who have to measure minute quantities find the factors of 12 to be of very little use, and almost all scientific men are obliged to adopt a decimal system between certain limits. It is also difficult to understand how scientific refinement, if I may be allowed the expression, is content to put up with a system of measurement whose unit is the length of a "barleycorn from the middle of the ear." This is theoretically still the case, although in practice a rod equal in length to 108 of these primitive barleycorns placed in juxtaposition, has been substituted and put by in the Exchequer Chamber for reference in case some of Her Majesty's subjects should find all their mid-ear barleycorns not precisely the same length.

On the other hand, it requires an excess of Francomania to adopt the French metre for our unit. It is true it originated with scientific men and not with the agriculturist, but it might indeed be contended that the English unit is the more philosophical of the two. Doubtless there was once upon a time such a barleycorn three lengths of which went to make up exactly an inch, the thirty-sixth part of the standard yard. But there never was and never will be, subject to the present laws of nature, such a quadrant of meridian that can be subdivided into 10,000,000 French metres. The metre again, should it be readjusted, is only in a degree better than the barleycorn. For it must vary as the place of measurement. The equator is slightly elliptical, and consequently there can be at most in the same hemisphere but four quadrants of meridian which are exactly the same length. Do what we may, the metre will only be a local and consequently a national unit. It stands to reason that the metre as well as the yard must go if we are to have a universal unit.

The length of a pendulum vibrating seconds is liable to the same objection. The experiment must be made at some particular spot, and will hold good for that spot alone. Should anything occur which would make that spot discoverable only by its longitude and latitude, complicated elements would have to be introduced into the calculations.

Sir J. Herschel suggested that the diameter of a sphere of the same volume as the earth should be taken as unit. The mean distance of the earth from the sun might also be suggested for a terrestrial as well as a celestial unit; but the infinite subdivisions of these enormously large units would appear at first sight, saying the least, somewhat clumsy. Should scientific men meet to discuss the matter, there is probably no doubt as to the unit they would adopt, namely, the wave-length in vacuum of a particular kind of

light emitted by some widely diffused substance, such as sodium, which has well-defined lines in its spectrum.

Assuming the wave-length as unit, there would scarcely be a doubt as to the adoption of the decimal system in proceeding with its multiples. The best unit for common use would be a matter for deliberation, and would take years to be adopted anything like universally.

If instead of taking the wave-length of yellow light, as has been suggested, we take that of the orange, which is slightly longer, our present measurements could be converted with peculiar facility. A million wave-lengths are equal to the mechanic's two-feet rule. The convenience of adopting this instead of the yard for a common unit would more than compensate for the ease with which the latter is extemporized by the matron's arm or the ploughman's pace. This rule, as it is generally folded up into two, can be carried about, without any inconvenience, by anybody to whom it ever becomes a matter of any importance to satisfy himself or others as to the dimensions of surrounding objects.

With the wave-length of orange light we should have

	Wave-lengths.		Inches.		Feet.		Yard.
	1,500,000	=	36	=	3	=	1
	1,000,000	=	24	=	2		
	500,000	=	12	=	1		
	$41,666\frac{2}{3}$	=	1				
In round numbers	1,000	=	$\frac{1}{40}$				
or	1	=	$\frac{1}{40000}$				

I confess my object is more to beat about the bush, to use a familiar expression, than to propose a modulus or to cut out a perfect system of measurement. This must be done by representatives of the different scientific societies meeting to discuss the matter. When they meet and deliberate upon the subject we shall, undoubtedly, have the best system of measurement possible. But then a sufficient number of them must meet, so that English spirit may have as much fair play in their deliberation as it has in that of the House of Commons. The Committee of the British Association have signally failed in this respect. From time to time they have recommended the French metre for our unit; but Englishmen will not become Frenchmen, much as they love their neighbours. They will not adopt a unit of measurement which practically is the exclusive property of the French nation, and theoretically holds good only when it is measured across the territory of the French Republic.

IV.—*The Microscope and its Misinterpretations.*

By John Michels.

The old adage that "seeing is believing" has long been exploded, and folks nowadays receive with caution the impressions conveyed by their eyesight.

There is still, however, a fixed idea with many people that, when the human sight is aided by powerful and correctly-constructed optical instruments, full reliance can be placed upon such united powers, and that the investigator may record that which he believes he sees as veritable and established facts.

In contradiction of such belief I shall place before the reader some curious results, which will show that the utmost caution is required by those using optical instruments for the elucidation of scientific problems or ordinary research.

Quite an interesting paper could be written upon the optical delusions with which astronomers have to contend in the use of the telescope, but I propose to confine my remarks to the difficulties which beset the path of the microscopist, in obtaining truthful and accurate results, while using the microscope, leading to the most contradictory statements from men whose powers of observation and skill in the use of the instrument are admitted.

Those who make use of a microscope for the first time are usually fascinated by the wonderful and beautiful appearances presented, and, having illuminated the object under examination with a flood of light, and focussed it to their satisfaction, congratulate themselves upon the ease with which they have handled the instrument, and fondly believe they have attained to a knowledge of its use. More extended study, however, and the use of high powers with the more complicated pieces of apparatus, soon convince the student that the instrument requires the most delicate manipulation, and that much practice is necessary before its true powers are developed.

Until full command over a microscope has been acquired, the most contradictory and perplexing results are obtained by those who use high powers in the examination of difficult objects, especially if the subject is very transparent. Things examined yesterday appear quite different to-day, both in form and colour; and even while the eye is still fixed upon the object, a slight change in the position of the mirror will alter its appearance, or present entirely new features.

Again, an object mounted in different mediums, or without any, will present the most varied appearances, and the honest investigator is thus embarrassed to decide which is the true form.

These complications follow the use of the instrument through

all its stages; but when the causes are well understood, the difficulties are reduced to a minimum, and even turned to account in the examination of difficult objects.

Great success in the use of the microscope can only be obtained by the skilful manipulation of the light, and he that is not acquainted with the numerous schemes, devices, and contrivances in its management, might as well be in the dark; no directions here avail, and nothing but diligent and constant practice will render the student efficient in this respect.

I once stood an hour watching a leading London optician struggling to show me the true markings of a diatom with a new object-glass he had recently constructed, with which he had had no previous difficulty. He at last gave up the attempt in despair. Of course an objective that has once performed a specific test will do so again. In this case the only thing in fault was the management of the light. This had disgraced the object-glass, and enraged its maker.

In contrast with the above case I may mention the real pleasure I experienced in witnessing the skill of a professional microscopist of this country. In his hands all difficulties appeared to vanish, and he showed me one of the most difficult objects known, with marvellous promptitude.

But to return to my subject. To enable the student to familiarize himself with the true power of the microscope, and to train his eyes to detect errors of vision, certain well-known test-objects are in general use, which are also convenient to test the quality and power of objectives. A favourite object of this class is the scale of the Podura, a minute insect, which dwells in remote nooks of dark and damp cellars, and similar localities.

This scale is usually mounted dry, and when viewed under the compound microscope with suitable objectives, presents a surface studded with marks similar to the well-known note of exclamation (!).

This test-object has been for years the delight of microscopists possessing high powers, and a sharp definition of its peculiar markings, as above mentioned, was accepted as its true appearance and form.

For twenty-five years this scale was under constant examination by every grade of microscopists, from the grandees of the Royal Microscopical Society to the humble tyro, without any new or special feature being noticed, when on November 10, 1869, Dr. G. W. Royston-Pigott, F.R.M.S., read a paper "On High-Power Definition," before the Royal Microscopical Society, and surprised the members by stating that all these years they had been gazing at the Podura scale, but had never yet seen its true markings. Dr. Pigott's paper described very fully what he had discovered as

the true markings, and illustrated it with drawings which represented them to be distinctly of a beaded character; in fact, as dissimilar from the old accepted idea of their form as contrast could depict them.

Every microscopist was now hunting Poduræ, and cellars damp and dismal were ransacked for the little scale-bearers, doubtless to the astonishment of numerous colonies of spiders, who must have been much provoked by this invasion, and thus commenced a controversy which is not yet concluded. Men equally eminent have taken opposite sides, and expressed the most contrary opinions; and I now propose to give a brief *résumé* of what has been said and done in regard to this subject, because the matter is full of instruction to those interested in microscopical research. Not that the markings of the Podura are of the slightest importance, or have any scientific significance, but the gravity of the conclusions which are sought hinges upon the fact that, if the views of Dr. Pigott are correct, our most eminent microscopists have been promulgating false and erroneous statements respecting the form of a well-known and common object; and in whatever light the controversy is viewed, the humiliating confession must be made that they are still unable to determine the correct focus or the proper method of illuminating it.

Dr. Pigott commences by calling resolving the Podura scale "a difficult enterprise," and then describes the beaded appearance in the following manner: "Under a low power, as 80 or 100, the Podura scale is remarkable for its wavy markings, compared to watered silk; raising the power to 200 or 250, and using a side light, the waviness disappears, and in its place longitudinal *ribbing* appears; with 1200 they divide themselves into a string of longitudinal beads; but with 2300 they appear to lie in the same plane and terminate abruptly on the basic membrane; in focussing for the beads attached to the lower side, the beadings appear in the intercostal spaces."

Respecting the old received views of the Podura scale, Dr. Pigott says: "With 300 to 500, the celebrated 'spines' appear, according to the size of the scale, as very dark tapering marks (like 'notes of admiration' without the dots ! ! !). To see these clearly with 2500 has been considered the *ne plus ultra* of microscopical triumphs, and it is consequently with no small diffidence that the writer ventures to traverse the belief of twenty-five years."

Dr. Pigott further states that he reckons these beads to be $\frac{1}{50000}$th to $\frac{1}{150000}$th of an inch in diameter, and that the "spines," which he calls spurious, really embrace in general three or four beads, while the intervening space abounds with beads seen through the basic membrane, and very difficult of observation without special management; and concludes with the remark that he expects in a

few months the Podura beadings, such as he described them, will be fully established.

Thus was the gauntlet thrown down, and the challenge was at once accepted by various members of the Society, who, on the conclusion of the reading of the paper, at once disputed the doctrine. Mr. J. Beck was the first to express an opinion, and rather increased the confusion of the subject by stating that both the spines and the beads were illusory, and that the true structure of the Podura scale was a series of corrugations on one side, and that the reverse side was slightly undulating or nearly smooth, and that the notes of exclamation were due to refraction of light.

Mr. Hogg, the Hon. Secretary of the Society, thought Dr. Pigott in error; he had never seen such appearances as beads; thought probably Dr. Pigott had seen them by using too deep an eye-piece, bad illumination, and drawing out the tube of the microscope to too great an extent; or, perhaps, to a disturbed vision caused by advanced age and presbyopia.

The President, the Rev. J. B. Reade, followed by stating that he agreed with the observations made by Mr. Hogg; and such was his faith in the skill of the opticians of the day, that he could not but feel that what he saw with their instruments really existed.

On the same date and occasion on which Dr. Pigott expounded his views, Mr. S. J. McIntire, a member of the same Society, read a paper "On the Scales of Certain Insects of the Order Thysanura." Now, Mr. McIntire, although a recent member, and young in microscopical research, is always listened to on this subject with respect by the Society, having devoted his attention specially to these insects, and shown a patient and intelligent power of observing not only their structure but their habits; he, in his communication, opposed Dr. Pigott's views, and calls the beads "optical illusions," and concurred with Mr. Beck's statement that the surface of the scale is corrugated, but flatly contradicts him by stating that both sides are alike.

December 8, 1869.—The President, the Rev. J. B. Reade, stated that he, with Dr. Miller and others, had interviewed Dr. Pigott, and was bound to say he had seen the beaded appearances, and it was clear to him *now* that in the best object-glasses small residuary aberration existed.

This slur upon the best object-glasses brought out Mr. Wenham with a paper in the Microscopical Journal of June, 1870, in which he repudiated such error, and described the beaded appearance as an illusion, obtained by a trick of illumination, and by examining the scale with the microscope out of focus.

At the June meeting of the Royal Microscopical Society a letter was read from Colonel Woodward, of Washington, enclosing photographs of the Podura scale, showing what he considered to be

the true appearance. These photographs showed the spines. Colonel Woodward, however, reserved his opinion, and asked for a specimen of the true test-Podura scale.

Dr. Maddox, in August, exhibited various photographs of Podura scales, which Mr. Wenham commented on in a paper to the Microscopical Journal of September following, which merely reiterated his views that the "spines" were the true appearance of Podura scales.

The Rev. J. B. Reade, in the 'Popular Science Review,' of April, 1870, appears to accept Dr. Pigott's views entirely, and writes: "I can now see with my own powers what has been before invisible, viz. the beautiful beaded structure of the whole test-scale, as discovered by Dr. Pigott."

It would be tedious to continue the subject and give even an outline of the papers and discussions that have been provoked by this knotty question; I shall therefore conclude by stating that Colonel Woodward has since produced two photographs, showing the two aspects of the question; they are made from authentic scales, and are pronounced very perfect.

In further illustration of the difficulty of obtaining a true and reliable image of an object when viewed under the microscope with high powers, I offer drawings which have been made by Mr. Ralph H. Westropp, B.A., T.C.D., of Allyflin Park, England. These figures all represent the same object, a scale of Podura viewed under different phases of oblique light; they are interesting as showing the effect produced by the play of light upon a refractive object.

The fact that the most skilful microscopists of the age all differ upon the true appearances of a common and not very minute object, and the microscope itself presenting to the vision the most opposite appearances of one and the same object, should act as a caution to those who accept too readily theories based upon microscopical research; and suggests that, in the cause of justice, when life is at stake, single-handed evidence relating to the microscopical examination of apparent blood-stains should be verified at least by a second person before being accepted.

Thus we see that the so-called revelations of the microscope are but hieroglyphics, needing the interpretation of a mind of the highest culture, and that while the microscope is a good servant it is a bad master—mighty in the hands of a Huxley, but as useless to a man without the powers of discrimination as the chisel of Michael Angelo would be in the hands of a Modoc.—*The American Popular Science Monthly*, June, 1875.

V.—*Double Staining of Wood and other Vegetable Sections.*

By George D. Beatty, M.D., of Baltimore.

In my paper on vegetable staining in the April number of this Journal,* I said the only aniline colour I had used with success for staining leaves was the blue. The statement was based on the fact that this colour did not come out when the leaves were put into absolute alcohol, or into oil of cloves, provided certain brands of these chemicals were used.

I have lately discovered that benzole fixes the anilines when they are used in staining vegetable and animal tissues. It not only instantly fixes any aniline colour in vegetable tissues, but also renders them as transparent as oil of cloves.

Finding that benzole possessed this property, led me to try double staining upon sections of leaves and sections of wood. The results have proved highly satisfactory. I have found the following processes successful: A section, say of wood, being prepared for dyeing, is put for five or ten minutes in an alcoholic solution of "roseine pure" (magenta), one-eighth or one-quarter of a grain to the ounce. From this it is removed to a solution of "Nicholson's Soluble Blue Pure," one half-grain to the ounce of alcohol, acidulated with one drop of nitric acid. In this it should be kept for thirty or ninety seconds, rarely longer. It should be frequently removed with forceps during this period, and held to the light for examination, so that the moment for final removal and putting into benzole be not missed. After a little practice the eye will accurately determine the time for removal.

Before placing the object in benzole it is well to hold it in the forceps for a few seconds, letting the end touch some clean surface, that the dye may drip off, and the object may become partially dry. By doing this, fewer particles of insoluble dye rise to the surface of the benzole, in which the brushing is done to remove foreign matter. The object should then be put into clean benzole. In this it may be examined under the glass. If it is found that it has been kept in the blue too short a time, it should be thoroughly dried, and, after dipping in alcohol, be returned to that dye. If a section of leaf or other soft tissue be under treatment, it should be put in turpentine or oil of *juniper*, as they do not contract so much as benzole.

When hæmatoxylon is used instead of magenta, it is followed by the blue as just described. As neither of these dyes comes out in alcohol or in oil of cloves, the section may be kept in the former for a short time before placing in the latter.

The hæmatoxylon dye I prefer is prepared by triturating in a

* 'Cincinnati Medical News,' June, 1875.

mortar for about ten minutes two drachms of ground campeachy wood with one ounce of absolute alcohol, setting it aside for twelve hours, well covered, triturating again and filtering. Ten drops of this are added to forty drops of a solution of alum; twenty grains to the ounce of water. After one hour the mixture is filtered.

Into this the section, previously soaked in alum-water, is placed for two or three hours, or until dyed of a moderately dark shade. When dyed of the depth of shade desired, which is determined by dipping it in alum-water, the section is successively washed for a few minutes each, in alum-water, pure water, and 50 per cent. alcohol. Finally it is put in absolute alcohol until transferred to the blue.

Carmine and aniline blue produce marked stainings, but they are rather glaring to the eye under the glass. I use an ammoniacal solution of the former, double the strength of Beale's, substituting water for glycerine. In this a section is kept for several hours. On removal it should be dipped in water, and then put for a few minutes in alcohol acidulated with 2 per cent. of nitric acid; then in pure alcohol; then in the half-grain blue solution before spoken of, from which it should be removed to alcohol; then to oil of cloves. Much colour will be lost in the acid alcohol. The acid is to neutralize the ammonia, which is inimical to aniline blue. Magenta aniline or hæmatoxylon may be used with green instead of blue aniline. The brand of green I prefer is the iodine brand, one grain to the ounce of alcohol.

Double stainings of sections of leaves in which red is first used have the spiral vessels stained this colour, other parts being purple or blue. Radial and tangential sections of wood have the longitudinal woody fibres red, and other parts purple or blue.

This selection of colour is, I think, due to the fact that spiral vessels and woody fibres take up more red than other parts, and are slower in parting with it. The blue, therefore, seems first to overcome the red in parts where there is less of it. It will entirely overcome the red if sufficient time be given.

If the blue be used before the magenta aniline, the selection of colour is reversed.

I would here call special attention to the importance of examining these stainings at night, as the red in them has a trace of blue in it which does not show at that time, but comes out so decidedly by daylight, as to change, even spoil, the appearance of the specimen.

I think they should be mounted in Canada balsam, softened with benzole, as the presence of the latter may be beneficial in preserving its magenta.

I would offer a few words upon section-cutting, and upon preparing sections for dyeing.

To cut a thick leaf, place a bit of it between two pieces of potato

or turnip, and tie with a string. Cuts may be made along the mid-rib, or across it, including a portion of leaf on either side, or through several veins. Fine shavings of wood may be used, or pieces rubbed down on hones.

Sections of leaves may be decoloured for staining by placing for some time in alcohol; but I would recommend the use of Labarraque's solution of chlorinated soda, for twelve or twenty hours after the alcohol. Especially do I recommend the Labarraque for all kinds of wood. In twelve hours wood is generally bleached; too long a residence in it will, however, often cause it to fall in pieces.

After removing from the soda, wash through a period of twelve or eighteen hours in half-a-dozen waters, the third of which may be acidulated with about ten drops of nitric acid to the ounce, which acid must be washed out. Next put in alcohol, in which sections and also leaves may be kept indefinitely, ready for dyeing.

Before closing this I would add a few suggestions concerning leaves not contained in my January article.

Magenta, when used for them, should be of the strength of one-eighth or one-quarter of a grain to the ounce of alcohol, and purples and iodine-green two or three times as strong. These anilines are inferior to the blue in bringing out all the anatomical parts of a leaf, including the beautiful crystals so often met with. On removal from the dye, leaves should be thoroughly brushed with camel-hair pencils.

One week, instead of forty-eight hours, is frequently required to effect the decolouration of large leaves in chlorinated soda, even when they are cut into several pieces, which is advisable.

Mr. L. R. Peet, of Baltimore, whose stainings in aniline are unsurpassed for beauty, thinks better results are attained by commencing with a weak dye, say from one-twentieth to one-twelfth of a grain, and slowly increasing the strength of the dye, at intervals of from one to three hours, until the required hue is obtained. This process certainly guards against too deep staining, and may give a finer tone to leaves under the glass.

VI.—*On Conjoined Epithelium.* By S. Martyn, M.D., F.R.C.P., Lecturer on Medicine and Pathological Anatomy, Bristol Medical School.

Plate CXII.

It does not appear that much attention has been given by English observers to the so-called "prickle and ridge" cells (*Stachel- und Riff-zellen*). At all events, they have scarcely found their way into our most recent books on pathology; while the drawing in our

newest text-book on minute anatomy is almost, if not quite, identical with the earliest sketch in Virchow's 'Archiv.'

It is, nevertheless, twelve years since these cells were first discovered and named by Professor Max Schultze; and their distribution in fishes and amphibia has been fully described (1867) by his brother, Professor Franz Schultze. They have been subsequently noticed by Kölliker, Frey, Rindfleisch, Stricker, Cornil and Ranvier, Rollett, and others. They have been called "spinous cells," "echinate cells," "cellules dentelées," "ribbed" and "spiny and furrowed" cells; but no observer seems to have added any careful researches to the original description of Max Schultze, by whom the name was given to them of "prickle and ridge" cells. And yet there is a certain beauty and interest attaching to these structures which seem to entitle them to more detailed notice; and while they offer problems in histogenesis of great interest to the anatomist and physiologist, they cannot but do so also in some degree to the pathologist. It is a good illustration of the old story of "eyes and no eyes," that a very remarkable structure, and not a difficult one either, in so thoroughly common a place as the borders of an epithelial cell, should have been persistently overlooked for many years, and long after high microscopic object-glasses had been brought to great perfection. No one need despair of seeing something new and important by looking with extra carefulness at what is, so to speak, straight before his face, after this fact, that the mere form of the simplest of all animal cells actually remained unnoticed until a dozen years ago.

The discovery of Max Schultze, then, was this: that in the epithelium of the mouth and conjunctiva firstly, and then in the epidermis of the skin, and indeed wherever there is true pavement or scaly epithelium in many layers, the second series upwards of cells have this peculiar character. Their surface is furnished with short projections, which are apt to appear as prickles; while sometimes there are ridges (reefs) marking portions of the cell with parallel groovings (see drawing). The lowest layer of epidermic cells consists of elongated vertical palisading-like elements, between and underneath which lie germinal masses of indeterminate form. Next above, and completing what was called "rete mucosum," is a succession of layers of spheroidal cells, becoming more and more flattened until they cohere under the form of mere scales into cuticle on the skin, or in a less firm manner in transitional situations like the mouth, conjunctiva, &c. It is these latter layers which are mostly spinous. There is some difficulty in seeing this well in thick skin. It was already noticed by Max Schultze, that the diseases involving the true skin showed the prickle-cells well; and other observers, especially Cornil and Ranvier, have followed in the same line (Fig. 1).

The anatomical structure, then, has been described thus: the cells are covered more or less with spines, which, when the cells are isolated, show as marginal prickles; and if the covering pressure chance to be *nil*, as besetting the surface. The spines are sharp-pointed, with a broadish base; and all observers, I believe, describe them as interlocking with the teeth of the next cells, thus giving the characteristic cohesion. Max Schultze compares this to the intermingled bristles of two brushes put together. Ranvier speaks of the cells as soldered by the interlocking teeth ("les dentelures au moyen desquelles elles sont engrénées et soudées"). Stricker uses the same phrase, and Rindfleisch compares their union to "a suture"; and I learn that the well-known Edinburgh *préparateur* Mr. Stirling, discovering them nine years ago, thought them like "watch-wheels" (see Fig. 1).

At first sight, this all seems satisfactory enough; but it is just as to the simple nature and plan of these processes that I venture to join issue with all observers to whose works I have been able to obtain access. So long, namely, as I looked at the contact between any two of these cells as that of toothed wheels, they really seemed to interlock. I confess, however, to a rooted distrust of real processes of definite form as being projected from a cell-wall of formed material, this having no analogy with the many changes of form in germinal or bioplastic matter. As far back as 1860, when the language of, I believe, all the books was of processes sent out by cells, I ventured to dissent from such views, and, in a paper on Connective Tissue published in Beale's 'Archives,' described cell-processes as being "spun out" by a receding body. This view is now generally accepted. It seemed, therefore, to me worth inquiry, whether these prickles were really processes which interlock, or whether they were really continuous delicate bands uniting cell to cell. In an epithelioma of the lip, and in a section of a hard preputial chancre, I found instances of a rich development of the prickle-cell. Probably, just as on a nasal polypus the ciliated epithelium often becomes monstrous, many fine examples of these cells may be encountered where there is unusual setting free of formative force.

In the example (Fig. 2), the cell (*a*) is incontestably united by bands to its neighbours, and the decisive experiment has been made; for, where these bands have broken across, the remaining stumps have become prickles; but where (two cells touching) the teeth seem to have strongly dark ends (Fig. 2, *b*), I believe this effect is universally an optical illusion, and that the dark spot is the cavern between bands (*b*). On the other hand, where (two cells touching) the ordinary *light* spines seem to be so truncated, a very careful view will show that the light track may be followed from one cell to the next, if I may use the illustration, like walking

across on a plank. In some cases, however, pointed teeth may be seen at touching cell-margins, as in Max Schultze's original plate. But it is an illusion, in my opinion, to see in this appearance a true second set of interlocking teeth, the outlines of which correspond precisely with the first. Rather let it be granted that prickles are broken bands of union in all cases, as we know they are in (e. g. *a*, Fig. 2), and then the whole thing becomes intelligible. In a microscopic preparation thin enough to be seen well with 1000 diameters, most cells are partly isolated, and consequently broken bands, i. e. prickles, abound, and those near the outer margin of the cells lie as detached teeth touching from each cell-wall, or even interlocking in some instances. Meanwhile the cell-margins of real contact deeper down are united by unbroken bands.

So much for the prickles; and next as to the "furrows," "reefs," "ribs," or "ridges" (see Fig. 1, *a*, and Fig. 2, *c*). I confess that at present the only explanation which occurs to me is founded on an insufficient number of observations. As a conjecture supported by some instances, I would suggest that these are *stretched bands* often broken off at one end, and lying parallel on the cell-wall. They are beautifully seen with careful oblique illumination under high powers.

This peculiar cell-structure which we have been describing may, then, be supposed to originate in this sort of way. In the lowest cell-layer of epithelium there are found the newly grown, long, vertically placed cells, side by side in contact. When these have a somewhat permanent character, as in many fishes, the next layer of cells consists of spheroidal cell-forms which have resulted from division or budding, and consequent "fissiparous" multiplication or budding of a *new* progeny. In all this there is a tendency of dividing cells to remain, at points, united by threads of formed material of hard cell-wall; e. g. amongst these very layers are often found large and branched pigment-cells united by long delicate threads. Thus I imagine the cells of rete mucosum, in multiplying originally by subdivision, retain numerous points of incomplete severance, and these points of cohesion are dragged out and become the uniting bands. These, when severed by any accident, assume the characteristic form of "prickles." In the normal ascending series, these cells, becoming older and flatter, lose all surface connection, and in the cuticle are simple polyhedral horny blocks. The uniting agency of these cells is a firm one; for in a scraping from a tumour section, as noticed by Ranvier, they almost always hold together strongly in groups. It is a firmer union than I should expect from any interlocking teeth, but of course one which would naturally result from uniting bands.

Lastly, as to the pathological anatomy. The primary meaning of the presence of these cells is an abnormal formative activity of the lower cells in the rete mucosum; and the occurrence of rib and

PLATE CXII.

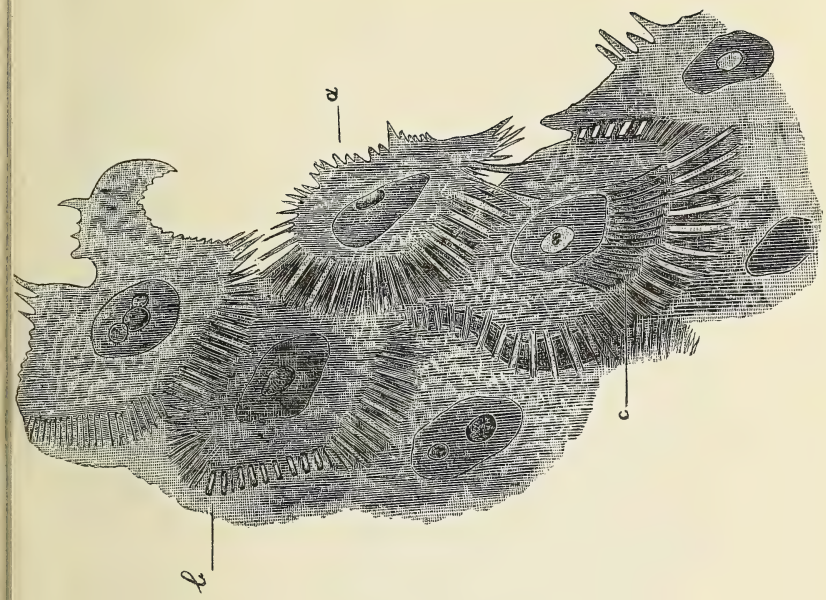


Fig. 2.

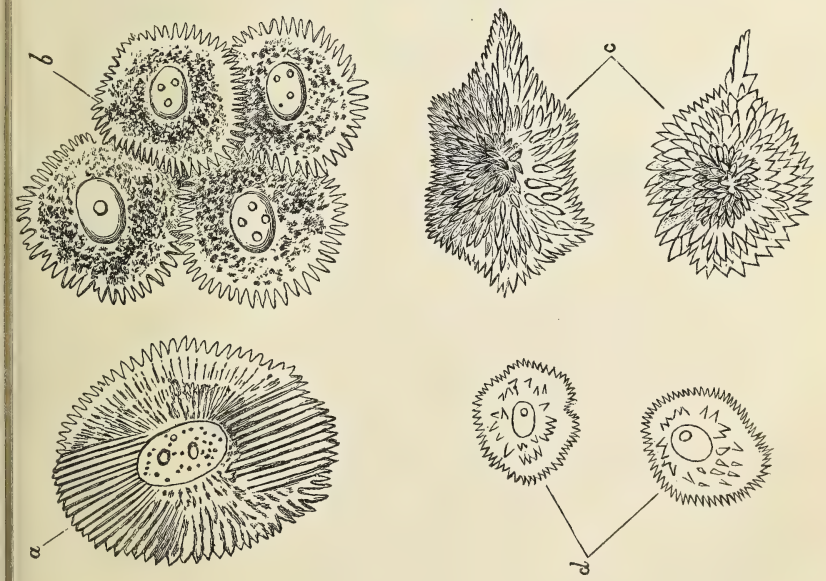


Fig. 1.

prickle cells may be taken as indicative of this structure having become involved. In epithelioma occurring in connection with *flattened* epithelium, both lobular and tubular, they mostly occur. Their development seems to be in direct ratio to the vigour of growth present, and accordingly they were very large in granulations snipped from a perineal fistula. Papilloma and rodent ulcer afford fine specimens; and probably always the best occur in diseases of the transitional epithelium extending from the lips to the cardiac orifice, and on the conjunctival, anal, or preputial epidermis. I have found them unusually fine in a section of preputial chancre. I believe it to be the opinion of Professor Lister, who discovered these cells independently, and named them "echinate," that rodent ulcer, when they are present in it, should be considered as allied to, if not identical with, epithelioma. The presence of prickle-cells in cylindroma leads me strongly to doubt the origin of that form of cancer from the sudariparous glands, according to the received notion. But, as I indicated at first, the real meaning, for medicine and surgery, of these curious cells, remains to be investigated; and it is necessary that some one should do that for their origin, varied forms, monstrosities, and pathological distribution which has been so laboriously accomplished for their anatomy in fishes and amphibia by Professor F. Schultze. My present attempt has been, so far, merely to see more of the true character of their intimate structure, and, if possible, to determine the true mode in which they are united together.—*The British Medical Journal*, June 26.

DESCRIPTION OF PLATE CXII.

Fig. 1.—Epidermic "prickle and ridge" cells; or "spinous," "furrowed," or "echinate."

a. Papillary tumour of tongue.
b. Human epidermis (Max Schultze).
c. Ext. layer, middle cells—Cornea of pig: Stricker and Rollett.
d. Cellules dentelées of cylindroma: Cornil and Ranvier (1869).

„ 2.—Conjoined epithelium (1000 diameters).

VII.—*The Microscopic Germ Theory of Disease; being a Discussion of the Relation of Bacteria and Allied Organisms to Virulent Inflammations and Specific Contagious Fevers.* By H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College.

When honoured by a request from the Council of this Society,* a few weeks since, to open a debate during the current session, compliance with such a wish was regarded by me as a professional duty. I was compelled, therefore, to do the best I could with the short time and limited leisure which presented themselves, though

* An address delivered before the Pathological Society of London.

these, I regret to say, have proved insufficient to enable me to bestow the attention I should have desired upon the vast accumulation of writings directly or indirectly related to the subject selected for discussion, and quite insufficient also to enable me to throw light upon it, to the extent I should have wished, by certain new observations of my own. The subject, however, large as it is —and consequently difficult to be dealt with satisfactorily in the space of one hour—seemed to recommend itself for several reasons: (1) It is a question lying at the root of the pathology of the most important and most fatal class of diseases to which the human race is liable—diseases which cause nearly one-fourth of the total number of deaths in this country. (2) It is a subject important alike to those engaged in almost every department of our profession. And (3) it is one which I happen to have very carefully considered for several years, and for the elucidation of which I was tempted in 1869 to undertake long and laborious investigations, though these may have seemed to many to have little practical bearing upon the science of medicine.

The subject of the relation of the lower organisms to disease has, moreover, a growing importance. The notion that there is a distinct causal relation between the two—though it has long existed in one form or another—is one which has been spread enormously within the last few years, partly owing to our increase of knowledge concerning these low organisms, and partly because of their ascertained presence in numerous diseased tissues and exudations. Medical literature both at home and abroad, now, in fact, teems with papers and memoirs bearing upon this relation, and such communications rapidly increase in number year by year.

In the short time allotted to me to open the debate, I shall be able to make specific allusions to but few of these contributions, as it would seem better to keep the broad issues well in view in my opening statement, and reserve questions of detail, as these may be taken up by other speakers and subsequently commented upon if necessary.

The one common and distinguishing feature peculiar to all the diseases whose pathology we are now about to consider is their "contagiousness." An individual affected by either of them throws off particles from the region specially affected, or from many parts of the body, and these particles, on coming into contact with suitable surfaces in other persons, may incite similar local or general diseases, though such results do not invariably follow. This peculiarity, by means of which such diseases are spread amongst the members of a community, was, even in the time of Hippocrates, compared to the property by which one fermenting mass may communicate its state of change to another mass of fermentable

material. Throughout all intervening periods such an analogy has never been lost sight of; it has rather been more and more strongly dwelt upon. Thus, more than two centuries ago, we find, as has been recently pointed out, Robert Boyle, one of our great English philosophers, and himself a pioneer in scientific investigation, giving strong expression to the then current view: "He that thoroughly understands," he says, "the nature of ferments and fermentations shall probably be much better able than he that ignores them to give a fair account of several diseases (as well fevers as others) which will perhaps be never thoroughly understood without an insight into the doctrine of fermentation." Again, in more recent times, it was doubtless under the influence of a belief in the same analogy between fermentations and the class of diseases of which I am about to speak that the term "zymotic" was proposed by Dr. Wm. Farr, and adopted as a general designation under which nearly all these diseases might be included. The consequence of the adoption of this nomenclature has been that views as to the nature of the infecting something, or *contagium*, have since been so powerfully influenced as to be actually led by views at the time entertained concerning the nature of *ferments*—the relationship supposed to exist between zymosis and fermentation has indeed been stamped and ratified by the very general consent of the profession.

Omitting for the present any remarks as to the real strength of this analogy, I would merely further point out that the foundations of the "germ theory of disease," in its most commonly accepted form, were laid in 1836 and shortly afterwards. The discovery at this time of the yeast plant by Schwann and Cagniard-Latour soon led to the more general recognition of the almost constant association of certain low organisms with the different kinds of fermentations. But it was not till twenty years afterwards that Pasteur announced, as the result of his apparently conclusive researches, that low organisms acted as the invariable causes of fermentations and putrefactions; that these, in fact, though chemical processes, were only capable of being initiated by the agency of living units. If, in accordance with this somewhat narrow and exclusive view, living units were to be regarded as the sole producers of fermentation and putrefaction, then they were sole ferments. The extension of this doctrine by medical men to contagious diseases, in face of the analogy sanctioned by the use of the term "zymotic," became only too easy. It was obviously nothing but the logical outcome of the two sets of views to hold that low organisms were the true contagia, or sole "germs" of the so-called zymotic diseases.

It so happens, therefore, that the very exclusive notion just mentioned as to the nature of contagia is at present almost as deeply rooted in the minds of the majority of writers on epidemic diseases and contagious fevers as was the opposite notion, founded

upon the physico-chemical doctrine of Liebig, some twenty years ago. Then a ferment was regarded as a portion of organic matter (not necessarily living) in a state of molecular change ("motor decay"), which, by virtue of its own unstable nature, was capable of communicating molecular movement (chemical change) to other unstable or fermentable mixtures. This broader notion was promulgated by Liebig at a time when less was known than at present as to the constant association of low organisms with the processes of fermentation and putrefaction. The nature of this relationship was, in fact, never adequately grappled with by him. Still, views of the kind promulgated by Liebig would not give anything like the same support to the germ theory of disease as that afforded by the doctrines of Pasteur. Those who have adopted and developed Liebig's views now hold that living organisms, though they may operate as ferments, act in this capacity merely by virtue of the chemical changes which the carrying on of their growth necessitates; and that other chemical changes taking place during the decay of organic matter may make fragments of it (in the dead state) almost equally capable of initiating fermentative changes in suitable media; whilst in either case bacteria or allied organisms are prone to be engendered as correlative products.

In the present day, therefore, two questions seem to need the serious consideration of medical men. In the first place, it may be asked, Are we justified in relying so strongly upon the analogy between fermentation and zymosis? Secondly, we may inquire whether the researches by which Pasteur claims to have established the sole nature of ferments are so conclusive as they have been commonly regarded? In reply to the first question, certain qualifying considerations will hereafter be stated, though it may be at once admitted that the analogy is so strong as to make it likely to continue to exercise a very considerable influence upon medical opinion. It therefore becomes all the more necessary for medical men to look to the foundations of Pasteur's doctrine, if they are not prepared blindly to follow his dicta on a subject which is of so much importance for medical science. It was with this view that I undertook, a few years ago, and shortly after I had been called upon to teach pathology, a series of investigations bearing upon this subject. In consequence of this work I was compelled, as others had been, to refuse assent to the exclusive doctrines of Pasteur concerning the nature of ferments. I do not enter upon this discussion now. I maintain, however, that my own investigations and those of others show that units of living matter are not sole ferments, since fermentation and putrefaction may be initiated in their absence, and since it can be shown that mere particles or fragments of organic matter may act in this capacity. For a brief exposition of the grounds of this belief I would refer those interested in the matter

to my recently published work, 'Evolution and the Origin of Life.'

Some time must be allowed to elapse before anything approaching to general agreement can be expected on such a subject; and meanwhile, standing as we do in the face of opposite doctrines as to the nature of ferments, we are free to look into the question of the relation of the lower organisms to disease on its own merits, apart, that is, from the overweening influence of any general theory of fermentation.

Leaving on one side, therefore, the influence of the analogy deemed to exist between the process of fermentation and that of zymosis, we may ask what other general evidence is forthcoming in favour of the notion that contagia are low organisms or living units, rather than dead organic particles from altered tissue-elements, or complex chemical compounds of alkaloidal constitution engendered in the tissues or in some of the fluids of the body. The consideration of this question may be introduced by a quotation from Dr. Burdon-Sanderson's valuable Report on the "Intimate Pathology of Contagion."* He says: "There are two obvious objections which stand in the way of the acceptance of any chemical explanation of the phenomena of contagion. The first is, that the multiplication of contagium in the body of the infected individual is a process which cannot be compared to any which is brought about by chemical agencies independently of organic development. The second is, that all contagia possess the power of retaining their latent virulence for long periods (often resisting the most unfavourable chemical and physical conditions), and only show themselves to be what they are when they are brought into contact with [the] living organism. Outside of the body the contagious material withstands all those changes to which, on chemical grounds, we should expect it to be liable; while in the body it manifests a degree of activity, and gives rise to an amount of molecular disturbance, which is quite as unaccountable. . . . . Neither of these difficulties stands in our way if we suppose that the contagious process is connected with the *unfolding of organic forms.*"

Now, though this is about as strong a statement as can be made, from an *a priori* point of view, against the mere chemical action of contagium, and in favour of a germ theory, I must confess that neither of the considerations seems to me to carry very much weight with it. I should be inclined to say, in reply (1) that proof is altogether wanting of the "multiplication of contagium" in the body in the same sense that a living unit multiplies; and that there are physico-chemical processes which may illustrate what occurs when contagium increases within the system. Instead of being an increase by continuous organic development and multipli-

* 'Twelfth Report of the Medical Officer of the Privy Council, 1870,' p. 243.

cation, it may be that contagium augments by some such process as that by which crystals of sulphate of soda increase or "multiply" when a fragment of such a body is thrown into a complex fluid containing its component elements. This is confessedly a very imperfect illustration, and one to which I resort merely to indicate the possible occurrence of another mode of increase of contagium within the body; though in an infected animal such increase may occur in a much more subtle manner, owing to the fact that fluids altered, directly or indirectly, by the original contact of contagium with some part of the body, are either locally or generally brought into intimate relation with the active, though modifiable, living units of the various tissues. And (2) in reply to Dr. Sanderson's other objection, which stands, as he supposes, in the way of any chemical explanation of the phenomena of contagion, I should say that, although our knowledge is at present extremely vague concerning the power possessed by the various contagia of retaining their virulence for long periods, and of resisting unfavourable physical and chemical conditions, we have no reason to believe that the more complex combinations of which living matter is composed are capable of resisting influences which would prove destructive to less highly complex not-living substances, such as snake-poison, woorara, or other compounds of this class. The general evidence is therefore, as I read it, certainly not more favourable to a vital or germ theory than to a physico-chemical theory as regards the nature and action of contagia.

I should here point out, however, that under the term "germ theory" two distinct views are included, each having their advocates amongst distinguished members of this Society. The side to which Dr. Sanderson leans is sufficiently obvious. Speaking of contagious particles, he says: * "With reference to their mode of action, we have examined into those considerations which seem to render it probable that they are *organized beings, and that their powers of producing disease are due to their organic development;* and we have accepted this doctrine as the only one which affords a satisfactory explanation of the facts of infection." †

This is the doctrine with which we are at present especially concerned, though it may be well for me to say a few words concerning the other sense in which a "germ theory of disease" is maintained by a distinguished member of this Society. Dr. Beale says: ‡ "We have therefore now to inquire what is the material substance which passes from the diseased to the healthy organism

* *Loc. cit.*, p. 255.

† These words occur in a summary which, it is only right to add, was immediately prefaced by the following statement: "The sentences which follow must therefore be accepted by the reader as nothing more than indications of the questions we are trying to solve, or as forecasts of what we hope to establish or disprove by experiment."

‡ 'Disease Germs, their Real Nature,' 1870, p. 5.

in small-pox, in measles, in scarlet fever, and other allied contagious diseases from which man and domestic animals suffer so severely. *The material in question grows and multiplies and produces its kind, as all living things do,* and as nothing that does not live has been proved to be capable of doing. We may therefore conclude that it is living matter." And as to the derivation of such matter, Dr. Beale says, "a disease germ is probably a particle of living matter derived by direct descent from the living matter of man's organism," though he supposes it to be altered and degraded as regards formative power by previous rapid multiplication of the tissue-elements or particles from which it has been derived. In many respects I am disposed to assent to this view, so long as it is not taken in too exclusive a sense. I will now, however, only mention what I consider to be its weakness. It seems to me that proof is wholly wanting as regards the statement which I have had printed in italics. That there is an enormous increase of germinal particles in the blood and in many of the tissues in these specific contagious diseases, Dr. Beale has helped to show us by his valuable researches upon the pathology of the cattle plague and other allied affections; but that such germinal or living particles are in any direct sense the descendants of the particles which acted as contagia, or, in fact, that the contagious particles really multiply to any extent in the body; these are propositions which at present appear to me to be wholly devoid of all proof. I and other pathologists are free to hold that contagious particles, whether composed of living or of not-living organic materials, may initiate changes in the tissues and fluids with which they come into contact, which changes may be exaggerated as they spread, so as at last to implicate the blood. And as one result of this altered constitution of the nutritive fluid and of the general febrile condition simultaneously excited, we may get that undue proliferation of tissue-elements and multiplication of their products which appear to go on in the blood and in the various tissues of persons suffering from these febrile diseases. Beyond these surmises we seem also to have to do with mere conjecture rather than with established facts.

Leaving this aspect of the question, therefore, I now turn to the special subject of this debate, viz. the truth of the germ theory as it is ordinarily understood, or the relation of the lower organisms to virulent inflammations and their sequelæ on the one hand, and to specific contagious fevers on the other.

Applicability of the germ theory to virulent inflammations and their sequelæ (gonorrhœa, purulent ophthalmia, erysipelas, hospital gangrene, puerperal fever, pyæmia, septicæmia, &c.)

A few years ago no one would have thought of connecting the contagiousness of gonorrhœa or of purulent ophthalmia with the presence of bacteria. The respective secretions were known to

contain some poisonous element either in the form of a chemical compound or altered product of tissue multiplication (pus), which, when it came into contact with a healthy mucous membrane, was capable of acting as a specific irritant, and there exciting a similar morbid process. It is by no means certain, however, that some pathologists would not at the present time connect this process with the presence of bacteria in the contagious fluids. Such a point of view has, indeed, been directly fostered by doctrines recently put forward by an eminent pathologist, Dr. Burdon-Sanderson. At this Society in 1871, whilst, strangely enough, professing to be indifferent to the mode of origin of bacteria, Dr. Sanderson said: "They afford a characteristic by which we may distinguish the products of infective inflammation from those which are not infective." And in a more recent paper on "The Infective Product of Acute Inflammation," * referring to his previous researches, he says it was inferred from these that, "if infective agents are particulate, they are probably comprised in that group of bodies to which I then applied the term microzymes, recognizing their identity with the *zooglæa* of Cohn, the *micrococci* of Hallier, and the various forms described by other authors under the terms *bacterium* and *vibrio*." And he then adds, as the result of subsequent investigations, the following passage: "With reference to these organisms, two entirely new and most important facts have been demonstrated by the observations to be now recorded. It has been discovered (1) that in all acute infective inflammations microzymes abound in the exudation liquids; and (2) that the same forms are to be found in the blood of the infected animals." And when Dr. Sanderson subsequently adds "that the relation of intensity between different cases of septicæmia and pyæmic infection is indicated by the number and character of these organisms," but little doubt seems to remain concerning his views as to the causal relationship of such organisms to the infectiousness of the inflammations referred to. And this view is not essentially modified by his subsequent concluding explanations, where he says: "Inasmuch as these organisms cannot have originated from the normal tissues or juices, they must have been derived from the external moisture." And also, "it does not at all follow because these organisms come in from outside that they bring contagium along with them; for it may be readily admitted that they may serve as carriers of infection from diseased to healthy parts, or from diseased to healthy individuals, and yet be utterly devoid of any power of themselves originating the contagium they convey." Such a doctrine still implies that bacteria are essential to a contagious process, though it seems to me to introduce certain very striking elements of weakness into the germ theory as thus interpreted. If this theory is

* 'Medico-Chirur. Trans.,' 1873, p. 354.

not tenable without the aid of some supplementary hypothesis, I cannot conceive that the introduction of the one above mentioned will be considered to have strengthened its foundations. Yet Dr. Sanderson apparently saw the difficulty of maintaining the germ theory in its integrity, and offered us this other view as a compromise. He considers it probable that, whereas true contagia, whether living particles or chemical compounds, may be engendered within the body in the tissues themselves, such contagia are not able to spread either within or outside without the aid of bacteria to act as "carriers." But why one set of particles should need others to carry them, or why bacteria alone should be able to bear about these mysterious contagious poisons which they are devoid of the power of originating, does not at all appear!

However complicated the doctrine may have been rendered, this is still practically the germ theory; and the same thing may be said with reference to a view which Professor Lister seems to entertain with some favour. He thinks that the lower fungi and their relations bacteria may contain within themselves some chemical compound absolutely peculiar to them, and forming part of their substance, which may act upon albuminous compounds after the manner of a ferment, such as emulsin.* "In this sense," he thinks, "as intervening between the growth of the organisms and the resulting decompositions, the theory of chemical ferments might be welcomed as a valuable hypothesis." This seems like the language of concession, but, practically, it is the germ theory still, and expressed too much, as all germ theorists who think out their views would have to formulate them. It would be no great concession to those who are not believers in an exclusive germ theory if, in the light of his views as above expressed, Professor Lister were to say that bacteria were "carriers of infection"; yet the apparent concession above referred to is no more of a concession to believers in a physico-chemical theory than the latter admission would be.

I will, however, now briefly enumerate the evidence which seems to me quite sufficient to disprove the probability of the existence of any causal relationship between the lower organisms and the diseases cited at the head of this section, and to establish, on the other hand, the position that the bacteria met with in diseased fluids and tissues are for the most part actual pathological products—that they are, in fact, engendered within the body, or are descendants of organisms owning such an origin, rather than of previously existing organisms introduced from without. It would take far too long were I to attempt to enter at any length upon a consideration of this evidence. I must therefore content

* 'Nature,' July 17, 1873.

myself with briefly summarizing the principal facts and arguments on which a judgment may be founded.

1. The experiments of many investigators prove that the alleged causes of diseases may be actually introduced into the blood-vessels of lower animals by thousands without producing any deleterious effects in a large proportion of the cases.

2. Bacteria, if not actually to be found within the blood-vessels of healthy persons, do nevertheless habitually exist in so many parts of the body in every human being, and in so many of the lower animals, as to make it almost inconceivable that these organisms can be causes of disease. In support of this statement I have only to say, that even in healthy persons they may be found in myriads in and about the epithelium of the whole alimentary tract from mouth to anus; they exist throughout the air-passages, and may be found in mucus coming from the nasal cavities, as well as in that from minute bronchi. They exist abundantly amongst the epithelial débris within the ducts of the skin, not only in the face, but in other parts of the body. Fresh legions of them are also being introduced into the alimentary canal with almost every meal that is taken, whence they may perhaps readily find their way into the mesenteric glands, if not farther within the system. And lastly, in persons with open wounds, bacteria are constantly to be found in contact with such surfaces, especially if the wounds are not well cared for, though the injured person does not necessarily suffer at all in general health.

3. It is no answer to these difficulties to say that there are distinct species amongst these lower organisms, some of which are harmless, though others are poisonous (or so-called "germs" of disease). In support of such opinion nothing can be alleged save some of the facts whose cause is doubtful; whilst against such an interpretation may be brought the experiments of several investigators, showing that bacteria are the creatures of circumstance, and modifiable to an extraordinary degree. The last position is even admitted by Professors Sanderson and Lister. The former acknowledges that they are "the lowest organisms," and that they are "much more under the influence of the conditions under which they originate and are developed, than organisms of any other class;" whilst Professor Lister's own work has compelled him to make an admission which, in the face of facts previously stated concerning the wide distribution of bacteria within the body, seems fatal to a consistent belief in the germ theory of disease. He says: "If the same bacterium may, as a result of varied circumstances, produce in one and the same medium fermentative changes differing so widely from each other, as the formation of lactic acid and that of black pigment in milk, it becomes readily conceivable that the same organism which, under ordinary circumstances may be com-

paratively harmless, may at other times generate products poisonous to the human economy." *

4. The consideration now to be mentioned suffices, in my opinion, to complete the discomfiture of the germ theory as an explanation of the mode of causation of the diseases with which we are at present concerned. It is this. It has been shown, on the one hand, that the virulence of certain contagious mixtures diminishes in direct proportion to the increase of bacteria therein; and on the other hand, it has been equally proved that fresh and actively contagious menstrua lose scarcely any of their contagious or poisonous properties after they have been subjected for a few minutes, when in the moist state, to a temperature which no living units can be shown to survive (212° F.), or after they have been exposed to the influence of boiling alcohol, which is well known to be equally destructive to all recognized forms of living matter. Such facts have been substantiated by Messrs. Lewis and Cunningham, Sanderson, and others.

Having said thus much in opposition to the germ theory, let me as briefly enumerate the facts and arguments which seem to me to show the real relations of bacteria and their allies to the diseases in question. I turn therefore to the construction of an opposite doctrine.

Admitting in part the very frequent presence of bacteria in diseased fluids and tissues, I consider that their presence and import should be differently explained. I say I admit the association in part, though I by no means admit it to the extent alleged. Bacteria are not, for instance, to be found in the blood of persons suffering from pyæmia, as might be inferred from former statements of Dr. Sanderson, which I have already quoted. My own experience in this matter seems to be entirely in accordance with that of Professors Bilroth and Stricker. Neither do I believe that the presence of bacteria in inflammatory fluids has the significance which Dr. Sanderson attaches to it, since it has been ascertained by myself and others that the exudation fluids of sick persons suffering from diseases of a totally different type are often similarly crowded with these lowest organisms, whilst the recent observations of M. Bergeron † seem to show that they may be found even in freshly extracted pus from ordinary abscesses occurring in elderly persons.

Now, it would seem quite obvious that the consistent advocate of a germ theory of disease can only successfully maintain such a doctrine if he can show, amongst other things, that bacteria are more capable of altering the character and chemical constitution of fluids of the body than they are themselves prone to be altered by

* 'Quarterly Journal of Microscopical Science,' October, 1873.

† 'Compt. Rend.,' February, 1875.

independently initiated changes taking place in such fluids. It seems, therefore, like unintentionally cutting himself free from the theory to which he has hitherto adhered, when we find Professor Lister, in speaking of the assumed "special virus of hospital gangrene," going on to say that "it is not essential to assume the existence of a special virus at all, but that organisms common to all the sores in the ward may, for aught we know, assume specific properties in the discharges long putrefying under the dressings." This passage has a similar import to that of a quotation previously made. In both a first place is assigned to the modifying influence of altered fluids; and however much the correctness of such a supposition would tell in favour of cleanliness, free exposure, or even of antiseptic dressings, it is none the less inimical to a consistent holding of the theory on which Professor Lister has chosen to base his system of treatment.

But though such statements are adverse to the holding of a germ theory in the only form in which it may be at all tenable, they are entirely in accordance with my own observations and views. I maintain, in short, that even the very existence of organisms in the fluids and tissues of diseased persons is for the most part referable to the fact that certain changes have previously taken place (by deviations from healthy nutrition) in the constitution and vitality of such fluids and tissues, and that bacteria and allied organisms have appeared therein as pathological products—either by heterogenesis, or by what I have termed archebiosis, or birth direct from a fluid.

The evidence on which my belief is founded is of this nature:

1. Bacteria and their allies are found in greatest abundance during the life of the individual in connection with dying tissue-elements, and apparently are as plentiful within the dying epithelium of the cutaneous ducts as in parts like the mouth, which are most liable to contamination with organisms from without. Again, they exist abundantly in and about the dying cells of bronchial mucus, although living bacteria appear to be almost completely absent from ordinary air.

2. The microscopical examination of such epithelial or mucous elements also favours the notion that the contained bacteria are products engendered within such cells rather than mere results of an external contamination and imbibition. This opinion is based upon the following considerations. Bacteria only appear within the cell when it is obviously dying; and in the case of epithelium, for instance, they manifest themselves at first as minute motionless particles scattered through the semi-solid substance of the cell, where each particle grows into a distinct bacterium, which still remains motionless, and does not appear to divide for a long time. This is precisely similar to what I have observed over and over

again, when amœbæ in vegetable infusions get into an unhealthy condition and become resolved into nests of bacteria. They may exist for days in a state of activity with bacteria in the fluid around them, though none are to be seen in their interior. After a time, however, the chemical constitution of the fluid seems to become no longer suited to the amœbæ; their activity ceases, they remain as almost motionless balls of jelly, and soon multitudes of the minutest particles appear throughout their substance, each of which straightway grows into a bacterium. The former amœba is converted into a mere bag of bacteria, which after a time ruptures, and thus liberates its swarming colony of newly-engendered living units. Multitudes of mucus-corpuscles seem to undergo the same kind of change, so that bacterial degeneration takes place in the same manner and is almost as typical amongst them as is fatty degeneration amongst pus-corpuscles. The two kinds of degeneration, moreover, commonly occur side by side in epithelial débris. Bacterial degeneration takes place where the vitality of the unit is lowered, but where it is not sufficiently degraded to permit of the still lower and more obviously destructive process of fatty degeneration; and if anyone wishes to see it in perfection let him examine some central portion of the kidney or other internal organ of a warm-blooded animal five days or more after its death.

3. Bacteria are admitted by nearly all pathologists to be absent from the blood of healthy persons during life, and yet in from eight hours to four or five days after death, according to the temperature of the air at the time, the previously germless blood of all individuals may be found to be swarming with these organisms in every stage of growth.

4. Whereas blister fluid or serum has been shown to be free from organisms in healthy persons, I have ascertained that, given a febrile patient with a temperature of 102° F., one can determine the presence of bacteria, at will, in any blister-bleb which remains intact for forty-eight hours or more, and this, too, where the patient does not suffer from any specific fever, but merely from pneumonic inflammation. I was led to ascertain this fact by finding, about eighteen months ago, myriads of bacteria in all the blebs of a patient suffering from acute pemphigus, with a temperature of 103°.

5. Lastly, as Dr. Sanderson has shown, a chemical irritant, such as liquor ammoniæ, may be introduced beneath the skin of some of the lower animals in such a way as to "preclude the possibility of external contamination," and yet here, amidst tissues which he has shown to be germless, we may thus within twenty-four hours determine the presence of swarms of germs and organisms in the pathological fluids effused under the influence of the local chemical irritant.

This constitutes, as it appears to me, an exceedingly strong

body of evidence tending to show that bacteria are pathological products capable of being engendered within the body after death, or in certain situations during life where tissue-elements are dying or where the fluids of the body are notably altered by disease. It is true that the facts and considerations mentioned under 1 and 2 are capable of receiving another interpretation. It may be said, for instance, and it has actually been said by Dr. Beale, that the higher forms of life are, as it were, interpenetrated by the lower forms of life. Speaking of bacteria and their allies, Dr. Beale says: "I have detected them in the interior of the cells of animals, and in the very centre of cells, with walls so thick and strong that it seems almost impossible that such bodies could have made their way through the surrounding medium."* And elsewhere the same observer says: "Probably there is not a tissue in which these germs are not; nor is the blood of man free from them." Noting by the way that this latter statement does not accord with the experience of others, I may further mention that some distinguished pathologists, and notably Dr. Sanderson, are also inclined to dwell strongly upon the fact of the wide distribution of bacteria throughout the body—not believing them to be innate or connate (in the mysterious manner imagined by Dr. Beale), but supposing that they have been introduced from without through certain definite channels.

Dr. Sanderson's views on this subject, and the means by which he supports them, are sufficiently remarkable to detain us a few moments. If what he says† concerning the assumed easy absorption of bacteria from the intestine by lymphatics, and their subsequent passage into the blood, were in correspondence with actual facts, then in face of the habitual prevalence of such organisms in the intestine, the blood of healthy individuals should scarcely ever be free from them. But this is surely proving too much, since Dr. Sanderson himself assures us that healthy blood is germless.

Again, the other main channel by which, as he says, bacteria may enter into the body abundantly from without is through the bronchi and the lungs. Now, as a result of Dr. Sanderson's oft-quoted experiments in 1871, he claims to have proved "in the most striking manner . . . . that *air is entirely free from living microzymes.*" Speaking of a previously boiled Pasteur's solution, he says that "no amount of exposure" to air "has any effect in determining the presence of microzymes therein." And yet Dr. Sanderson now talks of the air which is "entirely free from living microzymes" being the channel through which these organisms are introduced into the lungs. It is true that in his recently published lectures this distinguished investigator makes a tacit

* 'Disease Germs,' p. 72, 1870. † 'British Medical Journal,' Feb. 13, 1875.

retraction of his previous statement. He says, in fact, in his first lecture:* "It must not be understood that bacteria do not exist in the atmosphere. But their existence there in an active form strictly depends on moisture. They attach themselves *without doubt* to those minute particles which, scarcely visible in ordinary light, appear as motes in the sunbeam or in the beam of an electric lamp. It is by the agency of these particles that they are conveyed from place to place." Elsewhere in the same lecture† Dr. Sanderson repeats the statement that "solid materials in suspension in the air" play a principal part in the conveyance of bacteria from place to place, and claims that this was shown by the very experiments of 1871, which then entitled him to express the conclusion that "air is entirely free from living microzymes." All I can say is, that I have not been able to find in Dr. Sanderson's writings any explanation of this marked change of view, and that I certainly know of no experiments of his which at all establish the fact (extremely difficult as it would be to establish) that bacteria or their germs are conveyed from place to place on the surface of aerial particles, just as his assumed particles of contagion are supposed to be borne about by bacteria themselves. If the theory be true, the conditions for aerial locomotion of contagion are, at all events, getting a little complicated. The contagious particles cannot move about alone: they must engage the services of bacteria to carry them, and these latter porters are unfortunately so delicately constituted that they cannot exist alone in the atmosphere; they can only survive when borne on the backs of some moisture-containing fragments of atmospheric dust, which, though so much heavier than the contagious particles themselves, are freely borne through the air in all directions!

(*To be continued.*)

VIII.—*A Modification of Dr. Rutherford's Freezing Microtome.*

By William James Fleming, M.B.,

Assistant to the Professor of Physiology, Glasgow University.

The advantages derived from the examination of tissues in the fresh state are universally acknowledged by microscopists, but no means have hitherto been devised by which, in many instances, this can be effected except the hardening influence of cold. The difficulty of making frozen sections has prevented this process from being adopted with the frequency to which its merits seem to entitle it. A great advance in the mechanical appliances at our disposal for

* *Loc. cit.*, Jan. 16, 1875. † P. 70.

this purpose was the Stirling section-cutter surrounded by a freezing mixture contrived by Dr. Rutherford, and described in the 'Lancet' in 1873, vol. ii., p. 108. The objection to it is its cumbrousness and the difficulty which its size adds to the necessary manipulations. This same fault entails that the whole machine be firmly fixed—an arrangement which, in my opinion, very much reduces the value of any section-cutter, as it deprives the operator of the great assistance to be derived from the movement of the section upon the razor. It often occurs in practice that, if an imbedded object is held in the left hand, valuable assistance in making fine sections can be derived from slight movements of rotation, elevation, or depression, given to overcome some hitch which is noticed in the cutting of the razor edge. If the object to be cut is fixed, this power is of course lost. These considerations have led me to devise the instrument figured here.

The principle adopted is to freeze the substance in a Stirling's section-cutter by causing fluid, reduced to a low temperature, to flow round it. The mechanical means by which this is effected will be easily understood by reference to the accompanying section.

Fig. 1.

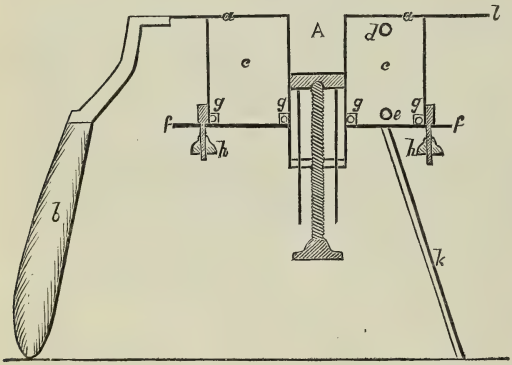
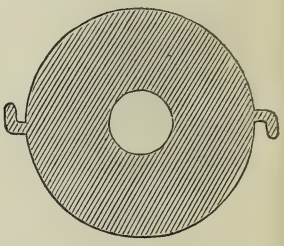


Fig. 2.



Scale one-third actual size.

Fig. 1.—A, Cylinder of ordinary Stirling's section-cutter. *a a*, Plate of ditto. *b*, Handle of brass and iron covered with vulcanite. *c*, Brass chamber surrounding A entered at top and bottom by two tubes, *d* and *e*. *f*, Movable bottom plate (Fig. 2, plan of this plate), which, when the nuts *h h* are screwed up, fits water-tight by the aid of two india-rubber rings sunk in grooves (*g g*, *g g*). *k*, One of the two legs forming with the handle a tripod stand. *l*, Beak to rest upon table or block when in use.

The manner of employing it is as follows: The substance to be cut is placed in the cylinder, about half an inch from the top, imbedded in scraped potato, muscle, brain-substance, or other suitable material. The tube *e* is connected by an india-rubber tube with a worm of block tin immersed in a freezing mixture, and placed a few inches higher than the section-cutter. The other end of the

worm is connected with a large funnel suspended above. An india-rubber tube is adjusted to *d*, and led into a suitable receptacle. Into the funnel is now poured a quantity of weak spirit, just strong enough not to freeze at 0° F. (one part methylated alcohol and two parts water is sufficient). This of course runs through the worm, and is thereby reduced to a low temperature (say 10° F.). Then it fills the chamber *c*, running in as we saw at the lower tube *e*, and out at the upper tube *d* into the vessel placed for its reception. As soon as most of the spirit has come through, the india-rubber tube conveying it away is compressed, and the contents of the vessel are returned to the funnel (this time at a very low temperature). After this has been repeated a few times (in about fifteen minutes) the sections may be cut with a razor with perfect facility.

The instrument is provided with two legs *k*, which with the handle form a tripod upon which it stands very conveniently. The upper plate *a*, is prolonged into a beak *l*, which is rested upon a table or block, while the instrument, being grasped by the handle, remains completely under the control of the operator. The vulcanite handle, from its non-conducting properties, prevents the heat of the hand influencing the result or the cold of the instrument affecting the hand. The spirit, of course, does over and over again, and is therefore no appreciable expense. Two steel guide-rods are introduced into the cylinder to prevent the piston rotating. In practice I find that if the screw is kept well oiled, there is no necessity for the introduction of spirit around the piston-rod, as recommended by Dr. Rutherford.

The advantages claimed for the instrument are, that it enables us to make fine sections of the softest tissues at once, and while they are in the fresh state. It is very handy, and when the subsidiary apparatus is once arranged, can be employed at any time with little trouble. It can be used as an ordinary section-cutter, and indeed for this purpose seems peculiarly adapted, as the arrangements before referred to enable it to be retained under the control of the left hand, an advantage not possessed by any other form of section machine with which I am acquainted. The microtome can be procured from Mr. Hilliard, Renfield Street, Glasgow.—*The Lancet*, June 19, 1875.

IX.—*On the Origin of Life.* By Lionel S. Beale, M.B., F.R.S.*

The far-fetched conjectures seriously advanced by some physical speculators concerning the origin of life serve to show what extreme difficulty has been experienced by those who have attempted to construct a plausible hypothesis by which the conversion of the non-

* Portion of a lecture delivered before the Royal College of Physicians.

living into the living might be reasonably accounted for. One great authority, dissatisfied with every suggestion, and being evidently convinced that no physical explanation of the origin of life upon our globe would ever be discovered, despairingly submits to us the proposition that life did not begin here at all, and that our earth was first peopled by the offspring of germs brought to us upon a fragment broken off from some distant orb that teemed with life. Whether even the simplest living forms would have survived after such a ride through space unfortunately had not been determined by experiment, so the idea of our fauna and flora being derived from those of another world found little favour, and probably all who have considered the subject would now agree that it is probable that life-forms originated upon our globe, though there might be great difference of opinion concerning the precise mode of their origin.

"Evolution" is now supposed to solve the difficulty of life formation; but this term has had at least two meanings assigned to it. By some it has been restricted to the living world, while others have given to the term "evolution" a much wider signification, and have maintained that it should include not only the evolution of living forms from pre-existing living forms, but the formation of the living out of the non-living. There is, it is scarcely necessary to point out, the widest possible difference between these two doctrines; for while the one teaches that all living forms came direct from living matter without accounting for the origin of life at all, the other is a tenet of the fiery-cloud philosophy which teaches as a cardinal point that the evolution of life is but one of the great series of changes in which the evolution of the cosmos is comprised. But surely such an idea may, for the present, be regarded as a conjecture so extravagant as to be unworthy of serious consideration. Facts are wanting, and the arguments advanced in favour of the hypothesis are such as cannot have much weight, since it has been deemed necessary to bring forward, in their support, utterances of a prophetic character.

If, then, evolution is restricted to the living world, the origin of the first living thing will be still unaccounted for. The presence of a very simple living form seems to have been assumed; but whether that being came of itself from the non-living, or arose in consequence of some prior changes, or was formed by an act of creative interference, is not suggested by the terms of the particular form of the hypothesis under consideration. Neither is the precise nature of the first living substance indicated, and we are even left in doubt whether one or two or many forms of living matter came into existence at the first formation of life.

Now with reference to the origin of the first living matter, several not improbable suggestions present themselves to the mind,

in all of which, however, it is assumed that the change from the non-living to the living was sudden and abrupt, and not gradual.

First, we may conceive that one form of living matter was produced direct from the non-living, and that from this all future living was evolved.

Secondly, we may prefer to imagine that more than one form of life originated from the non-living at or about the same time.

Thirdly, we might think it more in accordance with facts to conceive that several different kinds of bioplasm originated in the beginning of an epoch of life, from which all life of that epoch was derived. New forms originating anew in the next epoch, the results of evolution from the first gradually dying out as those of the second epoch increased and became dominant. As life-epoch succeeded epoch, new forms of bioplasm may have appeared as old forms of life died out.

But the above by no means exhaust the list of what I would term the reasonable hypotheses concerning the origin of life that may at once be suggested. All of them involve in some form or other the admission of a remarkable change in capacity or power not to be accounted for by physics. In all, the communication to matter of powers or forces which it did not always possess, and which it is conceivable might never have been communicated at all, is suggested.

Whether this communication of new powers occurred once only or was repeated at many successive periods in the remote past,—whether it be reasonable to consider a recurrence of the process in the future as probable or improbable, I shall not now venture to discuss.

What I particularly wish we should keep before our minds is that facts and arguments render it much more probable that the passage from the non-living to the living is *sudden and abrupt, than that there is a gradual transition or scarcely perceptible gradation from one state to the other.* I should, however, clearly state that this inference is in opposition to the views of many authorities, and in particular is opposed to the clearly expressed opinion of one of the greatest discoverers and most acute thinkers of our time, who maintains that the conversion of physical into vital modes of force is continually taking place. It is suggested that the change from non-living matter to living matter is a transition easily effected and continually occurring. Of the facts in support of so startling a proposition I confess I am ignorant, nor have I succeeded in my efforts to discover any facts in the writings of those who appear to have accepted the conclusion in question, which has never failed to enlist advocates in its support from the time when it was believed that highly complex living forms were produced from earth or dew, to the present day, when the advocates of the doctrine are so terribly restricted in the discovery of parentless living particles.

We have now reached the point where we are brought face to face with the modern developments of the old doctrine of spontaneous generation.

I cannot but remark that the more minutely investigation is carried out—the more thoroughly and intently facts bearing upon the matter are examined—the more improbable, in my judgment, does it appear that any living form should be derived direct from the non-living. Notwithstanding all that has been recently written upon this subject, I cannot but feel surprised that at this time many good reasoners should decide in favour of the *de novo* origin even of bacteria. Whether we consider the matter from the experimental side only, or study the evidence obtained in a general survey of nature, or carefully reflect upon the facts learnt from investigations concerning the properties of living and non-living matter, with the aid of the most perfect instruments of minute research now at command, or from other standpoints, the conclusion seems to me irresistible that the verdict of a jury of well-educated men would be against the direct origin of any form of living from any form of non-living.

Driven from one position to another, the advocates of spontaneous generation have entrenched themselves in the unassailable stronghold of experimental investigation. Here they may hold their own for any length of time, for no one can say what may not be demonstrated by new experiments in the time to come. Nay, although the conflicting results of different skilled experimenters, whose experiments have been conducted upon the same principles and professedly in the same way, even to the minutest details, may shake the confidence of some in the experimental method of inquiry, it is certain that the teachings of experiment will finally prevail over all other information.

But the modern advocate of abiogenesis should be skilled not only in explaining facts, but in explaining facts away. The fact that bacteria germs exist in all parts of the higher organisms, in the most internal parts as well as upon the surface of man's body, is to be accounted for by their spontaneous origin! Although millions are to be found about the mouth and upon the surface, and it can be shown that it is easy enough for them to get from the outside amongst the tissues within, we are asked to believe that those inside originated there direct from the non-living, or, as an alternative proposition, that they were derived not from parental bacteria, but by transmutation from some of the constituents of the tissues, on the principle that a living fungus comes not from a fungus germ, but from a dying tree. The next suggestion will be that man, after all, is but an aggregation of lower forms, peculiarly conditioned for a time, but which assume their ordinary forms when their environment shall be modified, as it must be at death.

Erroneous conclusions of many kinds have been employed as facts in support of abiogenesis. When one finds that it is believed that fungi may be developed from oil-globules and other living organisms of a much higher type, produced without parents out of organic matter, one fails to see any limit to the support that may be gained to the cause. Volumes of facts and arguments hitherto advanced in favour of abiogenesis may be republished without in the slightest degree modifying the real state of the case. What is now required is well-devised experiment, and that is all. No resuscitation of old arguments and doubtful facts, however ably the task is performed, will in the slightest degree increase the cogency of experimental proof, and in the absence of new experiment such facts and arguments will avail nothing.

I think we may be satisfied that before long the advocates of spontaneous generation will have to rely upon the production of the lowest organisms only. The only view in any way tenable at this time is, that such organisms as bacteria are the only ones that can, under any arrangement of conditions possible to an experimental inquirer, be formed anew, and that these alone, at any period of the world's history, sprang direct from the non-living. All are of extreme minuteness, many of the forms being so very small that they could not be identified with a magnifying power of less than eight hundred diameters. These are the smallest, simplest, and probably lowest forms of life known. That multitudes do now spring from pre-existing forms is absolutely certain, for the process can be seen. Whether some spring direct from the non-living is the question. Those that are supposed to be formed anew are very like those that have had a progenitor, and from those supposed to have been produced anew, forms exactly like those derived from undoubtedly pre-existing forms result. It cannot be pretended that new forms of existence are produced anew. No matter how the conditions are varied, the living forms supposed to result resemble known living forms, and give rise to forms of the same kind.

But, as I have before remarked, the question of the origin of bacteria can be only determined by experiment. All irrelevant considerations in favour of abiogenesis ought now to be left in abeyance. The assumed *de novo* origin is contrary to what goes on throughout the whole kingdom of nature, and the only exception which there is the remotest possibility of establishing is the spontaneous origin of some of these lower forms of life. While, therefore, it is allowable to permit ourselves to be influenced by general evidence against a new and exceptional doctrine, which a few observers seem very anxious to establish, we may fairly insist that only evidence of the most convincing and demonstrative kind should be accepted in its support. As regards the validity and reliability of the most recent experiments for and against the doctrine, I offer

no opinion. Time must be allowed for others to repeat the experiments; and, for my own part, I could express no opinion unless I had been present and had carefully watched each experiment in every stage. As far as I can judge, the reports of recent results are not more convincing than were those that were adduced years ago, many of which have been discarded and proved to have been unreliable from want of care, or from defects in the method of procedure.

If the formation of a bacterium germ, direct from non-living matter, be possible, three very remarkable series of changes, as it seems to me, will have to be brought about. Whether any means will ever be discovered of effecting these changes is surely most doubtful.

First, the atoms of the non-living substances must be separated from their combinations.

Secondly, the atoms will have to be rearranged to constitute groups of which the organic matter is made up.

Thirdly, the groups of atoms must be made to live.

What facts known, I would ask, render it likely that air, rarefied or condensed, or pressure of any degree or of any special kind, or any degree of heat, or light, or any conceivable modification of physical or chemical conditions, would at the same time account for the pulling asunder and joining together of atoms, and for the conference of new and peculiar powers of growth, of movement, of division, and the formation of new substances? In short, it is not easy to conceive, in the imagination, the several steps which result in the formation of a living bacterium even from *organic matter*. But the first germ must have sprung direct from matter that never had lived nor manifested phenomena in a way like those of life. Let us try to imagine a living germ being produced out of non-living matter. Atoms of many substances must be conceived as separating from one another, and then recombining. Attractions and affinities must, in the first place, be overcome, then the forces that effected the change must cease to operate; and these must, somehow, be exerted again. By what means the separation of atoms is effected cannot be suggested, neither can we conceive how the atoms are caused to recombine in a definite way. The supposed phenomena would be really more complicated than I have represented; for atoms are not related to one another—atom to atom, but group to group. How the atoms are grouped, and how the groups are related; how the groups act and react upon one another, and new groups are formed; what makes the atoms combine and begin a new course which may continue on and on for ever,—cannot be conceived. Upon the whole, the production from non-living matter of any living form, however simple, must be regarded as most improbable.

PROGRESS OF MICROSCOPICAL SCIENCE.

Mr. Archer's Opinion of the American Ouramœba.—In a paper read before the Philadelphia Academy, on April 20, Professor Leidy remarked that his description of the curious rhizopod he had named Ouramœba, in the Proceedings of May 12, 1874, having been noticed by Mr. Archer, of Dublin, this gentleman had directed his attention to notices of the same animal described in the 'Proceedings of the Dublin Microscopical Club' for Feb. 1866 and Oct. 1873. In these notices Mr. Archer regards the animal only as an *Amœba villosa* in another condition from that ordinarily observed. Mr. Archer's description clearly refers to the same animal as that named Ouramœba, in which he aptly compares the bunch of tail-like appendages to "a bundle of *dipt-candles*," and it is of some interest to know that the singular creature, like so many other rhizopods, is common to Europe and America.

While Mr. Archer regards the "Amœba with remarkable posterior linear processes" * as exhibiting another condition of existence of an Amœba from the one usually observed in the genus, he gives no evidence that such is the fact. Until this is proved to be the case the peculiar character of the animal justifies its separation as representing a distinct genus with the name of Ouramœba.

Since the latter was first noticed, many additional specimens have been observed; and though, as in the case of rhizopods generally, they exhibit considerable variation, it appears that several species may be distinguished.

The genus may be thus characterized:

Ouramœba.—Body, as in Amœba, consisting of an ever-changing fluctuating mass of jelly, composed of a granular entosarc, including a contractile vesicle and a discoid nucleus, and defined by a clearer ectosarc. Pseudopods usually digitiform, projecting anywhere, but usually in a direction differentiated as forward, and composed of extensions of the ectosarc closely accompanied by included extensions of the entosarc. Posterior part of the body furnished with one or more tufts of non-retractile, rigid, linear appendages, branching radically from common points in the vicinity of the contractile vesicle.

Ouramœba vorax.—Body active, usually ramifying forward from a median stock extending from the posterior blunt extremity. Posterior appendages numerous, originating in several tufts up to five or six, from one-third to nearly the length of the body, linear, straight or curved, uniformly cylindrical, or here and there contracted, commencing in a pointed manner from a common root, and terminating obtusely. Length of body, from $\frac{1}{5}$ to $\frac{1}{3}$ of a mm.; length of appendages from one-third to nearly that of the body.

The creature consumes multitudes of diatoms, desmids, and filamentous algæ. Found in springs and ponds, near Darby Creek, Delaware County, Pennsylvania.

Further observations have induced me to believe that the animal

* 'Proc. Dublin Micr. Club,' Oct. 1873, p. 314.

named *O. lapsa* is the same as the preceding. A variety has been observed in several instances in which the animal had a single pair of appendages springing from a common root.

Ouramœba botulicauda.—This species is predicated on the form alluded to in my previous communication as having a single tuft of three moniliform rays. I have seen it a number of times since, and its characters appear to be sufficiently constant to recognize it as a distinct species. It is much smaller than the preceding. The body measures about the $\frac{1}{16}$ of a millimeter. The appendages are usually in a tuft of three; each appendage consisting of from one to three sausage-like joints. Found with the preceding.

A curious Rhizopod: Biomyxa vagans.—Professor Leidy, who has recently been directing his attention to this branch of zoological research, remarked,* that in some water with aquatic plants, from Absecom Pond, N. J., preserved in an aquarium during the winter, he had detected a remarkable rhizopod, which he thought might best be compared to the reticular pseudopods of a Gromia separated from the body. The creature moved actively and assumed the most varied forms. At one time it appears as a cylinder or a ball of jelly which may spread itself into a disk of extreme thinness, from the edge of which emanate a multitude of delicate pseudopods minutely ramifying, and with the contiguous branches anastomosing, as in the extension of the net of a Gromia. At other times the creature divides up into branches from a trunk in the manner of a tree, but with the contiguous branches anastomosing. At times also the animal assumes the form of a cord, and the jelly accumulating along some portion of it will then move along the apparent cord like a drop of water running down a piece of twine. The branching pseudopods extending into a net, the large angular meshes gradually contract by the widening of the cords, so that the meshes become perfectly circular and appear like vacuoles imbedded in the jelly. A circulation of jelly with granules is observed along all the pseudopodal filaments exactly as in Gromia. No trace of a nucleus or investing membrane in any position could be detected, but the protoplasmic structure contained a multitude of minute vacuoles. Most of the specimens contained no food, and only one of the largest was observed to contain numerous minute Closteria.

The largest specimen, consisting of a net emanating from three divisions, occupied a semicircular space of $\frac{3}{5}$ of a mm. by $\frac{2}{5}$ mm. Another specimen with a central disk $\frac{1}{5}$ mm. by $\frac{1}{8}$ mm. with its net, occupied a circular space $\frac{2}{5}$ mm. in diameter. A small cord-like specimen was $\frac{1}{5}$ mm. long with an expanded end $\frac{1}{62}$ mm. wide; and another irregular cord-like specimen was $\frac{2}{5}$ of a mm. long with the widest portion $\frac{1}{50}$ mm.

Amœba porrecta, of Schultze, from the Adriatic Sea, most resembles the creature described. While it is nearly related with Gromia, Lieberkuehnia, Vampyrella, Nuclearia, &c., it appears sufficiently distinct in its characters to represent another genus, and with the species may be appropriately named *Biomyxa vagans.*

* 'Proceedings of the Philadelphia Academy,' April 20, 1875.

NOTES AND MEMORANDA.

An American View of the recently expressed opinion, as to Objectives, of the Academy of Natural Sciences of Philadelphia.—The following letter was addressed to the editor of the 'Cincinnati Medical News' (June, 1875) by Mr. J. E. Smith:—"In your last (May) number of the News I notice the report 'of the committee appointed to examine optically the $\frac{1}{25}$th and $\frac{1}{50}$th objectives displayed' at the late exhibition of the Biological and Microscopical Section of the Academy of Natural Sciences of Philadelphia.

"According to this report it seems that of the six objectives tested (?) a Tolles' wet $\frac{1}{50}$th of 140°, and a Wales' $\frac{1}{25}$th of 170°, were the only glasses that displayed the hexagons (?) of *P. angulatum* by central light.

"A few months since, a correspondent of the London Microscopical Journal proposed using *P. angulatum* with central light from an ordinary candle, as a *test* for objectives of medium power. I at once repeated his experiment, using a $\frac{1}{6}$th and $\frac{1}{10}$th four-system immersion glasses, made by R. B. Tolles, and was simply amused at the result, to wit: the hexagons were instantly displayed with either objective, using a common tallow candle for illumination—the angle of obliquity 0!

"A friend of mine, a well-known 'expert,' who had just purchased a Tolles' four-system $\frac{1}{10}$th, read this article in the London Journal, advised me that he too repeated the experiments, and with the same results as I obtained with my $\frac{1}{6}$th and $\frac{1}{10}$th.

"The 'committee appointed to examine optically, &c.,' having obtained the above-stated curious results, to wit: that the $\frac{1}{50}$th and the $\frac{1}{25}$th were the only glasses that would display the Angulatum hexagons by central light, proceed to 'deduce one *very important fact*' (italics mine), viz. that the different appearances of lines, dots, hexagons, &c., on *P. angulatum* are not only the varied results of angle of aperture, of amplification, and of illumination, but that they may be obtained with *less and less obliquity* of light *as we increase the power of the objective* (italics mine again), thus making it evident that high powers, with direct central light, show us clearly things which we rather guessed at than saw (owing to the increased chance of spherical and chromatic aberration and distortion from the employment of oblique light) with lower ones. (!)

"The committee therefore conclude by recommending these higher power lenses to those engaged in microscopic research, &c.

"This was too much for Dr. Hunt to stand. The Doctor desired it to be distinctly understood that *he* had nothing to do with the preparation of the report, and did not wish to be held responsible as a member of the committee for the views advanced in it. Dr. Hunt considered that it embodied the obsolete views of Carpenter and Beale in regard to penetration, which term, he says, should be dropped from the vocabulary of microscopists. 'He believed that penetration and resolution can be and *have been* combined in the best objectives.'

"Dr. Hunt, having thus washed his hands of this most curious report, makes a novel and startling proposition, '*the object being to test men* in regard to their technological skill.' This is a brilliant idea and to the point—rather *ominous* for the committee however!"

Angle of Aperture.—Mr. R. B. Tolles writes thus in the 'Cincinnati Medical News' (June, 1875):—"'Mr. Wenham is unquestionably right in stating that if an isosceles triangle be described, the base of which is ten times the measured diameter of the front lens, and the altitude ten times the measured distance of the focal point from the same surface, the vertical angle of that triangle will correctly represent the *maximum available* aperture.'*

"Taken as stated here by Mr. Brooke, the rule proves contradictory. Thus, Mr. Wenham gives the focus of the objective he measured† as ·013 of an inch *in air*. Diameter of front surface ·043 of an inch. From these data he deduces that 118° is the maximum angle *possible* in the case from *plain measurement*, &c. But Mr. Brooke says, the rule gives 'the *maximum available* aperture.' Apply this rule, then, to get the aperture *in 'balsam.'*

"Here are the data: Mr. Wenham, in the 'Monthly Microscopical Journal' for May, 1875, p. 225, gives the *focus in balsam* as 0·018 of an inch. Thus we have the elements for the triangle in balsam. Applying the rule we get a 'vertical angle' of 88°. But 82° in 'balsam' is equivalent to (infinitely near) 180° of pencil entering or emerging at a plane surface. Consequently their rule with Mr. Wenham's *own data*, viz. diameter 0·043, median height 0·018, proves the objective he declared‡ could not by any possibility have more than 118° of aperture, has, *by the same rule*, all the air-angle, i.e. angle for a dry mount, that any objective possibly can have."

How to Prepare the Diatomaceæ.—Herr J. D. Möller proposes to publish a work on this subject, and has offered the following as the plan on which it will be issued: "To the undersigned was repeatedly expressed the wish: to publish his procedure to prepare Diatomaceæ. He declares himself ready to do it for a corresponding indemnification, and has the intention to try the following. If a number of subscribers is obtained, he will publish a little work with illustrations, which will have the title 'The Preparation of the Diatomaceæ,' and which will contain: 1. The collecting; 2. The cleaning and purifying (a) of the living subjects, (b) of dead subjects in the mud, (c) of fossils. 3. The separation of the different species. 4. The preparation and mounting, (a) in the ordinary manner (in quantity), (b) as selected and arranged, (c) as 'Typen- and Probe-Platte,' &c. Price for the German edition, 30 marks; English, 1*l.* 12*s.*; French, 40 francs. Besides the undersigned, the following gentlemen will kindly receive orders: G. F. Otto Müller, Königgrätzer Str. 21, Berlin W.; Dr. E. Hartnack and A. Prazmowsky, Rue Bonaparte, 1, Paris; R. and J. Beck, 31, Cornhill, London, E.C.; Edmund Wheeler, 48, Tollington

* From the Annual Address of President Charles Brooke before the Royal Microscopical Society, London, February, 1875.

† 'M. M. J.,' March, 1874, p. 114.

‡ Ibid.

Road, London, N.; C. Baker, 244, High Holborn, London, W.C.; James W. Queen and Co., 924, Chestnut Street, Philadelphia. Orders must be sent in, at latest, by September, 1875; in October the subscribers will be informed whether or not the book will be published. If the enterprise succeeds, each subscriber, by remitting the price to the undersigned or to one of the above-named gentlemen, will receive the book at the beginning of 1876.

"WEDEL IN HOLSTEIN (Germany)." "J. D. MÖLLER.

A Micro-Ophthalmoscope.—We learn from a contemporary that "in order to facilitate the microscopical examination of the eye in cases of disease, M. Monoyer has contrived a modification of Siebel's ophthalmoscope, so arranged with prisms, that three persons can make simultaneous observations."

Dr. Fleming's Section-Cutter.—With reference to this instrument (see p. 79) the following letter has been published in the 'Lancet' (July 3), from Mr. Lawson Tait: "A very serious objection to Dr. Fleming's machine will at once occur to anyone who has done much work with the freezing method, in that the machine must require constant attention until the tissues are frozen and the sections are cut. In a University laboratory this is all very well; but for workers who are at the same time subject to the exigencies of practice the machine will be a constant source of disappointment. In the last number of 'Humphry and Turner's Journal' I describe a new section-cutter, which obviates this objection. When the tissue has once been frozen, it will keep so for at least twelve hours, and for a week if the machine be placed in a Norwegian chamber. Having the section-cutter screwed to the table has not been found by me to be a disadvantage; but, on the contrary, a very great advantage, as it leaves both hands free—a matter of the greatest importance in cutting fœtal sections, where the tissues are not continuous."

The Salmon Ova that were sent to New Zealand.—These ova were unsuccessful. That is to say, that the last shipment arrived in New Zealand dead. But what is worse is—if the officers of the Otago Association are to be believed—that microscopic examination showed that many of the eggs had not undergone any fertilization. It is strange that they should have arrived in a dead state, seeing that a large quantity of the ice alongside them remained unmelted on their arrival.

Microscopy at the American Association.—The American Association meets on the 11th of August, and it is endeavoured to get up a large amount of interest in the microscope. The 'American Naturalist' (July) gives the following notice: A full representation of those interested in the microscope is especially desirable at the Detroit meeting of the A.A.A.S., commencing on the 11th of next August, as it is desired to take steps toward the organization of a Microscopical Society, either as a separate society or club, or as a subsection of the large Association. There is a very general desire for a society of American microscopists, and it is believed that such a society can

obtain general attendance from the whole country only at the time and place of meeting of the A. A. A. S., to say nothing of the other very great advantages of meeting in connection with that prominent organization.

The Nutrition of the Protozoan.—Dr. Wallich, Hon. F.R.M.S., writes as follows to the 'Lancet' (June 12) on this subject. He says: "Having for fifteen years stood alone in maintaining that the law of nutrition which prevails in the case of the higher orders of the animal kingdom, and constitutes the fundamental distinction between it and the vegetable kingdom, fails in the case of the simplest and humblest creatures, whose body substance presents no trace whatever of special digestive apparatus, it is with no slight satisfaction that I am now able to state that my views on this subject have very recently been confirmed by evidence which seems to be incontrovertible. The fact is in itself so important and so intimately connected with the biology of deep-sea organisms, that it would be useless to attempt to indicate, within the compass of a few lines, the nature of the evidence and reasoning which influenced my conclusions. I must therefore content myself for the present with observing that in my 'North Atlantic Sea-bed,' published in 1862, pp. 130–132, as well as in various papers on the Rhizopods and Protozoa generally, contributed since that period to other scientific periodicals, I stated the reasons which appeared to me to be sufficient to establish the belief that the lower rhizopods provide for their nutrition and growth by eliminating from the medium in which they live the *inorganic* elements that enter into the composition of their protoplasm. What I contend for is, *not* that there exists in nature a hard-and-fast line between the extremes of its two great kingdoms, but a gradual transition and overlapping from both sides; and hence that the doctrine I have advanced is not the scientific heresy which its opponents, under the influence of foregone and, as I think, erroneous conclusions, have thought fit to consider it."

Microscopy at the Bristol Meeting of the British Association.—We are glad to see that Bristol is not going to be behindhand at the meeting of the British Association, but is determined to illustrate her local microscopy to the fullest. She has some men among her *savants* who are by their knowledge and work qualified in the highest degree to take a leading part in the discussions, and we trust they will not be absent. At the soirée on August 26, the Bristol Microscopical Society, assisted by the Naturalists' Society and the Bath Microscopical Society, has undertaken to give a systematic microscopic demonstration of the natural history of the neighbourhood; a novel feature will be the number of living objects which will be exhibited.

CORRESPONDENCE.

A Monstrous Form of Aulacodiscus.

To the Editor of the 'Monthly Microscopical Journal.'

Baltimore, *June* 16, 1875.

Dear Sir,—Allow me to offer you, for publication if you please, a photo of a drawing of mine representing a disk of *Aulacodiscus Oregonus* with two centres, conveying the idea of two half disks, each excentric, joined along a line of suture.

I am aware that this class of objects is not extremely rare, but such a valve as the one in my possession must give rise to questions of interest with regard to the manner of their production.

I have the honour to be, dear Sir,

Yours very truly,

Christopher Johnson.

[The photograph sent is not sufficiently distinct to make an engraving from. But it represents the two centres very well, and is certainly a curious departure from the ordinary form. We learn, however, from Mr. F. Kitton that monstrosities are not very rare in this department of botany.—Ed. 'M. M. J.']

Mr. Slack's Paper on Angle of Aperture.

To the Editor of the 'Monthly Microscopical Journal.'

224, Regent Street, London, *June* 23, 1875.

Sir,—So long as the question of angular aperture was being discussed by rival opticians—the one anxious to have it believed that everything worth knowing about the construction of objectives was known to, or discovered by, himself ever so many years ago—while the other, not content with having the splendid testimony of Dr. Woodward and Professor Renel Keith in his favour, must needs venture to speak in his own behalf with almost disastrous effect on his own lucidity,—I say, so long as the question of angular aperture was being discussed by rival opticians, we spectators might well stand by and be amused by the prodigious display of personalities. But when an Honorary Secretary of the Royal Microscopical Society comes forward with a paper "On Angle of Aperture in Relation to Surface Markings and Accurate Vision," we expect (at least some of us do) that interesting information will be given, and that some of the latest developments in microscopy will be discussed with all the fulness of knowledge to which his official position gives him access.

The interesting information in the Hon. Secretary's paper seems to be that he "has a microscope in his library, opposite a north window;" that the instrument "is pointed like a telescope towards the clear sky," and, "with Powell and Lealand's immersion $\frac{1}{8}$th, their last but one, a perfect [?] definition is obtained of *P. hippocampus*." (In

the next sentence he says it was "drowned in light"!) With sundry other objectives, such as Zeiss's C and D (= $\frac{1}{4}$th and $\frac{1}{6}$th), he also obtains admirable definition of this diatom. Further, that, taking a delicate valve of *P. angulatum* that was not satisfactorily exhibited by Zeiss's C and D objectives with illumination from sub-stage mirror or condenser, he has instantly increased the resolving power by the employment of the Reflex Illuminator. Later on he tells us he has had an opportunity of trying experiments with Powell and Lealand's new $\frac{1}{8}$th, and he says the view he obtained of *P. angulatum*, with an ocular giving a magnification of about 2000 linear, "surpasses in beauty and brilliancy anything seen before." For all this information we are thankful; if it be rather vague and fragmentary, it comes from good authority, and doubtless some of your readers will receive it with that polite deference which such authority seems to require.

From the limited nature of the severity of the tests he mentions, viz. *P. hippocampus* (which Möller has thought to be too vulgar a test to be admitted in his series on the Probe-Platte), and *P. angulatum* (which Hartnack exhibits with plane mirror axial illumination with every $\frac{1}{4}$th objective that leaves his hands), I am inclined to think that Mr. Slack has reached that stage in microscopy when the ardour of research palls, and we rest satisfied with mere amplification of an easily obtained image, rather than search for an image that taxes all our skill in manipulation and all the defining power of the finest lenses at our command; otherwise I should have thought he would have selected some such admittedly difficult test as the *Gnat's body-scale*, in which there is structure that challenges the power of the finest lenses to exhibit. It is clear, however, that Mr. Slack cared little for settling knotty questions; it was far easier for him to take the humbler position of a reviewer of old—old tests, the definition of which is pretty generally admitted: and this he has done. It is strange that he learned only within the last year "the amount of caution required" to regulate the quantity of light to be used with small and with large-angled objectives. Even now he seems not to have mastered that subject, for he speaks of a certain "$\frac{1}{5}$th with less aperture" showing *P. hippocampus* "with same eye-piece rather better" than Powell and Lealand's $\frac{1}{8}$th "because not so much drowned in light." Surely, a very important part of the process of manipulation in microscopy consists in duly proportioning the light to the capabilities of the lens? If an image appears to be *drowned in light*, it should be the duty of the microscopist to rescue it from that unfortunate predicament. He favoured Zeiss's C lens on the same object by carefully screening off a little superfluous light "by holding a sheet of white paper, so as to stop some rays from entering the field;" and he makes suggestions about placing the microscope "some way from the window, where there is a little shade," and about surrounding the eye-piece "with a screen of black cotton velvet, to keep all glare from the eye," with an air of *naïve* earnestness that is quite interesting.

I, too, have had an opportunity of examining some of Zeiss's objectives—about twenty of them—and, while making the general admission of their excellence and evenness of quality, I must at the same

time say that, taking his series from $\frac{1}{4}$th up to $\frac{1}{25}$th and testing them against similar objectives by Powell and Lealand and by Hartnack and Prazmowski, on the whole of those diatoms known as *test-objects*, —on *Podura*, on *Gnat's body-scale*, on Dr. Carpenter's "thread-cells," on various specimens of tissue, on the double-star test, on *Nobert's Test-plate*, by the test of deep oculars, and with all the methods of illumination that are in vogue,—the English and French objectives carried the palm. I do not here make allowance for any advantages there may be in the ease of manipulation in consequence of Zeiss's objectives being of smaller angular aperture and having greater working distance; my judgment is upon the results obtained, which I understand to be of the first importance. The mere item of one objective being a little less difficult to manipulate than another does not appear to me to weigh against superior clearness and definition in the image obtained.

And now, what, as to the latest developments in microscopy discussed by the Hon. Secretary? When Professor Abbe is eulogized for having "minimized the angles of aperture," it appears to me he is receiving praise for a backward march. In high powers—all those beyond $\frac{1}{4}$th—what we seek is magnification with clearness and definition. This is obtained if a lens be made that will support deep oculars. It is well known that the power of a lens for supporting deep oculars is in proportion to the perfection with which the corrections are made throughout a given angular aperture; if these corrections extend up to the maximum of possible aperture, such a lens bears the deepest oculars before the optical image is broken up.

Professor Abbe is largely quoted, as though he were an impartial authority criticising Zeiss's work; whereas, he is simply saying what he can as an advocate for the work which Zeiss is producing under his direction. When he says that "corrections cannot be well made with dry lenses exceeding 105° to 111° aperture, without a considerable reduction of working distance," he is simply stating a fact; but this fact does not presume that Zeiss's F lens ($= \frac{1}{14}$th) with an aperture of 105° can for a moment hold its own on the highest tests against Dallmeyer's or Powell and Lealand's $\frac{1}{8}$ths with apertures beyond 140°: the working distance is different, and so is the quality of the optical image. If Professor Abbe is satisfied with his result: so be it. But when I and my most critical friends have made the comparison, we first of all note that a deep ocular quickly finds the limit of light in Zeiss's lens, that it breaks up the crispness of image, that, in fact, the lens has the qualities of similar low-angled lenses such as were produced in England many years ago.

Again, according to Mr. Slack, the most recent investigations and experiments—those made by Professor Abbe—have led to the conclusion "that even in immersion systems, for the normal requirements of science, there would be no loss, but in many respects a gain, if they were constructed with smaller angles of aperture." I am unable to determine what Professor Abbe means by the *normal requirements of science*, but certainly one of the requirements is to render visible what was before invisible in the object: how this is attained by diminishing

the angles of aperture is not explained and certainly not proved to my mind by anything said or quoted by Mr. Slack.

In vol. xv. of 'Les Mondes' (p. 482 *et seq.*) Dr. Hartnack discusses the conditions under which the highest definition is obtainable, and he lays great stress on the importance of the utmost limit of angular aperture in high-power immersion objectives. Trying Zeiss's Nos. 1, 2 and 3 immersions (= $\frac{1}{8}$th, $\frac{1}{15}$th, and $\frac{1}{25}$th) by the test of deep oculars, I find Dr. Hartnack's observations borne out; the image with these comparatively low-angled objectives breaks up with any magnification beyond about 1000 diameters. On the other hand I have found similar powers by Hartnack and by Powell and Lealand, with their greater apertures, give far greater amplification, still retaining fine definition.

Taking Mr. Slack's summary of his results:—He says that "opticians have been encouraged to make excessive apertures substitutes for good corrections;" if this means anything, I take it to mean that opticians have been encouraged to make lenses that give bad images? In other phrase, the demand has been for lenses of inferior rather than superior quality? By way of absolute negation of this dictum I point to his own criticism of Powell and Lealand's new $\frac{1}{8}$th, in which aperture has been carried to the highest limit consistent with the accuracy of corrections for which these opticians are renowned. Secondly, he says that "naturalists and physiologists have been too contented with feeble resolving powers." If he had said that many English opticians have been too contented with mediocre lenses, that they slumbered over the introduction of improvements in high powers until both on the Continent and in America lenses were produced that have fairly given microscopy a new start, then he would have stated an important, though unpalatable, truth: as it is, the naturalists and physiologists have been rapidly providing themselves with high powers from Hartnack and others, so that the old objectives that were formerly in use in the medical schools are now looked upon as the stepping-stones to higher and more difficult investigations, such as require the aid of more powerful objectives. Thirdly, he speaks specially of the utility of the Reflex Illuminator. The Reflex Illuminator is a very ingenious contrivance by which new and curious effects of oblique illumination can be produced, both by reflexion and refraction; Dr. Woodward's photograph of *Podura* is the finest example I have seen of one of its uses. Mr. Wenham has hitherto seemed not to have observed that his Reflex Illuminator furnishes a most elegant practical refutation of his own position with reference to the "aperture" question. He has challenged "anyone, to get, through the object-glass with the immersion front . . . . any portion of the . . . . rays" that would "be totally reflected" with a pneumo-front (vide 'M. M. J.,' No. xxvii., p. 118). If he will try the experiment on Möller's Probe-Platte with his Reflex Illuminator and a high-angled immersion lens, he will see a *luminous* field; whereas, with a pneumo-lens he obtains a *dark* field. Whence comes the *luminous* field in the immersion lens if not from its having the power to collect rays which are *totally reflected* when the pneumo-lens is used?

Lastly, I think Mr. Slack might well spare himself the trouble of pleading *ad misericordiam* for those opticians whose specialty will be to "bring comparatively small-angled glasses to the highest degree of perfection in resolving as well as penetrating power." He may be well assured these opticians do not attach an exaggerated value to the "honour" of which he speaks as being within the power of the Royal Microscopical Society to award to them; what they ask for is a ready sale for their wares; what they deprecate is, that one who occupies the position of Hon. Secretary of the Royal Microscopical Society should give such an extraordinary meed of praise to the productions of a foreign optician whose work cannot be said fairly to rival the highest class of work produced by the best English opticians.

I am, Sir, your obedient servant,

JOHN MAYALL, jun.

CHROMATIC AND SPHERICAL ABERRATION.

To the Editor of the 'Monthly Microscopical Journal.'

BEDFORD SQUARE, *July*, 1875.

SIR,—The great interest and importance to microscopists of the question discussed by Mr. Slack, "On Angle of Aperture," &c., in the June number of the Microscopical Journal, is my excuse for asking you to permit me to occupy a small portion of your valuable space on the subject. On reference to the paper it will be found that it contains statements which not only involve the use of the microscope, but also certain intricate problems on the construction of the achromatic instrument, which will appear to other of your readers, as they do to myself, quite at variance with standard authorities on optical or rather physical science. For instance, at p. 233, the following irreconcilable statements are made, which for the purpose of discussing I place in juxtaposition:

"No one could deny that up to a certain date the best dot-displaying glasses had considerable chromatic errors, and that other glasses with better chromatic corrections did not show difficult dots so well."

"Had it been considered that all chromatic aberration involves spherical aberration, the belief in any theoretical necessity for leaving considerable chromatic error in order to ensure sharp definition would scarcely have become so prevalent."

The first it will be seen involves a matter of fact, one which needs no such qualification as "that up to a certain date" it obtained, since it is perfectly true at this moment. In the next paragraph, however, Mr. Slack says, "all chromatic aberration involves spherical aberration." Now this is simply a matter of theory, and for which I can find no evidence or authority in any work on optics with which I am acquainted. In Parkinson's 'Optics' (p. 167) he will find it stated that "the conditions of achromatism depend only on the focal lengths of the compound lenses, not at all on their *forms*, or the *order* in which they are placed. By a suitable arrangement of these latter qualities (i. e. *forms* and *order*), the conditions requisite for the destruction of spherical aberration can be secured, and a compound object-glass

constructed which shall be at the same time both *aplanatic* and *achromatic*." An object-glass may therefore be achromatic and not aplanatic; so that *all* chromatic aberration does not involve spherical aberration, and it necessarily follows that Mr. Slack's theory is not coherent, or consistent with actual observation and mathematical formula. This he will the more readily admit from the fact that in the objective which commands his admiration—the Powell and Lealand's new $\frac{1}{8}$th—the point of best adjustment for achromatism is not coincident with that of the best adjustment for aplanatism. I may say, I have been unsuccessful in my endeavours to discover an optician who believes "in any theoretical necessity for leaving considerable chromatic error in order to ensure sharp definition." I have, however, heard opticians confess to the enormous practical difficulties in the way of constructing a high-power objective combining aplanatism and achromatism.

Mr. Slack is in error in supposing that "large-angled glasses" were ever considered "nearly or quite useless for general purposes of natural history and physiological research:" physiologists were among the first workers with the microscope to recognize the value and importance of high-angled powers,* and in their investigations constantly employ them to obtain a magnification of from one to ten thousand diameters.

I remain, Sir, your most obedient servant,

JABEZ HOGG.

REPLY TO MR. MAYALL AND TO MR. HOGG.

To the Editor of the 'Monthly Microscopical Journal.'

ASHDOWN COTTAGE, *July* 14, 1875.

SIR,—I am obliged by a sight of Mr. Mayall's letter, as it enables me to correct the misapprehension it might occasion without delay. The first part is a wordy misrepresentation of my paper on angular aperture. The question raised by me related to the smallest apertures capable of showing lined objects. This would not be supposed from the totally irrelevant comments of Mr. M. I showed that Zeiss' C, angle 48°, resolved *P. hippocampus*, and that his D, angle 68°, sufficed to exhibit *Surirella gemma* in dots.

I do not think Professor Abbe's remarks will suffer from Mr. M.'s erroneous statement that "he is simply saying what he can as an advocate for the work Zeiss is producing under his direction." I feel no doubt of their value, and believe our opticians will find them well worth consideration. I can only attribute Zeiss' higher powers "breaking down at about 1000 ×" to bad arrangement. His $\frac{1}{8}$th, $\frac{1}{15}$th, and $\frac{1}{25}$th worked well in my hands, and in others', with Ross's D eye-piece, and even E.

Mr. M. is right in supposing me to mean that the demand of English observers for extreme angles has encouraged opticians to

* [We certainly disagree with Mr. Hogg as to this point.—ED. 'M. M. J.']

make lenses that do not give the best attainable images, but the term "bad images" would be too strong. The lesson I have learnt from Zeiss' glasses is, that small and moderate-angled objectives can be brought to a point of perfection far beyond what is generally supposed, or what has been accomplished by any other optician whose work I have seen. I have no doubt English opticians will excel with small angles and great working distance when their customers ask them to do it.

I remain, Sir, yours faithfully,

HENRY J. SLACK.

P.S.—Since writing the above I am further obliged by a sight of Mr. Hogg's letter. I regret that I am unable to understand some parts of it, but it seems as if he had not borne in mind the effect of the two kinds of aberration. In Ganot's 'Physics,' Atkinson's translation, he will find that "when the aperture is larger (than 10° or 12°), the rays which traverse the lens near the edge are refracted to a point nearer the lens than the rays which pass near the axis. The phenomenon thus produced is named spherical aberration by refraction." In chromatic aberration the violet rays are refracted to a point nearer the lens than the red ones, and the effect is a distortion of the image. The maximum of distortion will be where most rays are out of place, from failure of either or both corrections.

The accuracy of my remark about considerable chromatic error is proved by the fact that Messrs. Powell and Lealand's last objectives, and those next preceding them, are much more perfect in chromatic correction than their earlier high powers, and likewise much sharper in definition.

Mr. Hogg's last sentence will astonish physiologists and many others. With reference to the angles of the highest powers, Beck's $\frac{1}{20}$th was made with an angle (as per catalogue) of 140°, Powell and Lealand's $\frac{1}{25}$th with 160°, 15° less than their old $\frac{1}{16}$th, and their $\frac{1}{50}$th has an angle of 150°, moderate in proportion to its focal length. Mr. Wenham has informed us that the usual mode of estimating angles of aperture considerably exaggerates them, and I find my $\frac{1}{20}$th of Beck to be about 128° when measured with Mr. Stephenson's scale, and the angle taken when the edges of two real objects are distinctly seen.

HIGH-ANGLED *versus* LOW-ANGLED OBJECTIVES.*

To the Editor of the 'Monthly Microscopical Journal.'

MEMPHIS, TENN., *June* 25, 1875.

DEAR SIR,—Referring to your remarks† on a brief paragraph of mine in a late number of 'American Naturalist,' I beg to offer the following: not to enter into any controversy regarding it, but merely as a fuller statement of the facts set forth in the previous note.

* This letter was by a curious mistake addressed to Dr. Henry *Slack*.

† 'M. M. J.,' p. 258.

I am sure I am correct in stating that the generally received opinion regarding glasses of very high angle of aperture is, that they are "optical curiosities of no practical use" (Dr. Pigott); "their use limited to particular classes of objects" (Dr. Carpenter); in short, that a glass of nearly 180° is *per se* good for nothing but to show difficult test-objects under extreme obliquity of illumination, and comparatively valueless for the study, under central light, of objects coming properly within the sphere of the histologist and "working microscopist." My friend, G. E. Smith, referred our Society to Mr. Tolles' new "four-system" objectives as a practical refutation of this doctrine, and challenged us to the proof.

In my previous note I gave no names of makers of the various objectives, lest I might appear desirous of "puffing" the work of some favourite optician at the expense of others. Since this is considered an error, I will now say that the glasses tried were by Tolles, Wales, Zentmeyer, Smith and Beck, Powell and Lealand, Nachet, Scheik, and Siebert. All were bought for first-class specimens of the various makers' skill. As our object was not to make a qualitative analysis of the general merits of the various glasses, we confined ourselves to such objects as properly come under the notice of the histologist, and used only central illumination. After as full and fair examination as we were able to make, we unanimously agreed that the new "four-system" form of construction, as invented by Mr. Tolles, is an actual proof that objectives of the highest attainable angle of aperture CAN be so constructed as to meet the narrow-angled glasses on their own ground, and not only equal but actually excel them. When I state that certain glasses "broke down" under deep eye-piece, I meant that the difference in favour of the wide-angled glass under these conditions was so striking as to leave no room for comparison. In short, this glass—Tolles' new "four-system" $\frac{1}{60}$th of 180°—so far from being a mere "costly toy" of "no practical use," will, if properly handled, give better results in the hands of the "working microscopist," on any object suited to its magnifying power, than any of the best glasses of moderate angle of aperture which I have yet examined. In naming the angle 180°, I wish to be understood as simply giving it as named by the maker.

I feel sure that the best and most thoroughly scientific "working microscopists" in this country no longer discard an objective simply because of high angle, as being therefore unfitted for the best work. On the contrary, the narrow-angle is used for "blocking out" the work, as it were; the wide-angle for finishing it off. Whether their work is "valueless" or not, let the pages of this valuable Journal testify.

In conclusion, I wish to state that I in no sense submit this note as an official expression of the views of our committee on this subject; it is to be taken as a hasty expression of my own opinions alone.

Yours faithfully,

ALBERT F. DOD.

Mr. Taylor on Black Knot.

To the Editor of the 'Monthly Microscopical Journal.'

Washington, U.S.A., *July* 1, 1875.

Dear Sir,—Your Journal for May 1 has just come to hand. I have read with pleasure the remarks of Charles B. Plowright, on pp. 209 and 210, relating to my articles on Black Knot and *Erysiphe Tuckeri*. He says of the latter: "Of it in the ascigerous condition we have seen no specimens, and therefore offer no remarks, but would only suggest that Fückel, in his 'Symbolæ Mycologicæ' places it in the genus *Sphærotheca*, as a variety of *S. Castagnei*." As I am not a mycologist, but simply an observer, I would be much pleased to have the views of the Rev. M. J. Berkeley and Messrs. M. C. Cook and C. B. Plowright, and others who have given the subject particular study, and to this end will forward by this day's mail* a foreign grape-leaf, on which about one hundred specimens will be found in good condition. I have had it in my possession for several years, and plucked it from the grape vine of our foreign collection myself. I would like the specimen now sent to be placed first in the hands of Mr. Berkeley, as I understand he takes special interest in this subject, and he can give such specimens as he desires to others. When I have time I will review my experiments on Black Knot, and will forward specimens of the asci containing the sporidia on microscopical slide for examination in London.

Faithfully yours,

Thomas Taylor.

A Proposed Prize for the best Objective.

To the Editor of the 'Monthly Microscopical Journal.'

Brighton, *July* 10, 1875.

Sir,—There seems to be now no longer any doubt that an object-glass to suit all purposes cannot be made. Large angular aperture is a necessity for diatom work and the like; but on the other hand moderate aperture is considered best for physiological and scientific purposes. The names of Carpenter, Wenham, and Slack suffice to settle that point.

The inducements to make glasses of the first kind are obvious, and lie on the surface. They are such as are required by the largest portion of the makers' clientelle, the amateurs; they can be easily submitted to unanswerable tests, the resolution of difficult diatoms; and they can be judged of by everybody. Such glasses therefore acquire a finer reputation.

Not so, however, with physiological glasses; the facts in this case are all the other way. Men really doing scientific work are comparatively few, and there is no standard by which the work required of

* Specimen has not arrived.—Ed. 'M. M. J.'

their object-glasses can be either popularly, easily, or accurately estimated.

Under these circumstances opticians can hardly be expected to devote themselves with ardour to objectives of this sort. The finest glass ever made would have but a limited sale, and probably would be condemned by the public because it could not perform on a diatom so well as other known glasses of even moderate excellence.

Still it is most desirable that makers should be encouraged to throw their utmost skill into the manufacture of glasses best fitted for scientific work.

With this view I would suggest that the Royal Microscopical Society should appoint a committee to settle upon a standard for physiological glasses. That the Society should offer a gold medal every third year or oftener for such glasses, the same committee to judge of those sent in for competition. The prize should be open to all the world.

I have no doubt at all but that our great makers would add new laurels to those they have so long and with such good right worn. But whether that were so or not in the first instance, we may be sure they would accept no defeat; that they would be stirred up to new efforts, and that science would thereby be the great gainer.

The glass bearing off the Society's medal would be stamped in the public mind as the best of its kind; and while it would be a real and immediate boon to science, it would by its reputation teach the intelligent amateur to look to a more instructive field than that presented by mere diatoms, for the interest and pleasure he derives from his microscope.

Dear Sir, yours truly,

R. Branwell, F.R.M.S.

The Bucephalus parasitic on the Fresh-water Mussel.

To the Editor of the 'Monthly Microscopical Journal.'

Stoke-upon-Trent, *July* 10, 1875.

Sir,—In the account of the 'Proceedings of the Royal Microscopical Society' appended to your July number, a doubt is expressed whether an instance has ever occurred of the genus Bucephalus being found parasitical on the fresh-water mussel. Forty years back the writer of this communication found them in the ovary of that animal (Anodonta), and figured them in the 'Trans. Zool. Soc.,' vol. ii. pl. xviii. In the same year he found in the ovary of another Anodonta, in vast numbers, the mother-cells of a Distomus, each cell containing several individuals. This is figured in the same plate as the Bucephalus. These Distomi, when excluded, are probably the same which are found in the mantle or outer tunic of mussels generally, and constitute the ordinary nuclei of pearls.

Your obedient servant,

Robert Garner.

PROCEEDINGS OF SOCIETIES.

Quekett Microscopical Club.

Ordinary Meeting, May 28.—Dr. Matthews, F.R.M.S., President, in the chair.

Mr. M. Hawkins Johnson read a paper "On the Organic Structure of Flint and of Meerschaum." He referred to numerous examinations of the nodules found in sedimentary deposits, the structure of which, he said, might easily be made visible by staining thin splinters with acetate of rosaniline. The method adopted was to take a slice $\frac{1}{8}$ inch thick, to boil it in water to expel the air, then to boil it in a solution of acetate of rosaniline, dry it, saturate it with balsam, harden the balsam, and grind the sides, washing finally with oil of turpentine, but not covering the specimens with glass. Nitrate of silver and acetate of iron were stated to be sometimes even more successful than acetate of rosaniline, the object being to render one portion of the substance more opaque or more strongly coloured than the other. No less than fifteen substances were considered to possess a structure of a decidedly organic character, viz.: Meerschaum; Kunkur of the Doab in India; Phosphatic nodules of the Crag of Suffolk; Menilite from Ménil Montant, near Paris; Septaria of London clay; Race of the Woolwich beds; Chalk flints; Iron pyrites of Chalk; Green-coated nodules of Chalk rock; Phosphatic nodules of the Cambridge deposit; Phosphatic nodules from the Gault; the Oolitic bodies; Ironstone in Coal-measure sandstones; Chert of the Mountain Limestone; Phosphatic nodules of the Lower Silurian strata of North Wales. In addition to this he had found that by the aid of acetate of rosaniline the pale green substance in the green marble of Connemara could be shown to be a very beautiful fossil sponge, as also the soluble silica rock from near Farnham in Surrey.

The paper was followed by an animated discussion, in the course of which Mr. Lowne strongly contested Mr. Johnson's views. Drawings and specimens of the stained substances were exhibited.

Adelaide Microscope Club, South Australia.*

The monthly meeting of the club was held on April 2, 1875. Mr. Smeaton presided. A new instrument by Crouch was exhibited; the glass stage was much admired on account of its smooth gliding movements. Dr. Whittell exhibited a new form of diatom he had found in a collection from Edithburgh, S.A. He had been unable to find anything like it in the drawings available in the colony, or on Möller's typen platte, and promised to send it to England for identification.† The Chairman then gave a short address on the subject of

* Report supplied by Dr. Whittell, Adelaide.

† [This specimen, which was forwarded to us by Dr. Whittell, has been sent to Mr. Kitton, who informs us that it somewhat resembles the specimen described by the Rev. E. O'Meara ('Quart. Jour. Mic. Science,' New Series, vol. xi.), and which he calls *Amphiprora ramosa*. We believe, however, that Mr. Kitton thinks differently from Mr. O'Meara, and we shall probably have an expression of his decided views in an early number of this Journal.—Ed. 'M. M. J.']

study for the evening, viz. Hydrozoa and Polyzoa. After giving a lucid account of the general characters of each of these classes, illustrated by some well-executed drawings, he proceeded to show a collection of about fifty varieties he had collected at different points of the South Australian coast. Many of these he had been able to name, but several differed so much from any illustrations in the books that he was inclined to think they were peculiar to Australia. The collection was beautifully mounted, and about two hours were spent in examining it under several microscopes brought by members of the club.

Memphis Microscopical Society.

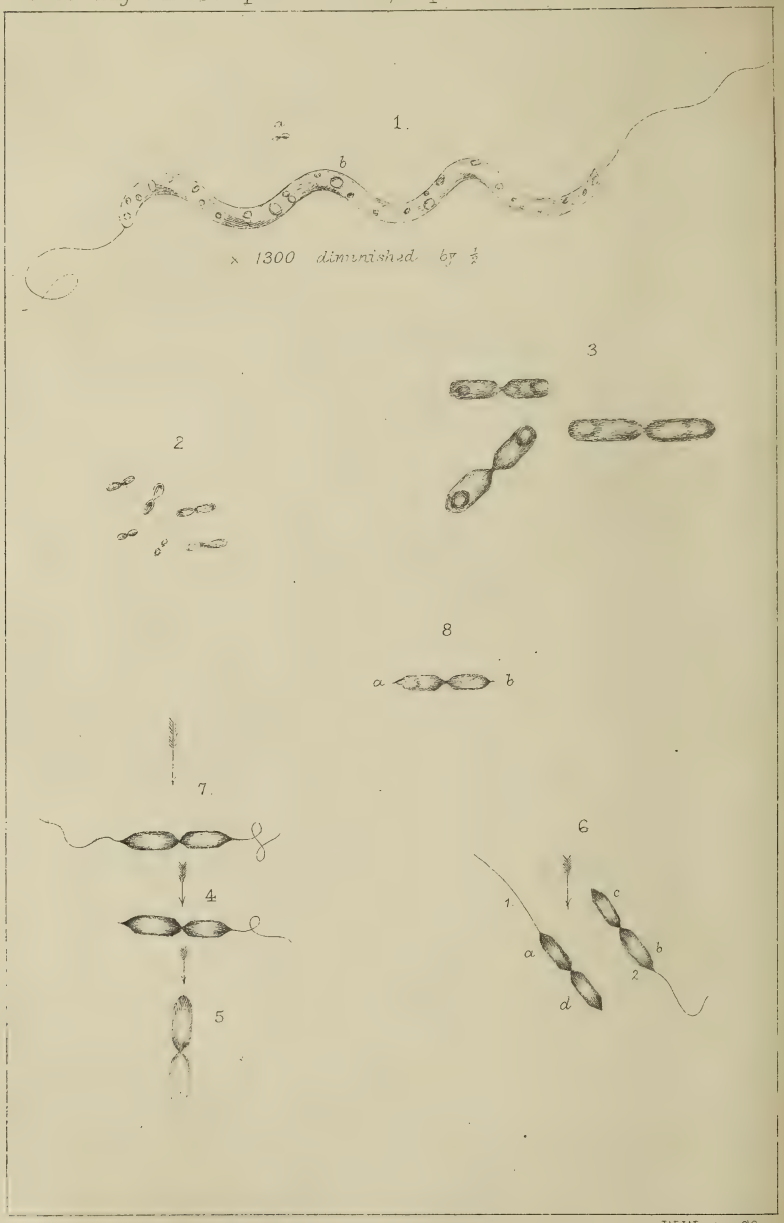
April 15, 1875.—The Society met at the usual hour, with a full attendance of members and visitors.

Donations were received; several handsomely prepared slides of various subjects from Mr. Frank Miller, of New York, and specimens of a somewhat rare species of insect, from Mr. N. N. Mason, of Providence, Rhode Island, for all of which a vote of thanks was passed. A pamphlet was received from Mr. R. B. Tolles, of Boston, giving a summing up of the angular aperture question, which of late has been with so much bitterness discussed on both sides of the ocean. A letter was read from Mr. Edwin Moulton, of Worcester, Massachusetts, asking for information as to the best method of preparing sections of coal, stating that the softening process described in the books proved very unsatisfactory in his experience. On this point all the members present agreed with his conclusion, and could offer no better way than to grind down like any specimen of rock.

A communication was read from Professor E. W. Morley, of Hudson, Ohio, giving in detail the measurements of another of Möller's probe plates. The results agree as closely as could be expected with the probe plate previously measured, the average variation between the two measurements being only about 5 per cent. The specimen of *A. pellucida* on the last plate gives a measurement of 95,000 lines to the inch. The care and patient skill of Professor Morley render it sure that the figures given may be implicitly relied on as correct.

The "scientific evening" of the Society having, from various causes, been deferred, it was decided to bring the matter to a conclusion by appointing Drs. Willett and Morse and Messrs. Omberg and Murray as a committee to carry out the plan and make all needed arrangements, unless unforeseen obstacles prevent. A report is to be presented by them at the next regular meeting, the first Thursday in May, when the Society can take final action.

A paper on "Plant Crystals" was read by Mr. A. F. Dod. All microscopists are familiar with these objects, but their place in the vital economy of the plant seems yet undecided. Mr. Dod concludes that they simply represent the *waste* of the plant's life-processes; in brief, that the useless and effete matter which in the animal kingdom passes off through the excreting organs, is in the vegetable kingdom represented by these raphides, &c. After considerable discussion of some of the points involved, the Society adjourned to first Thursday in May.



W West & C° sc.

Flagella on Bacterium termo.

THE

MONTHLY MICROSCOPICAL JOURNAL.

SEPTEMBER 1, 1875.

I.—*On the Existence of Flagella in Bacterium termo.*

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

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

Plate CXIII.

In the huge Bacterium known as *Spirillum volutans*, Cohn discovered, and figured in his 'Researches on the Bacteria,' * the presence of a pair of flagella, one at each end. This bacterium is gigantic in proportions in relation to most other varieties, but especially so to *B. termo*. An idea of their relative proportions is given in Fig. 1, Pl. CXIII., where *a* represents *B. termo* and *b* *S. volutans*. It is a spiral, and moves through the fluid by swift revolution upon its axis.

In the summer of 1872 some very fine specimens of *S. volutans* came under our notice, and were carefully examined. We were enabled fully to confirm Cohn's discovery, and demonstrated repeatedly the presence of a pair of swiftly lashing flagella; the drawing at *b*, Fig. 1, was made from a specimen magnified 1300

EXPLANATION OF PLATE CXIII.

Fig. 1.—*a*. *B. termo* magnified with the same power as *b*, which is a specimen of *Spirillum volutans* showing flagella at each end.

Fig. 2.—*B. termo* as seen with a power of about 600 diams.

Fig. 3.—The same, as seen with $\frac{1}{50}$th and second eye-piece (3700 diams.).

Fig. 4.—*B. termo* seen with flagellum at one end, the light coming in the direction of the arrow.

Fig. 5.—The same object when it moved at right angles to its former position, the light coming from the same direction, causing the sight of the flagellum to be lost.

Fig. 6 represents one *B. termo* which was in a still condition, but one flagellum moving. The light came in the direction of the arrow. When the end marked 2 *b* was in focus flagellum was seen, but none at the end *c*. When the end marked 1 *a* was focussed carefully, the flagellum at that end was seen, and lost at the end *d*.

Fig. 7.—The true form of *B. termo*.

Fig. 8.—The form as shown by the "supplementary stage" illumination *before* flagella were found, showing the pointed termination of the body at *a*, *b*.

* 'Beiträge zur Biologie der Pflanzen.' Zweites Heft, p. 127. Breslau.

diams. (diminished by $\frac{1}{2}$). The only difference between our specimens and those drawn and described by Cohn was, that we rarely saw them with so many turns in the spiral, and the granules were not so regular in arrangement, and were often very large. But these trifling differences may be accounted for in many ways.

It is extremely probable, as Cohn suggests, that Ehrenberg's *Ophidomonas janensis* is identical with *Spirillum volutans;* and if so Cohn only *demonstrated* what Ehrenberg inferred, from the presence of vortical action in front of the creature, viz. that there was a flagellum. But Cohn saw, and we confirm the fact, that there were two flagella in this form—one at each end.

Having closed for the present our Monad researches, we have been stimulated by the hope that the experience gained by these might enable us to prosecute similar investigations into the true life history of Bacteria. We have commenced the work this summer, and guided by the analogy of *S. volutans* we have been led to make several continuous efforts to find whether or not there existed a flagellum or flagella in *B. termo.* The task of course under the best circumstances must be a difficult one, from the extreme minuteness of the object. We tried each of Powell and Lealand's powers successively, from the $\frac{1}{12}$th to the $\frac{1}{50}$th, but with no definite result. Repeatedly we both saw vortical action at both the distal and proximal end of the *termo;* but could not absolutely see the organ causing it. But in the process of our investigations we made very close and careful observations on the *fission* of this form: we do not purpose now to describe the process, but merely to point out a phenomenon that further confirmed our suspicion of the presence of an invisible filament. In separating into two, the jointed rod of sarcode which is in process of division shakes to and fro at the constriction, as if the constricted part were a hinge; and at length a clear separation takes place to quite the length of the original *termo* (sometimes longer), and there is no *visible connection between them;* nevertheless *they act as one creature, so that if one moves in any direction the other goes with it just as the two parts did before separation;* showing that although we cannot *see* the connection there must be one; and the presumption was that it was a fine filament, such as we detected in the fission of some monads.* We could make no further progress in the question apparently; but our attention was called to the new $\frac{1}{8}$th objective prepared by Messrs. Powell and Lealand, with which we were soon supplied. We used it at first with the "supplementary stage" for very oblique illumination, supplied by the same makers; and this has the advantage of throwing the light in only from one direction. We were soon convinced of the exquisite performance of the glass when used as an immersion. *Amphipleura pellucida* was not merely seen to be

* 'M. M. J.,' vol. x. p. 55; and vol. xi. p. 8.

striated clearly and sharply, but the striæ were resolved into beads with the third and fourth eye-pieces. In like manner the fine striæ in *Surirella gemma* were instantly shown to be beaded with perfect and brilliant definition with the second eye-piece. *Navicula rhomboides*, and an extremely delicate specimen of *Pleurosigma attenuatum* which had resisted everything below a $\frac{1}{16}$th immersion, showed beaded striæ perfectly. We were therefore encouraged to try again to discover flagella in the *termo*. Some of our specimens, nourished in Cohn's nutritive fluid, were placed in a drop of distilled water, and put upon the supplementary stage on an ordinary slide covered with the thinnest cover. The utmost delicacy and tact in manipulation of the light is the great desideratum; but with this, enough may be secured to work with the fourth eye-piece. The light may be made to enter the objective at almost every angle, but it is always projected in a direction at right angles to the stage; and the first thing we observed when the objects were sufficiently slow in their movements, and at right angles to the light, was that the ends of the *termo*, which we (and all other observers as far as we know) had taken for round proved themselves to be conical, terminating in a sharp point. The usual appearance of *B. termo*, as seen with a magnification of about 600 diams., is seen in Fig. 2; whilst the same seen with a magnifying power of 3700 diams. ($\frac{1}{50}$th and second eye-piece) is seen in Fig. 3, where a globular granule is seen in the end of each half. But with the method above referred to, the best conditions being secured, the two ends of the bacterium were distinctly *pointed*, as seen at *a b*, Fig. 8, and after nearly five hours of incessant endeavour a flagellum was distinctly seen at one end of each of two *termo* which were moving slowly across the field. The discovery was not sudden and transient, but lasted for at least twenty minutes; the exquisitely delicate flagellum was lashing rapidly the whole time, and one of its frequent conditions is shown in Fig. 4, the arrow indicating the direction of the light: but if the *termo* turned round at right angles, as in Fig. 5, all trace of the flagellum was gone; showing that its discovery depended entirely, all things being equal, upon its position in regard to the light.

But this observation was made only by *one* of us, the other not being present; and in pursuance of our plan we determined to see it again, convincing ourselves separately, and then together. After many hours of labour this was accomplished; and Fig. 6 shows one of two instances which we both saw together at the same time and in the same instrument. It was lying still, obliquely across the field; the light coming in the direction of the arrow. Both ends were not perfectly in focus at the same time, but in focussing the end marked 2 *b* (Fig. 6) the flagellum was distinctly seen, and was seen also to coil and lash; but no flagellum was then seen at the end *c* of

the same object; but by bringing it into delicate focus it presented the aspect seen at 1 *a* (Fig. 6), which really represents the same object at the same time only with the other end in focus, while the end marked *d* corresponding to 2 *b* of Fig. 6 was in its turn slightly out of focus and the flagellum lost to view. This observation, made together, was as satisfactory as could be desired; and it was thus demonstrated that there was a flagellum at *both ends* of the ordinary *B. termo.*

It will of course be understood that it is by no means an easy matter to secure the demonstration; and whenever we repeat it, it must always be with nearly the same amount of trouble and patience, although we can now with the ordinary condenser detect the vortical action both in front and (occasionally) behind the *termo* as we never did before. But the demonstration of the ultimate structure of a fixed object—as for instance *Amphipleura pellucida*—must be looked upon as a matter of great ease in comparison; and that for many reasons, the foremost being the motion and minuteness of the object, the swift play of the flagella, their similarity in optical properties to the fluid in which the bacteria live, the difficulty of retaining them in focus, and of getting them in such a position in relation to the light as to make demonstration possible. Of course all this would be removed if dead bacteria would answer, but they very rarely (indeed only once) have done so with us. The flagellum needs to be in slow motion to properly show itself; for even with the more delicate and minute monads it is a difficult thing to show the flagella in dead forms, since in the majority of cases they appear to be attracted round the body of the creature.

II.—*A New Mode of Illuminating for High Powers.*

By Dr. Whittell.

I have lately been testing some high-power objectives, and have discovered by accident a mode of illuminating which I have never seen described, and which appears to be worth further trial.

I was working in a room lighted by a window to my left, which is protected by a balcony so as to admit the sun's rays during the winter months only. The microscope was Beck's larger model, with rectangular prism and achromatic condenser, the central rays being stopped. The object-glass was Powell and Lealand's $\frac{1}{16}$th, with immersion front. The test-object was the *Surirella gemma*, and I had just succeeded in getting a fair view of the so-called longitudinal markings, when the sun peeped from behind a cloud and shone brightly on the microscope. The object became iridescent and the outline rather indistinct, but I could still distinguish the markings. I turned the prism towards another part of the sky, but found that this did not affect the appearance. I could scarcely imagine that the sun's rays from above could affect the view of an object seen under a glass working so close to the cover as a $\frac{1}{16}$th, but by way of experiment I shut off all light from below. I was surprised to see the object, although not very brightly illumined, showing all the markings as before. I now took the small condenser for opaque objects which fits into the stand, and condensed the sun's rays upon the upper surface of the cover. The field became bright, and the object and its markings beautifully distinct. The object was not white upon a dark field, as in what is called dark-ground illumination, but had the same appearance as when lighted by oblique light from below. The markings, however, were more distinct. I substituted the dry front for the immersion one, and found the result equally good.

I have since tried the same experiment with one of Zeiss' $\frac{1}{25}$th immersion lenses, with splendid results. With this lens I found that after obtaining a fine view of the longitudinal markings, I could, by slightly altering the focus, cause them to disappear, and a series of dots running transversely took their place.

I have also tried the experiment with one of Beck's $\frac{1}{8}$-inch objectives, and with No. 2 eye-piece I had a good view of the longitudinal lines.

If any microscopist be desirous of testing the observation, I must caution him not to be satisfied with a mere dark-ground illumination, but to gently toy with the condenser until he obtain a field having all the appearance of being illuminated from below. In my last experiment I found the condensing lens close to the

objective, and nearly parallel with the stage, the microscope being in an upright position.

My present impression, which is open to correction, is that the winter rays of the sun strike obliquely on the upper surface of the slide, and some of these being reflected, pass through the object at an oblique angle, and cause the same appearance as if the object were illumined by light transmitted from the mirror.

ADELAIDE, SOUTH AUSTRALIA.

III.—*The Resting Spores of the Potato Fungus.*

By WORTHINGTON G. SMITH, F.L.S.

PLATES CXIV., CXV., AND CXVI.

THE Potato disease in this country is rarely seen before the month of July, but this year I received some infected leaves for examination from the Editors of the 'Journal of Horticulture' at the beginning of June, and my reply to the correspondent was printed on June 10. The leaves were badly diseased, and I detected the Peronospora in very small quantities here and there, emerging from the breathing pores. This was a week or ten days before Mr. Berkeley brought the matter before the Scientific Committee of the Royal Horticultural Society;* and when I heard Mr. Berkeley's remarks about the Protomyces, I immediately accused myself of great carelessness in possibly overlooking it; but I was equally certain of the presence of the Peronospora in the specimens I examined.

On receiving authentic specimens of diseased plants from Mr. Barron of Chiswick, the brown spots on the Potato leaves at once reminded me of the figures of some species of Protomyces, and the dimensions agreed tolerably well with some described plants of that genus; but the spots, when seen under a high power, appeared very unlike any fungus, and they were very sparingly mixed with other bodies much smaller in diameter, and with a greater external resemblance to true fungus spores. These latter spore-like bodies were of two sizes—one transparent and of exactly the same size as the cells of the leaf (and therefore very easily overlooked), and the other darker, possibly reticulated, and smaller. A few mycelial threads might be seen winding amongst the cellular tissue, and these threads led me to the conclusion that the thickened and discoloured spots on the leaves were caused by the corrosive action of the mycelium, in the same way as Peach, Almond, Walnut, and other leaves are thickened, blistered, and discoloured by the spawn

* See *ante*, vol. i., 1875, p. 795.

of the Ascomyces, as illustrated at the last meeting of the Royal Horticultural Society.

My opinion, therefore, was soon formed that the "new" Potato disease (as it has been called) was no other than the old enemy in disguise, or, in other words, that it was the old *Peronospora infestans* in an unusual and excited condition. That climatic conditions had thrown the growth of this fungus forward and out of season was probable; but the idea that the pest would not at length attack all and every sort of Potato was to me most unreasonable, though the more tender sorts might be the first to suffer.

Suspecting the two-sized small bodies before mentioned to be of the nature of spores, and remembering my experiments during last autumn with ketchup, in which I observed that the spores of the common Mushroom might be boiled several times, and for lengthened periods, without their collapsing or bursting, I thought I would try to set free the presumed spores in the Potato leaves by macerating the foliage, stems, and tubers in cold water. This maceration was necessary because the tissue of the diseased leaves was so opaque and corroded, and the cell-walls were so thickened, that it was difficult to distinguish the threads and suspected spores from the cellular tissue. I did not treat the leaves with boiling water, because I wished to keep the threads and spores alive.

From day to day I kept the diseased leaves, stems, and tubers wet between pieces of very wet calico, in plates under glass, and I immediately noticed that the continued moisture greatly excited the growth of the mycelial threads; this to me was quite unexpected, as I had merely wished to set the spore-like bodies free. So rapid was now the growth of this mycelium that after a week had elapsed some decayed parts of the lamina of the leaf were traversed in every direction by the spawn. Thinking the close observation of this mycelium in the now thoroughly rotten and decomposed leaves might end in some addition to our knowledge of *Peronospora infestans,* to which fungus I had no doubt from the beginning that the threads belonged, I kept it under close observation, and in about ten days the mycelium produced a tolerably abundant crop, especially in the diseased tubers of the two-sized bodies I had previously seen and measured in the fresh leaves. The reason why these objects, which undoubtedly occur in and about the spots, are so extremely few in number in those positions is, I imagine, because they require a different set of conditions for their normal growth, and these conditions are found in abundant and continued moisture.

The larger of these bodies I am disposed to consider the "oogonium" of the Potato fungus, and the smaller bodies I look upon as the "antheridia" of the same fungus, which are often terminal in position. The filaments of the latter are commonly

septate, and sometimes more or less moniliform or necklace-like. Both oogonium and antheridium are very similar in nature and size to those described as belonging to *Peronospora alsinearum* and *P. umbelliferarum*, and this is another reason (beyond my seeing undoubted *P. infestans* on Potato leaves at the beginning of June) why I am disposed to look upon these bodies as the oogonium and antheridium of the Potato fungus.

The larger bodies are at first transparent, thin, pale brown, furnished with a thick dark outer wall, and filled with granules; at length a number (usually three) of vacuities or nuclei appear. The smaller bodies are darker in colour, and the external coat is apparently marked with a few reticulations, possibly owing to the collapse of the outer wall. I have observed the two bodies in contact in several instances. After fertilization has taken place, the outer coat of the oospore enlarges, and soon gets accidentally washed off in water. Both antheridium and oogonium are so slightly articulated to the threads on which they are borne that they are detached by the slightest touch, but with a little care it is not really difficult to see both bodies *in situ;* and my observations lead me to think that conjugation frequently takes place after both organs are quite free. The antheridia and oogonia are best seen in the wettest and most thoroughly decomposed portions of the tissue of the decomposing tuber, but they occur also in both the stem and leaf. I consider Mr. Alexander Dean's remark, as reported in the 'Gardeners' Chronicle' for June 19 last, p. 795, to have a distinct bearing on this point, where he says, "In all cases where the seed tubers were cut they were quite rotten."

Before I referred to De Bary's measurements of similar organs in other species of Peronospora I was disappointed with the results of my observations, and felt disposed to refer the bodies and threads in the Potato leaves to Saprolegnia, but a glance at the figures now published and the similar figures copied from De Bary to the same scale, will show that if the bodies observed by me are Saprolegnia-like, the oogonia and antheridia figured by De Bary show an exactly similar alliance. Still, as the Saprolegnieæ are at present defined, I am by no means inclined to describe the bodies observed by me as *really belonging to that tribe of plants.*

The Saprolegnieæ have the habit of moulds and the fructification of Algæ, and they live on organic matter, animal and vegetable, in a state of putrefaction in water. One of the best known of these plants is *Botrytis Bassiana*, the parasite which causes the disease of silkworms. Now the genus Botrytis amongst fungi is almost or quite the same with Peronospora, to which the Potato disease belongs; and I consider it a strong argument in favour of my Saprolegnia-like bodies being the oogonia and antheridia of the

Peronospora when such an authority as Mr. Berkeley * considers one of the Saprolegnieæ (Achlya) "may be an aquatic form of *Botrytis Bassiana*"—the silkworm disease.

The common fungus which attacks flies (so frequently seen on our window-panes in autumn), *Sporendonema muscæ*, Fr., is said to be a terrestrial condition of *Saprolegnia feraæ*, Kutz., which latter only grows in water; and if a fly infected with the fungus be submerged the growth of the Saprolegnia is the result. It would now seem to be somewhat the same with the Potato when diseased, in the fact that when submerged a second form of fruit is produced.

Between the two moulds, Botrytis and Peronospora, there is little or no difference; the characters of Corda, founded upon the continuous or septate filaments, cannot be relied upon, and even De Bary himself figures *P. infestans* with septate filaments, like a true Botrytis. The intimate connection, however, between the Saprolegnieæ and some moulds cannot be denied, as the instances above cited clearly show; and I am therefore disposed to think that the fungus which produces the Potato disease is aquatic in one stage of its existence, and in that stage the resting spores are formed.

Reference may here be made to the bodies found germinating in the intercellular passages of spent Potatoes by Dr. Montagne (Artotrogus), and referred by Mr. Berkeley to the Sepedoniei. Ever since Mr. Berkeley first saw these bodies he has had an unswerving faith in the probability of their being the secondary form of fruit of *Peronospora infestans*, but unfortunately, as far as I know, no one has ever found a specimen of Artotrogus since Montagne.

The question may therefore be naturally asked,—How does Artotrogus agree with the presumed resting spores here figured and described? And has Mr. Berkeley been right or wrong in clinging so tenaciously to his first idea? Fortunately for the investigation of the Potato disease (which can never be cured till it is understood), Mr. Berkeley has given in the 'Journal of the Royal Horticultural Society' the number of diameters his figures are magnified to, and I have here engraved those figures so as to correspond in scale with my own drawings, which latter are sketched with a camera lucida. It will be seen that they are the same with each other both in size and habit, with the exception of the processes on the mature spore of Artotrogus—which processes may possibly be mere mycelial threads, or due to the collapsing of the inflated epispore. The reason these resting spores have evaded previous search is that no one has thought of

* 'Micrographic Dictionary,' p. 6.

finding them amongst leaves which had been macerated for a long period in water. There is, however, nothing unreasonable in fruit being perfected in water or very damp places, as it is common in the Saprolegnieæ and amongst Algæ in general. To sum up, there are four reasons why the bodies here described belong to the old Potato disease:

1. Because they are found associated with the Peronospora and upon the Potato plant itself.
2. Because they agree in size and character with the known resting spores of other species of Peronospora.
3. Because some other moulds are aquatic in one stage of their existence.
4. Because they agree in size with Artotrogus.

Now that these drawings illustrative of the fungus which causes the Potato murrain are reproduced in the following Plates,* it may be as well to explain at once some of the terms used and the nature and habit of the bodies hereafter referred to, for such readers as may not be thoroughly acquainted with the life history of the destructive parasitic moulds to which the Potato fungus belongs. For that purpose reference must be made to Fig. 1, which shows (greatly enlarged) a transverse section through the leaf of a Potato plant; the two great bodies at A A represent two minute hairs on the leaf, and at B B are seen the individual cells of which the leaf is constructed. When these hairs and cells are compared with the fine thread at C, which represents a branch of the Potato fungus coming out of a breathing pore of the leaf, it will be seen how very minute the fungus is in comparison with the dimensions of the leaf. This fine thread is no other than a continuation of a thread of spawn or mycelium which lives inside and at the expense of the assimilated material of the leaf. When this thread emerges into the air, as here shown, it speedily ramifies in different directions, and bears fruit at the tips of the branches, as at D D; these fruits are termed simple-spores, or conidia, because from their smallness they are dust-like. It is quite possible they may be an early state of the vesicles which contain the zoospores as seen at E, F. However this may be, they are commonly arrested in growth when still small, and they germinate in an exactly similar manner with the zoospores themselves, and may be considered somewhat analogous with seeds. The Potato fungus has another method of reproducing itself in the "swarm-spores" shown at E, F. These are so called because, on the application of moisture (as supplied by dew or rain, or when applied artificially), the vesicles set free a swarm of from six to fifteen or sixteen other bodies known as "zoospores," so named because they are furnished with two lash-like tails, and are capable of moving

* Plates CXIV., CXV., and CXVI.

PLATE CXIV.

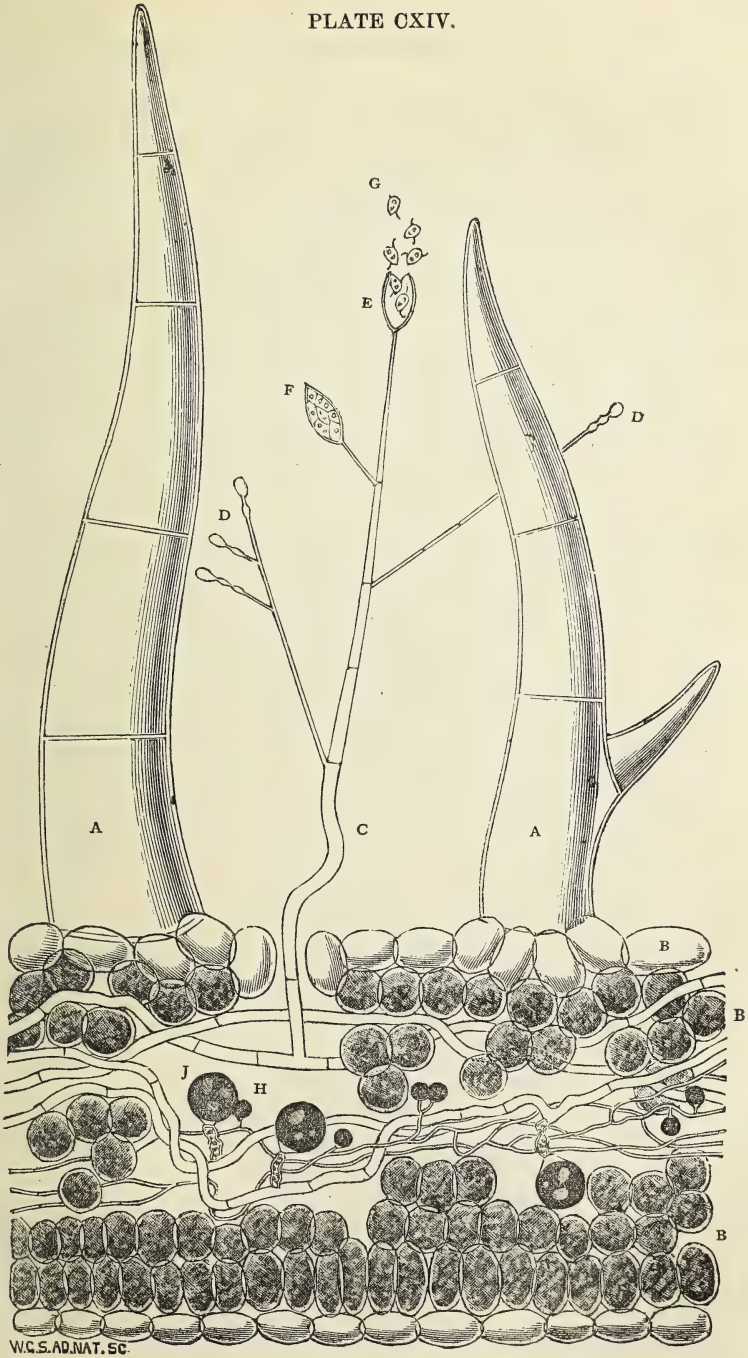


FIG. 1.—Transverse Section of a Fragment of Potato Leaf with *Peronospora infestans*. Enlarged 250 diameters.

rapidly about like animalcules. This rapid movement usually lasts for about half an hour, and (like the dust-like conidia or "simple-spores" before mentioned) the swarm-spores generally enter the breathing pores of the leaf, and there germinate. So potent, however, is the contents of these bodies when set free, that it is capable of at once corroding, boring, and entering the epidermis of the leaf, or even the stem or tuber itself. These zoospores are best seen when within the vesicle F, where they arise from a differentiation of the contents, but when once set free (G) they are, from the extreme rapidity of their movements, very difficult to make out. In about half an hour they cease to move, their lash-like tails (cilia) disappear, and having burst at one end, a transparent tube is protruded, which is a similar mycelium in every respect with that produced by the simple-spore, and which grows, branches, and fruits in a precisely similar manner.

Now the great difficulty which has beset botanists for so many years has been to account for the winter life of the Potato fungus. Simple-spores and zoospores are lost in the production of the mycelium or spawn, and this latter fine thread-like material cannot of course survive the frosts and rains of winter, but must utterly perish with the perished leaves and haulm.

A study of other species of Peronospora allied to the one which produces the Potato disease, reveals the fact of a third mode of reproduction. Simple-spores and zoospores are termed asexual, because they are without sex, as distinguished from other bodies called oospores, which are produced by the contact of two sexual spore-like bodies, known as the antheridium, which is the male, and analogous with the anther, H, and the oogonium, the female, and analogous with the ovary of a flower, J. The oospores, not till now seen for certain in the Potato disease, are the true resting spores. Instead of being transparent and unenduring, as are the simple and zoospores, these bodies are at length dense in substance, black-brown in colour, and covered externally with reticulations or warts. They are produced from the mycelium, by the contact of the antheridium and oogonium in the substance of the decaying plant; they are washed into the earth, and there they rest till a certain set of conditions makes them germinate in the year following their production, just as a seed falls and rests in the autumn and starts again into life during the following spring.

The terms here used will be better understood if the following note is borne in mind: The oogonium is analogous with a pod, the oosphere within answers to the ovule, and the oospore (or resting spore) is the matured seed. The antheridium with its contents is analogous with the anther and its pollen.

In various other fungi nearly allied to the Potato fungus these resting spores have been seen, measured, and illustrated, but till

now the resting spore of the Potato fungus has eluded all search. The reason generally given and accepted for its absence is, that the Potato is not the plant on which the fungus luxuriates to the greatest extent, and that if we only knew the plant it most affects (probably some South American species of Solanum) we should then find plenty of resting spores easily enough, for it must not be forgotten that the Potato fungus is by no means confined to the Potato. It grows on various species of Solanum besides *Solanum tuberosum;* it is even not unfrequent on the woody Nightshade of our hedges, and it grows upon the Tomato and other Solanaceous plants, together with at least one plant which belongs to quite a different natural order. On these latter, however, it makes less headway than upon the Potato. As an instance in point the allied pest of the garden Lettuce may be mentioned—*Peronospora gangliformis*—first described by Mr. Berkeley. Here, if the resting spores of the parasite are wanted, they must not be sought for in the Lettuce itself, where they are only sparingly produced, but in a plant belonging to the same natural order also commonly afflicted with the same parasite, viz. the common Groundsel; the resting spores are said to be even more common in Sow-thistles than Lettuces.

Therefore, although it is probable we shall have yet to look to some other member of the natural order Solanaceæ to find the resting spores in any abundance, yet, as the resting spores of the Lettuce mould can by searching be found in the Lettuce itself, so the resting spores of the Potato fungus have without doubt been found this year in the Potato plant.

How this came about is now pretty generally known. Mr. Murray exhibited some specimens of Potato leaves badly diseased before the Scientific Committee of the Royal Horticultural Society. In the corroded spots of these leaves Mr. Berkeley's sharp eye detected dark-brown warted bodies (but no mycelium), which he referred to the genus Protomyces. Assuming these bodies to be the true resting spores, which they doubtlessly are, they were necessarily free, as the coat of cellulose disengages them from the mycelial threads. But some similarly spotted leaves had been previously sent on to me, from the 'Journal of Horticulture,' upon which I detected the old Potato fungus, mycelial threads within the leaves, and some circular transparent bodies of two sizes, new to me.

In attempting to wash the circular bodies out of the leaves and stems, by maceration in water, I found the moisture greatly accelerated the growth of the mycelium, and that the long-sought-for oogonium and antheridium was at length the result. These bodies were at first most sparingly produced, so that for many days, and after most careful searching, I could only find one or two. Afterwards I found them more abundantly in different stages of maturity,

especially in the very putrid stems and in the tubers when in the last stage of decomposition. Mr. Berkeley afterwards found them with abundant mycelium, after the meeting of the Royal Horticultural Society on July 7, where he exhibited a drawing of one resting spore still attached to its thread. Mr. Broome (from material sent by me) has also detected and sketched, together with the immature sphærical bodies, one of these brown, coarsely-marked resting spores, but it was so involved in the mycelial threads (so he writes me) that he could not set it free. It is quite possible that the condition of the Potato, as seen during the present season, is quite exceptional, and that it may not occur again for a long series of years. Mr. Broome has written me to say he has never seen anything similar in diseased Potatoes.

In the accompanying illustration, which is an exact copy of the first sketch taken, the oogonia and antheridia are seen in the substance of the lamina of the leaf, the two bodies being in contact at H, J. In Fig. 5, Pl. CXV., many more of the same bodies are shown; some in actual contact; the two upper figures, K and L, show the resting spores some time after fertilization, when a coat of cellulose is the result. In K the spore is surrounded by this coat, whilst at L the spore is accidentally washed out by maceration in water. The semi-mature resting spores, as shown in these figures at M M, are furnished with a dark coat or skin; this coat, when further maturity is reached, clearly resolves itself into two layers, the inner one being termed the endospore, and the outer, which in *Peronospora infestans* is almost black in colour and warted, the exospore. The latter resembles in outward aspect, instead of one spore, a dense concreted mass of minute brown-black bodies. The antheridia are shown at N N. The perfected resting spores are slightly egg-shaped, and on an average are one-thousandth of an inch in diameter. The oosphere is fertilized by the contact of the antheridium; when the two bodies accidentally touch the latter fixes a small branch or tube, called a pollinodium or fecundating tube, into the wall of the oogonium, and discharges part of its contents into the protoplasm of the infant resting spore; when these resting spores are mature the mycelial threads soon vanish, and the spores are free.

When I read my first notes before the Royal Horticultural Society I had not been able to detect this fecundating tube, but since then I have several times seen it. After the Potato plant has been badly attacked and destroyed by the fungus, every part of the plant and its parasite perishes, except the dark-brown warted resting spores just described, and these find their way into the earth and hibernate. When they awake to renewed life in the summer they must germinate in the damp earth, and if no Potato plants are near they perish, as the earth cannot support them; in this

they are not unlike the seeds of germinating Dodder, for if they cannot find a proper host they die. But if Potato plants happen to be near the corrosive mycelium, it at once penetrates and enters the tuber or haulm. The tuber cannot produce simple or zoospores if buried, but in the haulms the mycelium doubtless soon grows and produces both these forms of fruit. These are at once carried by the air into the breathing pores, and the whole history of the fungus here described is re-enacted.

Since my observations on these bodies were published in the 'Gardeners' Chronicle' for July 10, I have (by the courtesy of the Rev. M. J. Berkeley) had an opportunity of carefully examining and measuring the original specimens of Dr. Montagne's Artotrogus, found long ago in the intercellular passages of spent Potatoes, and from the first considered to be the secondary form of fruit of the Peronospora by Mr. Berkeley. I have no hesitation whatever in saying that the bodies lately seen and now figured by me are positively the same with Dr. Montagne's in every respect, and when reflected and traced with the aid of a camera lucida no difference whatever can be detected. The bodies seen in Dr. Montagne's specimens are, without doubt, the fertilized and half-mature resting spores, and therefore dense, uncollapsed, and exactly the same in size, habit, and colour with mine when in the same stage of growth. After the lapse of so many years the threads, as might be expected, have more or less perished, but it is not difficult to find traces of antheridia in the specimens.

For comparison, the original figure of Artotrogus (Fig. 3) is here exactly reproduced to the same scale as my drawings, from vol. i. of the 'Journal of the Royal Horticultural Society,' to show the similar nature of the bodies illustrated. Since this was engraved Mr. Berkeley has kindly forwarded Dr. Montagne's original drawings to me for examination, and I may as well say they contain many more threads and oogonia than are shown in this cut, and they are also more like my organisms now brought forward. As for some of the bodies being shown as if within the threads by Dr. Montagne, I consider this of little moment, as the oogonia are at times almost or quite sessile, and consequently, when seen in some positions, they put on an appearance of being within the mycelium, whilst in reality they are upon or under it. As for the echinulate body at O, described as a "mature spore," it is not exactly like Dr. Montagne's original drawing, which is shown as furnished with a thick wall, and there are no "mature" spores in his specimens. After a most careful and searching examination of the latter I can find no such bodies, but there are several spores on the two mica slides which put on a *spuriously* echinulate appearance, which is owing to the collapse of the coat of cellulose, as suggested by me as a possibility when I read my first paper.

PLATE CXV.

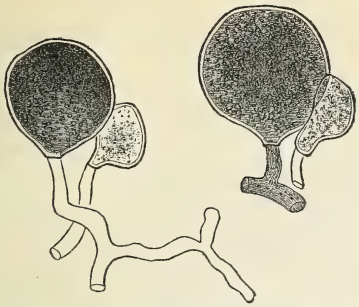


FIG. 2.—*Peronospora alsinearum.*
Oogonia and Antheridia enlarged 400 diameters.

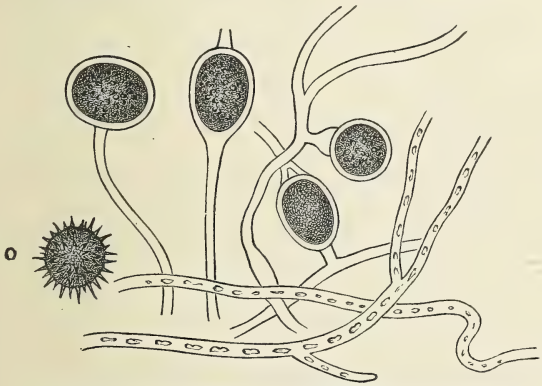


FIG. 3.—The Artotrogus of Montagne and Berkeley.
Enlarged 400 diameters.

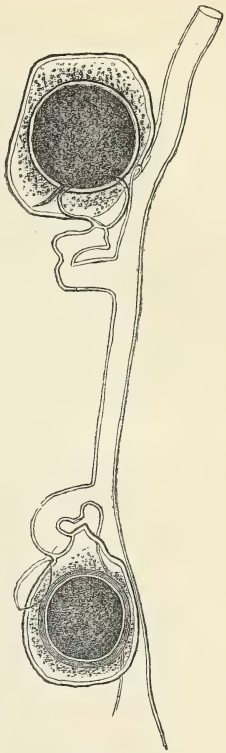


FIG. 4.
Peronospora umbelliferarum.
Oogonia and Antheridia
enlarged 400 diameters.

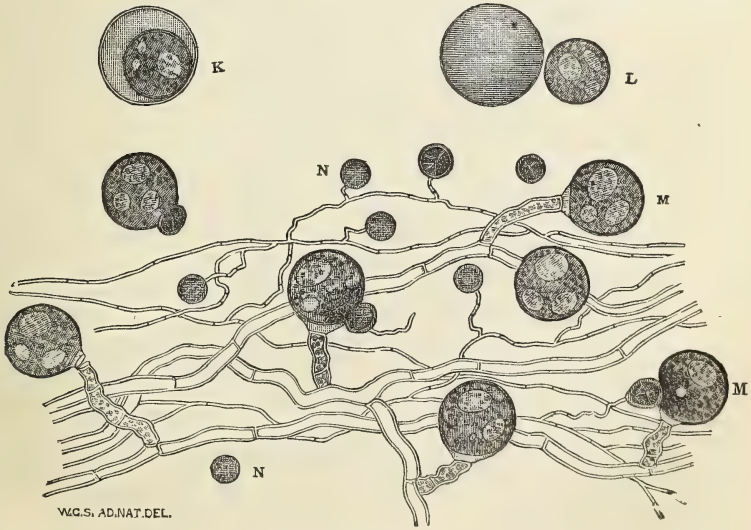


FIG. 5.—*Peronospora infestans.*
Oogonia and Antheridia from badly diseased leaves of Potato, after a week's maceration in water; enlarged 400 diameters. Antheridium $\frac{1}{2500}$ = ·0004 inch. Oogonium $\frac{1}{1000}$ = ·001 inch. Coat of Cellulose $\frac{1}{700}$ = ·00142 inch.

It will be observed there is a little difference in size between my oogonia (Fig. 5) and those copied from the 'Journal of the Royal Horticultural Society;' this is because the figures in the latter are somewhat incorrect. When the *actual specimens* are examined and measured side by side they are in every way identical.

Mr. Berkeley has also most obligingly sent me a specimen of another (new species ?) of Artotrogus, found in decayed Turnip by Mr. Broome in 1849. Here the threads and semi-mature bodies are in the same style as the oogonia and threads from the Potato, and the mature spore is not truly echinulate; it is globular, with a slight tendency to an oval shape, and is covered with warts. It is probably the resting spore of *Peronospora parasitica*, the pest of the Cabbage.

In Figs. 4 and 2 are given copies of the oogonium and antheridium of *Peronospora umbelliferarum* and *P. alsinearum*, enlarged from De Bary to the same scale as the other figures, to show the close similarity in size and habit.

Since this subject has been made public Mr. Carruthers has kindly furnished me with a copy of Dr. Farlow's paper on the Potato Rot, extracted from the 'Bulletin of the Bussy Institution,' part iv., a paper I had not previously seen. As some of Dr. Farlow's practical observations seem to have a direct bearing on some of the points raised by me, I will conclude by extracting one or two sentences: "The disease is first recognized by brown spots on the leaves" (p. 320). "If we examine any Potato plant affected by the rot, even before any spots have appeared on the leaves, we shall always find these threads in the leaves, stem, and, in fact, nearly the whole plant" (p. 322). "The Peronospora is much more easily affected by moisture than the Potato plant itself." "Suppose the temperature to keep equally warm, and the atmosphere to become very damp, then the absorbing power of the mycelium is very much increased, while the assimilating power of the leaf-cells is little altered. Thus it happens that a sudden change from dry weather to moist will cause the mycelium to increase so very much beyond the power of the Potato plant to support it, that in the struggle for existence the latter blackens and dies." "When the disease has arrived at a certain point, viz. just about the time of the appearance of the spots on the leaves, these mycelial threads make their way into the air" (p. 323).

I give in conclusion an illustration of the perfectly mature resting spore of *Peronospora infestans*, as seen imbedded in the substance of the Potato leaf (Pl. CXVI.). These resting spores, which carry on the winter life of the fungus, are not restricted to the leaves, for I find them sparingly in both haulm and tuber, although I have at present seen the best specimens in the leaves. The engraving given

herewith (Fig. 6) shows a transverse section through a black spot of one of the leaves from Chiswick, and the resting spore is seen at A nestling in amongst the cells of the leaf. An antheridium, B, and two oogonia (C, C), from which such resting spores arise, may be seen in the cut, and the old common form of the fungus will be noticed breaking through a hair on the upper surface of the leaf, which is a very uncommon occurrence. The situation of the resting spores can generally be ascertained on the leaves by noticing the slightly thickened and very dark spots, for the bodies are commonly in these spots. It is, however, an extremely difficult matter either to get them out, or, indeed, to see them when imbedded, for, when mature, they are black-brown in colour, and only a little larger in size than the leaf-cells. These leaf-cells are also intense brown-black in colour from contact with the hurtful mycelium, and almost as hard as wood. The best way to see the resting spores is to macerate the leaves for several days in water, and then set them free by crushing the spot between two slips of glass. The presence of the fungus in the leaf makes the cells very thick and woody as well as black, so that in crushing the leaf-cells the resting spore is not uncommonly crushed at the same time. With care, however, they can be got at, when they will be seen, as at D, covered with warts or coarse reticulations, and beautifully regular and perfect in outline: when young they are of a pure warm sienna colour, and when perfectly mature, brown-black and shining. They are sphærical or slightly egg-shaped, and measure on an average about one-thousandth of an inch in diameter. I consider it worthy of special note that these resting spores are almost exactly the same in size, conformation and colour with *Peronospora arenariæ*, Berk., an allied species found parasitic on *Arenaria trinervis*. In looking for these bodies care must be taken not to confound them with corroded cells, granules of starch injured by the disease, or foreign bodies.

At E is shown a semi-mature resting spore with pollinodium attached, accidentally half washed out of its coating of cellulose by maceration in water.

I may say as an addendum that to me there is a marked analogy in size and habit on the one hand between the oogonia and the vesicles which contain the zoospores, and on the other hand between the simple-spores and the antheridia. I consider that the oogonia and antheridia are merely the intercellular condition of the vesicles which contain the zoospores and conidia, which latter are the aërial state of the former.

The facts which point in the direction just indicated are these: Sometimes there is no differentiation in the contents of the vesicles, but the plasma is discharged in one mass and not in the zoospore condition, the vesicle then resembles the oogonium. At

PLATE CXVI.

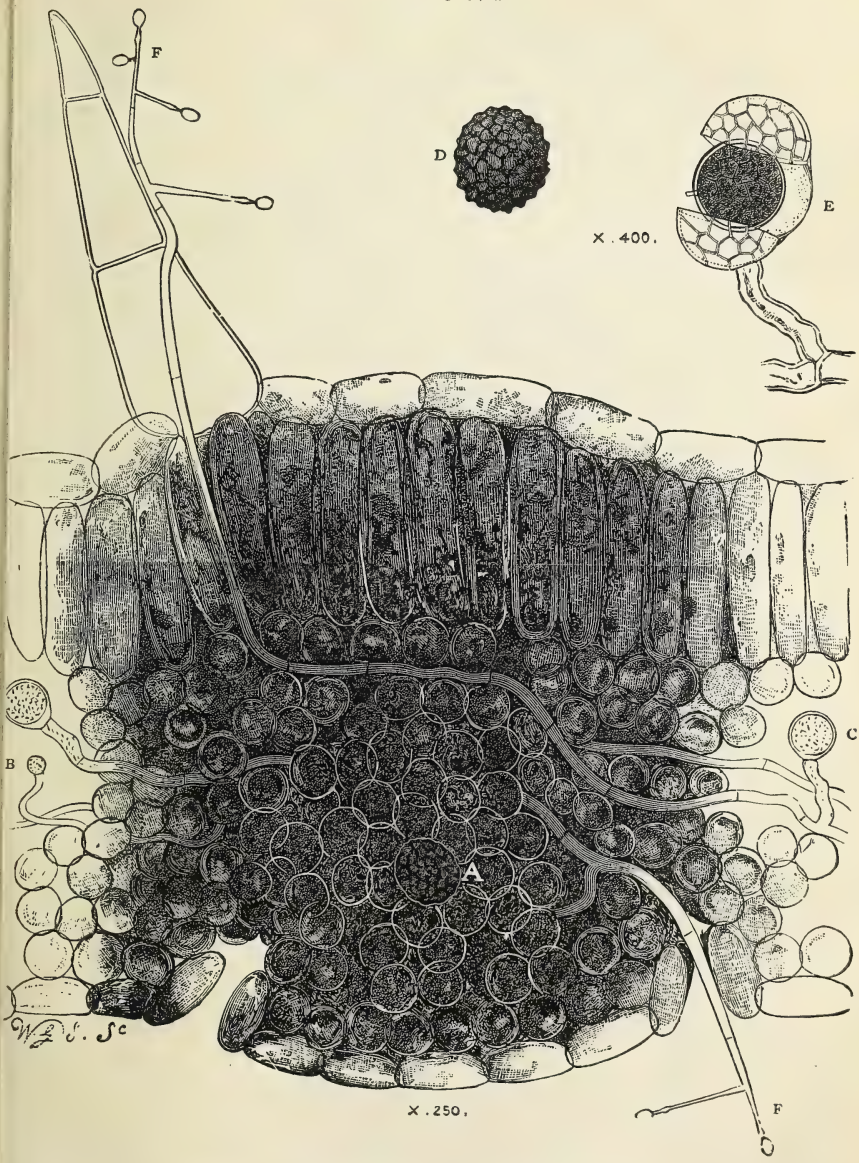


FIG. 6.—The Resting Spore of the Potato Fungus (A) imbedded amongst the Leaf-cells. Enlarged 250 diameters.

Semi-mature Resting Spore (E); Mature ditto (D). Enlarged 400 diameters.

other times the oogonium shows a distinct differentiation in its contents, and matures from one to three resting spores, which to me shows an approach to the condition of the vesicle which usually gives birth to the zoospores.—See also the 'Gardeners' Chronicle,' July 17 and 24, from which the above Plates have been taken.

Since the above observations were printed, the following facts have been observed by me, and recorded in the 'Gardeners' Chronicle' for July 31.

1. Some plants sent to the Royal Horticultural Society by Mr. Dean on July 21 were covered with the Peronospora far beyond anything I had ever seen before. The haulm, the leaves (on both sides alike), and the berries, were covered. Some of these plants, after being placed on a garden bed, and covered with leaves (to keep them moist), were the next day one white mass with the Peronospora.

2. The Potato fungus (as commonly seen) bears a far larger number of simple-spores than inflated vesicles containing the zoospores or swarm-spores, but in Mr. Dean's plants the fungus produced zoospores almost exclusively, and in the greatest abundance. As the zoospore is a higher development of the plant than the simple-spore, this latter observation points to the unusually robust health of the fungus this season.

3. On suspending the infected leaves over a glass of water for from twelve to seventy-two hours, the swarm-spores fell in abundance (either free or in the vesicle) on to the water, and there germinated. No single drop of the water could be taken up for examination without meeting with the germinating spores, the threads radiating over the water in every direction, evidently in quite a congenial element. It brought the following fact to light, which is of importance—some of the vesicles which usually discharge the zoospores discharged instead a thick mass of mycelium; and this cord, when it had proceeded a considerable distance over the water, there had its contents differentiated in a necklace-like manner, and gave birth to the zoospores far removed from the original vesicles. The same thread also produced two true oogonia on the water.

4. At the meeting of the Scientific Committee of the Royal Horticultural Society, held on July 21, Mr. Renny showed a species of Saprolegnia which, he said, might be mistaken for Peronospora. But if reference is made to my original paper it will be seen from the first that I have perceived the intimate connection between the new condition of the Potato fungus and the Saprolegnieæ. On my side I have the high authority of Thuret and Berkeley for similar alternation in the diseases of silkworms, flies, &c. I am quite prepared, therefore, to consider Mr. Renny's plant,

if not the same, some close ally with mine, even if it should turn out to be a true Pythium, and its oogonia produce zoospores in water, especially after what is known of the nature of Cystopus, the close ally of Peronospora. Two strong points in favour of this view are these: (1) The resting spores of Pythium are *unknown*, but if I find Pythium inside Potato stems and leaves mixed up with the Peronospora, and the same Pythium in the very centre of the tuber of the Potato (as I have done), there maturing itself and forming its resting spore, then the identity of the two may reasonably be assumed, and the resting spore of the Pythium, as well as the Peronospora, is found. (2) The same cells in the Saprolegnieæ will alternately produce, under the same (or different) conditions, zoospores or resting spores; therefore, if zoospores are produced in Mr. Renny's oogonia in water, it is reasonable to assume that under different conditions resting spores would be formed by similar cells. I have, from the first, believed the Saprolegnia condition of the fungus to be widely diffused, and when in that state it quite possibly grows on diverse plants and substances in watery places, as was explained by me. The Saprolegnia is the caterpillar condition (belonging to the water, like the larva of the dragon-fly), the Peronospora somewhat analogous with the perfect butterfly, and the resting spore with the dormant chrysalis.

5. I find by experiment, when badly diseased haulm, fruit, and tuber are partly submerged for from one to four days, the Peronospora changes its character, and produces the Pythium or Saprolegnia-like growth on the submerged parts. On examination of the plants this may be easily overlooked, as the Saprolegnia commonly frees itself and floats on the surface of the water, and must be carefully taken off (invisible as it is) with a camel-hair pencil. If the oogonia now produce zoospores in the water, as in Pythium, which is possible and even probable, it in no way invalidates my views, or makes the connection less probable between Pythium and Peronospora.

6. The aërial spores of the Peronospora never become globular in water, whilst the oogonia and antheridia are always so.

7. A superabundance of water excites the growth of the mycelium, but it retards the proper production of the resting spore, just as a superabundance of water in most plants makes leaves and retards flowers.

8. In my calendar of the weather I find we had here only five wet days from May 7 to June 10 (no wet between May 8 and 20), and it was during this dry weather that the Potato fungus this year lived inside, and at the entire expense of the plant, and there perfected its resting spores. With the twenty-two wet days after June 10 the Peronospora put on its usual shape, and came to the surface.

9. I have got my most abundant materials from the tuber when soft and almost transparent, like painter's size; in this state the starch is utterly destroyed, and, what is most curious, there is no offensive smell. The tuber frequently decomposes with a horrible fœtor, and turns whitish inside; the starch is then present and more or less injured, and very little can be seen of the fungus.

10. The season is too far advanced, and the fungus has already caused too much destruction, to think of grappling with it this season, but when it is remembered how the Vine, the Corn, and Hollyhock parasites have been restrained, it certainly does not seem impossible that means may be found to mitigate the damage done every year by the Potato murrain.

IV.—*The Microscopic Germ Theory of Disease; being a Discussion of the Relation of Bacteria and Allied Organisms to Virulent Inflammations and Specific Contagious Fevers.* By H. Charlton Bastian, M.D., F.R.S., Professor of Pathological Anatomy in University College.

(*Continued from p.* 79.)

Turning from these statements, therefore, as to the assumed modes by which bacteria habitually gain an entry into the healthy human body, I may say that many of the methods by which Professor Kühne, Dr. Sanderson, and others, have attempted to ascertain whether the different tissues contain actual or "potential" germs are pointless in the face of the statements of heterogenists, since their methods cannot enable them to say, when positive results are obtained, that the "potential" germs from which, as they assume, the organisms have been developed are other than elementary particles of the previously healthy, though now altered, tissues, or that they have not been produced from the fluids which the tissues contain. These experimental observations are not only almost valueless on this account, but they are altogether needlessly complex. Why resort to heated knives, boiled thread, rapid movements, frequent immersions in paraffin at 260° F., paper boxes, warm chambers, &c., when precisely similar results might be obtained by simply leaving the dead animal alone for three or more days, and then subjecting the central tissues of either of the viscera to microscopical examination? So far as the principle of the method is concerned, or the kind of results which it may yield, it makes no difference whether we keep an extracted portion of tissue enveloped in paraffin in a warm chamber for hours or days, or resort to the much simpler method of leaving the animal unopened for several days before submitting its tissues to examination. In

either case where organisms are found this fact alone would give us no right to infer that they had developed from pre-existing germs (in the natural history sense of that term); they may, on the contrary, have arisen either by heterogenesis or by archebiosis.

The weight of probability in favour of either of these two possibilities can only be judged of by resort to a different method of procedure. Because in view of the observed absence of bacteria from the tissues of such organs as kidney, liver, or brain, immediately after death, the subsequent multitudinous presence of organisms in these situations would, in the face of satisfactory independent evidence, be more easily accounted for by heterogenesis or archebiosis than by the hypothesis of pre-existing latent or potential germs. By an appeal to evidence of this kind, moreover, we are enabled to test the probability of the hypothesis previously referred to as being supported by Dr. Beale and others—viz. that which assumes the existence of invisible and mysteriously derived germs of bacteria and fungi throughout the elements of the tissues—an hypothesis somewhat wild in character, which has, I believe, no other foundation than the frequently observed prevalence of organisms in some of these situations.

With the view of settling these questions, therefore, we may carefully prepare an infusion from some animal tissue, be it muscle, kidney, or liver; we may place it in a flask whose neck is drawn out and narrowed in the blow-pipe flame; we may boil the fluid, seal the vessel during ebullition, and, keeping it in a warm place, may await the result as I have so often done. After a variable time the previously heated fluid within the hermetically sealed flask swarms more or less plentifully with bacteria and allied organisms, even though the fluids have been so much degraded in quality by exposure to this high temperature, and have thereby, in all probability, been rendered far less prone to engender independent living units than the unheated fluids in the tissues would be. We operate, however, under these disadvantageous conditions in order to make thoroughly sure that, by the preliminary heating, we have destroyed all pre-existing life within the flask; and, notwithstanding such adverse circumstances, we are able to obtain evidence of the occurrence of archebiosis. The researches of Kühne and others have fully shown that the protoplasm entering into the composition of the tissues of warm-blooded animals is coagulated and killed at a temperature of about 111° F.; whilst my own investigations * also show that bacteria and allied organisms are killed by exposure in the moist state to a temperature of 140° F.

Hence I contend that the wide distribution of bacteria throughout the human body in connection with dying tissue-elements in

* 'Evolution and the Origin of Life,' 1874, p. 101.

the most varied situations, and also in diseased fluids, is explicable most easily by assigning for many of them an origin by heterogenesis and by archebiosis (though when so produced they multiply rapidly in the ordinary fashion); and that my position—that bacteria are pathological products—is one which may claim to have been fairly established.

On this subject I would only add a word or two concerning the point of view and reasoning employed by those who seem willing to believe in almost any infringement of natural uniformity rather than admit the occurrence of heterogenesis and archebiosis, or either of them alone. The most remarkable recent utterances on this subject are those of Dr. Sanderson, though it is only fair to say that they are somewhat typical of the line of argument adopted by many others.

Whilst admitting that bacteria in their "ordinary state" have been proved to be killed at a temperature of 140° F., and also by immersion in absolute alcohol, Dr. Sanderson assumes that other bacteria germs may exist in an extraordinary state, in which they have the power of resisting the influence of this temperature, the influence of absolute alcohol, and even the simultaneous action of both these destructive agents. But if we ask on what amount of evidence this assumption is founded, many may be astonished to find that such an extraordinary belief has been adopted simply because bacteria make their appearance in an organic infusion which has been prepared by macerating an organic extract previously submitted to the influences above mentioned—just as bacteria make their appearance within our closed flasks, whose contents have been previously heated to the higher temperature of 212° F. Has it ever occurred to Dr. Sanderson that another interpretation might have saved him from the necessity of adopting this extraordinary belief?

Again, in his third lecture, the same investigator shows himself for the time similarly oblivious of the point of view of those who believe in archebiosis, whilst the argument made use of to support his own position is of a very surprising nature. After remarking* that "of all perishable things, protoplasm is amongst the most perishable," he goes on to state that bacteria possess "a wonderful property of passing into a state of persistent inactivity or latent vitality." This is nothing more than an explicit expression of the notion previously referred to, though I wish especially to call attention to the additional "evidence" upon which the view is now based. Dust, containing organic débris, in which, as Dr. Sanderson confesses, he has no proof that anything living is contained, may be added to a fluid at the time barren, though freely capable of supporting life. One of the results of this addition is the appearance,

* 'British Medical Journal,' March 27, p. 403.

after a short time, of bacteria. A physicist or chemist might conceive it possible that, as a consequence of such admixture, a compound not previously existing might have been more or less slowly formed—as this, at all events, is one of the modes by which new chemical compounds are engendered. But this point of view Dr. Sanderson will not seriously entertain—indeed, his remarks seem only explicable from the point of view of a foregone conclusion that archebiosis is an impossible process, and therefore on no account to be admitted as an interpretation of the facts. In reply to an imaginary objection, alleging that he had no proof that the dust contained anything living, he says with great *naïveté*: "True; but I have proof that it contains that which produces life, and express this state of things—viz. the absence of manifestations of life on the one hand, and on the other the fact that the stuff in question possesses the power of impregnating something else which before was barren—by saying that the dust possesses latent vitality." The legitimacy of the inference does not seem very apparent to me, if it is to be taken in any other than a poetical sense; yet this is the only evidence adduced in favour of the assumed existence of an extraordinary state in which bacteria may exist—a state in which they are assumed to be capable of resisting influences which are admitted to be destructive to all actually known forms of life. Of course, on the same grounds, the physicist might argue that "friction possesses latent electricity," or the chemist that "oxygen possesses latent acidity," but it seems very questionable whether such statements would be regarded as serviceable additions to science. Neither can we consider that any further light is thrown upon this notion of "latent vitality" by Dr. Sanderson's concluding observations upon the subject, in which he says:* "The vital activities of the organism are stored up for the future, *the individual being for this very end endowed* with the power of resisting external agencies, and thereby of enduring for an indefinite period." As to such teleological notions I have nothing to say; I prefer keeping to the region of fact and warranted inference. These, however, are the arguments by which a belief in the occurrence of archebiosis and heterogenesis is for the time averted.

Before drawing my remarks on this section of the subject to a close, I would point out that the views admitted by Dr. Beale, and those who think with him, those admitted by Dr. Sanderson, Professor Kühne, and Dr. Tielgel, as well as those recognized by myself and others, all coincide with one another on a certain common ground. We are agreed as to the fact that bacteria are abundantly present within the body, or that they may appear therein under certain conditions independent of any immediate external contamina-

* 'British Medical Journal,' April 3, p. 436.

tion—however much we may differ amongst ourselves as to the interpretation of their presence, actual or possible. Yet this common ground contains an admission which is decidedly inimical to Mr. Lister's theories. Following M. Pasteur, this distinguished surgeon would have us believe that whilst bacteria are disease germs, they do not naturally exist within the body. He has based his antiseptic system of treatment on the assumption that air, or surfaces which have been exposed to it, coming into contact with wounded portions of the body, are the means by which his assumed animated poisons are introduced into the system. But it is, I think, now well known that the whole pyæmic process may be met with occasionally, even where there is no abrasion of the surface of the body. And, moreover, as regards the cause of the disease in persons with open wounds, I may say that Pasteur never seriously attempted to discriminate between the respective effects of the living and the dead elements entering into the composition of atmospheric dust. Effects which were often due to the action of mere organic débris he attributed to the influence of living germs,* and in this respect M. Pasteur has been followed by Professor Lister.

But, as I take it, the essential practical fact which Professor Lister wishes to enforce is that the putrefactive processes apt to take place in wounds ought to be reduced to a minimum, because it seems certain that during such processes poisons are liable to be engendered whose absorption or local influence upon the system may be attended by the most fatal results. Such a notion, which is assuredly thoroughly well founded, may, however, be acted upon by the adoption of the antiseptic system of treatment (or by free exposure of wounds and frequent removal of secretion), quite independently of the question whether mere organic débris may act as ferments, and also quite independently of the further question whether the poisons engendered in wounds are living entities or complex chemical compounds not endowed with the attributes of living matter.

Applicability of the germ theory to artificial tuberculosis, syphilis, typhoid, typhus, relapsing fever, cholera, measles, scarlet fever, small-pox, and other contagious fevers.

I now pass to a consideration of the germ theory in its relation to another class of diseases, although I do not wish to convey the idea that there exists in nature a distinct boundary line, such as my division of the subject might indicate. It must be clearly understood that the local morbid processes or inflammations of a virulent type—which may or may not gradually entail a more general morbid condition—pass insensibly by means of such affec-

* 'Evolution and the Origin of Life,' pp. 103–114.

tions as artificial tuberculosis and syphilis into that class of diseases under which are included such affections as typhus, typhoid, relapsing fever, cholera, measles, scarlet fever, small-pox, and other contagious fevers. Affections like artificial tuberculosis and syphilis might therefore have been placed with equal appropriateness in either of the divisions I have adopted.

The treatment of the present part of my subject may be disposed of in a more summary manner than the last, principally because many of the facts and considerations which were advanced in reference to virulent inflammations and their sequelæ, and the presence of independent organisms in the altered fluids and tissues of the body, are also applicable to the question of the relation of such organisms to the more specific contagious fevers.

The case to be made out in favour of the germ theory as applied to these latter fevers is also, in my opinion, much weaker than it is in respect to the virulent inflammations and their sequelæ, since, although such contagious fevers have always been regarded as general and essentially "blood diseases," in only one of those occurring at all commonly in the human subject does it appear that anything like an independent living organism is to be met with in the blood. There is, therefore, here a *primâ facie* inherent weakness in the whole theory, which I think a thorough examination of the question will strongly tend to confirm rather than dissipate.

The reasons relied upon in favour of the germ theory, as applied to these diseases, are of a purely *a priori* or theoretical nature, and such as I have already referred to. They are, in fact, based upon the assumed nature of contagium, and upon its assumed mode of increase within the body. How little conclusive such *a priori* reasons are, and how the facts may be otherwise explained, I have already endeavoured to show, and as the theory in its applicability to these diseases rests upon absolutely no positive evidence that I am aware of, I am compelled to leave a gap here, and pass on to a brief enumeration of the facts and considerations which seem to tell so strongly against the existence of any causal relationship between organic germs and these specific contagious fevers:

1. The fact that, with two exceptions, no definite germs or organisms are to be met with in the blood of patients suffering from these diseases, during any stage of their progress.

2. The fact that the virus or contagium of some of these diseases, whatever it may be, does not exhibit the properties of living matter.

3. On the other hand, the virus of most of these contagious diseases with which definite experiment has been made, is most potent in the fresh state, whilst its power very distinctly diminishes in intensity as organisms reveal their presence more abundantly

therein—facts which would seem to point to the conclusion, or at least are quite consistent with the notion, that the contagious poison may be a chemical compound which gradually becomes destroyed or modified by the successive changes taking place in association with processes of putrefaction.

4. There is the extreme improbability of the supposition that this whole class of diseases should be caused by organisms known only by their effects.

5. The fact of the sudden cessation, periodical visitations, and many of the other phenomena of epidemics, however difficult they may be to explain upon any hypothesis, seem to oppose almost insuperable obstacles to the belief that living organisms are the causes of such epidemics of specific contagious diseases.

It would seem little better than an ill-timed attempt at jesting to postulate the existence of distinct germs for these several specific fevers, and at the same time to endow such imaginary entities with properties different from those of all known germs. To remain always in the germ stage in media favourable for their multiplication would, even if the imaginary germs were visible, be contradictory to all previous experience; but to suppose, in addition, that such hypothetical invisible entities are capable of resisting the influence of agencies which have been proved to be destructive of all known living matter would seem to be going altogether beyond the bounds of probability. And if we look at the question from this point of view, we may regard it as a definitely established fact that the virus of cholera, for instance, is not composed of living germs or particles. Messrs. Lewis and Cunningham have shown* that the virus is not appreciably impaired in activity when the fluids containing it have been raised for a few minutes to a temperature of 212° F.; and in reference to this subject they say, "We have seen no living object preserve its vitality after exposure in a fluid to a temperature approaching to 212° F., nor have we been able to satisfy ourselves that anyone else has done so.

In only one of the specific fevers commonly met with in the human subject have organisms been found in the blood: this exception is relapsing fever. There is, however, an affection occasionally communicated from cattle (sang de rete, or splenic fever) in which organisms are also met with in the same situation. But the fact of the existence of actual visible organisms in these cases seems altogether robbed of its significance after the occurrence of archebiosis and heterogenesis in diseased fluid and tissues has been demonstrated. The view, indeed, that the organisms found in these affections owe their origin to certain changes prone at times

* 'Report, &c., into the Nature of the Agent or Agents producing Cholera,' pp. 46 and 57, 1874.

to occur in the fluids of the body is directly supported by some of the most interesting results of Dr. Sanderson's experiments concerning pyæmia. He tells us that in some of the lower animals artificial tuberculosis and pyæmia are often only different effects of the same cause. That is, that some of the same inoculating material may be introduced beneath the skin of two rabbits, and in the one a slow and more chronic set of morbid changes is induced (tuberculosis), whilst in the other more acute and rapidly fatal processes are established (pyæmia). In the former animal no organisms are to be found in the blood, whilst the blood of the latter, according to Dr. Sanderson, is swarming with them. Changes in the character of the morbid process therefore may occasionally favour the presence of organisms. Nay, further, we see the same kind of difference in another way. Pyæmia and septicæmia as they occur in some of the lower animals differ in one very notable respect from the same diseases as they occur in man. Whilst in the lower animals bacteria are to be found in the blood of the living animal, in man they are always absent during life. With such facts as these before us, and others previously referred to concerning the absence and presence of organisms in blister-fluid from different individuals, it need not excite much surprise if we find that organisms are to be found in the blood of persons suffering from one or more of these specific contagious fevers.

There are, however, three other diseases of this class in which organisms, though absent from the blood, are to be met with in those parts of the body which are severally the special seats of morbid change. The three diseases are—vaccinia, ovine small-pox (which seems to be altogether similar to the disease occurring in man), and typhoid fever.

That the organisms of the vaccine vesicle have any significance other than from being possible instances of heterogenesis or archebiosis, I find it difficult to believe. Even if the contagious property of the fluid be resident in some of its particles, as the observations of M. Chauveau and Dr. Sanderson seem to prove, still such particles may exist and yet not be the independent organisms existing in the same fluid. The fact that as the organisms increase in the fluid with age the virus loses its intensity, and the fact that it may remain potent even after prolonged periods of desiccation, are both of them strikingly opposed to the notion that the living organisms of the fluid are its active elements in a specific sense. On the other hand, it does appear, from the experiments of the late Dr. Henry, of Manchester, that vaccine virus loses its intensity when subjected to a temperature of 140° F.

In ovine small-pox we have, as Dr. Klein's very interesting researches have shown,* a local appearance and active growth of

* 'Proceedings of the Royal Society,' No. 153, 1874.

organisms taking place in the skin in connection with its characteristic pustules; whilst in typhoid fever we have also an active growth of rather different organisms in the substance of the ileum, and more especially in the tissues constituting Peyer's patches—that is, in connection with the anatomical marks of this disease.* But just as a mere chemical irritant (ammonia) injected beneath the skin of a rabbit produces, as Dr. Sanderson tells us, a local inflammation in which the fluids effused swarm with bacteria, why may not the morbid processes taking place within the skin in small-pox engender irritants which may lead to the appearance of somewhat similar products? So that, in the face of the evidence already detailed concerning the occurrence of heterogenesis, the presence of organisms in connection with small-pox lesions may be readily accounted for, without the necessity of attaching any very important *rôle* to them. And as regards the presence of organisms in Peyer's patches and adjacent parts, in cases of typhoid fever, no greater importance could be accorded to this association by any but enthusiastic germ theorists. For, even if the reasons above alluded to were not very influential with them, there is another mode of looking at the matter, from quite an orthodox point of view, which would equally assign to the local development of organisms a very subordinate *rôle.* Morbid tissues are generally admitted to form a favourable nidus for fungoid growths, and the intestine is known to contain the germs or spores of such bodies. The flourishing growth of leptothrix and fungi in the diseased mucous membrane may therefore be only another example of an already well-known class of effects; so that, looking at the question from all sides, it seems to me, in the present state of our knowledge, to be extremely improbable that these newly discovered organisms have any causal relationship to typhoid fever.

It only remains for me now to make a few very brief concluding observations concerning—(1) Pasteur's recent important modifications of his germ theory of fermentation; (2) upon the degree of relationship existing between fermentation and zymosis; and (3) as to the probable mode of action of ferments and contagia.

1. Pasteur has within the last two years made a most important modification in his theory of fermentation.† Whilst he formerly held that fermentations and putrefactions are chemical processes initiated by independent organisms (bacteria and their allies), and taking place in correlation with their growth and multiplication, he has of late shown that similar phenomena may be initiated by the chemical processes taking place in the tissue-elements of certain fruits and vegetable tissues, when these are placed under certain

* 'British Medical Journal,' December 5, 1874.

† 'Compt. Rend.,' 1873–4.

abnormal conditions. Grapes, for instance, suspended in an atmosphere of carbonic acid will undergo fermentation, so as to generate alcohol and other products, even without the presence of torulæ or allied organisms. Other fruits and vegetables treated in the same way behave more or less similarly. Organic multiplication of independent organisms has therefore now been shown by Pasteur himself and his followers, not to be an essential factor in the process of fermentation. With this admission I believe it will be found impossible hereafter consistently to entertain an exclusively "vital" theory of fermentation, and equally impossible to resist accepting the broader physico-chemical theory, and with it the almost inseparable correlative doctrines of archebiosis and heterogenesis.

M. Pasteur, in fact, now proves that fermentation takes place under the influence of altered chemical (nutritive) processes taking place in unhealthy vegetal tissue, just as we know that similar processes may be initiated under the influence of a physico-chemical process brought about by finely divided platinum. As Döbereiner pointed out, this material "has the power—and many organic substances have a similar power—of absorbing oxygen from the air, and bringing it into a condition in which it can unite with other substances with which it would not otherwise enter into combination at low temperatures."*

And MM. Lechartier and Bellamy, following up the recent experiments of Pasteur, have found † that in these modified processes of fermentation, taking place in vegetal tissues, independent organisms, though they are usually absent at first, not unfrequently make their appearance after a time. In the process as it occurs in beet-root and in the potato, on the other hand, bacteria habitually spring into existence or reveal themselves in great abundance soon after the commencement of a well-marked process of fermentation. M. Pasteur will, I suspect, find it difficult consistently to account for these facts without admitting his long-postponed acceptance of doctrines of "spontaneous generation."

2. Respecting the degree of relationship existing between fermentations and zymotic processes, something more definite may now be said. Between the ordinary previously known forms of fermentation and zymosis, a most fundamental difference exists which has hitherto been far too much lost sight of. It is this: Whereas in an ordinary fermenting fluid the changes initiated by a ferment take place in a mere isolated mixture of organic substances, in zymotic processes the changes initiated by contagium occur in the fluids and tissues of a complex living body. That this latter fact does exercise a very important influence, and that the two

* 'Beginnings of Life,' vol. i. p. 409.

† 'Compt. Rend.,' November 2, 1874.

processes are not so similar as they have been supposed, we may now more readily recognize since the processes of fermentation occurring in vegetal tissues have been investigated. The relationship existing between zymosis and these modified processes of fermentation taking place in fruits and tubers seems, indeed, far more close than that between the zymotic processes in animals and ordinary kinds of fermentation.

In the process occurring in vegetal tissues, as well as in those morbid processes which take place in the animal organism, the presence of rapidly multiplying independent organisms is an occasional rather than a necessary feature. Though usually absent in other allied processes, yet we find organisms invariably manifest themselves throughout the tissues of beet-root and of the potato, when these are placed under certain abnormal ("unhygienic") conditions. And though usually absent from the blood of persons suffering from specific contagious fevers, yet do we also find organisms invariably showing themselves in the blood of persons suffering from some of them, such as relapsing fever and splenic fever. Nay, further, under the influence of a "change of conditions" alone we may initiate these modified fermentative processes in vegetables; that is to say, in ordinary parlance, the processes may originate "spontaneously" or *de novo.* But if the modified activity of tissue-elements suffices to initiate such morbid processes in the vegetal organism, why may it not occasionally do the same in the animal organism? This is a point of view which seems too valuable to be lost sight of, more especially in the face of the results yielded by our flask experiments.

3. In conclusion, I would maintain that the facts already known abundantly suffice to displace the narrow and exclusive vital theory, and to re-establish a broader physico-chemical theory of fermentation.

Whether the "ferment" in any given case be an independent living organism, a tissue-element, a fragment of not-living organic matter, or some mere physico-chemical influence (as in the case of the action of finely divided platinum), the initiated fermentative change is in each case a result of chemical action. And similarly with regard to "contagium," whether it be an altered though living tissue-element, a fragment of dead organic matter, a chemical compound (or even the more vague influence of a "set of conditions" which may suffice to generate contagium *de novo*), we have in each case to do with gradually initiated chemical changes, distinctive in kind and gradually terminating in one or other of the recognized varieties of zymotic affections. The changes in each case where we happen to have to do with living ferments or living contagia would be due only to an infinitesimal extent to the organic multiplication of such living units, though the decompositions set up by

them in their respective fluids may be such as to lead to the formation of a continuous new birth of independent organisms, all of which exhibit most active powers of multiplication. The organisms produced in such cases are therefore only to an infinitesimal extent lineal descendants of the original living ferments or contagia under whose influence such fermentative or zymotic processes were originally established.*

Thus it would appear that the original notion borrowed from the vital theory of fermentation, that all the organisms met with in a fermenting mixture are in the strict sense of the term lineal descendants of those originally introduced as ferments, would disappear with the vital theory itself. Yet this has been the notion upon which upholders of the germ theory of disease have always relied so confidently in explanation of the mode of increase of contagium within the body.

Looking, however, at this question from our new point of view, may we not say that chemical changes established in some one tissue, or in many, may, by dint of altered blood and other secondary processes, spread so as to be initiated also in previously sound parts; and that thus throughout the body, or in some special regions of it, living tissue endowed with peculiar and poisonous properties, or complex alkaloidal compounds, may be engendered in enormous quantities, some of which may be thrown off from this or that surface, and act after the fashion of "contagia" generally?

* 'Beginnings of Life,' vol. ii. p. 361.

PROGRESS OF MICROSCOPICAL SCIENCE.

Atmospheric Dust.—The 'Academy' (May 8) says that a microscopical examination of atmospheric dust which fell in parts of Sweden and Norway on the night of March 29–30, 1875, has led M. Daubrée to believe that it proceeded from a volcanic eruption in Iceland, as it closely resembled the pumice powder from that country, and especially that of Hrafftnurhur. M. Nordenskiöld, telegraphing from Stockholm, said: "Grey vitreous and fibrous powder fell here with snow on March 30: several grammes collected." M. Kjerulf sent to M. Daubrée a specimen of the same dust collected from the snow by Dr. Kars, between Söndmöre and the valley of Romsdal in the west, and Tryssil, in the direction of Stockholm, in the east. The dust was found to be composed of fragmentary transparent grains, some colourless, others more or less brownish yellow. Most of them were finely striated, fibrous, and full of vesicles, round or elongated, the latter being most common. Few of these reached the dimension of $\frac{2}{10}$ millimeter in length, and many were only from $\frac{1}{200}$ to $\frac{1}{300}$ millimeter. M. Daubrée also recognized minute crystals of pyroxene and felspar. He reminded the French Academy of several instances of dust being conveyed by air-currents to great distances. Thus in February, 1863, sand, apparently from Sahara, fell in the western parts of the Canaries, transported 32 myriameters; and more recently, ashes from the Chicago fire reached the Azores in four days, accompanied with an empyreumatic odour, which made the inhabitants suppose that a great forest was in conflagration. In 1783 the dry fog, which covered most of Europe for three months, was occasioned by dust from an Iceland eruption.

The Functions of the Frontal Ganglion in Dytiscus.—The functions of the frontal ganglion of *Dytiscus marginalis* are elucidated by M. E. Faivre in a paper which will be found in 'Comptes Rendus' (May 31, 1875). After detailing a variety of experiments, he states, as a result of his researches, that "the frontal ganglion specially presides over the movements of deglutition, determining not only the contraction, but also the dilatation of the pharyngeal sphincter, while it reacts at the same time by the recurrent nerve on the cardiac sphincter. The action of this nervous centre may be excited by impressions from back to front or the opposite. It associates together by means of its connection with the encephalon, acts of prehension, mastication, pharyngeal deglutition, and ingestion of food to the stomachs and the intestine. The subœsophagal ganglion is the centre, under the influence of which it reacts with the most energy. In fine, the frontal ganglion, distinguished by special functions from all other nervous centres of the ganglionic chain, is allied to them by its essential properties, and, as we may be assured, by its structure also."—See *Academy*, June 12.

The Development of the Nervous System in Limulus.—A very valuable paper on this subject was read by Mr. A. S. Packard, jun., before the

National Academy of Sciences (U.S.A.), and appears in the 'American Naturalist' (July, 1875). Mr. Packard observes: After a good many unsuccessful attempts at discovering the first indications of the nervous system in the embryo of Limulus, I at length, in making fine sections, with the aid of the skill of Prof. T. D. Biscoe, discovered it in a transverse section of an embryo in an early stage of development, corresponding to that figured on plate iv., fig. 10, of my essay on the Development of *Limulus Polyphemus*, in the 'Memoirs of the Boston Society of Natural History.' The period at which it was first observable was posterior to the first blastodermic moult, and before the appearance of the rudiments of the limbs. The primitive band now surrounds the yolk, being much thicker on one side of the egg than the other, the limbs budding out from this disk-like thickened portion which represents the outer or nervous layer of the germ. At the time the nervous cord was observed it was entirely differentiated from the nervous layer proper, and in section and relation to the nervous layer appeared much as in Kowalevsky's figure (33) of the germ of Hydrophilus.*

At a later stage in the embryo (represented by pl. v., fig. 16, in my memoir), at a period when the body is divided into a head and abdomen, and the limbs are longer than before, by a series of sections parallel with the under surface of the body, I could make out quite satisfactorily the general form of the main nervous cord. It then forms a broad thick mass, the two cords being united with small holes between the cords opposite the sutures between the segments, and situated between the primitive ganglionic centres. The nervous cord, as in the Acarina, is formed long before the other internal systems of organs; the development of the dorsal vessel some time after succeeding that of the nervous cord, while the alimentary canal is not formed until some time after the larva is hatched.

The next stage observed, and one of exceeding interest, was studied in longitudinal sections of the larval Limulus. If we make a longitudinal section of the young king crab when a little over an inch long, the disposition of the cephalothoracic portion of the cord is exactly as in the full-grown individuals. The nervous ganglia are then united into a continuous nervous collar surrounding the œsophagus, no ganglionic enlargements being observed, the collar in fact consisting entirely of ganglia, the commissures being obsolete; in front of the œsophagus and in the same plane as the œsophageal collar lies the supraœsophageal ganglion, or so-called brain; not as usual in the normal crustacea, raised above the mouth into the roof of the head. On the contrary, the œsophagus passes behind the brain and through the collar at a right angle to the plane of the œsophageal collar and brain taken collectively. Now a section of the larva before moulting shows a most important and interesting difference as regards the ganglia which supply nerves to the appendages of the cephalothorax. These are entirely separate, the spaces between them, where they are connected by commissures, being as wide as

* 'Embryologische Studien an Würmen und Arthropoden,' 1871.

the ganglia themselves are thick. Five ganglia were observed corresponding to five anterior pairs of members. It is probably not until after the first moult at least that the adult form of the nervous cord is attained.

Some interesting questions in the morphology of Limulus arise in connection with this discovery of the original separation of the ganglia of the head, which I will simply touch upon. The brain of Limulus differs remarkably from that of the normal crustacea, i. e. the Decapods, in sending off no antennal nerves, but only two pairs of optic nerves, there being in fact in Limulus no antennæ. In the spiders and scorpions the disposition of the nervous system only resembles that of Limulus in the thoracic and cephalic ganglia being somewhat consolidated, but the brain is situated far above the plane of the thoracic mass, and the commissures are very long, and here also there are no antennal nerves, no antennæ being present, but a pair of nerves is distributed to the mandibles. The general analogy in the form of the anterior portion of the nervous cord to the Arachnidan, by no means proves satisfactorily to my mind that the Limulus and Merostomata generally are Arachnida, as some authors insist, for, besides the remarkable difference in the form and position of the supraœsophageal ganglion above mentioned, there are other differences of much importance, which separate the Merostomata from both the Arachnida on the one hand, and the Crustacea on the other.

It will now be a matter of interest to study the development of the nervous cord in the Arachnida, at the stage where the cephalothoracic ganglia are separate, and compare them with the same stage in Limulus.

The result may possibly show that the appendages of the anterior region of Limulus are in fact cephalic appendages or mandibles and maxillæ or maxillipeds, and in part truly thoracic; as in the spiders and scorpions the nerves to the maxillæ and legs are distributed from a common cephalothoracic mass of concentrated ganglia.

The Siliceo-Fibrous Sponges.—A valuable paper has been read on this subject before the Zoological Society of London, by Dr. J. S. Bowerbank, F.R.S. It is styled, "A Monograph of the Siliceo-Fibrous Sponges," part iii., being the third of a series of memoirs on this class of sponges. A second communication from Dr. Bowerbank to the Zoological Society contained the seventh part of his contributions to a General History of the Spongiadæ.

The Spermatozoa of the Petromyzon have been investigated by Mr. G. Gulliver, F.R.S., who has recently read a paper on this subject before the Zoological Society of London.

Structure of the Volcanic Dust of Barbadoes.—The structure of this dust, which fell in such quantities in the island of Barbadoes in 1812, has been examined by Professor Hull, F.R.S., who gives the following account in the 'Geological Magazine' (June, 1875): With an objective power of fifty-five diameters, the dust is seen to consist of angular, or subangular grains of a translucent reticulated mineral, amongst which

are dispersed black particles, sometimes angular, and a very few others of a rounded form and bronze colour. On examining the translucent grains with the polariscope, and under several different magnifying powers, it became evident they consist of felspar. The structure is reticulated and in a very few cases banded; but owing to the irregularity of the forms of the grains, I was unable to determine to which class of the felspars they are referable. My impression is that they are the dust of sanidine, and of a small proportion of plagioclase; such, in fact, as would result from the pounding up of trachyte. The black grains are those of magnetite, and on placing a small magnet near the dust, a movement is immediately observed amongst the grains, which increases in intensity as the magnet approaches contact.

The Law of Embryonic Development in Animals and Plants.—We take from the last number of the 'American Naturalist' (July, 1875) an important letter, in which Mr. C. R. Dryer, of Ontario Co., N.Y., objects in very strong terms to the views expressed in a paper published by that journal (May, 1875). He says that the paper in question opens with the startling proposition that "it is a well-known law in the animal kingdom, that the young or embryonic state of the higher orders of animals resemble (*sic*) the full-grown animals of the lower orders." If such a law had ever been discovered to exist, the tadpole and the caterpillar, which are cited in proof, would certainly be good illustrations of it. But this statement is so far from being "a well-known law," or "one of the causes of the recent rapid progress in the study of the animal kingdom," that no eminent living naturalist or biologist recognizes the existence of such a law; or at least no one of them gives a hint of it in his writings. Agassiz claimed that ancient animals resembled the embryos of recent animals of the same class, and that the geological succession of extinct forms is parallel with the embryological development of existing forms. But if this principle be true, it is far from meeting the requirements of the "law" of this article. The writer of it may have had in his mind a vague idea of the law of Von Baër, which *is* well known, and which *has* enabled naturalists "to correct their systems of classification," viz. "That, in its earliest stages, every organism has the greatest number of characters in common with all other organisms, *in their earliest stages*." Or, to put it in language parallel to that of the "law" of this article, false syntax excepted, the embryonic state of the higher orders of animals resembles *the embryonic* (not the full-grown) *state* of the lower orders. The germ of a human being differs in no visible respect from the germ of every animal and plant: it never resembles any full-grown animal or plant. It successively loses its resemblance to vegetable embryos, then to all embryos but those of Vertebrates, then to all but those of Mammals. Finally, it resembles only the embryos of its own order, Primates; and at birth the infant is like the infants of all human races. But never at any period of its successive differentiations does it resemble the *adult form* of fish, reptile, bird, beast, or monkey.

The principle stated is not a law of the animal kingdom. If it be a law at all, it is a newly discovered one, and applies only to the vegetable kingdom.

The proposition to be established then is, that the young or embryonic state of the higher orders of plants resembles the full-grown plants of the lower orders. The writer finds his first proof in a comparison of the fovillæ of a pollen grain with full-grown Desmidiæ. The points of resemblance are these: both are minute; each consists of a single cell; and both have an apparently aimless motion. Surely, these resemblances are not numerous or striking enough to found a law upon; and if they were, they have not the remotest bearing upon the supposed law. Admitting that the fovillæ "may be regarded as one of the first steps towards the reproduction of plants of the highest type," yet they are not in any sense a *young or embryonic form* of a plant. The fovillæ constitute the male element, and are homologous, not to the *embryo*, but to the *spermatozoa* of animals. The supposed analogy between a Protococcus and a pollen grain is open to the same criticism. Nor is the correspondence between a full-grown Botrydium and a pollen tube of greater value. A pollen tube cannot, in any legitimate sense, be called embryonic. The superficial resemblance of a mould fungus to a stamen is obvious enough; but in reality no analogy can exist between them. The spores of the mould are embryos, and will develop, under favourable circumstances, into mould again. But pollen grains are not embryos, and never, under any circumstances, grow into what, by any stretch of terms, can be called a new plant. Neither stamens nor pollen constitute a part of the embryo; and no analogy drawn from them can have any bearing upon the laws of embryonic development. If such a law as the writer claims really exist, it must be found by studying the development of the *ovule*, the true homologue of the animal embryo. In view of such facts, all "similar analogies" and all similar "proofs of the unity of design of the Creator" may be easily dispensed with.

The article proposes to extend the domain of a certain supposed law of the animal kingdom, so as to include the vegetable kingdom also. It has been shown, first, that no such law exists in the animal kingdom; second, that not a single fact cited as proving it to be a law of the vegetable kingdom has the remotest bearing upon the question. If such hasty conclusions as these, wildly jumped at from no data, are to be allowed under the name of Science, her students will richly deserve all the ridicule and sarcasm which a certain class are so fond of pouring upon them.

The Structure of Connective Tissue.—Dr. G. Thin has read a paper on this subject before the Royal Society, of which the following is an abstract:* Transparent animal tissues, when sealed up fresh in aqueous humour or blood-serum, by running Brunswick black round the edge of the cover-glass, undergo a series of slow changes, by which, generally within a period of two to five days, anatomical elements mostly otherwise invisible become distinct. The paper is chiefly a record of observations made by this method. The author shows:

1. After a horizontal section of the cornea has been sealed up for about twenty-four hours, the stellate branched cells are seen to

* 'Proceedings of the Royal Society,' No. 158.

consist of a mass of protoplasma, sharply defined on every side, except where it is continuous for a scarcely perceptible distance into the processes. The nucleus is flattened. The processes become very fine, glistening, and thread-like almost immediately after leaving the cell, and, by dividing and anastomosing with the processes of other cells, form a rich and very delicate network.

2. It sometimes happens, although only in rare instances, that, in gold preparations, fine dark lines extend between the nuclei, and correspond in outline and course with the processes seen in the aqueous humour; and it is then evident that they are surrounded by the dark-coloured tracts which form the ordinary network seen in gold preparations, and which correspond, in outline and varying degree of development in different animals, ages, and pathological conditions, with the corneal spaces.

3. Similar appearances to those described in paragraph 1 are seen in sections of cornea which have been five to ten days sealed up in a 10 per cent. solution of common salt.

4. The quadrangular and long narrow flat cells shown by the author to exist in the cornea by means of a saturated solution of potash, are also rendered visible by the above method. They are best seen in oblique sections, from which, after two to five days, they fall out singly and in rows. A row of the long narrow cells is often seen to terminate in quadrangular cells at either end. These cells have a perfectly hyaline appearance; their nucleus has a very faint yellowish tinge, and projects beyond the surface of the cell.

In exceptional instances, in the uncut cornea of the frog, the long flat cells may be seen, after several days' maceration, lying on the primary bundles.

5. In tendon, flat masses of cells are found, on the third to fifth day, lying on the edge of the preparation and free in the fluid. The cells are accurately fitted to each other, after the manner of an epithelium. In the tendo Achillis of the frog they are seen of three sizes: (*a*) large cells, corresponding to the flat cells seen on the surface by nitrate of silver; (*b*) smaller quadrangular cells, similar in size to those described by Ranvier, and which have been described by the author as investing the secondary and tertiary bundles in double layers; and (*c*) long, narrow, flat cells, similar to those described by the author as being isolable by potash, and as covering the primary bundles.

The masses of the cells of the surface, and of the secondary and tertiary bundles, can be usually seen to consist of a double layer separated by a very thin transparent medium.

6. The perimysium and neurilemma are respectively represented by a double layer of quadrangular and hexagonal cells, identical in general appearance with an epithelium. Between the two layers there is a thin transparent medium.

7. From the neurilemma of the sciatic nerve of the frog, when cut in narrow longitudinal strips, after a few hours, branched cells of different types of form are seen isolated in the fluid near the cut edges. These cells are of two well-marked general types. In one

a small smooth-contoured elongated mass of protoplasma is continuous at both ends with a fine long thread-like fibre; in another an irregularly contoured, but generally somewhat elongated, mass gives off numerous sharply defined, very fine glistening fibres in all directions. Sometimes a protoplasmic centre terminates at one end by a single fibre, and by two at the other. These fibres are often of great length, and the protoplasmic mass can sometimes only be found by carefully tracing them whilst moving the object-glass.

8. Fibrillary tissue is seen to be composed of uniform flat ribbon-like bands, whose breadth approaches the diameter of a human red blood-corpuscle. These are seen in their simplest form when extruded from the neurilemma of the sciatic nerve of the frog, which takes place within twenty-four hours' maceration. From their position in this membrane they form part of the transparent medium which exists between the two layers of quadrangular cells. They are mostly marked by a puckered appearance transversely.

In skin and tendon, after a few days' maceration in the sealed fluid, the fibrillary tissue is seen to be composed of extremely fine but sharply contoured fibrillæ, arranged in parallel bands, which are of the same breadth as the soft ribbon-like bands which are isolable from the neurilemma.

The respective appearances in the neurilemma and in tendon indicate extremes in the condition of this tissue, and represent, according to the author, primary bundles of connective tissue.

9. The primary bundles of the cornea are seen only exceptionally by this method, but can be demonstrated with great precision by sealing up a frog's cornea in a mixture of equal parts of half per cent. solution of chloride of gold and concentrated acetic acid.

10. In nerve-bundles, after twenty-four hours' maceration in aqueous humour, some of the medullated fibres may be seen to have their contour broken transversely by straight hyaline spaces. The author assigns this appearance to the peculiarity of structure described by Ranvier.

11. The breadth and appearance of the rods of the frog's retina are nearly identical with those of the primary bundles of neurilemma.

The transverse markings described by Max Schultze as being produced by the action of osmic acid on the rods and cones, resemble the transverse puckerings in the primary bundles. In both rods and primary bundles, after prolonged maceration in aqueous humour, the free ends of each individual element bend in one direction until they join, and the substance of the ring thus formed undergoes in both a similar and peculiar process of disintegration. From these facts the author infers that the rods and cones of the retina are composed of fibrillary tissue in its simplest form.

12. Transverse sections of muscular fibre, when examined at intervals, show varying appearances, only a small minority of such preparations being successful. Successful preparations show one or more of three appearances; (*a*) primary bundles, corresponding to Cohnheim's fields; (*b*) groups of these (secondary bundles), the aggregate of which fill up the space bounded by the sarcolemma;

(*c*) a threadwork of fine fibres surrounding the primary bundles, in meshes.

13. Examination of connective tissue, in various stages of inflammation, yields strongly confirmatory evidence in favour of the interpretation given by the author to the appearances above described.

Lymphatics of the Choroid and Retina.—The 'Lancet' (July 10) says that this subject has been recently investigated by Morano. He last year announced the discovery of minute perivascular lymphatic channels in the choroid, communicating with the perisclerotic space, and penetrating to the chorio-capillaris; and he has now ascertained the presence of stomata in the pigment layer of the retina (formerly called "choroidal epithelium" similar to the stomata described by Recklinghausen in the serous membranes. These retinal stomata are best marked in the frog, and are mostly formed by the separation of three or four of the epithelial cells; they present internally a curious valvular arrangement due to the presence of three or more small processes from the sides of the orifice, and covered by a delicate endothelium. In mammals they are less constant, and may be destitute of valves, if present.

NOTES AND MEMORANDA.

Preparing Sections of Coal.—Dr. C. Johnson gives the following as the method adopted by him in preparing sections of coal, in the 'Cincinnati Medical News' (July, 1875):—1. Macerate suitable pieces, $\frac{1}{4}$ or $\frac{1}{2}$ inch thick, in liquor potassa until they swell and soften. 2. Soak for a few hours in pure water, and drain. 3. Macerate in nitric acid until the colour changes from black to brown. 4. Soak for a few hours in water, and drain. 5. Put aside in alcohol for a day or two. If for future use, let the pieces remain in alcohol. 6. Fasten in a cutter with paraffin, and make sections. 7. Place in absolute alcohol. 8. In oil of cloves. 9. In balsam; and 10. Set aside with a small weight or cover, having, before the mounting, attached the label.

A Mode of Counting the White and Red Blood-corpuscles.—The 'Medical Times and Gazette' (August 14) gives the following account of M. Malassez's mode of counting the number of corpuscles in blood: "He draws the blood into a peculiar kind of pipette, in which he dilutes it with a hundred parts of an indifferent liquid, so that its constituents may be as equally distributed as possible. The liquid he uses consists of one volume of a solution of gum of specific gravity 1020, and three volumes of a solution containing equal parts of sodic sulphate and sodium chloride, also of specific gravity 1020, to which one drop of a concentrated solution of sodic carbonate may be added. The diluted blood is transferred from the pipette into a fine capillary tube of elliptical section, whose dimensions are accurately known; this is brought directly under the object-glass of the

microscope, and the corpuscles are then counted by means of a micrometer eye-piece. In counting the white corpuscles the blood is only diluted fifty times instead of a hundred, and a longer capillary tube is used than in the case of the red corpuscles. To count the latter it is not necessary to use a tube longer than twice the breadth of the field of the microscope, whereas for the former it is better to use one about ten times as long. The number of corpuscles thus counted gives what the author calls 'the actual richness of the blood,' i.e. the number of blood-cells in a unit of volume. To determine the 'relative richness' in the two kinds of corpuscles for any particular blood, their number must be estimated separately, and then their relative percentage calculated. M. Malassez has already obtained the following results by his method. He finds that while the number of red blood-cells is uniform throughout the arterial system, in the venous it varies with the kind of organ from which the blood comes. Thus there is an increase in their number in the veins of the skin and muscles, especially during muscular action. There is also an increase in the blood from the secreting glands; but, curiously enough, this is most pronounced, not during secretion, but when the organ is at rest. The red corpuscles in the splenic vein are most numerous when digestion is going on; whereas, in the intestinal veins, they are diminished at that time, and increased during fasting."

CORRESPONDENCE.

Von Baër's and Mr. Badcock's B. polymorphus.

To the Editor of the 'Monthly Microscopical Journal.'

3, Queen Street Place, Upper Thames Street,
London, *July* 30, 1875.

Sir,—In answer to Mr. Garner's letter of July 10, 1875, in your August number, I beg leave to say that, in the discussion to which he refers, the "doubt" was not as to the *Bucephalus polymorphus* of Von Baër being "parasitical," because it was well known that the creature so named by him was found in *that state only*, and had been *so* found by other observers since. *There had been a doubt* previously expressed (see April number of Journal) as to whether the creature I had found, and which was the subject of discussion at the time alluded to by Mr. Garner, was really *identical* with Baër's Bucephalus, and this doubt begat another, viz. as to whether it was *parasitical* or not. I had not found it as a *parasite*, but always *free;* and this perfectly *free* state, combined with certain structural and functional differences in comparison with Von Baër's *Bucephalus polymorphus*, pointed to the bare possibility of its being another species or variety. In addition to other differences already pointed out by me to the Society, I may here mention that a few weeks since I found the creatures with two additional balls, in size and shape exactly like

the two ordinary ones at the base; but these extra ones were at the extreme tip of the long arms or appendages. Seen thus with four balls, they were extremely curious.

Will Mr. Garner kindly inform us if he ever found the Bucephalus in a *free* state, and with the characteristic quick movements which those I found possessed?

Your obedient servant,

JOHN BADCOCK.

ON MR. SLACK'S REPLY TO MR. J. MAYALL, JUN.

To the Editor of the 'Monthly Microscopical Journal.'

224, REGENT STREET, LONDON, *August* 2, 1875.

SIR,—As you have allowed Mr. Slack to make criticisms on my letter (before its publication), perhaps you will permit me to make a few observations in reply to him?

Mr. Slack is dissatisfied with my interpretation of his paper on "Angle of Aperture, &c.": I was neither so simple nor so sanguine as to expect otherwise. I willingly admit that my notions of clearness of thought and accuracy of language are often opposed to his. But I had no wish to misrepresent him. As I read his paper, its general tendency—whether avowed or not—was to make the unwary believe that an optician had come into the field with new lenses of wondrous excellence. He now says the question raised by him "related to the smallest apertures capable of showing lined objects." Well, the new lenses are supposed to be the practical embodiment of *the smallest apertures capable of showing lined objects;* to criticise them by their results was therefore not irrelevant to the question at issue—but the contrary.

He now points to his exhibition of *Surirella gemma*, with Zeiss's D ($\frac{1}{6}$th) lens at the recent Scientific Evening of the Royal Microscopical Society, in confirmation of his views. I refrained from commenting on that exhibition, thinking, after his apology for its failure on the evening, the least said about it the better. But, since he again insists on referring to the exhibition, I venture to inform those of your readers who did not witness it, that, compared with Dr. Woodward's photograph of *Surirella gemma* (which may be accepted as the finest definition hitherto obtained of it), Mr. Slack's image was only recognizable by the general contours—not by the superficial definition. It was the old story—Amplification *versus* Definition—and he seems to prefer the former.

He says Zeiss's lenses have taught him a lesson on the utility of small apertures. The information he seems to have received from their examination has been commonly accessible for at least forty years past: for example:—In vol. ii. of the Natural Philosophy published in 1832 by the Society for the Diffusion of Useful Knowledge, p. 50 of the Treatise on Optical Instruments by Sir David Brewster, we find the lesson summarized:

"It should be remarked, that when the objects are used as *opaque,*

a smaller aperture will do best, viz. about $\frac{2}{5}$ of its focus, (for any power less than 300 *decimal standard*,) but the magnifier requires to be more free from aberrations."

Thus far Mr. Slack has seemingly got with the help of the Reflex Illuminator.

Sir D. Brewster then presages the most modern developments of the microscope: "For transparent objects a larger aperture is absolutely necessary; and for some *tests* it should be equal to its focal distance, to show the cross *striæ* between the lines on many of the scales, when the power of the instrument or lens is considerable."

The finest English, French, and American lenses are practical examples of the truth of this statement.

Sir D. Brewster then says: "It is worthy of remark, that the same aperture that with advantage will develope one class of objects will not show another with the same success."

This is evidence that there is room for both large and small-angled lenses.

Against this we have Professor Abbe quoted: "that we should look for improvement in the direction of making objectives of 3 or 4 millimètres focus do the work now done by higher powers." I maintain in opposition to Mr. Slack that Zeiss's lenses made "under the direction of Professor Abbe" do not substantiate his position.

High-power microscopy has been hitherto very little indebted to reasonings *a priori*, from the practical impossibility of working exactly to formulæ; but the lenses, when made, can be put to the test of comparison: this is what I did in a series of trials which I endeavoured to make exhaustive, and I placed the statement of them as evidence for the consideration of your readers.

That Mr. Slack should seize upon Professor Abbe's view of the question does not surprise me; it is novel, even if it be not susceptible of practical proof. In these days of eager competition, novelty is at a premium.

I am, Sir, your obedient servant,

JOHN MAYALL, jun.

To the Editor of the 'Monthly Microscopical Journal.'

ASHDOWN COTTAGE, FOREST ROW, *August* 8, 1875.

SIR,—I am again obliged by your giving me an opportunity of preventing a month's misconception arising from Mr. Mayall's inaccuracy.

The statement regarding *Surirella gemma*, as shown by me at the Scientific Evening referred to, does not correspond with facts. Numerous Fellows saw the markings distinctly divided into beads, as in Dr. Woodward's photograph. My apology related to loss of time in getting the illuminating apparatus into fair action, and to not obtaining quite enough light for the D eye-piece. A public room and a tremulous floor are not good conditions for doing, or seeing, anything difficult. Several well-known microscopists have seen the object

perfectly at my house with the objective in question, angle 68°. I find the high opinion I expressed of Zeiss' glasses abundantly confirmed by other observers; and Messrs. Powell and Lealand have just proved the correctness of Professor Abbe's view by making a $\frac{1}{6}$th, or, more properly, a $\frac{1}{7}$th (for Mr. de Souza Guimaraens, and kindly lent to me by him for trial), which with a moderate angle reconciles in a remarkable manner the three demands for great working distance, penetration, and resolution. I find the angle, when the objective is corrected for pretty thick covering glass, about 105°. This glass, while fit for looking right into a macerated potato leaf for fungi, displays difficult diatoms very beautifully. I said that when our opticians were asked to do so, they would make small angles preserve their own special merits, and do what disproportionately large ones have hitherto been required for.

I remain, Sir, your obedient servant,

HENRY J. SLACK.

ANGLE OF APERTURE, CHROMATIC AND SPHERICAL ABERRATION.

To the Editor of the 'Monthly Microscopical Journal.'

1, BEDFORD SQUARE, *August* 5, 1875.

SIR,—I regret to find that Mr. Slack is "unable to understand some parts of my letter" on Chromatic and Spherical Aberration. If he will do me the favour to particularize the parts he does not understand, I will endeavour to make my meaning clear. Let me assure him, however, that the quotation he gives from Ganot's 'Physics' simply confirms my statement, that spherical aberration is due to the *forms* of lenses; and when he gave his *own* version of chromatic aberration I most assuredly dissented, and now leave it to those who are familiar with the subject to decide whether they will have Mr. Slack's definition of achromatism or that of well-recognized authorities on optics, who not only assert, but prove, that the conditions of achromatism depend only on the *focal lengths* of the compound lenses.

With regard to Mr. Slack's statement that the more recently made objectives of Messrs. Powell and Lealand have more perfectly balanced corrections, this, I presume, is a well-deserved compliment to these opticians; but so far from its adding weight and strength to his assertion that "*all chromatic aberration involves spherical aberration,*" it certainly proves quite the reverse.

If proof were needed as to the value of large-angled glasses, I have but to refer Mr. Slack, and those who hold his views, to the Jurors' Report on the Microscope and its accessories, exhibited in the Great International Exhibition of 1851. He will there find a well-digested expression of the opinions of the highest authorities in favour of a new $\frac{4}{10}$th, made by Messrs. Smith and Beck under Lister's direction, angular aperture upwards of 75°, and which appears to have called forth the wonder and admiration not only of the Jurors, but of

rival opticians, who at once seized upon the improvement and immediately set to work to produce similar lenses. I may remind Mr. Slack that the best glasses of this period had only about one-half this angular aperture for the same focal length. *These are the object-glasses of the past, and such as he would now resuscitate, and call improvements.* I would further remind Mr. Slack of the statement of an authority on these matters: that the separating power, i. e. that which constitutes the real power of discovery in the ultimate structure of muscular fibre, tissue, or cell, increases with or is proportionate to the chord of the angle of aperture, at least up to 150°. Has Mr. Slack any proof to offer to the contrary? Is it that he sees *P. hippocampus* with Zeiss's $\frac{1}{4}$th with an angle of 48°? I am in possession of a $\frac{1}{4}$th by Dallmeyer, angular aperture 125°, which exhibits none of the defects Mr. Slack dilates upon as pertaining to large-angled objectives. This lens has a fair working distance for a thick cover-glass; it can be converted into an immersion lens by means of another front, giving about the same angle of aperture; with the immersion front sufficiently unscrewed the aberrations can be balanced for a dry object when the angular aperture is reduced to 90° or thereabouts, and I detect no chromatic aberration. Thus it appears to me that Mr. Dallmeyer has succeeded in placing at our disposal a large-angled objective—for both wet and dry objects, and also as a *comparatively* small-angled glass if that be desired—nearer to perfection than any objective I have seen or worked with. Nevertheless, I must confess my preference for the large-angled glass, because of its precision of focus, the *sine quâ non*, as I deem it, of perfect corrections. With this new objective of 125° I can see all that I have ever seen with any $\frac{1}{8}$th of the same angle, and I need not dilate upon the facility of using a $\frac{1}{4}$th as compared with an $\frac{1}{8}$th.

As to the assertion that a Beck's $\frac{1}{20}$th, advertised aperture 140°, only measures 128°, I would much rather place my faith in the high character of the optician and his experience in recording an exact measurement than upon the amateur performances of Mr. Slack.

I do not apprehend anything like astonishment on the part of physiologists who may read my letter. I may, however, refer those unacquainted with the work of practical histology to the 'Transactions of the Pathological Society of London,' and extending over a period of upwards of twenty-eight years. It is scarcely possible to point to more able testimony to scientific microscopy, or to more important evidence of the eagerness with which the medical profession has availed itself of the latest improvements in high-angled powers, than that contained in the twenty-five volumes of this Society. Again, turn to Continental work; take, for example, the splendid volumes of Stricker, 'Manual of Human and Comparative Histology.' The main results in high-power definition spoken of in these volumes have been produced with high-angled lenses. It is scarcely necessary to say that the greater part of the valuable pathological work of the schools has been done with high-angled objectives; for what are we to infer when such a man as Dr. J. Hughes Bennett, the eminent Professor of Pathology in the University of Edinburgh, tells us, in a lecture delivered to the

College of Surgeons of Edinburgh, January 17, 1868, "On the Atmospheric Germ Theory," "I have employed for these investigations the $\frac{1}{8}$th of Ross, and frequently a $\frac{1}{12}$th by the same maker, and the immersion No. 10 of Hartnack with varying powers of from 600 to 800 diameters linear, and I have confirmed my observations with a lens made for me by Messrs. Powell and Lealand, of $\frac{1}{25}$th of an inch focus, whereby I obtained a magnification varying from 1250 to 2000 diameters linear. My observations were made in 1841 and 1842, and carefully repeated and extended to October, 1864, and they have been since repeated by my assistant, Dr. Rutherford."

The high powers used by Dr. Beale, Dr. Bristowe, Dr. Bennett, Dr. Payne, Mr. Lister, Mr. Tomes, Dr. Burdon Sanderson, Dr. Pritchard, Mr. Lankester, Dr. Michael Foster, Dr. Klein, &c., together with a host of French and German physiologists, are, it is well known, lenses of large angular aperture; and they, in their published works, constantly write of Ross's $\frac{1}{8}$th and $\frac{1}{12}$th, of Powell and Lealand's $\frac{1}{16}$th, $\frac{1}{25}$th, and $\frac{1}{50}$th, or of Hartnack's Nos. 10, 11, and 12 immersions ($\frac{1}{15}$, $\frac{1}{18}$, and $\frac{1}{21}$), as being the lenses with which their finest work has been done.* And, lastly, I would point to the wonderful series of photographs of muscular tissue, blood-corpuscles, &c., by Dr. Woodward, of America, all of which were produced by the aid of high powers with the largest angle of aperture. Can Mr. Slack produce or indicate any work done with low-angled powers that will for a moment compare with these photographs?

I remain your obedient servant,

JABEZ HOGG.

OBSERVATIONS ON MR. SLACK'S OPPONENTS.

To the Editor of the 'Monthly Microscopical Journal.'

SIR—"Trying Zeiss's Nos. 1, 2 and 3 immersions (= $\frac{1}{8}$th, $\frac{1}{15}$th, and $\frac{1}{25}$th) by the test of deep oculars, I find . . . . the image with these comparatively low-angled objectives breaks up with any magnification beyond about 1000 diameters."

Such is the verdict of Mr. John Mayall, jun., in his observations on Mr. Slack's paper on Angular Aperture, which appeared in your last number.

Would Mr. Mayall be "surprised to hear" that the linear magnifying powers of a $\frac{1}{15}$th and $\frac{1}{25}$th are, with even a C eye-piece, 1500 and 2500 respectively, and that an $\frac{1}{8}$th has, with a No. 4 eye-piece, a power of 1440, according to Smith and Beck's catalogue.

* On the question whether an increase of angular aperture is a real advantage to the physiologist, Dr. Leopold Dippel, in his work 'Das Mikroscop und seine Anwendung,' vol. i. p. 36, says: "The result obtained by the immersion system is therefore equivalent to an increase in the angle of aperture. This arrangement is consequently a real step in advance, *especially with regard to the most difficult physiological investigations*, as the use of my Hartnack's object-glass convinces me more completely every day."

So much for Mr. Mayall's notions of the magnifying power of deep objectives.

Proceeding onward, in the same letter, Mr. Mayall naively remarks, that if Mr. Wenham will try an experiment "on *Möller's Probe-Platte* with his Reflex Illuminator and a high-angled immersion lens, he will see a *luminous* field; whereas, with a pneumo-lens he obtains a *dark* field"; he then asks the following question: "Whence comes the *luminous* field in the immersion lens if not from its having the power to collect rays which are *totally reflected* when the pneumo-lens is used?"

If Mr. Wenham replied to the question put by Mr. Mayall, he might very properly inquire what on earth "Möller's Probe-Platte" had to do with the question; and whether a plain glass slip would not answer the same purpose. The answer to Mr. Mayall's question "Whence comes the luminous field?" is obviously that there is nothing whatever in his letter to show that any rays totally reflected when the dry lens was used, were picked up by the immersion; for anything that appears to the contrary, the dry had a smaller angle than the immersion lens, which would perfectly account for the phenomenon.

Following this amusing letter by Mr. John Mayall, jun., comes one on the same subject from Mr. Jabez Hogg, in which we find a most charming proposition. After quoting from Parkinson, who says a compound object-glass can be constructed "which shall be at the same time both *aplanatic* and achromatic," Mr. Hogg says: "An object-glass may therefore be achromatic and not aplanatic," and hence he concludes that some object-glasses which are *not* achromatic must be aplanatic.

Mr. Hogg thinks Mr. Slack's theory is not coherent.

Your obedient servant,

CRITO.

ANGLE OF APERTURE OF OBJECT-GLASSES.

To the Editor of the 'Monthly Microscopical Journal.'

LONDON, *August* 10, 1875.

SIR,—I have no intention of entering into the present object-glass question as an advocate of any particular opticians. This I have always avoided, even in the case of those who are guided by my advice in optical matters. If a partisan comes forward to publish the merits of an object-glass of one maker, an opponent is sure to appear in favour of another, and the consequence is that a scientific question becomes a party one, and degenerates into a mere trade squabble.

In the last 'M. M. J.' we have a verbose letter from Mr. Mayall, assuming the calm indifference of a looker-on amused at "the prodigious display of personalities." It may, however, appear to another indifferent "looker-on" that his own letter, from its tone, may vie

with any of them in the failing that he deprecates in others,* and so the cavil, embittered by feelings of rival interest, spreads to all who use object-glasses.

My attention has been called to a letter from Mr. Stodder, appearing in the 'Cincinnati Medical News' for July last, wherein he attempts to claim pre-eminence for Mr. Tolles, and asserts that all recent improvements have been taken from him. I cannot reply in any of the American journals, particularly as the letter in question is such a manifest puff of Mr. Tolles' object-glasses, from one professedly his agent, that I am surprised at its insertion as a scientific article. Not to lose the chance that now offers, in the present small-angle controversy, Mr. Tolles is there boldly put forward as the maker of the best object-glasses in the world. It is imputed that Messrs. Powell and Lealand have based their recent improvements on the fact of having seen Mr. Tolles' four-combination $\frac{1}{6}$th (belonging to Mr. Crisp). As these gentlemen make it a rule never to answer insinuations against themselves, I venture to state that I have examined their new glass, as well as the notorious $\frac{1}{6}$th, and am in a position to say that they have not copied, and further, that they also consider this much-vaunted object-glass not a subject for imitation.

The four-system combination is claimed by Mr. Tolles as his invention, but it is no novelty. Andrew Ross made a great number of them about twenty years ago. Mr. Tolles' agent (Mr. Stodder) refers to me with his characteristic rancour as claiming arrangements belonging to others. That in which one single front works both wet and dry is not "copied from America," neither do I claim it, as many of the four-combination lenses just referred to by the late Andrew Ross having a single front, work equally well both wet and dry. The effect partly depends upon the perfection of the correction, and whether there is sufficient range in the adjustment to enable the lenses to occupy a closer position than usual.

Having shown that the four-combination system is no novelty, I must say the same of the doublet or "duplex" front now claimed by Mr. Tolles as the great improvement of his lenses. This was tried and suggested by myself years ago: I then formed a high opinion of the arrangement, and consequently described and figured it in my essay "On the Construction of Object-glasses," page 172 of the first vol. of this Journal (March, 1869), which makers of object-glasses may have noticed. I am gratified to find that my idea of its merits is confirmed by practice, and that it will find a place amongst improvements in the history of the object-glass.

The real question at issue at present, viz. the superior value of small apertures *versus* large ones on a certain class of objects, appears

* I shall not revive the controversy of the theory of dry and immersion apertures; but in referring to my "Reflex Illuminator" Mr. Mayall has simply mistaken the action as described by me. All rays are made to fall at such an angle on the top surface of the *slide* (not the cover), that they are totally reflected, and the only light that can pass through is at the contact points of the object, which adhere to that surface. The total reflexion is the same irrespective of any aperture of object-glass, and the field equally dark whether this is used with water between the front lens or not.

to me to be a very simple one, and easily decided. I will undertake to produce a series of objects that can be well shown with the small-aperture glasses, upon which the higher ones will completely fail (even the 180°); these must be left for dots and striæ on the flat and most excruciating tests.

I need not look to, or care for the maker's name, *as this has nothing to do with the question*, provided the glasses are well made and corrected, as so many of them are by different makers: thus all jealousies may be avoided. Mr. Slack's views concerning the use and value of apertures considerably less than 180° for general observation, are so obvious, and have been endorsed by eminent microscopists who have really made all the most important investigations, that I am surprised at the opposition which, however, is evidently biassed by trade feelings.

As yet English makers need have no anxiety on the score of superiority; they will not be found behind either Continental or American in the most recent improvements.

I am, Sir, yours very truly,

F. H. Wenham.

PROCEEDINGS OF SOCIETIES.

Quekett Microscopical Club.

Ordinary Meeting, June 25.—Dr. Matthews, F.R.M.S., President, in the chair.

Nominations were made of candidates to fill the offices of President, Vice-Presidents, Secretaries, and vacancies on the Committee. Dr. Matthews was proposed as President for the ensuing year.

The Secretary explained the method of using the scale for the measurement of angular aperture devised by Mr. J. W. Stephenson, who had kindly presented to the club a number of copies for the use of the members.

The Secretary described two lamps which had been patented by Mr. Parkes, of Birmingham. One had a circular and the other a flat wick, and in each case the light was placed in the focus of a parabolic reflector, throwing a beam of approximately parallel rays. There was also a tinted glass in the front of each reflector to correct the yellowness of the flame.

Mr. B. T. Lowne gave a continuation of his lecture on the Histology of the Eye, treating principally of the minute structure and functions of the retina, and giving the latest and most approved methods of hardening, cutting, and mounting sections of it for microscopical examination.

Adelaide Microscope Club, South Australia.*

The monthly meeting of the club was held on April 30. Dr. Whittell presided.

Mr. Francis showed some glasses he had succeeded in ruling with fine lines to show the effect of diffraction.

The Chairman said the subject for study that evening was "The Animal and Vegetable Parasites of the Human Body," and after giving a general summary of the facts known respecting these bodies, proceeded to show several varieties of them under the microscope. In referring to the *Microsporon furfur*, he noticed the statement of some dermatologists that it required considerable skill to demonstrate it. He had not found this to be so. If some of the scrapings from a skin affected by it be soaked in Beale's staining fluid for a few hours, the sporules will be found to be coloured, while the dead epithelial scales will be scarcely touched. In referring to Hydatids, the Chairman showed a slide containing a minute portion of the contents of a cyst in the hepatic region. There were hooklets of the echinococcus and thousands of minute ovoid bodies, which Dr. Cobbold, of London (to whom similar slides had been sent), had pronounced to be protospermial bodies, although differing somewhat from any he had before met with. The patient from whom they were obtained by tapping had ultimately made a good recovery.

The Rev. Dr. Bleasdale, a distinguished visitor from Victoria, called attention to the effect of oblique light obtained by removing the mirror of the microscope and viewing an object illumined by the direct light of a candle or lamp placed to the right or left of the observer.

A committee was appointed to arrange for the holding of a conversazione.

Microscopical Section of the Academy of Natural Sciences of Philadelphia.

February 1, 1875.—Director W. S. W. Ruschenberger, M.D., in the chair.

Dr. J. Cheston Morris, chairman of the committee appointed to examine optically the $\frac{1}{25}$th and $\frac{1}{50}$th objectives displayed at the late Exhibition of the Section, made a report, which was referred to a committee, and of which the following is an abstract:

The lenses submitted for examination were a $\frac{1}{6}$th, $\frac{1}{10}$th, and $\frac{1}{50}$th made by Tolles, all immersion, a $\frac{1}{25}$th immersion by Wales, and a $\frac{1}{50}$th, dry, made by Powell and Lealand. The points which we considered it requisite for us to examine were, 1st, flatness and clearness of field; 2nd, definition; 3rd, penetration; 4th, resolution; 5th, angle of aperture; 6th, achromatism; 7th, amplification; 8th, working focus. Penetration or depthing is the property by which a lens shows us with tolerable distinctness objects or structures lying just within or beyond the best focal point or plane, and is of the greatest importance in

* Report supplied by Dr. Whittell, Adelaide.

tissue investigations. [As a method for testing penetration (hitherto a desideratum), Dr. Morris proposes to examine a cover ground so as to be $\frac{1}{100}$th of an inch thinner on one edge than its opposite, and to measure with an eye-piece micrometer the breadth of the band of ground glass distinctly visible (flatness of field being presupposed)]. Again, resolving power is the property of showing certain lines, markings, or shadows on diatoms, &c., and may or may not coexist with best defining power; it depends in great measure upon angle of aperture, and is to a great extent an antagonistic property to penetration. The dependence of resolving power upon angle of aperture is very well shown by placing a *Pleurosigma angulatum*, for instance, under a $\frac{1}{4}$th or $\frac{1}{10}$th, with such an eye-piece as will amplify sufficiently, and putting the compound body horizontally in front of a direct light. In this position no lines will be seen, but by rotating the compound body on its axis an oblique light is obtained, which at different angles, according to the power of the objective, will bring out transverse or oblique lines, and finally dots appearing as hexagons. The following results were thus obtained:

Power employed.	Transverse Lines.	Oblique Lines.	Hexagons.	Total Obscuration.	Angle of Aperture.
	°	°	°	°	°
Spencer's $\frac{1}{2}$-inch, C eye-piece (lines beginning to show at 25°)	25	..	..	30	60
Spencer's $\frac{1}{4}$-inch, C eye-piece	20	25	50	60	120
,, $\frac{1}{8}$-inch, C eye-piece	..	0	40	65	130
Tolles' $\frac{1}{10}$th, immersion (lines broken into dots)	..	8	20	85	170
Wales' $\frac{1}{25}$th, immersion	..	..	0	70	140
Powell and Lealand's $\frac{1}{50}$th, dry	..	15	20	63	126
Tolles' $\frac{1}{50}$th, immersion	..	..	0	70	140

We found that the $\frac{1}{50}$th of Tolles gave good results as to flatness and clearness of field, penetration, resolution, amplification, and working focus or distance. Its definition is only fair, as also its working angle of aperture, while as to achromatism there is much improvement to be desired, and in working focus and general usefulness much might be gained by setting the front lens less deeply and reducing the brasswork of the face. We were, however, agreeably surprised by the facility with which it can be handled.

The $\frac{1}{50}$th of Powell and Lealand was not equal to the above in clearness of field, nor in definition, nor in working focus. Its penetration was equal, as also was its amplification, but its angle of aperture was 14° less.

The $\frac{1}{25}$th of Wales is a very superior lens, giving good definition, resolution, and penetration, while its other qualities are very fair.

The $\frac{1}{10}$th of Tolles, although constructed mainly for use with oblique light, showed itself a good lens, with direct central rays,

as to flatness and clearness of field, definition, amplification, and resolution; its angle of aperture is wonderful, while its achromatism and even its penetration are very fair, and its working focus sufficient.

From the observation noted above we deduce one very important fact, viz. that the different appearances of lines, dots, hexagons, &c., on *Pleurosigma angulatum* are not only the varied results of angle of aperture, of amplification, and of illumination, but that they may be obtained with less and less obliquity of light as we increase the power of the objective; thus making it evident that high powers with direct central light show us clearly things which we rather guessed at than saw (owing to the increased chance of spherical and chromatic aberration and distortion from the employment of oblique light) with lower ones.

We would conclude, therefore, by recommending these high-power lenses to those engaged in microscopic research, not as capable of doing all work—a 1-inch is as indispensable to a histologist as a $\frac{1}{4}$th—but as likely to be proportionately useful in unravelling the mysteries of organic life.

Dr. J. Gibbons Hunt desired it to be distinctly understood that he had nothing to do with the preparation of the report, and did not wish to be held responsible as a member of the committee for the views advanced in it. He considered that it embodied the obsolete views of Carpenter and Beale in regard to penetration, which term should be dropped from the vocabulary of microscopists. He believed that penetration and resolution can be and have been combined in the best objectives.

Dr. J. Cheston Morris stated that the report was based upon a careful and conscientious examination of the objectives by the committee, and was in accordance with the well-known laws of reflexion and refraction of light. He had submitted the report to Professor George F. Barker (of the University of Pennsylvania), who had approved it so far as the optical questions were concerned, except, perhaps, on the subject of penetration, which he attributed to imperfect spherical correction. He could have wished that Dr. Hunt had expressed his dissent from the document as freely in private, when it was shown to him, as he had just done. All he did at that time was to suggest some slight alterations and additions, which being made, Dr. Morris was led to expect his adhesion to the report. As to the question of penetration being a useless one, he considered the presence of this quality in the lenses of Tolles of great moment. High angle of aperture and penetration have not been combined in the objectives of the German, English, and French makers to the same degree.

Dr. Hunt said that what one man calls penetration another does not, terms often being used without an exact knowledge of the meaning intended to be conveyed by them. He preferred a lens that will give one absolute focus rather than three indistinct ones. The conditions of testing are frequently fallacious, and imperfect illumination is one of the most prolific sources of error. With low objectives, as, for example, a $\frac{1}{5}$th, used with the amplifier, very satisfactory and reliable results can be obtained.

In conclusion, Dr. Hunt proposed that at some time during the

next year any resident member or members of the Section should prepare one dozen microscopical preparations, extemporaneous or otherwise, representing the many different departments of microscopical work, and exhibit the same at a meeting or meetings of the Section, under his own apparatus and in his own way, the object being to test men in regard to their technological skill, he offering to do the same. Those competent may compare and judge the results.

On motion of Dr. Morris, it was resolved that the Curator be authorized to purchase for the Section a Nobert's test-plate.

San Francisco Microscopical Society.

The regular meeting of the Microscopical Society was held in its rooms, on Thursday evening, March 4, with a full attendance of members. President Ashburner in the chair. Messrs. Robert Munch, C. Troyer, and Theo. H. Hittell, were present as visitors.

Dr. W. F. McAllister, United States Navy, was elected a corresponding member.

The Secretary announced the reception of the recent edition of the 'Micrographic Dictionary,' a very valuable work for reference.

Dr. D. V. Dean, city chemist and microscopist of St. Louis, presented a copy of the Seventh Annual Report of the Board of Health of that city, which contained valuable reports of microscopic investigations of meats and parasites.

A communication from the committee appointed to receive contributions for the National Polish Museum was read, also interesting letters from Messrs. W. H. Walmsley, of Philadelphia, J. W. Deems, and Captain John H. Mortimer, of New York, corresponding members.

Dr. A. Mead Edwards, of Newark, N.J., and Mr. A. F. Dod, of Memphis, corresponding members, favoured the Society with lengthy and valuable letters containing assurances of interest and assistance in the good work, the latter gentleman accompanying his letter with a very interesting paper on Mr. Tolles' new four-system immersion $\frac{1}{10}$th objective. The notes prepared by Mr. Dod were a memorandum of the tests made by him in the way of comparison with other first-class objectives of a less angular aperture, both with central and oblique illumination, and various test-objects. His results point directly to the fact that when the low-angled objectives failed to give as satisfactory results as the $\frac{1}{10}$th, using low eye-pieces, they were utterly vanquished when increased eye-piecing was applied to amplify the image; and this too with central light. The generally received doctrine that the wide angles are only valuable for work with oblique light, would seem to be overthrown; and the conclusion of Mr. Dod and the gentleman who aided in the test was unanimous that this tenth of Tolles', with the highest attainable angle of aperture, can meet the narrow angles in the field that has been hitherto regarded as peculiarly their own, and not only successfully compete with, but actually and undoubtedly surpass them, one and all.

So much has been said and written on this subject that any addi-

tional testimony is useful and interesting to all students in microscopy; and Mr. H. C. Hyde, Vice-President of the Society, who has given the mechanical features of the microscope in their adaptability to test objects a large amount of his attention, was called upon to make some remarks on the subject. Using the black-board and accompanying his statements by the reading of a paper by J. Edwards Smith, of Ashtabula, O., published in the microscopical department of the 'Cincinnati Medical News,' Mr. Hyde was able to so explain the matter as to interest and instruct all present.

Mr. Munch exhibited some fine drawings from the microscope, of various minerals and rock sections, which were peculiarly beautiful and valuable for their accuracy and detailed finish.

Mr. Hanks exhibited a binocular microscope which he had caused to be made from a pattern of his own, and a number of which he had imported for miners and mineralogists. The want of such an instrument has long been felt, and the combination of its many features, in the way of portability, movements, and cheapness, were noted satisfactorily.

Mr. W. H. Pratt, corresponding member, donated four slides, mounted by him with the anthers and pollen of sweet elysium, pollen from the osage orange, fly's foot, and fine gold.

Dr. J. W. Winter donated two slides with objects, prepared and mounted by him in balsam, being a longitudinal and transverse section of human cuspidata, showing very clearly the enamel, dental tubulars, cementum, and periosteum.

Mr. Henry Edwards, honorary member, donated two specimens of the *Dytiscus marginalis*, from Europe, and which is a favourite object with microscopists for the tarsi; also a great variety of material, for examination and mounting, in the way of insects.

Mr. C. G. Ewing donated a slide, mounted with a microscopic barnacle in glycerine taken out of a shell, found on the bottom of the steamship 'Vasco de Gama.'

Perhaps the most valued acquisition for future research is that of the donation by Mr. Fisher, of the U.S.S. 'Tuscarora,' of samples of a series of twenty-three deep-sea soundings from Cape Flattery to the Aleutian Islands, and of twenty-six from San Diego to Honolulu. These have been arranged, numbered, and labelled, with statistics regarding latitude, longitude, depth and temperature, in each case, by Dr. H. W. Harkness, assisted by Messrs. J. P. Moore and Kinne, and form a field for study for months to come.

After the exhibition by Mr. Hyde of a series of very beautifully and wonderfully arranged slides, particularly one, which was a picture made up of butterfly scales, arranged as a bouquet of flowers in a vase of diatoms, &c., with two birds, lizard, and various insects, mounted by Dalton, London, the meeting adjourned.

The stated meeting was held at the Society's rooms, Thursday evening, March 18, with President Ashburner in the chair, and a fair attendance of resident members. Dr. Eisen, corresponding member, and Mr. W. A. Skidmore, visited the rooms.

Two proposals for resident membership were received, and Henry Molineaux, Esq., was elected as such.

Under the head of donations to the cabinet, Mr. C. G. Ewing presented a slide mounted with a colony of polyps, *Sertularia*, in glycerine, from San Pedro Bay.

Colonel C. Mason Kinne donated five slides, mounted by him, comprising the elytron of a beetle, showing very marked peculiarities; raw cotton from near Visalia, Cal.; scale of salmon; raw cotton from New Mexico; and white horse-hair; the three last named being mounted in balsam, for the polariscope, and which proved worthy objects for observation with that accessory. Colonel Kinne exhibited some living *protococcus*, which vegetable, moving freely in the same drop of water with the animal forms *Paramœcium vorticella* and others, aided to show how nearly the two great kingdoms are allied in the so-called lower forms of life.

Dr. Eisen exhibited the tentacles of a barnacle (*lepas*), and a variety of marine algæ (*ulva*).

An adjourned meeting was held at the rooms of the Society on Thursday evening, April 8, with a full attendance of members, Mr. H. Edwards, honorary member, Dr. A. Barkan, Dr. Geo. H. Powers, Dr. A. P. Hayne, Prof. Wm. H. Brewer, and Messrs. S. Heydenfeldt, jun., Chas. E. Case, Sam. B. Christy and B. B. Redding, were present as visitors.

Messrs. Wm. A. Woodward and W. F. Myers were elected resident members.

The Society was also the recipient of a valuable gift from Dr. J. N. Eckel of this city, in the work of his distinguished countryman, Ehrenberg, on microscopic geology. It is truly a great work—one now very rare—embodying the results of the life-long labour of one of the most celebrated scientists of the age on microscopic research, illustrating the microscopic marvels of earth and ocean, as found in various parts of the world, in a series of large and beautifully coloured plates, with all that perfection of detail for which the German engravers are so justly celebrated. The Society may be congratulated on the acquisition of so valuable an addition to its library.

The following was unanimously adopted:

Resolved,—That the thanks of the San Francisco Microscopical Society be tendered to Dr. J. N. Eckel of this city, for his magnificent and valuable donation of Ehrenberg's 'Mikrogeologie,' and that the trustees be authorized to extend to Dr. Eckel the courtesies of the rooms and apparatus, with a cordial invitation to use them at his pleasure.

Under the head of donations to the cabinet, Mr. C. D. Gibbes presented a portion of a nest supposed to have been made from wild hemp; a sample of hard-pan, twenty-one feet below the surface of the ground, and a fungus found on wild rose, all from Middle River, San Joaquin.

Dr. Gustaf Eisen presented four slides, mounted by him with fresh-water algæ, palate of *Physa radula*, pedicellarium of echinus, and sphœridium of echinus. Dr. Eisen explained the position and formation of the latter, using the black-board, and stated that it was considered the organ of taste in the well-known sea-urchin.

Mr. Hanks called attention to a sample of infusorial earth in the

cabinet, and stated that a fragment of the same, placed in the hands of a London microscopist by Captain Mortimer, corresponding member, had proved exceedingly interesting, and to contain some rare minute forms of fossil Diatomaceæ.

The feature of the evening was a lecture by Dr. Adolp Barkan "On the Construction and Uses of the Ophthalmoscope." The Doctor used the black-board with facility, giving a technical description of the various parts of the human eye, aided by frequent diagrams, and illustrated by reference to the eyes of a rabbit and a hog, from which the light of other days had departed.

Commenting on the need of an instrument to enable one to see the interior of a living human eye, he stated that up to 1845 no such had been obtained; but acting on the fact then discovered, that light entering the eye was not absorbed but reflected, and that the interior of the body could be made luminous, Professor Helmholtz, in 1851, invented an instrument which had the desired effect of illuminating the eye; and catching the rays in the rebound, so to speak, the observer found a new field for his scientific powers.

The practical uses of the ophthalmoscope are of enormous value, as the diagnosis of a disease in and on the rear portion of the eye can be made without guesswork, the various substances and coats of the optic being shown with astonishing clearness.

After exhibiting patterns of the instrument, and stating that although some forty odd varieties had been invented, none were of any practical improvement on the one first produced by Helmholtz, who had seemed to hit on the right thing at once, Dr. Barkan brought out his menagerie of living animals and proceeded to demonstrate what could be seen.

An immense frog, strapped to a piece of board, stared and blinked at the interested scientists, and proved himself a tractable and worthy aid to the evening's study. A subcutaneous injection of a solution of cinnabar filled his blood-vessels with minute crystals, and pouring down the interior of the eye could be seen the coursing blood, with now and then a flashing sparkle of the cinnabar.

A rabbit claimed its share of attention, and the instrument brought out the peculiar construction of its eye, the absence of pigment and insertion of the optic nerve being noticeable.

A cat, swathed and enclosed in a heavy sack for obvious reasons, attracted her quota of observers, and richly repaid them all. The peculiar manner in which the light was reflected in colours of red, green, and yellow, made a picture not before deemed possible to the casual observer.

A pleasantly disposed dog was interviewed with the instrument, and quietly permitted an intelligent gleam to sparkle forth, showing much the same beauties as were by his feline friend and co-labourer in the good cause.

After voting the Doctor a hearty vote of thanks for his delightful and interesting lecture, the meeting adjourned.—*Cincinnati Medical News.*

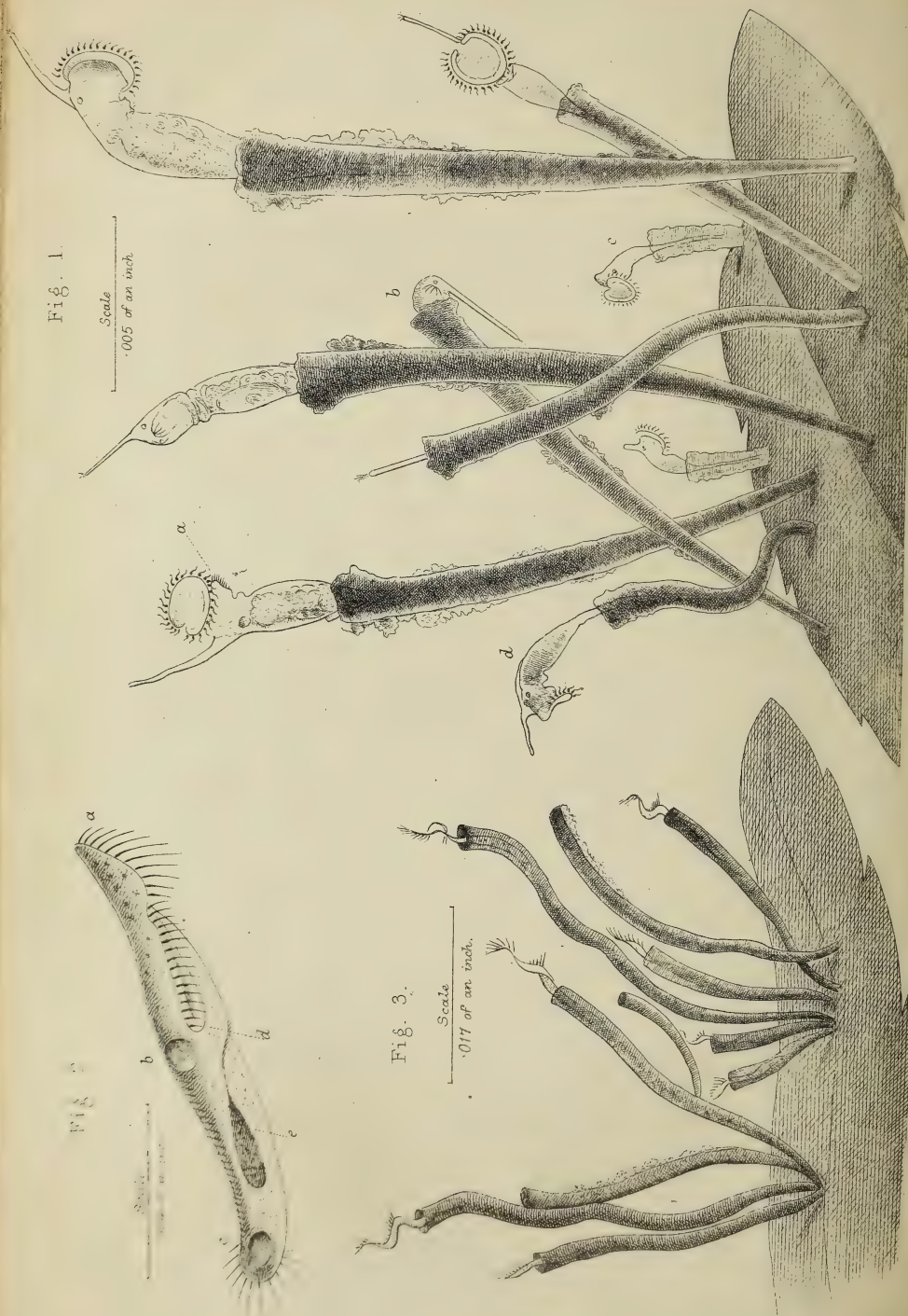


Fig. 1.
Scale
·005 of an inch.
a
b
c
d
Cephalosiphon Limnias.
Fig. 3.
Scale
·017 of an inch.
Archimedea (Chaetospira?) remex.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1875.

I.—*On Cephalosiphon and a New Infusorion.*

By Dr. C. T. Hudson, LL.D.

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

Plate CXVII.

Nature is certainly feminine, for she is the most capricious of deities; now refusing the slightest favour to her ardent worshipper, and then, when he has lost heart and is about to give up the pursuit, overwhelming him with a lavish kindness that is almost as embarrassing as it is tardy. For years I had hunted in every brook, pond, and ditch, within some miles of my house, and always with the hope that some day among my other captures I should light on *Cephalosiphon* and *Ptygura;* two rotifers that I had never seen, and which, so far as they had been described, stood sadly in the way of what seemed to be a tolerably satisfactory way of classifying the Rotifera. So at last I determined to accept provisionally some suggestions that had been made about these two doubtful species, and to consider that *Ptygura Melicerta* and *Cephalosiphon Limnias* had no right to their rank, but that the former was the immature condition and the latter a temporary state of some other species.

When I had struck out their names from the list of rotifers, my classifying went gaily on; and as I had got rid of two of my chief stumbling-blocks I soon finished my scheme to my own satisfaction. It was now Dame Nature's turn. I had given up all hope of winning her favour and of finding the creatures, and I had resolved to do without both it and them; I had made (as I thought) a perfectly satisfactory classification in spite of her; I had played my best card and it remained for her to trump it; which she promptly did, proving to me in a trice that perfect classification is often but another name for imperfect knowledge; for on my going to Nailsea ponds (an old haunt of mine) to get something fresh for my microscope, on the very first weed I plucked were *Cephalosiphon* and *Ptygura*, both on the same leaf; and a short inspection gave me an uneasy suspicion that the latter was probably a mature form, and that the former was a permanent one; so that my knot was not to be cut by the simple method of striking their names out of the list of rotifers.

It seems a paradoxical thing to say that ignorance makes classi-

fication easy, but it will hardly be denied that after our knowledge of a natural group of animals has reached a certain point, their classification is made more difficult by a wider acquaintance with the species that the group comprises. For the more common forms, which are always the great majority, fall readily enough into divisions and subdivisions whose boundaries have that delightful sharpness which seems to make it so easy to study Nature in books, but which unluckily Nature herself never offers to the observer. Further research generally brings to light those rarer intermediate forms (of which *Cephalosiphon* and possibly *Ptygura* are examples) which are at once the despair of the classifier and the delight of the naturalist; and, when the attempt is made to fit these into the original scheme, it is then found that Nature knows no sharp lines of demarcation and has no symmetrical system.

The best classification of Nature's facts will always lack that precision and symmetry which is so dear to most men—who, if they had had the making of their own skeletons, would have substituted cylinders, spheres, and triangles for Nature's free curves and flowing surfaces.

What had made it so difficult to include *Cephalosiphon* in any system of classification was this; that while it appeared to be a tube-making rotifer of the Melicertan family, yet, unlike all other tube-makers, it had only one antenna instead of two, and that one on the wrong side of its body, namely on the side opposite to that of the mouth.

Now the Philodines have only one antenna, and that antenna is on the side opposite to the mouth: moreover (as Mr. Cubitt had pointed out) some of the Philodines occasionally form tubes round themselves. I have several times met with *Rotifer macroceros* so encased. Its long flexible antenna seemed to resemble that of *Cephalosiphon*, as also did its habit of moving it about from side to side before emerging from its tube; so that Mr. Cubitt's suggestion, that *Cephalosiphon* was a temporarily encased Philodine and not a Melicertan at all, seemed worth entertaining; though adopting it was to fall into another difficulty, as Mr. Slack's picture in 'Marvels of Pond Life' showed that the creature had only one large wheel of cilia, and not two small ones.

In fact *Cephalosiphon* had no right to exist if there was to be any comfort in classifying; for its single antenna opposite to the mouth seemed to throw it out of the Melicertans, and its large single wheel to prevent its entering among the Philodines.

Ptygura too was a difficulty; for here was a creature whose form seemed to place it among the tube-makers, and yet which made no tube. Ehrenberg's description of it had made me think that it was some young Melicertan seen before it had begun to construct its tube. I had overlooked Mr. Gosse's statement (in the

'Popular Science Review,' vol. i. p. 490) that he had found two specimens of a fine wheel-animal without a tube, and which he had reason to think was *Ptygura*. "One of the animals," says Mr. Gosse, "laid a large egg actually under my eye, so that it was in adult age. Its general figure was somewhat trumpet-shaped, slightly swelling in the middle The disk was very large, forming a nearly circular outline, and partially surrounded by a layer of granular tissue. The foot terminated in an adhering sucker, whose figure resembled that of a glass stopper in a phial; the dilated extremity of this was capable of adhering to any foreign substance."

The specimen I found at Nailsea—unluckily a solitary one—tallied very closely with this description; but it was very awkwardly placed, half bent round a leaf, so that I could only get occasional glimpses of the body and head, while of the foot I could see but little. I satisfied myself however that it had no case, though I was not quite sure whether there was not a sort of fluffy gathering at the foot. The foot too was thicker than it is in other Melicertans, and more coarsely wrinkled.

For the present, at all events, I am afraid that *Ptygura* must be retained as a Melicertan that does not make a tube.

I was more fortunate with *Cephalosiphon*. There were many specimens on the weed, and of various sizes; and the very first I looked at was a half-grown one protruding sufficiently from its tube to show that it had a smooth, jointless foot, precisely like that of a Melicertan, but quite unlike that of a Philodine.

The trochal disk was oval, with a gap in it on the side opposite to the mouth. Two parallel rows of cilia—the upper large, the lower small—ran round the edge of the disk separated by a groove which led down into the mouth (Fig. 1, *a*); and the lower row of cilia were continued round the edges of the mouth after the usual fashion in Melicertans. But what struck the eye at once was the long flexible antenna. When the creature was withdrawn into its tube the antenna generally projected above it like a thin wand. It was turned too to a curious use; for when *Cephalosiphon* had made up its mind to emerge from its retreat, it hooked its antenna (see Fig. 1, *b*) over the side of its tube, and getting a good purchase, hoisted up its body into a great curve, and then letting go its grip of the case, unbent itself, and at once unfolded its disk. The antenna bore on its end a brush of diverging setæ, and was often not quite straight. It was attached to the animal in an unusual way, for it had a broad base something like that of the thorn of a rose, as if its great length required extra support. I am not aware that there is any Melicertan that at all resembles *Cephalosiphon* in these two particulars—that is to say, that makes so odd a use of its antenna, or that has its antenna broadening out into a bracket-like base.

There were, as I have already said, many specimens of various sizes; and the variations in size were so great that I had little doubt that they represented considerable variations in age, and that it was in consequence most probable that *Cephalosiphon* was a genuine Melicertan forming its tube from early youth, and not a temporarily encased Philodine.

I searched therefore all over the weed in the hope of finding an infant *Cephalosiphon*; and I was not unrewarded. Close to a large cluster of the tubes of *Archimedea remex* (a new infusorion, to be described lower down) was a very tiny tube, barely the $\frac{1}{400}$ of an inch long, and out of which protruded a wheel-animal (Fig. 1, *c*) Plate CXVII., about $\frac{1}{200}$ of an inch in length, and like a *Cephalosiphon* only with a hump instead of the characteristic antenna. I isolated the creature and kept it under daily observation for nearly a fortnight, and during that time had the pleasure of seeing it grow rapidly into the form of an adult *Cephalosiphon* (Fig. 1, *d*).

In twenty-four hours the hump had developed into a short antenna; in four days the young *Cephalosiphon* had grown from $\frac{1}{200}$ of an inch to $\frac{1}{80}$; in six days to $\frac{1}{65}$; in twelve days to $\frac{1}{55}$.

I was too busy to look at it during the next two or three days, and when I did find the leisure the creature was gone—tube and all. It was perfectly healthy on the twelfth day, and about half grown. This conclusively shows that *Cephalosiphon* is not an encased Philodine—even if there were no other reasons to be brought forward.

Since I made the above observations I lighted on a reference to papers of Mr. Gosse and Mr. Slack on *Cephalosiphon* in the 'Intellectual Observer,' vol. i., 1862. I have read these papers carefully, and can quite confirm Mr. Slack's statement that the trochal disk is not bi-lobed with a tendency to further division. It is, as he says, nearly circular with a deep notch in front of the antenna. Mr. Slack too is quite right in saying that the antenna carries setæ at its extremity. It is clear that Mr. Gosse's specimens (sent to him by Mr. Slack) were not in perfect health, and did not, in consequence, give that admirable observer a fair chance. He himself says that his specimens "were chary of exposing their facial charms," and that his "delineation of the form of the disk rests on a single individual."

No doubt the rotifers had suffered in their journey; for when they are fresh from their pond they expand their disks freely.

I have omitted to state that after numerous trials I succeeded in getting nearly the whole of a full-grown specimen out of its tube; I could not see the extremity of the foot, but on the part which I did see—quite seven-eighths of the whole—there was no trace of any segmentation, nor of a telescopic joint, nor of any

processes whatever. There were a few faint transverse wrinkles just as there are in ordinary Melicertans.

Cephalosiphon was not my only prize. I found attached to the same weed (—*Anacharis alsinastrum*—) clusters of long thin brown tubes which at first I thought to be those of *Cephalosiphon*. They proved however to contain a curious infusorion which I believe to be new, and which I have named from its frequently assumed corkscrew shape, and from its rows of cilia used like banks of oars, *Archimedea remex*. It really makes its own tube; for I have seen very young specimens in tubes just begun, and I have left a group of tubes each with its full-grown *Archimedea* in it, and after two days have found them still with their inhabitants inside them, and each tube lengthened by about $\frac{1}{125}$ of an inch. The full-grown *Archimedea* is about $\frac{1}{90}$ of an inch, and the longest tube I have met with is $\frac{1}{20}$ of an inch. The tube of course is far too long for its inhabitant, who as a rule lives in the top of it, though occasionally it backs down the tube nearly to the bottom. When inside the tube the animal is extended to its full length as in Fig. 2, where *a* is the ciliated front down which a groove ciliated on both sides leads to the entrance of the mouth *d*. On protruding from its home it first remains straight quivering its cilia: then, if not alarmed by any shake, or by anything passing, it thrusts quite half its body from its tube, and twists it up into the screw-like form seen in Fig. 3.

On watching the groove, along the edges of which the cilia are in full action, tiny atoms may be seen hurried along to the mouth: tap the table, and *Archimedea* flashes down the tube, quick as a serpula; and if it is in the upper and more transparent end it may be seen moving itself gently to and fro as if hesitating whether to return to the aperture or to seek a safer refuge down below.

These are very tantalizing creatures, for they readily desert their tubes; and in spite of all my care I frequently found every tube empty after the third or fourth day's captivity. Under ordinary circumstances it is a great piece of good luck when a tube-maker leaves its tube, as then the observer gets a capital chance of making out points of structure that could not be seen when the animal was encased; but *Archimedea's* tube is so frail, and the animal so shy, that the leaf it is on must be transferred to the glass cell on the stage of the microscope with the least possible handling; and as the leaf is broad the cell must be so too, so that when the creature swims out of its tube (as I have seen it do) it is almost impossible to keep in the field for more than a few seconds. I tried the heroic method of scraping a group of tubes off the leaf on to my compressorium, and after many failures did at last get one animal out alive and uninjured, as well as free from the débris of the tubes, the rubbish on the leaf, &c., &c.: all of which would

have rendered it hopeless to try to confine it within reasonable limits. I have already described its front. In the middle of the animal and close to the side was a contractile vesicle, Fig. 2, *b*, which opened and shut every two seconds or so. There was a clear space *c* at the end, but this I did not see contract. The shaded portion *e* might have been a stomach, but this I could not determine. The whole of the surface was spotted with granules, and was such as to impair greatly the definition of any internal organ. There was an anus at the side, towards the end opposite to the mouth; and twice I saw the creature dart up the tube (so as to bring the vent above the top of it), shoot out its excrement, and instantly dart back again.

The groups in Fig. 3 are carefully copied from two that were together on the same leaf, and which retained some of their inhabitants for about four days, and then dropped to pieces gradually, and disappeared in about four days more.

II.—*Recent Progress in our Knowledge of the Ciliate Infusoria.*

By G. J. Allman, M.D., F.R.S.

Plate CXVIII.

I believe that the object contemplated by the addresses which it has been the custom of your Presidents to deliver year after year to the Fellows of the Linnean Society will be best fulfilled by making them as much as possible the exponent of recent progress in biological science. The admirable addresses with which my distinguished predecessor has during his long tenure of office so greatly enriched our journal, afford an example as regards the exposition of botanical research which may well be followed in biology generally. The field, however, which thus offers itself is so wide, the activity in almost every department so intense, that the necessity of restricting the exposition within a limited area becomes imperative if it be expected to produce anything like a definite picture instead of a

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EXPLANATION OF PLATE CXVIII.

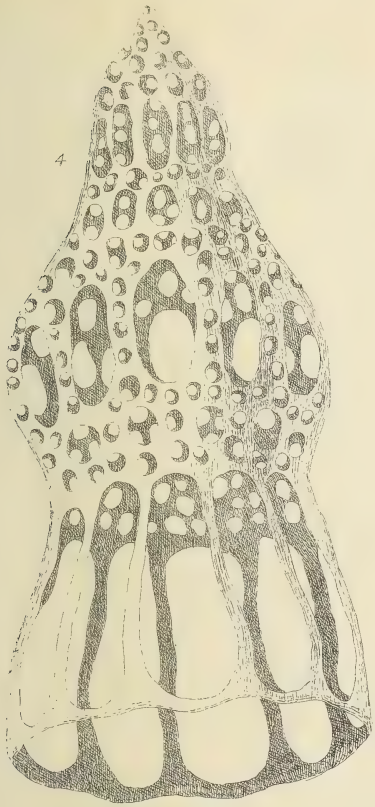
Fig. 1.—*Strombidium sulcatum.* 2 *a*. Crystalline bodies which have been liberated from it, and show amyllaceous reaction with iodine. After Bütschli.

,, 2.—*Polykricos Swartzii.* 2 *a*. Thread-cell-like body liberated from its outer layer. 2 *b*. The same with the filament ejected. After Bütschli.

,, 3.—*Torquatella typica.* After Lankester.

,, 4.—Silicious lattice-like case of a *Dictyocysta.* After Haeckel.

,, 5.—*Didinium nassutum.* 5 *a*. The same in the act of seizing a Paramœcium. 5 *b*. The same with the prey passing through the oral orifice into the interior of the body. After Balbiani.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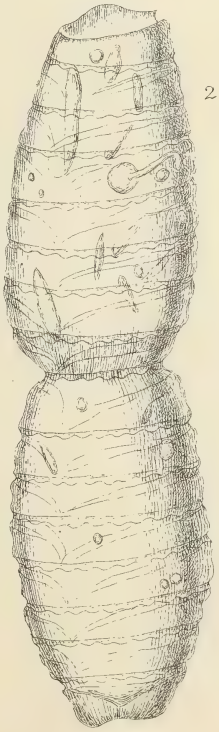
1


1a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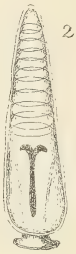
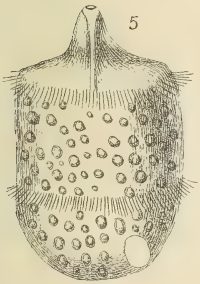

2a


2b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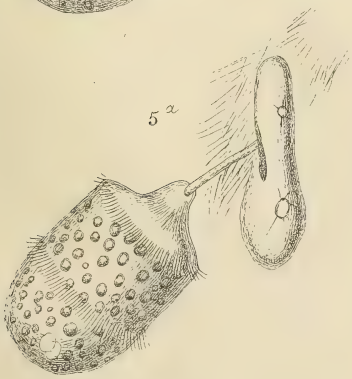
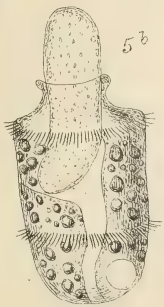

5a


5b


3

vast assemblage of images confused and ill-defined by their very multiplicity and by the condensation which would be inseparable from their treatment.

While thus imposing on myself these necessary limits, it is almost at random that I have chosen for this year's address some account of the progress which has recently been made in our knowledge of the CILIATE INFUSORIA—a group of organisms whose very low position in the animal kingdom in no way lessens their interest for the philosophic biologist, or their significance in relation to general morphological laws.

To enable you to form a correct estimate of the value of recent researches, it may be well to bring before you in the first place, as shortly as possible, the chief steps which have led up to the present standpoint of our knowledge of these organisms.

It is scarcely necessary to remind you that the first important advance which during the present century was made in our knowledge of the Infusoria dates from the publication of the great work of Ehrenberg,* whose unrivalled industry opened up a new field of research when, by his expressive figures and well-constructed diagnoses, he made us acquainted with the external forms of whole hosts of microscopic organisms of which we had been hitherto entirely ignorant, or which were known only by such figures and descriptions as the earlier observers with their very imperfect microscopes were able to give us.

Ehrenberg, however, as we all know, did not content himself with portraying the external forms of the microscopic organisms to whose study he had devoted himself, but sought also to determine their internal structure, of which scarcely anything had been hitherto known. In this direction, no less than in the other, the perseverance of the celebrated microscopist never flagged; but, unfortunately, at the very commencement of his researches he slid into a misleading path, and was never again able to find the right one.

Everyone knows how Ehrenberg, in accordance with preconceived notions of the high organization of *all* animals, attributed to the Infusoria a complicated structure; how, while he rightly distinguished them from the Rotiferæ with which they had been confounded by previous observers, he yet regarded them as intimately related to these representatives of a totally different type; and how, in attributing to them a complete alimentary canal with numerous gastric offsets, he took this feature as their most important character, and designated them by the name of *Polygastrica.* And it is probably a matter of surprise to many of us, that with the overwhelming mass of evidence which subsequent research has brought to bear against the truth of the polygastric theory, the

* 'Die Infusionsthierchen als vollkommene Organismen.' Leipzig, 1838.

great Prussian observer should still adhere with undiminished tenacity to his original views.

Among the authors who, since the publication of the 'Infusionsthierchen,' have contributed most to a correct estimate of the morphology, physiology, and systematic position of the Infusoria, the names of Dujardin, Von Siebold, Stein, Balbiani, Claparede, and Lachmann, and most recently, Haeckel, stand out conspicuous.

The way to a philosophic conception of the Infusoria and of other beings which occupy the lowest stages of life was undoubtedly opened up by Dujardin* when he drew attention to the existence of a peculiar form of matter of semi-fluid consistence and of nitrogenous composition, and which, though totally undifferentiated, is yet endowed with properties essentially characteristic of vitality. To this remarkable substance he gave the name of "sarcode." The sarcode of Dujardin has of late years been described chiefly under the name of protoplasm, and its wide extension and importance in the economy of all living beings, whether plants or animals, has been recognized as one of the most comprehensive facts in biology.

After Dujardin, the first who from a strong position offered battle to the authority of Ehrenberg was Carl Theodor von Siebold.† Von Siebold rejected *in toto* the polygastric theory, and, so far from admitting a complexity in the organization of the Infusoria, he regarded them as realizing the conception of almost the very simplest form of life, and attributed to them the morphological value of a cell.

Let us see what is involved in this most significant comparison. The essential conception of a cell is, as you know, that of a more or less spherical mass of protoplasm with or without an external bounding membrane, and with an internal nucleus or differentiated and more or less condensed portion of the protoplasm. It was to a form of this kind that Siebold compared the body of an Infusorium. He called attention to the soft protoplasmic mass of which the body mainly consists; to the external firmer layer by which this is surrounded; and to the variously shaped body differentiated in the protoplasm, to which Ehrenberg had gratuitously attributed the function of a male generative organ. Here then were, according to Siebold, the protoplasm body-substance, the bounding membrane, and the nucleus of a true cell.

The morphological value thus attributed to the true Infusoria —under which were included the Flagellatæ—was extended by Siebold to Amœba and its allies, and to the whole assemblage so constituted he assigned the position of a primary group of the

* "Sur l'organisation des Infusoires," 'Ann. des Sci. Nat.,' 1838; and 'Hist. des Infusoires,' Paris, 1841.

† Siebold, 'Lehrbuch der vergleichenden Anatomie,' 1845.

animal kingdom, to which he gave the name of PROTOZOA, whose essential character was thus that of being unicellular animals. He then divided his Protozoa into those which had the faculty of emitting pseudopodial prolongations of their protoplasm (Amœba, &c.), and those in which the place of the pseudopodia was taken by vibratile cilia or by lash-like appendages. To the former he gave the name of *Rhizopoda;* to the latter he restricted that of *Infusoria;* and lastly, he divided the Infusoria into the mouth-bearing, *Stomatoda* (Ciliata), and the mouthless, *Astomata* (Flagellata). From every point of view Von Siebold's conception of the morphology of the Protozoa, and his sketch of their classification, however much this may have been subsequently modified, must be regarded as marking out an epoch in the history of zoology.

Shortly after this the unicellular theory was strongly supported by Kölliker,* and received further confirmation from the researches of Stein,† who, however, was unable to accept it to its full extent. With an industry almost equal to that of Ehrenberg, Stein had the advantage of the more philosophic views of organization which had emanated from the newer schools of biology, and to him we are indebted not only for more accurate views of the structure of the Infusoria, but for the first important contributions to our knowledge of their development; and though the opinion which he at one time entertained, that the true Acinetæ are only stages in the development of the higher Infusoria, has been abandoned by him, he has nevertheless demonstrated the presence in an early period of the development of certain species, of peculiar pseudopodial processes resembling the characteristic capitate appendages of the Acinetæ, an observation of importance in its bearing on the relations of these last to the true Infusoria. No doubt can remain, after Stein's observations, that the Infusoria in their young state have the morphological value of a simple cell, and it is only after their development has become advanced, and that a marked differentiation has begun to manifest itself in this primordial condition, that there can be any difficulty in accepting their absolute unicellularity.

About this time Balbiani drew attention to some very important phenomena in the life history of the Infusoria.‡ It had been known even to the early observers that the Infusoria multiplied themselves by a process of spontaneous fission. They had been frequently observed in the act of transverse cleavage, and had also been noticed in what appeared to be a similar cleavage taking place in a longitudinal instead of a transverse direction. Balbiani, how-

* Zeitschr. f. Wissens. Zool., 1849.

† Stein, 'Der Organismus der Infusionsthiere,' 1867.

‡ Balbiani, "Recherches sur les organes générateurs et la réproduction des Infusoires," 'Comptes Rendus,' 1858, p. 383.

ever, showed that this apparent longitudinal cleavage had in many cases an entirely different significance; that it was, in fact, not the cleavage of a single individual, but the conjugation of two distinct ones; and he connected this phenomenon with what he regarded as a true sexual act.

It was then known that besides the nucleus which occupied a conspicuous position in the protoplasmic mass, there existed in many Infusoria another differentiated body similar to the nucleus but smaller, and either in close contact with it or separated from it by a greater or less interval. To this body the ill-chosen name of "nucleolus" had been given. Now, Balbiani's observations led him to believe that under the influence of conjugation this so-called nucleolus underwent a change and developed in its interior a multitude of exceedingly minute filaments or rod-like bodies, to which he attributed the significance of spermatozoa; while at the same time the nucleus became divided into globular masses, which Balbiani regarded as eggs, and in which he believed he could recognize a germinal vesicle and germinal spot. We should thus, according to this interpretation, have in the Infusoria the two essential elements of sexual differentiation, the spermatozoa and the egg.

Stein, though differing from Balbiani in certain details, accepts in its general facts the sexual theory, and maintains the spermatic nature of the rod-like corpuscles to which the nucleolus appears to give rise. But however real may be the phenomena described by Balbiani and by Stein, the correctness of assigning to them a sexual significance may be called in question; and it is certain that subsequent observation has not tended to confirm the hypothesis that we have in the Infusoria true eggs fecundated by true spermatozoa.

Claparede and Lachmann, two able and indefatigable observers fresh from the school of the great anatomist Johannes Müller, now entered the field, and their joint labours have given us a valuable work on the Infusoria.* In this an entirely new view of the morphology of the Infusoria has been introduced. Receding widely from the unicellular theory of Siebold, they approximate towards the views of Ehrenberg in assigning to the Infusoria a comparatively complex structure; but instead of adopting the polygastric theory of the Prussian microscopist, they attribute to the Infusoria a single well-defined gastric cavity occupying the whole of the space limited externally by the outer firm boundary walls of the softer protoplasmic mass; while this mass is regarded by them as nothing more than a sort of chyme by which the gastric cavity is filled. According to this view, the nearest relations of the Infusoria would

* Claparede et Lachmann, 'Etudes sur les Infusoires et les Rhizopodes.' Genève, 1858–61.

be found among the zoophytes, and their proper systematic seat would be in the primary group of the Cœlenterata.

Though few zoologists will now be prepared to accept the conclusions of the Genevan naturalist and his associate, the cœlenterate relations of the Infusoria has recently found an advocate in Greeff.* In an elaborate memoir on the Vorticellæ, Greeff sees in the very well-marked distinction between the external or cortical layer and the internal soft body-substance, a proof of the views maintained by Claparede and Lachmann; and he considers this position still further confirmed by the presence in *Epistylis flavicans* of numerous oval or piriform, brilliant, well-defined capsules, which are generally distributed in pairs below the outer layer, and which, under the influence of a stimulus, emit a long filament, thus closely resembling the thread-cells so well known as characteristic elements in certain tissues of the Cœlenterata.

It must be here remarked that the presence of similar bodies in the Infusoria, where they have been described under the name of trichocysts, has long been known. Though varying in form, they all possess a more or less close resemblance to the thread-cells of the Cœlenterata. Their presence undoubtedly indicates a step upwards in the differentiation of the organism, but, as we shall presently see, it offers no valid argument against its unicellularity.

In his admirable 'Principles of Comparative Anatomy,' † Gegenbaur expresses doubts as to the sexual nature of the reproductive phenomena of the Infusoria, and is disposed to regard the so-called embryo-sphere, to which the nucleus gives rise, in the light of a proliferous stolon, from which several zooids are in some cases thrown off. Arguing from the Acineta-like form of the young in the higher Infusoria, as shown by Stein, and comparing the transitory condition of this with the permanent condition of the true Acinetæ, he believes that we are justified in regarding the Acinetæ as the ancestral form from which the proper Infusoria have been derived. He further compares the contractile vesicle and its canals in the Infusoria with the water vascular system of the worms, and believes that a parentage with these higher forms is thus indicated. Gegenbaur, moreover, expresses himself strongly against the unicellular theory. He regards, however, the absence of distinct cell nuclei in the substance of the Infusoria as affording evidence of their composition out of several "Cytodes" or non-nucleated protoplasm masses rather than out of true nucleated cells.

Still more recently, Bütschli has given us the results of observations on the conjugation of *Paramœcium aurelia*.‡ He is led,

* Greeff, "Untersuchungen über den Ban und die Naturgeschichte der Vorticellen," 'Archiv für Naturg.,' 1870.

† 'Grundsüge der Vergleichenden Anatomie,' 1870.

‡ O. Bütschli, "Einiges über Infusorien," 'Archiv f. Microscop. Anat.,' 1873.

however, to doubt the validity of the sexual interpretation of the conjugation. He found that in certain cases in *Paramœcium aurelia* and in *P. colpoda* the so-called spermatic capsule into which the nucleolus had become converted, had entirely disappeared without any evident change in the nucleus; and he concludes that fecundation of the bodies regarded by Balbiani as eggs cannot be here entertained. Indeed, he will not allow that we have evidence entitling us to regard the appearance of filaments in the interior of the nucleolus as affording any indication of true spermatozoa. He offers no explanation of this appearance, but he calls attention to the fact that both Balbiani and Stein noticed that in *transverse* division of the Infusoria—a phenomenon with which conjugation can have nothing to do—the nucleolus frequently enlarges and acquires a longitudinal striation like that of the nucleolus in the supposed production of spermatozoa during conjugation. Balbiani maintains that this striation during cleavage is only superficial, but it nevertheless affords an argument against assigning any more important significance to the very similar appearance in the case of conjugation.

On the whole it would appear that the spermatozoal nature of the striæ visible in the nucleolus of the conjugating individuals—even admitting that these striæ represent isolatable filaments—has not by any means been proved, while the phenomenon of conjugation in the Infusoria would seem to correspond rather with the conjugation so well known in many lower organisms, where it takes place without being in any way connected with the formation of true sexual products.

In the same memoir the results of observations on some other points in the structure and economy of the Infusoria have also been given by Bütschli. He records the occurrence of minute crystal-like laminæ in the interior of a marine Infusorium (*Strombidium sulcatum*) rendered remarkable by a conspicuous girdle of trichocysts which surround its body (Pl. CXVIII., Figs. 1 and 1 *a*). The crystal-like corpuscles seem to be of the nature of starch, for on the application of iodine they assume a beautiful violet colour. It does not appear from Bütschli's account of these bodies that they have not been introduced from without, and the chief interest of the observation seems to be in the discovery of an amyllaceous body assuming a crystalline form. He had previously met with similar bodies in a parasitic Infusorium (*Nyctotherus ovalis*), as well as in a Gregarina (*G. blattarum*).

He also describes, under the name of *Polykricos Swartzii* (Fig. 2), a new Infusorium which he frequently found in the fjords of the south coast of Norway and in the Gulf of Kiel, and which he regards as especially interesting, from the fact that with a true infusorial organization it contains, irregularly distributed in the outer layer

of the body, numerous capsules (Figs. 2 *a* and 2 *b*) indistinguishable from the true cœlenterate thread-cells. These bodies, however, are never included in a special investment, and he justly regards their presence as affording no argument against the unicellular nature of the Infusoria. He lays it down as a probable distinction between the trichocysts of the Infusoria and genuine thread-cells, that the former have the power of ejecting their contained filament from both ends of the capsule, while we know that in the thread-cell it is only one end which gives exit to it. This double emission of a filament appears to have been observed by Bütschli in the trichocysts of a large Nassula, but the distinction is certainly not a generally valid one. There is no doubt that in the majority of cases the trichocyst emits its filament from only one end of its capsule, exactly as in the thread-cells of the Cœlenterata, and it is hard to see in what respect the bodies noticed by Bütschli in his *Polykricos Swartzii* essentially differ from true infusorial trichocysts. In conclusion, he declares himself strongly in favour of the unicellularity of the Infusoria.

The reproductive process was lately followed by myself through some of its stages in a very beautiful Vorticellidan obtained abundantly from a pond in Brittany.* The zooids which form the colonies in this Infusorium are grouped in spherical clusters on the extremities of the branches. They present near the oral end a large and very obvious contractile vesicle, and have a long cylindrical nucleus curved in the form of a horseshoe.

In the internal protoplasm are also imbedded scattered green chlorophylloid granules. No trace of the so-called nucleolus was present in any of the specimens examined.

Among the ordinary zooids there were usually some which had become encysted in a very remarkable way, and without any previous conjugation having been noticed. These encysted forms were much larger than the others and had assumed a nearly spherical shape; the peristome and cilia disk had become entirely withdrawn, the contractile vesicle was still obvious, but had ceased to manifest contractions; brownish spherical corpuscles with granular contents, probably the more or less altered chlorophylloid granules of the unencysted zooid, were scattered through the parenchyma, and the nucleus was not only distinct, but had increased considerably in length. Round the whole a clear gelatinous envelope had become excreted.

In a later stage there was formed between the gelatinous envelope and the cortical layer of the body a strong, dark-brown, apparently chitinous case, the surface of which in stages still further advanced had become ornamented by very regular hexagonal spaces with slightly elevated edges. In this state the chitinous

* British Association Reports, 1873.

envelope was so opaque that no view could be obtained through it of the included structures, and in order to arrive at any knowledge of these it was necessary to rupture it. The nucleus thus liberated was found to have still further increased in length, and to have become wound into a convoluted and complicated knot. Along with the nucleus were expelled multitudes of very minute corpuscles with active Brownian movements.

In a still further stage the nucleus had become irregularly branched, and at the same time somewhat thicker and of a softer consistence; and finally, it had become broken up into spherical fragments, each with an included corpuscle resembling a true cell nucleus in which the place of a nucleolus was taken by a cluster of minute granules.

In this case the original nucleus of the Vorticellidan had thus become broken up into bodies identical with the so-called eggs of Balbiani, but this was unaccompanied by any conjugation or by the formation of anything which could be compared to spermatozoal filaments.

What I believe we may regard as now established in the phenomena of reproduction in the Infusoria is, that besides the ordinary reproduction by spontaneous fission of the entire body, the nucleus at certain periods, and after more or less change of form has occurred in the Infusorium body, becomes broken up into fragments, each including a corpuscle resembling a true cell-nucleus; and that this takes place without necessarily requiring the influence of conjugation or the action of spermatozoa; that these fragments after their liberation from the body of the Infusorium become developed—still without the necessity of spermatic influence—directly or indirectly into the adult form.

Whether proper sexual elements ever take part in the life history of the Infusoria remains an open question.

Everts * has given an account of observations which, with the view of testing the statements of Greeff, he made on *Vorticella nebulifera.* Greeff, as we have seen, followed Claparede and Lachmann in attributing to the Vorticellæ a true cœlenterate structure; and Everts, by his own investigations, has convinced himself of the untenableness of this view, and has been led to regard the Vorticellæ as strictly unicellular.

He recognizes the distinction between the cortical layer (which forms not only the periphery of the body but the whole of the stalk on which this is supported), and the central mass in which the nutriment is deposited, collected into pellets, and digested; but instead of regarding this central mass as chyme, he looks upon it as an integral constituent of the entire body, like the central

* Everts, Untersuchungen an *Vorticella nebulifera.* Sitzungsberichte der Physikalisch-Medicinischen Societat zu Erlangen. 1873.

portion of an Amœba. The nucleus is imbedded in the inner side of the cortical layer, which is itself differentiated into certain secondary layers. He describes the deeper part of the cortical layer as exhibiting a rotation of its granules independent of the rotation which occurs in the central parenchyma, and moving in a direction opposite to that of the latter. Everts's account of the structure of Vorticella is thus in accordance with the conception of it as a cell with a parietal nucleus; a cell, however, in which differentiation is carried very far without the essential character of a simple cell being thereby lost.

Everts regards the external wall as corresponding with the ectoderm, and the internal softer body-substance with the endoderm of higher animals. If by this the author meant to indicate a homological identity between the structures thus compared, it is plain that he would have taken an entirely mistaken view based on a misconception of the essential nature of an ectoderm and endoderm. These membranes are essentially multicellular, and are always results of the segmentation of the vitellus in a true ovum. They can therefore never be attributed to a unicellular animal, in which no true segmentation process ever takes place. In his rejoinder, however, to an elaborate criticism of his memoir by Greeff, he explains that he intended to compare the two layers of the Infusorium body analogically, not morphologically, with an ectoderm and endoderm.

The same author has further made some interesting observations on the development of Vorticella. He has noticed that reproduction is here ushered in by a longitudinal cleavage, in which after division of the nucleus the body of the Vorticella becomes cleft into two halves, still seated on the common stalk. Each of these develops near its posterior end a wreath of vibratile cilia, while the peristome and the cilia disk over the mouth are entirely withdrawn, and then breaks loose from its stem and swims freely away. These free-swimming Vorticellæ now encyst themselves, the cilia disappear, and the contents of the encysted animal acquire a uniform clearness with the exception of the nucleus, which persists unchanged. In the next place the nucleus breaks up into eight or nine pieces, and then the wall of the cyst becomes ruptured and gives exit to these fragments, which now appear as spontaneously moving spherules. These increase in size, develop on one end a cilia wreath, within which a mouth makes its appearance, and the free-swimming nucleus-fragment becomes gradually changed into a form which entirely agrees with the *Trichodina grandinella* of Ehrenberg.

These Trichodinæ now multiply by fission, first developing a posterior wreath of cilia, and then dividing transversely between the anterior and posterior wreaths. After this each fixes itself by

the end on which the mouth is situated; a short stem becomes here developed, and the cilia wreath gradually disappears. Then upon the free end the peristome and cilia disk make their appearance, and the growth of the stem completes the development.

Everts remarks that in this process we have an example of alternation of generations. There is one point, however, in which he has overlooked its essential difference from a true alternation of generations, namely, the absence of any intercalation of a proper sexual reproduction.

Ray Lankester * has subjected to spectrum analysis the blue colouring matter of *Stentor cæruleus.* This occurs in the form of minute granules in the cortical layer of the animal, and Lankester finds that it gives two strong absorption bands of remarkable intensity, considering the small quantity of the matter which can be submitted to examination. He cannot identify these bands with those of any other organic colouring matter, and to the peculiar pigment in which he finds them he gives the name of *stentorin.*

He has also examined the bright green colouring matter of *Stentor Mülleri*, and finds that instead of giving the stentorin absorption bands, it gives a single band like that of the chlorophylloid matter of *Hydra viridis* and of Spongilla.

Ray Lankester † has also described, under the name of *Torquatella typica* (Fig. 3), a remarkable marine Infusorium, which, though quite destitute of true cilia, can scarcely be separated from the proper Ciliata. With the general structure of the ciliate Infusoria, the place of a peristomal cilia wreath is taken by a singular plicated membrane, which forms a wide, frill-like, very mobile appendage, surrounding the oral end of the animal, and projecting to a considerable distance beyond it. The author regards *Torquatella typica* as the type of a distinct section of the Ciliata to which he gives the name of *Calycata.*

Of all the authors who since Von Siebold have applied themselves to the investigation of the Infusoria, Haeckel must be mentioned as the one who has brought the greatest amount of evidence to bear on the question of their unicellularity. In a very elaborate paper which has quite recently appeared, ‡ and which is remarkable for the clearness and logical acuteness with which the whole subject is treated, Prof. Haeckel, resting mainly on the observations of others, and partly also on his own, argues in favour of the unicellularity of the Infusoria from the evidence afforded both by the phenomena of their development and by the structure of the mature organism. He confines himself chiefly to the Ciliata—

* 'Quart. Journ. Mic. Sci.,' 1873. † Ibid., 1874.

‡ Haeckel, "Zur Morphologie der Infusorien," Jenaische Zeitschr., Band vii. heft 4, 1873.

which, indeed, he regards as the only true Infusoria—while he considers the unicellularity of the Flagellata as too obvious to require an elaborate defence. The value of this paper will be obvious from the analysis of it which I now propose to give.

In stating the argument derived from development, Haeckel does not accept as established the alleged sexual reproduction of the Infusoria, and he believes it safest to regard as non-sexual "spores" the bodies (*Keimkugeln*) which result from the breaking up of the nucleus, and which Balbiani regarded as eggs.

These bodies consist of a little mass of protoplasm usually destitute of membrane, and including a nucleus within which one or more refringent granules admitting of comparison with a true nucleolus may sometimes be witnessed—characters which are all those of a simple genuine cell. From this spore the embryo is developed by direct growth and differentiation of parts; but however great may be the differentiation, there is never anything like the formation of a tissue.

The development of the Infusoria is thus entirely in favour of the unicellular theory. This theory, however, is just as strongly supported by the study of their mature condition; and here Haeckel gives an admirable exposition of the structure of the true or Ciliate Infusoria.

The parts which are common to all Ciliata and which first differentiate themselves in the ontogenesis or development of the spore, are the cortical layer, the medullary parenchyma, and the nucleus, which is situated on the boundary between the two. The differentiation of the protoplasm of the naked spore into a clearer and firmer cortical substance, and a more turbid, granular, and softer medullary substance, corresponds entirely with what we see in the parenchyma cells of higher animals. These two products of differentiation are designated by Haeckel "exoplasm" and "endoplasm."

The exoplasm is originally a perfectly homogeneous and structureless, colourless hyaline layer distinguishable from the turbid granular soft protoplasm of the internal body mass, by containing in its composition less water, by absence of included granules, and by its high independent contractility. All the mobile appendages of the body, the cilia, bristles, spines, hairs, hooks, &c., are nothing but structureless extensions of this exoplasm and participate in its contractility. In this respect they entirely correspond to the cilia and flagellæ of the cells which form the ciliated epithelium of multicellular animals.

In many Ciliata we find this cortical layer or exoplasm itself subsequently differentiated into distinct strata. In the most highly differentiated Ciliata four layers may be distinguished as the result of this secondary differentiation of the exoplasm. These are:

(1) the cuticle layer, (2) the cilia layer, (3) the myophan layer, (4) the trichocyst layer.

The *cuticle* is nothing but a lifeless exudation from the surface. In the majority of Ciliata there is no true cuticle, and in those which possess it, it presents itself under various forms, as seen in the thin, chitine-like, hyaline homogeneous pellicle of Paramœcium and Trichodina, the outer elastic layer of the stem of the Vorticellinæ, the protective sheath of Vaginicola, the chitine-like cases of the Tintinnodeæ and Codonellidæ, the beautiful lattice-like silicious shells of the Dictyocystidæ (Fig. 4), and many other shells, cases, and shield-like protections.*

The *cilia layer* occurs in all Ciliata; it lies immediately beneath the cuticle where this is present, and the whole of the cilia and other mobile appendages are its immediate extensions. These must therefore perforate the cuticle or its modifications when such protective coverings exist.

The *myophan layer* is identical with that which most authors describe as a true muscular layer. It has been demonstrated in most of the Ciliata. It appears as a system of regular parallel fine striæ in the walls of the body, and in the Vorticellidæ occupies also the axis of the stem, where it forms the characteristic "stem-muscle" of these animals. There can be no doubt that these striæ represent contractile fibrils, which, by their contraction, effect the various form changes of the animal. They are thus *physiologically* analogous to muscles. From a *morphological* point of view, how-

* In the same number of the 'Zeitschrift,' Haeckel ("Ueber einige neue pelagische Infusorien") describes some highly interesting Infusoria which spend their lives in the open sea and are distinguished by the possession of variously formed shells. His attention was first directed to them by finding their elegant empty shells in the extra-capsular sarcode of Radiolariæ. These pelagic Infusoria appear to belong to two different groups, which stand nearest to the Tintinnodeæ of Claparede and Lachmann. He designates them as *Dictyocystidæ* and *Codonellidæ*.

The family of the Dictyocystidæ is based on Ehrenberg's *Dictyocysta*, and is characterized by the possession of a silicious perforated lattice-like shell so closely resembling that of many Radiolariæ, that Haeckel at first mistook it for the shell of one of these. The shell is in all the species bell-shaped or helmet-shaped, and the body of the animal, which is fixed to the fundus of the bell, and can be projected far beyond its margin, has a wide funnel-shaped peristome on whose edge are two concentric wreaths of strong cilia. He describes four species, distinguishing them by characters derived from their silicious latticed shell.

The family of the Codonellidæ, based on the genus *Codonella*, Haeckel, is also provided with a bell-shaped case, but this, instead of being formed of a silicious lattice work, consists of a chitine-like organic membrane, through which silicious particles are scattered. The family is, however, chiefly characterized by the peculiar form of its peristome. This is funnel-shaped and provided on its margin with a thin collar-like expansion. The free edge of this collar is serrated, and each tooth carries a stalked lobe of a piriform shape, regarded by Haeckel as probably an organ of touch. At some distance behind the circle of piriform lobes is situated a ring of long, strong, whip-like cilia, which form powerful swimming organs. The three species described are distinguished by the form of their chitinous cases.

ever, we must regard them as only differentiated protoplasm filaments. In the morphological conception of true muscle, its cell nature is absolutely indispensable. The so-called muscle fibrils of the Infusoria never show a trace of nucleus. They can be viewed only as *parts* of a cell due to the differentiation of the sarcode molecules of its protoplasm; and as they are thus only sarcode filaments, Haeckel designates them by the term "myophan," as indicating a distinction from proper muscle.

The *trichocyst layer* occurs also in many Infusoria, but not in all. It is a thin stratum of the exoplasm lying immediately on the endoplasm, and including in certain species the trichocysts. The presence of these bodies, which possess a striking resemblance to the thread-cells of the Cœlenterata, has, as we have already seen, been urged as an argument in favour of the multicellularity of the Infusoria. But, as Haeckel argues, no evidence of multicellularity can be derived from this fact. The thread-cells of the Cœlenterata are themselves the products of a cell, and we often find many of them originating in a single formative cell quite independently of the nucleus; the formative cell may in this respect be compared with the entire body of the Infusorium.

It is the endoplasm, or internal parenchyma of the Infusoria that has given rise to the most important differences of opinion, and in his account of this part of the Infusorium organism Haeckel chiefly directs his criticism against the views advocated by Claparede and Lachmann, and by Greeff.

These authors, as we have already seen, compare the Infusoria with the Cœlenterata, and regard the endoplasm not as a real part of the body, but merely as the contents of the alimentary canal—as a sort of food mash or chyme contained in a spacious digestive cavity whose walls are at the same time stomach wall and body wall, and into which the mouth leads by a short gullet. As Haeckel urges, however, it needs only a correct conception of the intestinal cavity throughout the animal kingdom and of its distinction from the body cavity, in order to show the untenableness of this position. The main point of such a conception lies in the fact that the intestinal cavity and all extensions of it (gastro-vascular canals, &c.) are always originally clothed by the endoderm or inner leaflet of the blastoderm, while the body cavity is always formed on the external side of the endoderm, and between this and the ectoderm or outer leaflet of the blastoderm. The body cavity and intestinal cavity of animals are thus essentially different; they never communicate with one another, and always arise in quite different ways.

Again, the contents of a true intestinal cavity consist only of nutritious matter and water, in other words, of chyme; while the fluid which fills the body cavity is never chyme, but is always a

liquid which has transuded through the intestinal wall, and which may be called chyle, or blood in the wider sense of the word.

Haeckel has thus taken, I believe, the true view of the intestinal and body cavities of animals. He had already advocated it in his work on the Calcareous Sponges. It necessarily involves a belief in the homological identity of organization between very distant groups of the animal kingdom, a belief which all recent embryological research has only tended to confirm.

It follows from this view that the cavity of the Cœlenterata would represent an intestinal cavity only, while a true body cavity would be here entirely absent. This way of regarding the cavity of the Cœlenterata is at variance with the conclusions of most other anatomists who regard the cœlenterate cavity as representing a true body cavity, or a body and intestinal cavity combined. I had myself long entertained the generally accepted opinion that the cavity of the Cœlenterata represents a body cavity. I must, however, now give my adhesion to the doctrine here advocated by Haeckel, and regard the proper body cavity of the higher animals as having no representative in the Cœlenterata. I believe that this is supported both by the facts of development and by the structure of the mature animal. Indeed, the body cavity first shows itself, as Haeckel has pointed out, in the higher worms, and is thence carried into the higher groups of the animal kingdom.

If such be the real nature of a true intestinal cavity and of a true body cavity, it is plain that neither the one nor the other can exist in the Infusoria, for there is here nothing which can be compared with either the endoderm or the ectoderm.

The whole, then, of the alleged chyme of the Infusoria is nothing more than the internal soft protoplasm of the body. It is quite the same as in Amœba and many other unicellular animals.

The peculiar currents which have been long noticed in the endoplasm of many Infusoria must be placed in the same category with the rotation of the protoplasm observed in many organic cells. Von Siebold, indeed, had already compared the endoplasm currents of the Infusoria to the well-known rotation of the protoplasm in the cells of Chara.

The presence of a mouth and anal orifice in the ciliate Infusoria has been urged as an argument against the unicellular nature of these organisms. The so-called mouth and anus, however, admit of a comparison not in a *morphological* but only in a *physiological* sense with the mouth and anus of higher animals. They are simple lacunæ in the firm exoplasm, and have, according to Haeckel, no higher morphological value than the "pore canals" in the wall of many animal and plant cells, or the micropyle in that of many egg-cells. Kölliker had already compared them to the excretory canal of unicellular glands. Since, therefore, they do not admit of

being homologically identified with the orifices of the same name in the higher animals, Haeckel proposes for them the terms "*Cytostoma*" and "*Cytopyge*."

So also the presence of a contractile vesicle and of other vacuoles affords no solid argument against the unicellularity of the Infusoria. The physiological significance of the contractile vesicles has been variously interpreted. In certain cases a communication with the exterior appears to have been demonstrated, and Haeckel regards them as combining two different functions of nutrition, namely, respiration and excretion. They are in all cases destitute of proper walls, and they have been long recognized as morphologically nothing more than lacunæ filled with fluid. Regular contractile vesicles differing in no respect from those of the ciliate Infusoria are often found in the Flagellata and in the swarmspores of many Algæ.

Besides the constant and regular contracting vacuoles, there occur also others less constant and less regularly contracting. These are found in the softer endoplasm, while the constant and regularly contracting vacuoles occur for the most part in the firmer exoplasm. One is just as much a wall-less vacuole as the other, and the difference between them is to be traced to the difference of consistence in the surrounding protoplasm. Haeckel regards the less constant ones as the original form from which the others have been phylogenetically derived, that is, by a process of inheritance and modification through descent.

The last and most important of the parts which enter into the formation of the Infusorium body, namely, the nucleus, is next discussed. Viewed from a morphological point, it has been already demonstrated that the nucleus is in all Ciliata originally a single simple structure, resembling in this respect a true cell-nucleus. As the Infusorium body approaches maturity we find that with its advancing differentiation peculiar changes occur in the nucleus just as in the rest of the protoplasm, but these changes are entirely paralleled by differentiation phenomena which are known in other undoubted cell-nuclei, as, for example, in the germinal vesicle of many animals, in the nuclei of many unicellular plants, the nuclei of many parenchyma cells of the higher plants, and the nuclei of many nerve-cells. The mature Infusorium nucleus is often vesicle-like, and consists of a delicate investing membrane and fine granular contents, precisely as in the differentiated nucleus of many other cells. In many Ciliata, if not in all, there is within the young nucleus a dark, more refringent corpuscle, which has quite the same relations as the nucleolus of a true cell-nucleus.

Regarded from a physiological, no less than from a morphological point of view, the Infusorium nucleus and true cell-nucleus admit of a close comparison with one another. It may be considered as established by the concurrent observations of all investigators, that

the nucleus of the Infusoria performs the function of a reproductive organ, though the opinions entertained as to the mode in which it thus acts are extremely divergent.

It is now admitted that in the reproduction of unicellular organisms both in the animal and vegetable kingdom, the nucleus takes an important part, and by its division as a primary act ushers in the division of the rest of the protoplasm. Even in the cells which form constituents of tissues, the part played by the nucleus is altogether similar, its division always preceding the division of the cell itself.

In quite a similar way does the nucleus behave in the ciliate Infusoria. The non-sexual reproduction of the Infusoria by division is perhaps universal. In such cases the division always begins by the spontaneous halving of the nucleus, and this is followed by a similar division of the surrounding protoplasm, exactly as in the ordinary simple cell.

Another phenomenon in which the nucleus plays an important part is named by Haeckel "spore formation." Under this designation he comprehends all those cases in which—the idea of a previous fecundation being rejected—the nucleus breaks into numerous pieces, and each of these, apparently by becoming encysted in a portion of the protoplasm of the mother body, shapes itself into an independent cell—a so-called germ-globule (*Keimkugel*). Now this is a true spore—just as much so as the spores which arise quite in the same way in unicellular plants. The whole process is to be regarded as a case of the so-called endogenous multiplication of cells.

Most authors, however, take a different view of the nucleus. Following Balbiani, they regard it as an ovary; and to the fragments into which it breaks up they assign the significance of eggs; while the so-called nucleolus, which lies outside the nucleus, is, as we have seen, believed to be a testis in which spermatozoa are developed for the fecundation of the eggs.

We must bear in mind, however, that this "nucleolus" has been hitherto found in but a disproportionately small number of species, while the spermatozoal nature of the apparent filaments which have been noticed in it has by no means been proved; and we have already seen that some observed facts, such as those adduced by Bütschli, are opposed to the view which would assign to them the nature of true spermatozoa.

As Haeckel remarks, however, even though the so-called nucleolus be really a testis fecundating the eggs or fragments derived from the breaking up of the nucleus, this would afford no valid argument against the unicellularity of the Infusoria, for precisely the same sexual differentiation and reproduction are found in unicellular plants.

It may now, then, be regarded as proved that the process by

which the body of the ciliate Infusorium attains a certain degree of differentiation is repeated not only in other unicellular organisms, but in many parenchyma cells both of plants and animals. The difference, as Haeckel with much force points out, between the differentiation process of these parenchyma cells and that of the Infusorium body consists in the fact that in the parenchyma cells the differentiation is a one-sided one, conditioned by the division of labour in the organism of which they form the constituents, while in the Infusorium it is a many-sided one related to all the different directions in which cell-life manifests itself, and resting on a physiological division of labour among the "plastidules" or protoplasm molecules. In other words, the differentiation processes which in multicellular organisms are found distributed among different cells, are united in the single cell of the ciliate Infusorium, thus leading to the formation of an animal very perfect in a physiological point of view, but which morphologically does not pass the limit of a simple cell.

In some rarer cases the Infusorium body is found to enclose two or more nuclei, and Haeckel admits that such Infusoria must strictly be regarded as multicellular, since the nucleus in itself alone determines the individuality of the cell; but these exceptional cases have no significance for the main conception of the infusorial organism. The multiplication of the nucleus exerts almost no influence on the rest of the organization, and such "multicellular ciliata" are to be compared with the colony-building forms of the Acinetæ, Gregarinæ, Flagellatæ, and other undoubtedly unicellular organisms.

In conclusion, Haeckel considers the systematic position of the Infusoria. That they are genuine *Protozoa*, having no direct relation to either the Cœlenterata or the Worms, must be now admitted. To this result we are led in the most convincing way by all that we know of their development. In all the animal types which stand above the Protozoa, the multicellular organism is developed out of the simple egg-cell by the characteristic process of segmentation, and the cell masses so arising differentiate themselves into two layers—the endoderm and the ectoderm, or the two primary germ lamellæ.* Resting on the fundamental homology of these two layers in all the six higher types of the animal kingdom, Haeckel had already† directed attention to the fact that all these types pass in their development through one and the same remarkable form, to which he gives the name of Gastrula, and which he regards as the most important and significant embryonal form of the whole animal kingdom. This gastrula consists of a multicellular, usually oviform uniaxial, body enclosing a simple cavity—

* The comparison of the endoderm and ectoderm of the Cœlenterata to the two primary germ lamellæ of the Vertebrata was first made by Huxley.

† 'Die Kalkschwämme,' 1872.

the primordial stomach or intestine cavity, which opens outward on one pole of the axis by a simple orifice—the primordial mouth, and whose walls are composed of two layers, the endoderm or inner germ lamella, and the ectoderm or outer germ lamella.

This larval form has now been shown by the researches of Haeckel, Kowalevsky, Ray Lankester, and others, to occur in members of all the six higher primary groups of the animal kingdom; and Haeckel, in conformity with what he has called the biogenetic fundamental law*—the recapitulation of ancestral forms in the course of the development of the individual—had already in a former work† concluded in favour of a common descent of all the six higher types from a single unknown ancestral form which must have been constructed essentially like the Gastrula, and to which he gives the name of *Gastræa.*

From this common descent the Protozoa alone are excluded, these not having yet attained to the formation of germ lamellæ or of a true intestinal cavity.

He regards this difference between the development of the Protozoa and that of all the other animal types as so important, that he founds thereon a fundamental division of the whole animal kingdom into two great primary sections—the *Protozoa* and the *Metazoa.* The former never undergo segmentation, never develop germ lamellæ, and never possess a true intestinal cavity; the latter, which include all the other types of the animal kingdom, present a true segmentation of the egg-cell, have all two primary germ lamellæ—endoderm and ectoderm—a true intestine formed from the endoderm, and a true epidermis from the ectoderm; they all pass through the form of the gastrula, or an embryonic form capable of being immediately deduced from it, and (hypothetically) are all descended from a Gastræa.

The only Metazoa which in their existing condition have no intestine are the low worm-groups—Cœstoda and Acanthocephala; but these form only an *apparent* exception, for the loss of their intestinal canal is a secondary occurrence caused by parasitism, and Haeckel regards them as having descended from worms in which the intestine was present.

Several years ago Haeckel united into a separate kingdom, under the name of Protista, certain low organisms, some of which had been previously placed among the Protozoa, while others had been assigned to the vegetable kingdom. To this neutral group he refers the Monera, the Flagellatæ, the Catallactæ, the Labyrinthuleæ, the Micromycetæ, and the Acytariæ and Radiolariæ. After the elimination of these there remain as genuine Protozoa the Amœbinæ, the Gregarinæ, the Acinetæ, and, above all, the true Infusoria or Ciliata.

* 'Generelle Morphologie.' † 'Die Kalkschwämme.'

The union of the Protista into a distinct kingdom equivalent in systematic value with the animal or vegetable kingdom, can, however, scarcely be maintained. We already know enough of some of them to justify our assigning these to one or other of the two generally accepted organic kingdoms; and there can be little doubt that, did we know the whole history of the others, and were able to formulate the essential difference between the animal and vegetable kingdom, these, too, would be referred without hesitation either to the one or to the other, some passing to the former and others to the latter. The group of the Protista is thus at best but a provisional one, based partly on our ignorance of the structure and life history of the beings which compose it, and partly on our inability to assign to the animal its essential difference from the plant. Haeckel, however, has done well in specially directing attention to it, and in his admirable researches on many of the organisms which he has thus grouped together he has largely contributed to our knowledge of living forms.

I have thus dwelt at considerable length upon this important paper of Haeckel's, because I think that it not only brings out in a clear light the essential features of infusorial structure and physiology as demonstrated by recent research, but that it goes far to set at rest the controversy regarding the unicellularity and multicellularity of the Infusoria.

Balbiani * has quite recently published a very interesting account of the remarkable Infusorium long ago described by O. F. Müller under the name of *Vorticella nassuta*, and more recently taken by Stein as the type of his genus Didinium.

The animal (Fig. 5), which is somewhat barrel-shaped, with an anterior and a posterior wreath of cilia, has one end continued into a proboscis-like projection which carries the oral orifice on its summit, while an anal orifice is situated on the point diametrically opposite to this. There is a very distinct cuticle, though the rest of the cortical layer is very thin, and can scarcely be optically distinguished from the internal parenchyma, which exhibits manifest currents of rotation. These flow in a continuous sheet along the walls from the anal towards the oral side, and on arriving at the mouth turn in towards the axis and then flow backwards along this until they complete the circuit by once more reaching the anal side of the body. No trichocysts are developed in the walls of the body. The contractile vesicle is large, and is situated near the anal end; it presents very distinct pulsations, and Balbiani is disposed to believe in a communication between it and the exterior.

During the act of digestion a tubular cavity can be seen running through the axis of the body, and connecting the oral and anal

* 'Arch. Zool. Exper.,' vol. ii.

orifices. This is regarded by Balbiani as a permanent digestive canal. The post-oral or pharyngeal portion of this tube possesses a very remarkable feature, namely, a longitudinal striation caused by rigid rod-like filaments which are developed in its walls, and which can be easily detached and isolated by pressure or by the action of acetic acid. They then resemble some common forms of the raphides developed in the cells of plants. The function of these rods becomes apparent when the animal is observed in the act of capturing its prey. The Didinium is eminently voracious and carnivorous, and when in pursuit of other living Infusoria, such as Paramœcium, the prey may be seen to become suddenly paralyzed on its approach. A careful examination will then show that the Didinium has projected against it some of its pharyngeal rods, and to the action of these bodies the arrest of motion is attributed. A curious cylindrical tongue-like organ is now projected from the mouth towards the arrested prey, to which it becomes attached by its extremity (Fig. 5 *a*). By the retraction of this tongue the prey is now gradually withdrawn towards the mouth, engulphed in the distended pharynx (Fig. 5 *b*), and pushed deeper and deeper into the axial canal, where it is digested, and the effete matter ultimately expelled through the anus.

From all this Balbiani concludes against the unicellular doctrine. He sees in the axial cavity a permanent alimentary canal, and in the surrounding parenchyma a true perigastric space filled with a liquid which corresponds with the perigastric liquid of the Polyzoa, and of many other lower animals. He is not, however, disposed to make too broad a generalization, and to insist on the presence of an alimentary canal distinct from a body cavity in all the other Infusoria. Here, however, he falls in with the views of Claparede and Lachmann and of Greeff, and maintains that as a rule the digestive and body cavity in the Infusoria are confounded into a single gastro-vascular system.

Independently, however, of the untenableness of the conception of a united digestive and body cavity, it does not appear to me that Balbiani makes out any case against the unicellularity of the Infusoria. He admits that except in the pharyngeal and anal portion there is no evidence of a differentiated wall in his so-called digestive canal, and even though it be conceded that the middle portion of this canal constitutes a permanent cavity in the parenchyma, it would not differ essentially from other lacunæ permanently present in the protoplasm of many undoubtedly unicellular organisms. It has been already remarked that a communication between these lacunæ and the external medium is paralleled in many simple cells, and these external communications in Didinium present no feature essentially different.

The pharynx appears to be bounded by an inflection of the

cortical layer, and I believe we may regard the rod-like corpuscles here present as a peculiar modification of the trichocysts which in many other Infusoria are developed in the cortical layer of the body. The projectile tongue-like organ is one of the most remarkable features of Didinium; we must know more, however, than Balbiani has told us of it, before we can decide on its real import. It is not improbably a pseudopodial extension of the protoplasm.

Balbiani has followed the Didinium through the process of transverse fission. This is preceded by the formation of two new wreaths of cilia, between which the constriction and division take place, each half previously to actual separation developing within it such parts as it had lost in the act of division. The only part which in this act becomes divided between the two resulting animals is the nucleus. The so-called nucleolus was not seen by Balbiani; and though he observed two individuals in conjugation by their opposed oral surfaces, he never witnessed anything like the formation of eggs or embryos.

I believe I have now laid before you the principal additions which during the last few years have been made to our knowledge of the Infusoria. But though it will be seen that the labourers in the special field of microscopical research, to which I have confined this address, have been neither few nor deficient in activity, it must not be imagined that the subject has been exhausted, or that many questions, more especially such as relate to development, do not yet await the results of future investigations for their solution.—*Anniversary Address to the Linnean Society*, May 24, 1875.

III.—*Extracts from* Mr. H. E. Fripp's *Translation of* Professor Abbe's *Paper on the Microscope.*

A careful consideration of the means at the disposition of the optician, and a critical comparison of the difficulties serving as a guide to the discussion of the conditions influencing them, have led me to the conclusion that lenses and systems of lenses of which each part has prescribed dimensions, can be executed with an exactitude that fairly ensures correct action, and with greater facility than any other mode of procedure offers for the fulfilment of the same conditions with equally good results.

In the workshops of C. Zeiss, of Jena, the construction of objectives, from lowest to highest power, is regulated by strict calculation for each single part, each curve, each thickness of glass, each degree of aperture; so that all guesswork and "rule of

thumb" is avoided. The optical constants of each piece of glass are obtained from trial-prisms by means of the spectrometer. Each constituent lens is ground as nearly as possible to its prescribed dimensions and accurately fitted. In the highest-power objectives only is the lens distance left variable, in order that slight deviations from accuracy may be adjusted. And thus it has been shown beyond dispute that a well-grounded theory, combined with rational technical processes, may be successfully substituted for empirical practice in the construction of the microscope.

The fact that an amount of angular aperture, which is unknown in any other instrument, comes here into question, renders the accepted ideas of "aberration" entirely useless, and the result of investigations which were undertaken in order to bring the question to some issue, was the discovery that an important feature in the optical functions of the microscope had been hitherto overlooked. In all previous explanations or interpretations it has been accepted as a self-understood proposition that the formation of an image of an object in the microscope takes place in every particular, according to the same dioptric laws by which images are formed in the telescope, or in the camera; and it was, therefore, tacitly premised that every function of the microscope was determined by the geometrically traceable relations of the refracted rays of light. A rigorous examination of the experiences upon which the traditional distinction of "defining" and "resolving" powers is founded, has shown that the proposition is not admissible. It holds good, indeed, for certain cases, capable of definite verification, but for the generality of objects, and particularly for those objects on which the microscope is supposed to exhibit its highest quality of performance, it appears that the production of microscopic images is closely connected with a peculiar and hitherto neglected physical process, which has its seat in and depends on the nature of the *object* itself, although the measure of its effect stands in direct dependence upon the construction of the *objective*. It is hence possible not only to fix the limits of the visible, beyond which no further resolution of structure could be expected, but also to bring to light the fact that a microscopic image which may be entirely free from error in itself, and therefore be supposed to represent in all cases the true structure of an object, nevertheless does *not* do so for a whole class of objects and observations.

In addition to those images *of the object* which are thrown off by the lenses of the microscope, a series of *associated images of the aperture* are simultaneously thrown off, which together form an image of the outwardly projected plane of aperture. This latter (aperture image) is thus associated with the final virtual image of the object, and appears at the eye-point, so called, above the ocular

where it may be examined with a lens. But the image of the object, so far as it is produced by the objective alone, lies in or close to the upper focal plane of the objective, where also it may be seen by looking down the tube of the microscope with the naked eye. These two sets of images are interconnected by common relations, the determination of which affords a key to the solution of questions scarcely to be approached by any other means. All the characteristics of the *object images* hang together with certain characteristics of the *aperture images*, and *vice versâ*.

The principle on which is founded the study of these aperture images leads to various results, depending for their full development upon a principle which constitutes at the same time a law of fruitful application throughout the whole theory of the microscope, and which may be thus formularized.

When an objective is perfectly aplanatic for one of its focal planes, every ray proceeding from this focus strikes the plane of the conjugate focus at a point, whose lineal distance from the axis is equal to the sum of the equivalent focal length of the objective × the sum of the angle which that ray forms with the axis.

Now as this condition must be fulfilled in every correct instrument, both for the objective and for the whole optical part of the microscope, the formula above given establishes a relation of quantity between the *angle of aperture* of the microscope and the lineal diameter of the aperture images above the objective and ocular.

Moreover, it is thus possible to determine, by micrometric measurement of the position in the upper focal plane of the objective which the track of any ray occupies, the direction which it took before entering the microscope. Consequently the aperture images formed above the objective, when examined with a suitable micrometer eye-piece, can be used for measurement of the divergence which the rays coming from the object undergo.

In the next place, we need a more characteristic exposition of the optical functions which, in the case of images formed under larger angles, by rays having a *great* inclination to the axis, differ greatly from the abstraction by which theory represents the action of a set of lenses in forming an image. And such an exposition offers itself when we can define by axioms of general validity the mode in which an image is focussed and spread out on the focal plane of an optical system, and distinguish the *focussing function* and the *extension of image* over a surface as the two principal factors of the image-forming process, alike independent in their abstract idea, and distinct in actual specific function. Apart from the fact that no exhaustive analysis of a faulty image nor any means of perfect correction are possible until such characteristic distinction can be laid down, we have no other means of determining

the part taken by each constituent element of a compound system of lenses in the joint performance of the whole. When then we define the function of the objective to be the production of a real image, and the function of the eye-piece the amplification of this image,—such explanation does not by any means reach the essential principle of action of the compound microscope. This is obvious at once when we consider that by such a definition the combination of objective and eye-piece is made only to indicate *magnifying power*, whereas on the contrary the remarkable superiority of compound over simple microscope consists in the *quality of its performance*. By the *objective* an image is formed and spread out in what is an almost perfect accordance with the laws by which images of infinitely small elements of a surface are formed. By the *eye-piece* a displacement of focus is effected; that is to say, a change of divergence of each separate pencil of light takes place till the divergence is almost imperceptible, and the pencils infinitely fine.

The first step or act in the image-forming process consists, not in the production of a reversed image by the objective in front of or within the ocular, but rather in the production of a "virtual" image at an infinite distance with parallel rays. The *second* act comprises the last refraction through the posterior surface of the objective, and the several refractions taking place in the ocular by which the image is re-formed at the distance of clear vision with diverging visual angles. The first act answers plainly to the function of an ordinary "magnifying glass"; while the second, taking all the changes comprised therein together, answers as obviously to the functions of the telescope (possessing only a small objective aperture) to which the virtual image formed by the first process serves as "object."

This interlocking of objective and ocular functions—presenting the combined effect of a magnifying glass and that of a telescope—must be laid down as the most general and correct characteristic of the principle upon which the compound microscope of the present day is constructed.

From the foregoing remarks may be gathered a theory of aberrations, sound and strong enough to master the difficulties which the application of exceptionally large angles of aperture to microscope objectives has occasioned.

It appears that the faults of image formation are separable into two distinct classes, one comprising faults of the focussing act (aberrations in the strictest sense), the other comprising faults of the amplifying function. To the first class belong those spherical and chromatic aberrations commonly studied; in the second class must be placed a series of peculiar deviations of rays of light from their normal course, which arise from the circumstance that the separate rays of a homofocal beam occupying the aperture

of the lens yield unequally magnified images, according as their inclination to the axis varies, and according also to the unequal refrangibility of the different colours—an inequality which obtains just as much whether the several partial images are compared with each other, or whether within the area of each image different positions in the field of vision are compared.

This class of anomalies affects exclusively the constitution of the image outside the centre of the field. The perfection with which the rays unite in the central region, and therewith the maximum capacity of performance, depends on the contrary entirely on the real aberration spherical and chromatic, as commonly understood.

Chromatic aberrations, as they show themselves where a large angular aperture is used, do not depend alone on those differences of focus which affect the image-forming beams as a whole; but quite as much in an unavoidable inequality of coincidence of colours of variously inclined pencils of rays within the angle of aperture, which manifests itself in this, that an objective which is perfectly achromatic when direct illumination is used must be more or less *over*-corrected for use with oblique illumination. Although the first-mentioned ordinary form of colour dispersion (primary and secondary) may be entirely removed or rendered scarcely noticeable, the last-named source of chromatism cannot be counteracted or removed by any known material or any known technical treatment.

Spherical aberration on a stricter examination of its causes resolves itself into a series of independent elements which as they increase in number, follow, with the increasing inclination of the rays towards the axis, a more and more unequal course. An absolute effacement is only possible theoretically for the two first members of the series. As soon as the angular aperture exceeds a small number of degrees, the counteraction of spherical aberration can be effected in no other manner than by compensating the irremovable errors of the higher elements through intentionally introduced residual aberrations of the lower ones. The accumulation of unavoidable deficits which this method of compensation necessarily leaves unremedied, compels a limitation of the angle of aperture. For angles of aperture exceeding 60° and *a fortiori* for the very large angles of modern objectives, the pre-supposition of an adequate compensation is found in the well-known type of construction where a plain nearly hemispherical front lens is combined with a strongly *over*-corrected system of lenses. The discovery of this mode of construction must be looked upon as the basis of every improvement which has been introduced since. For a system of lenses made to use in air, the limit of serviceable aperture proves to be from 105° to 110°, beyond which it is not possible to counteract sufficiently the spherical aberration, except by lessening the focal distance of

front lens from the object to a degree which makes it practically useless. The application of the immersion principle renders it possible to overcome spherical aberration, where even the *maximum* angular aperture is used. It is in this power of using very large angles of aperture, and also in avoiding loss of light, that the real advantage of the immersion plan lies. It will indeed be seen from what follows, that these two facts fully explain the undoubted superiority of the immersion lens.

Every appliance by which the amending of spherical aberration has been attempted—whether by correcting lenses placed above the objective or by construction of ocular—will produce no better result than what is already effected by changing the distance of the front lens of the objective from those behind it. They simply permit the existing residual aberration to be transferred—shifted backward or forward between the centre and outside border of the aperture—and by this means to keep, for a time, some particular zone of the objective more or less free from aberration, *at the cost of the rest!*

In an analysis of the conditions which belong to a perfect construction, it becomes obvious that the factors on which correctness of image in the centre of the field, and the maximum of good performance depend, namely, chromatic and spherical aberration, pertain to the functions of the *objective* alone, upon which no influence of the eye-piece, however constructed, can produce any marked effect. Arguments advanced in favour of a long tube or of a short tube are untenable in theory; and the supposed differences of effect have no real existence when examined under conditions which are truly comparable. There will be found in every objective a particular angular amplification obtainable at will by means of length of tube and strength of ocular, which must exactly suffice to enable any eye possessing normal capacity of vision to recognize all the details that can possibly be delineated in the virtual image formed by the objective. And this, which may be termed "necessary *angular* amplification," may be looked upon as the measure of the relative perfection of the objective.

Theoretical study of the aberrations of the image-forming rays, and practical experience involving the application of methods to be hereinafter described, and the careful testing of a considerable number of objectives of recent date from the best workshops on both sides of the Channel, have led Professor Abbe to the conclusion that the numerical value of "necessary amplification" yet arrived at or attainable at present, is altogether much lower than might be supposed from the liberal way in which microscopists deal with thousands and tens of thousands. According to his experience, the capacity of the most perfect objectives, the usual forms of illumination being assumed, is exhausted with an eightfold *angular*

amplification, so that every detail that can be possibly delineated by an objective in its "virtual" image is certainly accessible to any eye possessing normal vision, when the tube and ocular, taken together, represent a telescopic magnifying power of eight times. Even this performance is only reached in the case of low and middle power objectives; for when the focal length is less than $\frac{1}{8}$ inch, the relative perfection of construction perceptibly fails, on account of the rapidly accumulating technical difficulties, and there certainly does not exist an objective of $\frac{1}{25}$ inch focus whose optical capacity exceeds a fivefold *angular* amplification.

From all this may be gathered how utterly futile any efforts to obtain disproportionately high amplifications by means of specially constructed eye-pieces must prove; and, as regards any expectation of exalting the performance of the instrument by further shortening of the focal length of the objective, there stands in the way one objection, which, in the present state of our knowledge, is absolute and insuperable—namely, that the imperfections resulting from residual aberrations and defective technical manipulation increase with every addition of magnifying power. This form of diffraction, likewise, turns the image of each point in an object into a dispersive circle of greater or less diameter; but the resulting diminution of optical capacity, while scarcely noticeable in objectives of moderate power, compared with the effect of residual aberrations, becomes very serious with the higher powers. Assuming the magnitude of angle of aperture 180° in air, which cannot be exceeded beyond a few degrees, even by immersion systems, we find, e. g. for an amplification of 1000, the diameter = $\frac{1}{50}$ inch, and for amplification of 5000 = $\frac{1}{250}$ inch, without reference to the mode in which the amplification is obtained (through objective and ocular). And if we would know what conditions are involved in such amplifications — as, for instance, 5000 fold — we have only to make a puncture of $\frac{1}{250}$ inch diameter with a needle in a card or piece of tinfoil, and through this opening to look at some brightly illuminated object, which has well-defined edges (e. g. a candle flame), and we shall have before our eye of what must be the appearance of the outlines of a microscopic object magnified 5000 times, even if the microscope itself were absolutely perfect, the diffractive effect excepted.*

Taking all these circumstances into consideration, it must be concluded that no material exaltation of the absolute power of the microscope, beyond what is attainable at present with objectives of $\frac{1}{25}$ inch focal length, is to be expected in the future, either by shortening of focus or by further improvement of construction. And as there exists at this moment no microscope whose *serviceable*

* Due to smallness of aperture of a minute lens, and to be carefully distinguished from the diffraction which is caused by the *structure* of objects.

magnifying power reaches even to 4000, so will there be none in the future. On the contrary, the facts just stated show that amplifications of less than half 4000—such as are readily obtained with objectives of $\frac{1}{25}$ inch, and seem really *serviceable*—are nevertheless not available in practice. The final inference from these data is that improvement of the microscope should no longer be sought for by aiming at still higher magnifying power and amplification, but rather at a more correct performance of the middle and moderately high powers. It will be a real advance of the optician's art, and of infinite service to the scientific use of the microscope, when we succeed in accomplishing with objectives of $\frac{1}{6}$th and $\frac{1}{8}$th what is now only attained with much higher powers. Such an aim is within the range of what is possible.

In the account of Professor Abbe's researches, to be hereafter published, new and exact methods will be given by which every determinable point in the construction of the microscope, e. g. focal length of each lens, angle of aperture, character and limits of objective and ocular functions may be empirically ascertained; and, in addition to this, a mode of procedure described which renders it possible, with very simple means, to examine in instruments already made, every fault of definition of image, and thus to determine their relative excellence. The methods commonly recommended for testing the state of spherical and chromatic correction of the objective are not adequate to the actual requirements of the case, and quite fail to explain the true character of the aberrations.

The principle upon which the mode of proceeding to which reference has been made above, may be here generally indicated. As test-object, a preparation is used which presents only sharply outlined black and white lines alternating with each other, and *lying in the same plane*, so that no deviation can occur in the course of the rays transmitted through it. A preparation of this kind, sufficiently perfect for all practical purposes, may be made by ruling groups of lines, with the aid of a dividing machine, on the metallic film of silver or gold fixed by known methods on glass, and having no greater thickness than a fractional part of a micro-millimeter (1 micro-mm. = $\frac{1}{25000}$ inch). Covering glasses of various thicknesses (accurately measured) are ruled on their under surfaces with lines $\frac{1}{250}$ to $\frac{1}{1250}$ to the inch, and cemented on a glass slide with balsam, one beside the other. A preparation of this kind serves for the highest as well as lowest powers. The illumination must be such that light may be reflected simultaneously from several sides upon the object, and means provided for regulating at will the course of any pencil entering within the angle of aperture of the objective to be tested.

The testing process has for its aim to view the co-operation of every zone of the aperture, whether central or peripheral, and yet,

at the same time, to be able to distinguish and recognize the images which each zone delivers separately. For this purpose the illumination is so regulated that every zone of the aperture shall be represented in the image formed at the upper focal plane by tracks of the entering pencils of light, yet so that for each zone a small streak only of light be let in, and that the tracks be kept as widely apart from each other as possible. If an objective be absolutely perfect, all these images should blend *with one setting of focus* into a single, clear, colourless picture.

A test image of this kind at once lays bare in all particulars the whole state of correction of the microscope, With the aid which theory offers to the diagnosis of the various aberrations, a comparison of the coloured borders of the separate partial images, and an examination of their lateral separation and their differences of level, as well in the middle as in the peripheral zones of the entire field, suffice for an accurate definition of the nature and amount of the several errors of correction, each of them appearing in its own primary form.

Assuming the theoretical knowledge and practical experience necessary to carry out such an inquiry properly, and to estimate its results correctly, the mode of procedure above described affords so exhaustive an analysis of the qualities of an objective, that when, in addition, its focal length and angle of aperture are ascertained, its whole capacity of performance may be determined beforehand. Whoever has once examined in this manner even good objectives which have proved to be excellent in practice, will be as little disposed to accept childish assertions of their perfectness as to advance on his part absurd pretensions which no one has yet made good.

That the performance of the microscope does not always depend solely on the geometrical perfection of the image, but also, in addition to this, in certain classes of objects, upon amount of angular aperture, is a fact long recognized. The exact significance of this fact has nevertheless remained as problematical as the exact nature of the quality of "resolving" or discriminating power. It remained a question, What value might be assigned to the quality thus related with angular aperture, and does its significance extend any farther than to certain cases in which shade effects were supposed to be produced by oblique illumination?

In the endeavour to establish a theoretical basis for the construction of the microscope, it was a matter of the first importance to define the exact function of angular aperture in the normal performance of the microscope, lest I should fall into a misdirection of my labours towards aims of very problematical worth.

As, then, it was important above all things to ascertain more exactly than has been hitherto set forth the actual facts respecting

the operation and effect of angular aperture, I endeavoured to determine by experiment in what cases a distinct advantage resulted from larger angular aperture, and in what cases no such advantage could be perceived. For this purpose a series of objectives, differing widely in focal length and angular aperture, were constructed, according to my calculations, and their accuracy tested, so as to afford a certainty of correctness. The test-objects employed included prepared insect scales of various kinds, diatom valves, striped muscle fibre, diamond-ruled lines on glass, groups of lines on silvered glass, fine and coarse powdered substances, and, besides these, the minute optical images of natural objects (lattice bars, wire-net) obtained by means of air-bubbles, or, preferably, by objectives of short focus, fitted to the stage of the microscope.

These experiments yielded the following results:

(i.) So long as the angle of aperture remains within such limits that no noticeable diminution of sharpness of image results from its diffraction effect, no sensible improvement in the delineation of the outlines of the object takes place, provided these parts are not of less size than $\frac{1}{2500}$ inch.

(ii.) On the other hand, the difference is wholly in favour of the larger aperture for every object which yields details minuter than the limits above given; and this quite irrespective of the question whether such details are due to unevenness of surface or to unequal transparency in an infinitely thin layer, or whether the detail takes the form of striation, granulation, trelliswork, or images of natural objects reflected from bubbles or produced by refraction of lenses.

(iii.) The smaller the linear dimension of such details, so much the larger must be the angle of aperture of the objective, if they are to be made out with any definite kind of illumination, e.g. whether purely central or very oblique: and this irrespective of the more or less marked character of the delineation and of the focal length and necessary amplifying power of the objective.

(iv.) When the detail in the real object appears in the form of striation, groups of lines, &c., a given angular aperture always reaches much finer details with oblique than direct illuminations, and this irrespective of the circumstance that the constitution of the object admits or excludes the possibility of shade effects.

(v.) A structure of the supposed kind, which is not revealed by an objective used with direct illumination, will not be rendered visible by inclining the *object itself* at any angle to the axis of the microscope, even when, lying at right angles with the axis, it is perfectly resolved by oblique illuminations. Resolution, however, follows at once when the incident light is directed perpendicularly to the plane of the object, as it lies inclined to the axis. Hence the increased effect of oblique illumination depends solely on the

inclination of the rays towards the axis of the instrument, and *not* upon the oblique incidence of light on the object.*

The facts here brought forward show, on the one hand, the reality of a special optical quality, directly related with the *angular aperture* of the objective, yet independent of any special perfection or amplifying power possessed by it, and also show it to be a "resolving" power or capacity of separating minute detail, conformably with the literal sense of the term employed. On the other hand, they show unequivocally that the delineation of images of minute details of structure must take place under conditions essentially different from those under which the contour outlines of larger parts are formed. In all cases where a "resolving" power of this kind is in operation, the reunion of rays proceeding from the several points of the object in the focal plane of the image is most certainly not to be accounted an adequate explanation of the images of such details of the object, for on such a supposition the differences would remain absolutely inexplicable. The result, then, of this preliminary study is to give the following form to the inquiry, namely, to find out the special causes *outside* the microscope which operate in the formation of images of small structural details, and then to determine individually the mode and manner of their intervention.

* Vide Wenham, in 'Monthly Microscopical Journal,' April 1, "On a Method of obtaining Oblique Vision of Surface of Structure," &c. The optical principle enunciated by Mr. Wenham is totally irreconcilable with Professor Abbe's theory and experimental investigations.

(*To be continued.*)

PROGRESS OF MICROSCOPICAL SCIENCE.

Bony Tissue, not prepared Plain, but with Aniline Dye.—M. Ranvier—no mean authority on the subject of bone tissue—recommends the adoption of the following method in the 'Archives de Physiologie,' which is abstracted by the 'Medical Record': A portion of the shaft of a long bone is procured, and immediately on removal from the body is plunged into water. It is allowed to macerate in this for the space of a year; the water in the mean time being repeatedly changed. At the end of that time the bone will be found to have become as white as ivory, and quite free from any adhering tissue. The object of immediately plunging the bone in water is to prevent the infiltration of the canals and substance of the bone with fat.

When the bone is thoroughly macerated, sections of it are made with a saw. These sections are ground down on pumice-stone, and finally polished on a harder material. In order to remove the powdered fragments of bone which have been ground off from the canals and lacunæ on the surface, it is sufficient to scrape the section with a scalpel. It is then placed in a warm solution of the aniline, and allowed to remain there for two hours, and afterwards dried on a water-bath.

The section is next rubbed on a hone, moistened with a 2 per cent. solution of common salt. It is then washed in this solution, and permanently mounted in a mixture of equal parts of the solution of salt and glycerine.

In objects prepared in the above manner three important facts, not previously noticed, may be observed.

The first is the existence of lacunæ or corpuscles, consisting of a simple slit, not much larger than a canaliculus. The fact of their being lacunæ is proved by the relation in which they stand to the canaliculi, which is precisely the same as that of other lacunæ. The name given to these fine atrophied corpuscles or lacunæ is *confluents lacunaries.* They are lacunæ either partially or completely atrophied. This observation bears out the theory of the disappearance of the lacunæ with age. But this disappearance is not due to the lacunæ being filled up with fresh bone, but rather to a process of atrophy.

The second interesting fact rendered clear by this method is, that the canaliculi which are given off from the outer sides of the external lacunæ of each Haversian system proceed for a short distance as though they were going to inosculate with a neighbouring system. They then turn on themselves, and inosculate with other canals belonging to their own system. These are called *canalicules récurrents.* From this fact we may conclude that each Haversian system forms a complete structure by itself, and represents the elementary bone.

The third fact relates to the structure which intervenes between the Haversian system. In transverse section there may be observed, in these islets of bone, certain small circles which represent the fibres of Sharpey divided transversely. These circles are only to be seen in

the intermediate structure, never in the Haversian system. This fact proves that the substance in these localities is developed from the periosteum. The relation which the corpuscles and canaliculi bear to the fibres of Sharpey may be briefly stated as follows. The corpuscles are placed in the angles formed by the intersection of these fibres. The canaliculi surround the fibres, but do not pass through them. This last fact, taken into consideration with that of the recurrent canaliculi of the Haversian system, proves that the canaliculi are spaces left in the substance of the bone at the time of its development, and not fissures made during the preparation of the section.

Fecundation of Thecaspore Fungi.—The 'Academy' (July 10, 1875), in its occasional and very interesting microscopical notes, gives the following account of a paper in the 'Comptes Rendus,' to which the author's name is not appended. It gives a description of phenomena of copulation observed in *Hypomyces asteroplehorus*, and *Dothidia Robertioni*, the latter parasitic on *Geranium Robertianum* (Herb Robert). The generative process corresponds with what had been previously observed by MM. de Bary, Woronin, and Tulasne, in other thecaspores. The report observes that "these interesting facts generalize the phenomena already observed in a very different group, and help to confirm the opinion that the fecundation of thecaspore fungi is effected in the mycelium, and thus precedes the formation of the organs that form the spores." Speaking of spermatia, the report continues: "It is known that M. Tulasne has given this name to bodies of very great tenuity developing regularly on the surface of many Thecaspores and Uredines, or in special conceptacles, and which have been considered as concerned in the work of fecundation." The discovery of the fecundation of these fungi by organs springing from the mycelium—a discovery to which M. Tulasne contributed—rendered very problematical the fecundating action attributed to the spermatia. The author of the memoir before us shows that the spermatia can germinate when placed under suitable conditions, which, for hypoxylous species, consist in adding to water a little tannin and sugar, and leaving them in contact with air. The spermatia of Uredines germinate in pure water, but their development appears to be very different from that of the hypoxylous sorts.

Development of the European Lobster.—A writer in a recent number of 'Silliman's American Journal,' whom we take to be Mr. Samuel H. Scudder, but whose signature is S. I. S., writes as follows on the above subject: Dr. Sars has also recently published a paper of twenty-seven pages, illustrated by two autographic plates, on the post-embryonal development of the European lobster (*Homarus vulgaris*, Edwards). He describes and figures in detail the three larval stages corresponding precisely with the first three stages which I have described in the American lobster. Dr. Sars did not receive my papers until after a part of his memoir was printed, so that his investigations were wholly independent. In a short appendix, Dr. Sars calls attention to the remarkable agreement in the results at which we had each arrived, and to the excellent opportunity afforded for a careful comparison of the

early stages of these two closely allied species. Although the corresponding stages agree so closely in form and structure, they are from the first readily distinguishable by well-marked specific differences in the form and armature of the appendages. In fact, the differences appear greater in the larval stages than in the adults. Dr. Stars was not able to trace the development beyond the third stage, which he had at first supposed could not be the last stage of the larva, but after comparison with the latter stage of the American lobster, he regards it as quite probably the last true larval stage.

Comparative Anatomy of the Placenta.—Professor Turner, F.R.S., who has been lecturing on this subject before the Royal College of Surgeons, devoted his second lecture to a consideration of the changes which occur in the uterine mucous membrane during pregnancy. He pointed out that as soon as the ovum was received into the uterus the mucous membrane swelled. The *epithelium* often, though not always, loses its columnar form, and the cells multiply in order to cover the increased surface. The *subepithelial tissue* increases enormously, which is due to the multiplication and development of the corpuscles already stated to be abundantly distributed through its tissue. The tubular *branched glands* are separated to a much greater distance from each other in consequence of the growth of the subepithelial tissue. They are augmented in size. The whole membrane becomes much more vascular. On its surface pit-like depressions may be seen, which were formerly thought to be the enlarged mouths of the glands, but which the Professor himself had ascertained to be new-formed pits or crypts in the interglandular tissue. The relation of these pits he next proceeded carefully to describe, taking the diffused form of placenta of the pig, which is the simplest form of placenta, as an example.

When the mucous membrane of the pig is examined after the entry of the ovum, it was found to present a series of folds, which, however, like the rugæ of the stomach, are only preparatory to its great subsequent dilatation, since they disappear in the distended state. On examination with a hand lens fine furrows are seen, which correspond to the ridges on the chorion. Also a series of spots corresponding to the spots of the chorion; these spots are feebly vascular. Between these are a number of crypts, which are highly vascular. The glands in the pig are tubular and much branched; they open upon circumscribed areas free from crypts; and hence, which is a very important point, there is no relation between the glands and the crypts. The crypts are lined by columnar and probably by ciliated columnar epithelium. The villi of the chorion are received into the crypts, and *not* into the mouths of the glands. The secretion of the glands is not brought into direct relation with the villi of the fœtus.

In the mare the mucous membrane is also thrown into folds, and there are here also polygonal areas with intervening ridges. Each area presents a pattern formed by numerous crypts; the walls of the crypts are highly vascular, the ridges between the collections of crypts are but feebly vascular; the glands here, again, do not open into the crypts, but irregularly upon the ridges; the crypts, therefore, as in the pig, which receive the villi, are interglandular, and are not the

glands themselves. Professor Turner has not satisfied himself that the cells in the crypts are ciliated, but they are more or less columnar, swollen, and sometimes binucleated. In the cetacean, *Orca gladiator*, which Professor Turner has studied specially, the ridges are tolerably regularly arranged in parallel rows. But the crypts exist both on the ridges and on the furrows, and Professor Turner at first thought the glands opened into some of the crypts, and it seemed as if the crypts were only the enlarged mouths of the glands, but he soon found that, as the crypts are pretty uniformly distributed, the glands must open into some of them, and that here again the crypts cannot be regarded as the mouths of the glands.

Professor Turner observed that some of these points had been clearly seen by Eschricht in the porpoise many years ago, the crypts being named by him cellulæ, and the bare comparatively non-vascular spots areolæ.

The cotyledonary placenta of the ruminants was then considered. It was shown that here also the crypts, with the intercryptal substance, enlarged immensely at certain spots, forming the cotyledons, whilst the glands were pushed aside, as it were, and the fœtal villi did not penetrate into their interior.

Effect of Curare on the Emigration White of Blood-globules.—An anonymous writer says that if the blood of a frog poisoned by curare be examined on the second or third day of immobility, it is found to contain no leucocytes; these, however, reappear as the power of voluntary movement is restored, and gradually resume their customary proportion to the red corpuscles. This observation was made by Drozdoff, who attributes the phenomenon to a specific poisonous action of the drug on the colourless corpuscles; he found that the addition of a minute proportion of curare to a drop of blood, after its removal from the body, speedily arrested the amœboid movements of the leucocytes, and rendered their protoplasm granular. Tarchanoff, working under Ranvier's direction,* repeated Drozdoff's experiments, the results of which he partially confirms, while explaining them in a very different way. The gradual disappearance of leucocytes from the blood of the curarized frog is an undoubted fact; but the phenomenon cannot be ascribed to any specific action of curare upon protoplasm. The addition of curare to drops of blood in the moist chamber yielded results which were by no means constant; some samples of the drug speedily destroying the colourless corpuscles, while others appeared in no way to influence their vitality. What then is the cause of their disappearance (which is never really absolute) from the circulating fluid? They migrate into the perivascular spaces, and accumulate in the lymphatic sacs and serous cavities. This emigration is associated with a considerable transudation of the fluid constituents of the plasma, so that, *pari passu* with its increasing poverty in leucocytes, the blood is observed to contain an abnormally large proportion of red disks. Both processes may be accounted for by the paralyzing effect of the drug upon the vasomotor nerves;

* 'Archives de Physiologie,' Janvier-Février, 1875.

the arterioles are everywhere dilated, the intravascular tension lowered, and the blood-current uniformly retarded. Exactly similar phenomena may be produced by destroying the cerebro-spinal axis, and so paralyzing the vasomotor nerves. But why does the lymph, after its escape from the vessels, accumulate in the serous and lymphatic cavities? Why does it not make its way back into the current of the circulation? For this there are two reasons: first, the paralysis of the voluntary muscles, whose contractions are largely instrumental in the onward propulsion of the lymph; secondly, the arrest of the lymphatic hearts. As the effects of the poison pass off, these causes cease to operate, and the exuded constituents of the blood return to their normal home within the vessels.

Cause of a Disease in Cabbages.—A recent writer says that whilst seeking for the cause of the swellings which occur so often in the roots of the cabbage tribe and injure their growth, Herr Woronin found a fungus in the parenchyme cells. He describes it, when young, as a plasmic body with a lively motion. After a time it settles down and grows; "the granules of the plasma collect into small bodies lying close together, and form a round mass covered with a membrane." These are spore-cells, and as the plant rots, "zoospores or small amœbæ escape from the fungus, and, penetrating young sound rootlets, cause fresh plasmic bodies to be formed in their parenchyme cells." When seed from healthy plants was sown in soil containing the decaying matter of sick plants, and wetted with water containing the fungus spores, the fresh plants were attacked, and their roots exhibited the characteristic swellings. M. Woronin supposes this fungus to belong to the Mucedines or the "Chytrideneen." *

The Germination of Chara.—The 'Academy' (August 28) says that in the 'Botanische Zeitung' Professor de Bary has recently contributed an exhaustive article on the germination of the *Characeæ* in general. In the main the results of his researches confirm Pringsheim's views, as published in 1864,† especially with regard to the nature of the pro-embryo, which is not a direct prolongation of the oospore, but the outgrowth of one of two divisions of the latter equal in all respects at the time of partition, though subsequently exhibiting a different development. The direction and nature of the earlier divisions, formation of nodes, appearance and development of adventitious pro-embryos and roots, &c., are fully described and illustrated. But it is impossible to epitomise an article of this nature, or rather the article itself is unintelligible without the accompanying figures. It contains, however, some additional notes on parthenogenesis in the genus *Chara*, which, the author asserts, is established beyond doubt.

* "Der Naturforscher," No. 24, 1875, cited from 'Botanische Zeitung,' 1875, No. 20.

† 'Jahrbuch für Wissenschaftliche Botanik.'

NOTES AND MEMORANDA.

Powell and Lealand's new $\frac{1}{8}$ inch.—The 'Academy,' which occasionally has valuable microscopical notes, states that the new $\frac{1}{8}$th of Powell and Lealand is a decided advance in accuracy of correction, and if, as there is some reason to expect, the glass-makers can succeed in producing a material rivalling the refracting powers of the diamond, still further progress would be within the reach of skilful optical artists. An aluminium glass is spoken of as likely to fulfil the requisite conditions.

Glass ruby-tinted with Gold, seen with the Micro-spectroscope.—The writer of the microscopical paper above mentioned says that his attention has been called by Mr. Lettsom to the remarkable spectrum afforded by glass ruby-tinted with gold. In a slide prepared by him, kindly forwarded to us, we find the luminous part of the green and blue darkly clouded by a very thin slice of the glass while the red and violet portions of the spectrum remain clear. At night, with the micro-spectroscope and a paraffin lamp, the cloudy band has a peculiar red tint, well seen if the lamp is screened so that little light can reach the eye except what passes through the spectroscope.

Microscopical Soirée at the British Association.—This is said by a contemporary to have been a very great success, and the Association owes its hearty thanks to Messrs. W. Tedder and J. W. Morris, the secretaries respectively of the Bristol and Bath Microscopical Societies, and to the members of those societies. A bold idea was well carried out, viz. that of exhibiting chiefly living objects. The 110 microscopes were arranged in classified divisions, devoted to Crustacea, Arachnidans, Insecta, marine and fresh-water fauna, ciliary action, vertebrate circulation, vegetable circulation, fertilization of flowers, Cryptogamia, micro-spectroscopes, &c. The idea of practically illustrating Sir John Lubbock's 'Fertilization of Flowers by Insects' was novel, and so far carried out as to give a vivid idea of the processes to those who were previously unfamiliar with them. The geological division included an exhibition of the perennial *Eozoon canadense*, which must be exhibited again and again to live down the hostility to its animal nature. Altogether the exhibition was a great evidence of scientific enthusiasm, which had led many ardent students to make special dredging and fishing expeditions both in inland and marine waters.

Anatomical Micro-photographs.—At the meeting of the British Association, Mr. H. B. Brady, F.R.S., exhibited a series of photographs, chiefly from physiological and pathological preparations, taken by a new and simple process devised by Mr. Hugh T. Bowman, of Newcastle. The apparatus was also shown, and described to consist of a simple mirror of spectrum metal placed at an angle of 45° in front of the eye-piece of the microscope, directed downwards. The image was received upon a collodion plate set in the frame of a

common photographic camera, and photographs taken in the usual way. About eleven seconds was stated to be sufficient exposure for the purpose.

Professor Abbe's new Book on Optical Instruments.—We are informed that Professor Abbe intends to give his papers on the microscope—one of which appears in print in the present number of this Journal—in the form of a complete book "on the theory of optical instruments."

CORRESPONDENCE.

Observations on Mr. Branwell's Letter.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, Mass., U.S.A., *August* 10, 1875.

Mr. Editor,—I have been much interested in the proposition by Mr. R. Branwell in the August number of this Journal. Some microscopists may at once accept his dictum that the opinions of certain persons named settle at once and for ever a certain disputed point; most others equally as competent may not accept that dictum, and the pages of that same number of the Journal afford ample evidence of the fact.

Mr. Branwell says, "The finest glass ever made would have but a limited sale, and probably would be condemned by the public because it could not perform on a diatom so well as other known glasses of even moderate excellence."

I challenge Mr. Branwell's theory. I claim that the "finest glass ever made" or yet to be made *will perform best on the diatom,* and I maintain the converse proposition that the glass that will not perform best on the diatom is not the best for all other work. These propositions I am ready to prove by glass that are the best. No opinion to the contrary can be of any value unless the parties giving the opinion have tried them.

It is time the humbug was exposed and done with, that glasses fit for the study of diatoms are only fit for them. The fact is, the diatoms are the best objects in nature for the study of microscope objectives.

Charles Stodder.

[Mr. Stodder, if not remarkably clear in some of his utterances, is at least bold in his expression of opinion. We fear, however, that those who have most studied microscopic anatomy will not accept his propositions.—Ed. 'M. M. J.']

An Error in Mr. Dod's last Letter.

To the Editor of the 'Monthly Microscopical Journal.'

Memphis, Tenn., U.S.A., *August* 17, 1875.

Dear Sir,—Please do me the favour to correct a typographical error in my note published in the August number of 'M. M. J.,' p. 100. The Tolles' "four-system" glass of $\frac{1}{60}$th of an inch, *should be* $\frac{1}{10}$th of an inch, only.

Yours truly,

Albert F. Dod.

A Query as to the Luminous Field in the Immersion Lens.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *August* 24, 1875.

Sir,—Your correspondent, Mr. Mayall, in the 'M. M. J.,' p. 96, alluding to the testimony of Mr. Wenham's Reflex Illuminator as deciding the "aperture question," says: "If he will try the experiment on Möller's Probe-Platte with the Reflex Illuminator and a high-angled immersion lens, he will see a *luminous* field; whereas, with a pneumo-lens he obtains a *dark* field. Whence comes the *luminous* field in the immersion lens if not from its having the power to collect rays which are *totally reflected* when the pneumo-lens is used?"

I beg to suggest that this is not quite so *definite* a statement as is needed to make the conditions of the case clear. Will Mr. Mayall tell us what is the *very least angle* of such "a high-angled immersion lens" as will give the effects he names; that is to say, of a *dark* field when *dry* (thus a "pneumo-lens"), and a luminous field when "wet," and becoming thus an immersion lens? I hope for an explicit statement of the fact.

Yours respectfully,

R. B. Tolles.

English and Foreign Preparers of Microscopic Specimens.

To the Editor of the 'Monthly Microscopical Journal.'

August 25, 1875.

Sir,—Although in some instances foreign preparers supersede the English, it is but fair to state that the English excel in several instances the foreign preparers.

Among the English preparers, Barnett, Cole, Enock, Norman, and Amos Topping take the lead in their *specialities*, although several *amateur* preparers produce preparations of the highest quality. Among foreign preparers, Bourgogne's (Charles, Eugène, and J.

Père) preparations are well known for their great excellence, and several of them are not obtainable, of equal perfection, from any other source. C. Rodig's botanical preparations, above all the sections of *Fungi in situ*, are well worth recommending — they are *superb*. J. D. Möller's Platten are well known, and they are undoubtedly marvels of manipulative skill. H. Dalton's arranged scales are deservedly admired as "pretty and showy" preparations. Some American microscopic specimens are *extremely* well mounted.

Möller has recently increased some of his prices to an *enormous* extent, although the quality of his productions has, to say the least, remained *stationary*. Several species of diatoms quoted in his (M.'s) catalogue at prices ranging from 2*s*. to 6*s*. can be obtained here from the different opticians at from 1*s*. to 2*s*. each, according to the perfection of the specimens and the hands in which they are. Möller may be looked upon by some as an authority on *Diatomaceæ*, but according to Mr. Hickie ('M. M. J.,' No. lxxix., p. 34), "wrong naming seems to be his speciality," and authenticity is not, therefore, the inducement to pay the increased prices for M.'s preparations.

Since this increase of prices has taken place, Möller *generously* offers to the microscopical world to divulge his procedure to prepare Diatomaceæ, for a corresponding indemnification, by publishing a *little* work, with illustrations, at the *moderate* price of 32*s*.

In such a state of things, one must look for some source whence reliable preparations, of excellent quality, and moderate prices, may be obtained.

In regard to the English preparers, Cole and Son, of Liverpool, the well-known preparers of Diatomaceæ, have recently extended their productions to series of pathological and physiological preparations, which are highly recommended by several authorities.

Among the physiological series will be found excellent preparations of the so much looked for *injected* human cerebellum and cerebrum, and many other first-class injections, which will be, no doubt, of great interest both to the professional and to the amateur microscopist.

Cole and Son have also produced lately series of Diatomaceæ, some of the specimens being of great rarity, mounted in their usual *clean* style, at very moderate prices.

When specimens like the above are met with, there should be no fear, when calling attention to them, that the idea of "puffing" may be, for a moment, justly entertained.

I bring the above before your readers, believing that several of them do not wish to pay *fancy* prices, and therefore deprive themselves from adding certain specimens (which they would have bought at reasonable prices) to their cabinets.

I am, Sir, your obedient servant,

A. de Souza Guimaraens.

On Improvements in Illumination.

To the Editor of the 'Monthly Microscopical Journal.'

27, Montague Street, Edinburgh,
August 30, 1875.

Sir,—Whether the present controversy on angular aperture and the rival merits of English and foreign lenses may, or may not, result in enlarging the defects while minimizing the excellences of our objectives, or whether the differences between the dot-showers and the Valentin-knife men, where the one party looks for glasses having only one definite focal plane, and the other for such as have half-a-dozen planes at once, are likely to be adjusted by a prize glass to be constructed under the superintendence of the "R. M. S.," which shall in some mysterious way combine both those idiosyncrasies, is a question I care not to meddle with. My purpose here is, not to obtrude opinions on others, but rather to seek advice and assistance in a matter, the importance of which is incontrovertible. I am alluding to improvements in illumination.

There are, of course, other points in which I should like to see improvement, or, at least, the liberty of choice afforded us; for instance, I should like to see some portion of that careful correction, which is said to be lavished on our objectives, extended also to eye-pieces; and some contrivance hit upon to enable the diaphragm with its iris arrangement, instead of being a fixture, as at present, to be smoothly traversed from left to right,* at right angles to the axis of the tube, so as to combine oblique illumination with complete exclusion of all extraneous rays, and this without adding materially to the thickness of the stage.

But these, I suppose, are things rather to be hoped for than expected.

With regard to illumination, though I have hunted through divers works to find what I wanted, my search has hitherto been fruitless. They either recommend specialities of limited application, or their experiments are like those of Mejnour in Bulwer's 'Zanoni,'—successful, it is true, in their own hands, but with the finishing touch concealed.

Many persons have a strong prejudice against all sources of illumination with which glass,—especially quicksilvered glass,—is mixed up, as invariably introducing disturbing elements, which it is the worker's business immediately to get rid of. Compare Dr. Pigott's remarks in the 'M. M. J.,' vol. xiii., pp. 152, 177. Therefore I have been thinking whether some improvement in this direction might not be effected by substituting for our present concave mirror of quicksilvered glass,—at least as an optional alternative,—a concave surface of some material perfectly white, and yet perfectly free from glistening, the surface to be brought to a state of *absolute smoothness.* As polished silver would probably be quite as offensive as our present arrangement, it occurred to me that the surface might be covered with white enamel,

* In the better German microscopes the diaphragm certainly does slide in on the right side (cf. 'Nägeli u. Schwendener,' p. 93); but there is nothing answering to the above requirements,—at least I have seen nothing.

after the manner of watch faces, though I am doubtful how far such a material would be susceptible of polish: but upon this point I would fain have the opinion of others. The amount of polish required and the difficulties in the way may be inferred from the following extract from Dr. Miller's 'Chemical Physics' (5th ed., p. 161): "Bodies in general do not possess surfaces actually flat. To common observation they may be flat; but, when optically examined, their surface is found to consist of an indefinite number of minute planes inclined to each other at all possible angles, and therefore receiving and reflecting light in all possible directions. When by the operation of polishing they are so much reduced as not to be elevated or depressed more than about the millionth of an inch, they appear to become incapable of acting separately, and produce the effect of a uniform surface." Compare also 'Nägeli u. Schwendener,' p. 86.

It will be seen from this that the degree of smoothness required to convert the surface I have spoken of into an efficient reflector is to the millionth of an inch. I have seen it stated that, if put under the microscope, the surface of few microscope lenses would fail to show lines and scratches left by the polishing material. Perhaps some may be tempted to submit their objectives to this ordeal, and then to calculate how far the marks so found exceed the millionth of an inch, that is, how far the polishing of their glasses comes short of perfection.

The difficulties, then, are considerable. On the other hand, popular report credits Mr. Whitworth with having constructed a machine to measure to the millionth of an inch! If this be anything better than a stupid *canard*, and if to *measure* to the millionth of an inch be really within the ability of our ordinary mechanics, surely to *polish* to that degree of exactness cannot be an insuperable difficulty to our London opticians, who are confessedly the very flower of artistic skill. At any rate, I think the attempt ought to be made; and what has been done in the way of reflecting telescopes may serve as a guide. It is just possible that such reflecting surfaces might fail to furnish sufficient light for very high powers; but they certainly would be a comfortable and *trustworthy* aid to all powers under a $\frac{1}{20}$ inch.

Speaking of opticians reminds me that, though I found most of the German opticians knew little more of Schacht, Harting, Frey, and Dippel than their bare names, yet I always found them furnished with a well-thumbed copy of 'Nägeli u. Schwendener,' and some of them were especially emphatic in their opinion of its merits, giving me to understand that, in their estimation, it was *the* book *par excellence*.* I set it down at the time for just an ordinary German book on optics, copiously dotted with trigonometry and optical diagrams, exhibiting a fair quantum of good sense,—and certainly of botany,—by two editors at once, one of them apparently supplying the good sense, and the other the botany. Perhaps there *is* a trifle too much of botany and crystallography, and what the editors call "Mikrochemie"; but

* 'Das Mikroskop, Theorie und Anwendung desselben, von Karl Nägeli, Prof. in München und S. Schwendener, Docenten der Botanik in München. Mit 276 Holzschnitten. Leipzig, 1867.'

this will be a venial fault with those of kindred tastes,—those, I mean, who believe with the poet,

"The proper study of mankind is—fungi."

I have since endeavoured to know it better.

It would be superfluous to call it profound; for all German scientific works at least try to be that. It is that, and something more. It is a thoroughly *practical* treatise on applied optics, that is, on optics applied specially to the construction and correction of microscopical instruments,—a regular optician's *vade mecum*, with all the whys and wherefores reasoned out to the end,—in short, a veritable book after Mr. Wenham's own heart. Indeed, in many passages its language is almost identical with some of Mr. Wenham's recent utterances; so that the editors might seem rather to have been translating than composing, only that they happened to publish their remarks a few years earlier. And throughout the book there is an absence of that spirit, so general in German writers, of affecting to ignore all that has been written on the subject by other nations. They quote Scotch, French, English, American, and Dutch authorities with perfect impartiality, thus recognizing that science, like goodness, is the property of no particular nationality. In their chapter, "How to determine Angular Aperture," after discussing the various methods proposed, and Mr. Wenham's amongst them, they remark, "This [Wenham's] method has indisputably the great advantage, that we are enabled by it to determine, not only the aperture of the objective in respect of the whole amount of light it admits, but also the really available part of it, that is, the part which supplies sharp and correct images." See p. 168.

I may add, that the work is written by men of acknowledged eminence as mathematicians, who are at the same time notable microscopists, and of high repute for their microscopical researches in their own particular line; so that their statements will hardly be open to the sarcasms that might be levelled at microscopical assertions by writers on optics ignorant of microscopy, or at optical remarks by microscopists careless of optics.

I do not mean that all their theories will be acceptable to our London opticians. The following, for instance, will, I know, be very unpalatable: "The use of condensers is in most cases superfluous, where the mirror is sufficiently large, and can be brought up near enough. The use of such things has a meaning only where one purposes to enlarge the aperture of the incident cone of light." . . . "Condensers, therefore, are efficacious only in two directions; they give to the cone of light, which illuminates a particular area of the field of view, an equal intensity in its entire cross-section; and, in the next place, enlarge its angle of aperture. As for the other assertions regarding the effects of condensers, that they dissipate the interference lines at the margin of the object, and resolve difficult details proportionately better, the more completely the correction of their aberrations has been carried out, that is pure imagination." See pp. 91, 255. This, of course, is rank heresy; but I suspect that

the editors, if assailed on this point, would be quite equal to the occasion.

But there is one passage (p. 169) so pertinent to the controversies of the day, that I must give it in the authors' own words: "Es ist vollkommen gleichgültig, ob der Oeffnungswinkel eines Mikroskops beispielsweise 70 oder nur 68 Grad betrage. Es ist geradezu lächerlich, wie Harting mit Recht bemerkt, wenn man bei stärkeren Objectiven, wie es Manche gethan haben, die Grösse des Oeffnungswinkels bis auf Bruchtheile eines Grades angiebt. Und ebenso lächerlich und unpraktisch ist es, Objective mit Oeffnungswinkeln bis zu 160° und darüber herzustellen, wenn hievon wenigstens 40–50° auf einen total unbrauchbaren peripherischen Theil des Systems fallen, wie diess bei manchen englischen Systemen wirklich vorkommt."

A translation of the entire work would be out of the question, owing to its great bulk,—628 pages large octavo. Of these about 358 belong to the microscope proper, while the rest of the book is devoted to an exposition of their own peculiar views on gases, crystals, protoplasms, cell-formation, plant-life, and what not; all very learned and very interesting, but which would have gone much better into a separate volume, to be entitled, 'The application of the Microscope to things in general.'

Yours faithfully,

W. J. Hickie.

Mr. J. Mayall, jun.'s, Critics; and the "Balsam Aperture Question."

To the Editor of the 'Monthly Microscopical Journal.'

224, Regent Street, London, *September* 2, 1875.

Sir,—Mr. Slack's defence of his apology amounts to this: He affirms that with Zeiss's $\frac{1}{6}$th pneumo-lens, angle 68°, and C eye-piece, with artificial light, he was able to rival the definition of *Surirella gemma* as seen in Dr. Woodward's photograph produced with Powell and Lealand's $\frac{1}{16}$th immersion, with sunlight. I am content to leave that statement to carry its own conviction.

"Crito's" attempt to answer my question "Whence comes the *luminous* field in the immersion lens if not from its having the power to collect rays which are *totally reflected* when the pneumo-lens is used?" lacks the sagacity he would affect, and does *not* "perfectly account for the phenomenon." He suggests, the luminous field might have been obtained by the immersion lens having a larger angular aperture than the pneumo-lens used. The very point of the experimental proof furnished against Mr. Wenham's position in the "Balsam aperture question" by his Reflex Illuminator is, that when the pneumo-lens is used on a balsam-mounted object (viz. Möller's Probe-Platte), the field rays are totally reflected by the cover-glass,—there are *none* to be "picked up,"—the total reflexion at the cover-glass is a barrier that excludes the pneumo-lens whatever increase might be given to the angle of aperture; but when the high-angled immersion lens is

used, field rays enter the lens, giving a luminous field—rays from balsam of greater angle than 41°—the very rays "challenged" by Mr. Wenham. The optical law that forbids the pneumo-lens to gather the field rays from the Illuminator also excludes it from all rays nascent from the object of greater obliquity than 41°; and conversely the law that has permitted the *field* to be *luminous* in the immersion lens with rays beyond 41° permits image-forming rays to pass from the object beyond 41°. Whether the rays pass direct from the total reflecting surface of the prism, or from the object, if they reach the posterior surface of the front lens at equal angles of incidence, they both follow the same process of refraction. That *image-forming* rays deflected by the object, or nascent therefrom, of greater angle than 41° do enter the immersion lens and are refracted into the optical image is, I conceive, abundantly proved,—theoretically by Professor Keith's computation, experimentally by the fact that definition entirely invisible to the pneumo-lens and manifestly the product of *extra*-oblique rays *is* visible with the immersion. Those who care to follow the subject will at once note that the *increase of angular aperture* due to the *immersion* principle is *not* the increase "Crito" speaks of.

The answer to my question is, as Dr. Woodward and Professor Keith demonstrated: *That immersion lenses made on certain formulæ transmit a greater angle of rays than corresponds to the maximum air-angle.*

I take this opportunity of saying that shortly after the arrival of Dr. Woodward's photographs of Professor Keith's computation, I drew up a brief history of the "Balsam aperture question," and submitted it with the computation to one of the highest mathematical authorities in England: the result was against Mr. Wenham. I should have made this known, but I understood Mr. Wenham to reject the experimental proof as being inadmissible in deciding the question of image-bearing aperture. It remained to provide means by which rays beyond 41° from balsam should be refracted through the immersion lens; and this was required to be done with legitimate means—such as admitted of no cavil on the score of being made only for the purpose of proof and of no moment in practical microscopy. The Reflex Illuminator is surely not open to this kind of objection?

I am, &c.,

JOHN MAYALL, jun.

MR. GARNER'S BUCEPHALUS.

To the Editor of the 'Monthly Microscopical Journal.'

3, QUEEN STREET PLACE, UPPER THAMES STREET,
LONDON, *September* 7, 1875.

SIR,—Having sent a copy of my letter to you of July 30th date to Mr. Garner, he has in return kindly lent me the paper published in the 'Zoological Transactions,' December 8, 1835, to which he referred in his communication to your Journal of July 11, and accompanied the same with the following note to me, viz:

"Dear Sir,—I send you my paper in which I figure a parasite which

I presume is allied to your Bucephalus; possibly not identical with it. I never found it *free*, but the individuals I found were remarkable for such rapid and sudden movements as were described in your animal. My short communication was written under the impression that the assertion of the non-parasitism of Bucephalus was not confined to your species or variety, whatever it is. I leave the matter in your hands, and remain,

"Dear Sir, yours very faithfully,

"R. Garner."

With Mr. Garner's permission I now enclose copy of the figure in his paper.* In reference to it he says that he found it in the foot of an Anodonta, and that it presented the following characters, viz: "In the mature state the body is more or less cylindrical in its shape, but varied much at the will of the animal. At one extremity it has two very long appendages, which are spiniferous at their terminations, and which in some individuals have a row of round bodies attached to one side for part of their length; these appendages are contracted with great rapidity, and are then very short. There is an opening by a circular lip between these appendages. A contraction separates this part, on which they are situated, from the rest of the body. There appears to be another opening at the opposite extremity of the animal." I think it will be seen from the above as compared with my animal, and also with the *Bucephalus polymorphus* and *Haimeanus*, that although there is some resemblance in form and character, yet that it more nearly approaches the *B. Haimeanus* than either, and that all three differ very much from the creature I have found, and which so far remains *unique*.

I remain your obedient servant,

John Badcock.

Remarks on Crito's Letter.

To the Editor of the 'Monthly Microscopical Journal.'

Dalston Vicarage, near Carlisle,
September 11, 1875.

Sir,—In "Crito's" letter, in the last number of the 'M. M. J.' there are some passages which must, I conceive, excite surprise in the minds of many of your readers.

Mr. Mayall had observed that, tried by the test of deep oculars, the image with certain specified comparatively low-angled objectives breaks up with any magnification beyond about 1000 diameters. In reply, "Crito" quotes the nominal linear magnifying powers of lenses of the same focal length as those referred to by Mr. Mayall, with different eye-pieces, as though that were a conclusive answer. But surely he forgets that the whole question at issue is not as to possible

* [We have received a copy of the figures from Mr. Badcock, but as they have already appeared elsewhere we think their reproduction here is unnecessary.—Ed. 'M. M. J.']

amplification, but as to actual definition. Of course, the nominal magnification can by the use of deep oculars be run up to almost any figure; but what practical worker with high-power lenses does not know by experience how useless and deceptive is the further enlargement of an object by such means, after that point is reached at which, for want of defining power in the lens employed, the image begins to lose its sharpness and crispness, and to become indistinct and woolly?

Mr. Mayall had further noticed that, with Wenham's Reflex Illuminator, a high-angled immersion objective gives a luminous field, when a pneumo-front gives a dark field; and he asks, Whence comes the luminous field? In reply, "Crito" makes the singular suggestion, that the dry may have had a smaller angle than the immersion lens. But what has the angular aperture of the dry lens to do with the question? If the rays were totally reflected from the upper surface of the cover, which they would reach after passing through the balsam-mounted slide, but beyond which they could not get according to the well-known optical law, there could be none to be picked up by the dry objective, whatever its angular aperture might be. What then could make the field luminous when the immersion front was used, but the entrance into the objective of rays which, with the pneumo-front, were totally reflected? But "Crito" asks whether a plain glass slip would not answer the same purpose as the Möller's Probe-Platte? No doubt it would, so far as the mere brightness of the field goes. But the Probe-Platte serves another purpose. For the objects it contains, though attached to the cover and not in contact with the glass slip, are brilliantly illuminated, thus proving that the rays which enter the object-glass do not arise from mere diffusion of light, but that they are *bonâ fide* image-forming rays. In the one case the image of the diatoms is not there; in the other it is there. This is, as it strikes me, Mr. Mayall's answer to Mr. Wenham's challenge.

The pleasantry which "Crito" discharges at Mr. Hogg at the close of his letter arises, I think, from a misapprehension of his meaning. Certainly he misrepresents his conclusion, which he could not do if he rightly understood his abridged line of reasoning. And yet the argument, when fully stated, appears to be simple enough. Dr. Parkinson proves that chromatic and spherical aberration, which arise from different causes, are to be corrected by different and independent means. Theoretically, therefore, as Dr. Parkinson states, both may be corrected together, and a perfect lens obtained. It is equally true from the reasoning that theoretically either of them may be corrected whilst the other is left uncorrected. In other words, a lens may be achromatic and not aplanatic, or aplanatic and not achromatic. From this the inference drawn by Mr. Hogg is, *not*, as "Crito" incorrectly states it, that "some object-glasses which are not achromatic *must* be aplanatic," which is simply absurd, but "that all chromatic aberration does not involve spherical aberration," which was the conclusion to be established.

I am, Sir, your obedient servant,

EDMUND CARR.

Mr. Branwell's proposed Prize for the best Objective.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Your Brighton correspondent, Mr. Branwell, F.R.M.S., makes a suggestion that "the Royal Microscopical Society should appoint a committee to settle upon a standard for physiological glasses. That the Society should offer a gold medal every third year or oftener for such glasses. . . . ."

It appears to me the appointment of a committee for that purpose would be fraught with danger to the existence of our Society. Every optician would have his candidate. Every optician's friend or acquaintance would be touted for his vote with such uncompromising energy, that the whole subject of microscopy would become tainted to the core with party spirit; and in the chaos of our bickerings we should lose all chance of getting access to the palatial realms of Burlington House, which many Fellows have yearned for as the one hope of reviving our declining reputation as a Learned Society.

When I endeavour to represent to myself in imagination the probable effect, among the opticians, of the sight of Mr. Branwell's suggested gold medal to be competed for every third year, I seem to see a whole Hogarthian series of delineations of human character flitting in and about our cheerless assembly room at King's College, scarcely one of them having the least confidence beforehand in the impartiality of the committee; and only *one* of them to be gratified every third year!

Such a prospect fills me with dismay. I much prefer our present anarchical system, where every optician who has a friend among the Fellows may induce him to swear by his—and only his judgment in recommending optical work. Thus competition is kept alive. Thus has every optician an interest in furnishing every one of us with choice lenses. Thus may we pride ourselves in thinking we each possess lenses of extraordinary excellence. The present system favours us *all* individually,—that is why I like it.

If Mr. Branwell's proposal were carried out, the very first award might put me out of conceit with my lenses. I should sell them at a loss, and get others from the "gold medallist." At the next award I might be driven back to my discarded lenses; and so on, until my microscopical proclivities would be so tortured by anxiety—hoping against hope ever to possess the very best lenses in existence (of which at present I am undoubtedly assured by Messrs. X., *my* opticians)—that, in despair, I should send the whole paraphernalia to the auction room. It would probably be sold for an "old song" to some dealer in second-hand apparatus, who would select out the valuable portions, replace them by thoroughly worthless ones, and resell to one of those aspiring amateurs who have perfect confidence in their own judgment.

I remember some years ago a large sum of money was subscribed to the "Quekett Memorial Medal Fund," to provide "a medal, to be called the '*Quekett Medal*,' and to be given at the discretion of the

Council (if possible annually) to such member of the Society who, in the opinion of the Council, has best promoted the interest of Microscopical Science;" but the Council have considerately stored it away for some other purpose. If they were to revive the subject, we should all put in our claims with such daring ambition, the Council might be forced to award the medal alphabetically, according to seniority,—or, perchance, have it scrambled for: the justice of the award would probably be as likely to satisfy us one way as another.

Your obedient servant,

F.R.M.S.

PROCEEDINGS OF SOCIETIES.

Quekett Microscopical Club.

Annual Meeting, July 23.—Dr. John Matthews, F.R.M.S., President, in the chair.

The Tenth Annual Report of the Committee was read, giving a very favourable account of the progress of the club during the year, which was fully borne out by the details entered into.

The President read the Annual Address, in the course of which, after reading the prospectus of the original constitution of the club, he proceeded to consider how far the intentions of its founders had been carried out. After a rapid review of the nature and motives of their work, and of the eminently social character of their meetings, he deprecated the supposed necessity for original research on the part of a society composed principally of amateurs and students, maintaining that the best thing they could do was to make themselves well acquainted—before all—with the labours of their predecessors and others, taking nothing for granted until personally verified as far as possible. That, he considered, was the nature of real scientific training. Adverting to the Reports of the Committee and Treasurer, he congratulated the members on the prosperous state of the Society, numerically, financially, and scientifically; and concluded with the inference that the Society *had* amply fulfilled the intentions of its founders.

The elections then took place for officers and members of committee. Dr. Matthews was re-elected President for the ensuing year.

Dr. Lionel S. Beale, F.R.S., &c., a past President of the club, was balloted for and unanimously elected an honorary member.

Votes of thanks to the President and officers were passed, also one of a very cordial character to the Council of University College, for the continuation of their permission to hold the meetings in the College library, a favour which had been accorded to the club from an early period of its existence.

Ordinary Meeting, August 27.—Dr. John Matthews, F.R.M.S., President, in the chair.

A paper was read by Mr. William Cole, M.E.S., on *Sphœrularia Bombi*, an entozoön parasitic in Humble-bees. This was first described by Leon Dufour in the 'Annales des Sciences Naturelles' for 1836, and subsequently formed the subject of two very instructive memoirs by Sir John Lubbock, in the 'Natural History Review.' Mr. Cole gave a *résumé* of the facts connected with the anatomy and morphology of this extraordinary creature. It is found in the abdominal cavity of the bee, in the form of a white worm-like animal nearly an inch long and thickly covered with wart-like projections or spherules, whence the name of the genus. Mr. Cole drew particular attention to the great abundance of the parasite in females of *Bombus terrestris* during the past spring. In several instances every individual examined contained one or more specimens, and as many as thirty-three were once found in a single bee. The young nematoids also occurred in immense numbers—as many as from 50,000 to 100,000 in each insect. Near the end of each adult *Sphœrularia* is invariably attached a small nematoid worm, which was described by Sir John Lubbock as a male, passing an epizoic existence on the body of his giant consort. By Schneider, however, it is held to be the *true* female—the immense structure to which it is affixed being regarded by him as a prolapsed uterus. Mr. Cole gave a brief account of the facts on both sides of the question, and after sketching the probable life history of the young *Sphœrulariæ*, pointed out the direction which investigation should take in an endeavour to solve the very interesting questions connected with this anomalous creature, which he recommended to the careful consideration of members in search of a subject. The paper was illustrated by enlarged figures, and specimens under the microscope.

The Fairmount Microscopical Society of Philadelphia.

This Society held its regular meeting on Thursday evening, April 15, 1875. The main topic of the evening was the kidney and its diseases, particularly "Bright's disease." It was illustrated by many fine and unique sections of the human kidney, stained by means of carmine. The explanations, volunteered by a member of the Society, were remarkably clear and to the point. Quite a series of drawings, made direct from the instrument, of urinary deposits, was shown at the same time, tracing the course of the above disease in several patients under treatment by the demonstrator.—*Cincinnati Medical News.*

Memphis Microscopical Society.

At the meeting of this Society, April 3, several matters of interest were presented. A communication was received from corresponding member, E. W. Morley, of Hudson, Ohio, giving his method of measurement, under very high magnification, of the striæ on the

entire series of diatoms on the Möller's test-plate. A pamphlet was also received from Dr. Geo. E. Blackham, of Dunkirk, New York, giving the history of the cases of *trichiniasis* caused by eating the diseased pork, of which specimens had previously been sent to the Society. From this article it appears that the dreaded *trichinæ* is by no means so uniformly fatal in its effects as is ordinarily supposed. Nevertheless, cases are on record where competent medical men estimate that 1 cubic inch of muscular fibre contained 85,000 of this parasite.

A pamphlet was also received containing an article by Dr. Chris. Johnston, of Baltimore, identifying a deposit of earth found on the banks of the Patuxent, in Maryland, as being of the same origin and composition as the well-known Bermuda "tripoli." The microscope at once demonstrated the identity of the two deposits, both being made up of the fossil shells of diatoms, invisible to the naked eye, but here aggregated in such countless numbers as to form extensive tracts of land. Prepared specimens were also received from Mr. Frank Miller, of New York, and Mr. H. G. Hanks, of San Francisco; for all which a vote of thanks was passed.

The President, Dr. Cutler, read a paper "On the Microscopy of the Cyclops," and also gave some new and interesting facts regarding certain diatoms found in the same specimen of water. Dr. Cutler states that he has detected in these organisms undoubted *cilia*, by means of which their peculiar and puzzling movements are effected. In this connection an interesting discussion was had as to *how far* motion is to be accepted as undoubted evidence of animal life.—*Cincinnati Medical News.*

San Francisco Microscopical Society.

The regular meeting of the San Francisco Microscopical Society was held in its rooms on Thursday evening, April 22, President Ashburner in the chair. In addition to a very full attendance of members, Professor Wm. H. Brewer, of New Haven, J. B. Bond, of New York, Dr. J. N. Eckel, Dr. Henry Terrer, Dr. Murray More, D. J. Staples, H. L. Hosmer, and W. N. Lockington, were present as visitors.

The Secretary announced the receipt of the April number of the 'Cincinnati Medical News,' containing valuable microscopical items; three numbers of the 'World of Science'; and from Mr. R. H. Ward, the 'Rules of the American Postal Micro-Cabinet Club,' with a letter desiring that a circuit be organized on the Pacific coast. Messrs. J. H. Carmany and Co. donated the May number of the 'Overland Monthly,' and Dr. Harkness presented a printed copy of his paper read before the Sacramento Society for Medical Observation, in April, 1868, regarding Salisbury's Ague Theory, which is a very interesting article to all microscopic botanists, and controverting the theory advanced. Mr. H. Edwards presented ten papers of a series on "Lepidoptera of the Pacific Coast."

To the object cabinet, Mr. W. H. Walmsley, of Philadelphia,

donated twelve beautiful slides, mounted with the following objects, viz.: Scales of *Lepisma saccharina*, wolffia, columbiana, fertile frond of maidenhair fern, fertile frond of *Lygodium palmatum*, coccinella, spinnerets of garden spider, American podura, culex (male and female), head and tongue of horse-fly, gizzard of cockroach for polariscope, and ovipositor of saw-fly.

Mr. W. G. W. Hartford donated seven slides, mounted with *Rhabdomena arcuatum* (Scotland), *Aulacodiscus scaber*, *Gyrosigma Spencerii* (New York), guano (Gulf of California), diatoms from San Francisco, transverse section of Indian corn, and wool from the Cape of Good Hope.

Mr. H. G. Hanks donated a slide mounted by him with a section of magnesian rock from Healdsburgh, and a number of seeds of *Paparium somnifera* from Turkey, the peculiar marking of the latter being interesting and making a beautiful opaque object.

Mr. C. G. Ewing donated a slide mounted by him with the palate of a land mollusc (Arion).

Mr. C. Mason Kinne donated four slides mounted by him with *Tingis hyalina*, obtained by Mr. Edwards from Calaveras county, California, and which proved to be a most beautiful object, the entire upper portion of the insect being covered with a curiously woven network of glassy appearance; the very remarkable egg of a California butterfly, *Polyommattus xanthoides*, and the cell structure of the pith of elder. The fourth slide was mounted with what was claimed to be worms taken from diseased teeth; and in presenting this slide Mr. Kinne read a short paper, which explained the matter fully.

Mr. Henry Edwards donated twelve additional specimens of the *Tingis hyalina* taken from the flowers of the cœonothis, mammoth trees, Calaveras county, California; two specimens of parasite from the chrysalis of a species of brassolis, Panama; and a gordius, class entozoa, from Colusa, California, which was accompanied by a paper giving a technical description of the characteristics of the genus; and if, as suggested by Mr. Edwards, this be a new variety, a careful study of its habits, life history, and microscopic examination, would furnish the material for a very valuable paper.

Dr. Harkness, who has just returned from a short trip to the Sandwich Islands, exhibited several slides representing different phases in the life history of the blight which is believed to have been the cause of disease in the coffee plant of those islands.

The Doctor was unable to obtain, at the time of his visit, any coffee leaves which were afflicted by the fungus, but brought with him several leaves of the guava plant, which were infested with a blight, said to be identical with that found on the coffee tree. These specimens belong to the genus Hyphomycetes, which appears upon the upper surface of the leaf as a black mould, and is a true fungus, the mycelium forming a network over the surface of the article, and its filaments dipping downward into the cell beneath. From the surface of the mycelium aerial hyphæ are thrown off in branches, made up of globose cells, adhering to each other, sometimes rising in a single

stem, in others dividing into two or more branches. From these branches arise, on the one hand, a capsule (*sporange*), with its cluster of stylospores, and on the other spermagonia, with imprisoned spermatia. The slides exhibited showed different portions of the plant, and proved that it was not the coffee blight—*Hemileia vastatrix*—which proved so injurious in the island of Ceylon.

The Doctor also exhibited a specimen of confervoid algæ, the œdogonium, from Saucelito, and explained the marvellous manner of the formation of its pores; also from the same locality the filaments of *Zygnema cruciata* in a state of conjugation. His lecture was listened to with interest, and was made plain by diagrams on the black-board, at the conclusion of which he was requested to speak of matters of microscopical interest which fell in his way at the islands.

Among them he mentioned that of the organization of the Royal Microscopical Society of Hawaii, and which, from the interest manifested by its members, will prove an adjunct to science; while it and our Society, its nearest neighbour, can maintain an intercourse to their mutual advantage.

After the unanimous adoption of the following, the meeting adjourned:

Resolved,—That the thanks of the San Francisco Microscopical Society be tendered to Dr. Adolf Barkan for his interesting and instructive lecture on the "Ophthalmoscope" at the last meeting, and that the trustees be and are hereby authorized to extend to him the privileges of the rooms and apparatus for one year.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening, May 6, with a large attendance of members and the following visitors: John H. Caswell, of New York, a corresponding member; John W. Young, Salt Lake City; J. M. Redway, Placerville; S. B. Christy, Berkeley; Henry Taylor, M. de Kirwan, Chas. G. Yale, and Arthur Hayne, of this city.

Under the head of donations the library received many important additions, among them being four numbers of the 'Monthly Microscopical Journal,' April number of the 'American Naturalist,' and three scientific works, entitled 'Fungi, their Nature and Uses,' 'Chemistry of Light and Photography,' and 'Nature of Life.'

Dr. Christopher Johnston, of Baltimore, donated material for mounting, in the way of spicules of *Euplectella speciosa*, rosette form, and teroxide molybdenum for the polariscope.

Mr. D. Mason Kinne presented two slides mounted by him with diatoms, *Isthmia nervosa*, Cliff House beach; and seeds of *Paparium somnifera*.

Letters from Drs. C. Johnston, R. H. Ward, J. A. Thacker, Messrs. Eugene Bourgogne and J. Edwards Smith, were read, each containing matters pertaining to microscopy, and evincing interest in our Society, after which Mr. H. G. Hanks placed on the table his spectroscope, and proceeded to make a few remarks relative to the construction and use of this instrument. His remarks were devoid of technicalities, and with the use of the black-board he made the subject quite clear.

After showing the characteristic absorption bands of various coloured solutions, the spectra of flames in which different substances were volatilized were shown, and their beauty and fixed position noted.

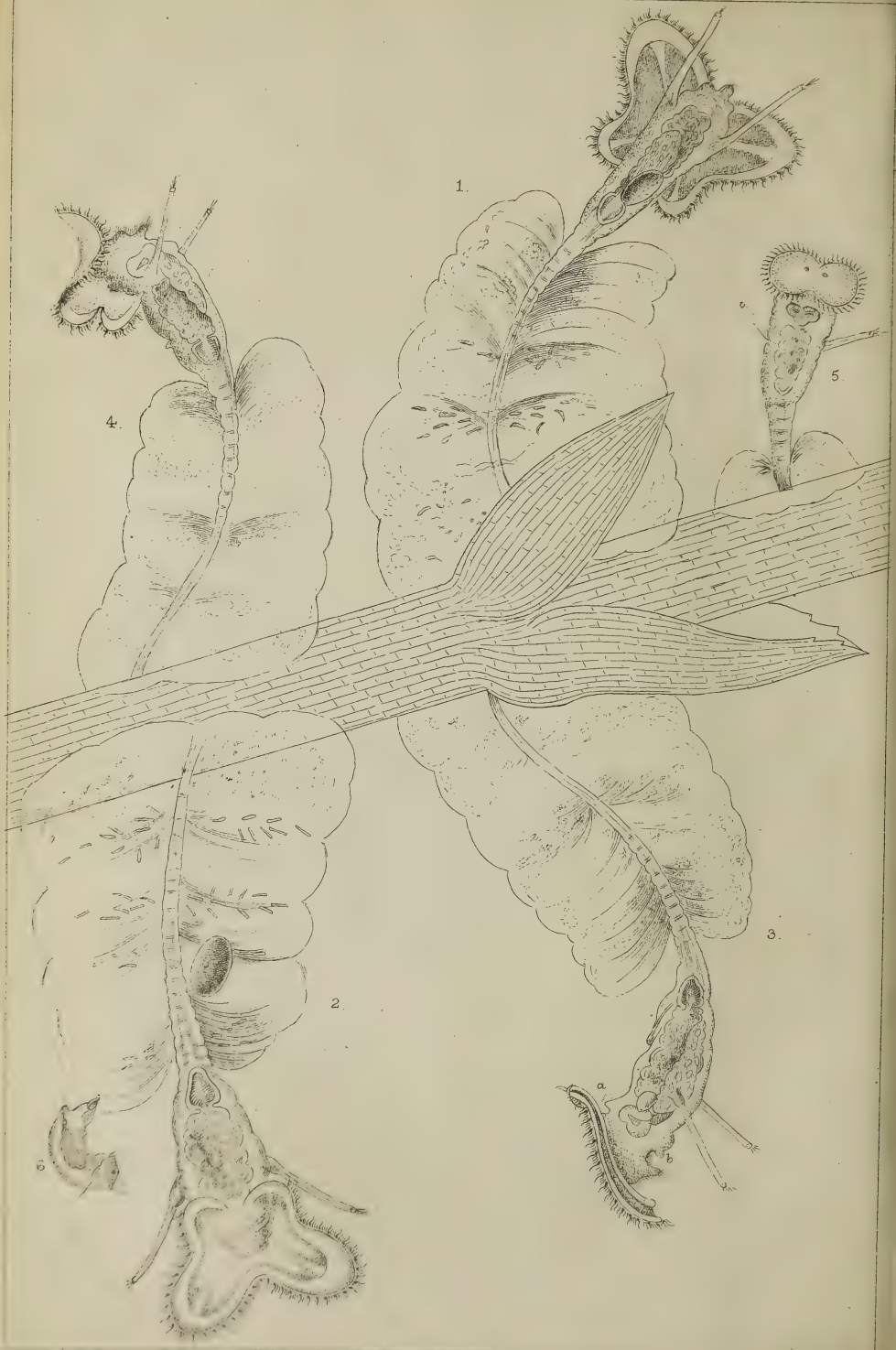
Perhaps there is no single discovery of the last quarter of a century of so much scientific interest, and which has enabled the educated observer to learn of the constituents of bodies unapproachable, as the spectroscope, and combined as it now is with the microscope, the student in spectrology has a wide field for original and interesting work.

After an announcement by the President that the annual reception of the Society would be held in Mercantile Library Hall some time during the last week of this month, the meeting adjourned.

Adelaide Microscope Club, South Australia.*

The monthly meeting was held on May 28, 1875, Mr. G. Francis in the chair. The following objects were exhibited: Pollen from one of the Wattle trees growing in the far north, by Mr. Babbage; peculiar forms of Foraminifera (?) from the salt lakes on the peninsula, by Mr. Young; thin sections of the bitumen-like substance found near the Coorong and called here Coorongite, by the Chairman. The consideration of the time for holding the annual conversazione was postponed till the July meeting. The Chairman then introduced the subject for the evening's study, viz. Ferments, and gave a succinct account of the views held by different sections of the scientific world in reference to fermentation. The subject was a difficult one, but he thought the balance of evidence was in favour of Pasteur's theory. The various kinds of fermentation were briefly referred to, and the saccharine, acetous, and lacteous were more fully discussed. Numerous specimens of the organisms found in yeast, beer, "diseased" wines, and other fermenting fluids, were shown during the Chairman's address.

* Report supplied by Dr. Whittell, Adelaide.



W. West & Co. lith.r

Melicerta tyro.

THE

MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1875.

I.—*On a New Melicerta.*

By C. T. Hudson, LL.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *Oct.* 6, 1875.)

Plate CXIX.

In a review, that I read some time ago, of Mr. Wood's interesting book 'Homes without Hands' the critic remarked that the author had omitted to notice one of the most curious of such structures, namely "the tube of Melicerta."

The criticism was just, and yet the critic was inaccurate; and while pointing out an omission in Mr. Wood's book he was guilty of a blunder in his own review, for he spoke of "the tube of Melicerta," just as one would of the cestus of Venus, or of the club of Hercules;—as if the rotifer had as inalienable a title to his tube, as the hero had to his weapon, or the goddess to her girdle.

No doubt the critic erred in good company; for what microscopist has not viewed with delight the opening of this living flower, and who has ever seen it set otherwise than in a pelleted tube, and who would have hesitated to assent to the proposition that all Melicertæ have these marvellous homes? Nature however seems to have but one law that knows no exception; and that is *that all general statements are untrue.* Fifty years ago every naturalist would have considered it an axiom that every vertebrate animal had a skull; and yet all the while burrowing in the sands of Cornwall there was the headless Lancelet, a vertebrate animal with the heart of a worm and the lungs of a mollusc, apparently created for the express purpose of driving classifiers mad.

EXPLANATION OF PLATE CXIX.

Fig. 1.—Female of *Melicerta tyro;* oral view.
" 2.— " " aboral view.
" 3.— " " side view.
a, a third antenna; *b*, ciliated cup.
" 4.— " " side view, from below.
" 5.— " " a young specimen.
" 6.—Supposed male of *M. tyro.*

All the figures are on the same scale, and all are magnified about 125 times linear.

Who would once have scrupled to say that no monkey has the teeth of a rat, or that no rat has the hands of a monkey? Yet the Aye Aye presents the systematizer with either absurdity he pleases.

That every rotifer has a rotary apparatus seemed an absurdly self-evident proposition, till Gosse's *Taphrocampa*, Claparède's *Balatro* and Mecznikow's *Apsilus*, showed that Nature will not submit even to the widest generalization, and that rotifers may exist without the very apparatus to which the whole class owes its name.

In truth Nature has no such fancies as those Man is ever ready to credit her with. She has but one law—endless variety; and her varieties blend into one another by such fine gradations that no natural system of classification can be other than unsatisfactory; doomed to be destroyed and re-cast by every succeeding generation of naturalists, as fresh discoveries destroy the value of those differences which earlier classifiers have relied upon for separating allied forms.

So long as the tube-makers were represented only by *Melicerta* with its four-lobed disk and pelleted tube, *Limnias* with its two-lobed disk and smooth compact tube; and *Œcistes* with its one-lobed disk and irregular fluffy tube, the genera of the Melicertidæ were comfortably distinct from each other. But the discovery of several new species, and the more thorough investigation of the older forms, have been steadily bringing all these genera closer and closer.

Gosse, Huxley, Leydig, Williamson, Claparède, and others have studied the structure and use of their trochal disks, their internal organization, and the nature and mode of secretion of their gelatinous sheaths; and the result of their labours is that such a similarity of structure and habits is seen to run through the whole group that Gosse has even proposed to make but one genus of them all.

The new rotifer, which is the subject of this paper, will, I think, prove a notable ally in helping to break down the barriers that have hitherto separated the genera of the Melicertidæ.

I found it this autumn on a piece of Anacharis given to me by Mr. W. Fiddes of Bristol, who had brought it from Sutton Park in the hope of getting something worth exhibiting at our Microscopical Soirée. I placed a bit of the weed at random in a zoophyte trough, and on bringing down my inch objective on it, had the pleasure of seeing this charming rotifer fully expanded in the very centre of the field. It is a *Melicerta* with a gelatinous sheath, very like that which invests the Floscules, and yet with a distinct character of its own. Two striking peculiarities at once catch the eye; namely, the great size and butterfly shape of the trochal disk (Fig. 1), and the wonderful length, backward setting, and great

flexibility of the antennæ. The animal reminds one at once of Mr. Davis' *Œcistes longicornis.* Of course I was very curious to know if it possessed its kinsman's ciliated cup for making pellets. Whether it was a Melicerta without a pellet-cup, or whether, having the apparatus for making pellets, it failed to make use of it,—in either case its condition was a strange one. With some difficulty I so clipped the leaf which a specimen was on, as to get the creature comfortably placed in a compressorium; and although it was limited to a very thin plate of water, it still continued to come out of its sheath and to unfold its disk. I then distinctly saw that it possessed a ciliated cup (Fig. 3, *b*) with thick edges, but without a trace of any extraneous matter revolving in it. Now the gelatinous sheath is excreted in some way by the animal, but as similar sheaths are excreted by *Stephanoceros, Œcistes,* and *Floscularia,* which have no ciliated cups, what is the use of the cup in this new *Melicerta?* Has it really had the organ given it not for its own use, but in order that a more highly developed relative might have an improved and effective form of it, without too great a break in the ascending series of animal life?

I think it possible to give a simpler solution of the riddle. Most of the rotifers have in some parts of their bodies organs for secreting a viscous fluid; in the great majority of cases they lie at the extremity of the foot; either at the ends of the pincers as in *Synchæta* or *Euchlanis,* or in a ciliated cup as in *Pterodina,* or on two ciliated projections at the posterior end of the body as in *Pedalion.* The Philodines can secrete this fluid in great abundance; and, as Mr. Davis has conclusively shown, so encase themselves with it as to resist the destructive effects of a high temperature and absolute dryness. The tube-makers have this secreting organ below the mouth and nearly midway between the antennæ, and on or near it fall atoms which have made the circuit of the trochal disk in the groove leading to the mouth, and which, rejected by the organs of taste at the entrance to the buccal funnel, have been hurried over the ciliated chin.

These atoms mix with the viscous secretion and accumulate round about the neighbourhood of the gland; till the animal, apparently annoyed by their presence, contracts itself with a sudden twist and rubs them off on to its tube by a sideway motion of the head, which is characteristic of the Melicertidæ. In this way the gelatinous sheaths are strengthened and frequently rendered quite opaque. When the creatures grow in very clear water their sheaths are so free from foreign bodies as often to be quite transparent. If paint is put into the water (as has been often shown) it soon appears in the newly formed portion of the case. In the 'Transactions of the Royal Microscopical Society,' 1867, Mr. Davis has given an excellent drawing of the tubes of Œcistes enlarged by

the addition of atoms of carmine. Both *Melicerta ringens* and my new rotifer, which I have named *Melicerta tyro*, have their secreting gland below a ciliated cup, the cilia doubtless in their case, as in that of *Pterodina*, acting to keep the sticky surface of the gland free from constant clogging. But the cup of *M. tyro* differs from that of *M. ringens* in its not being united by lines of cilia to the ciliated chin. In *M. ringens* two constant streams of very fine particles may be seen to trickle through two notches on either side of the chin, and to be conducted gently into the ciliated cup there to be converted into pellets; while the larger ones are hurled furiously over the ciliated chin. *M. tyro* has not got these lines of fine cilia from the notches in the chin to its ciliated cup, and so all the atoms large and great rejected at the mouth fly swiftly off together over the chin, and never reach the cup which lies just beneath. It is the lack therefore of these special cilia which seems to prevent *M. tyro* from making the same use of its cup that *M. ringens* does.

The sheath of *M. tyro* is generally tolerably transparent, and like that of all the tube-makers, it is not a hollow cylinder, but (if I may use the term) a solid one with a tubular axis of very small bore, up and down which the rotifer moves. Viewed as a transparent object, it is sometimes difficult to make out the real structure of the tube, but with dark-ground illumination and with the binocular there can be no doubt about it for a moment.

The central tube down which the stem of the rotifer passes is often widened at the top into an inverted cone by its frequent semi-contractions; and this portion too is often rendered conspicuous by the débris of confervæ, or by diatoms rubbed off on it by the twisting Melicertan. When the creature contracts *wholly* it does not tend to make such a cone; and as now and then it has fits of complete contractions, and afterwards of semi-contractions, so at one time it forms a dirty cone at the top of its tube, and at another buries the cone with fresh accumulations of evenly distributed viscous secretions. Thus it happens that as the animal ages and the tube grows, tiers of discoloured cones occur one above another; and all of these, as the animal works up and down in its tube, are pushed at last into horizontal layers. This is seen in all the figures in the Plate, but especially in Fig. 2, in which the layers—the flattened surfaces of the discoloured cones—are sprinkled with diatoms.

The trochal disk acquires its butterfly shape from a habit which the creature has of bending the corners of the two top lobes of the disk. The four lobes are really all rounded just like those of *M. ringens*, and are often seen fully expanded and round. The two antennæ are of prodigious length; and, owing to their size and transparency, it is easy to see how the muscle that runs up to the bunch of setæ at the top can withdraw them, by infolding the tube

even till the knob on which the setæ are placed comes right down to the very base of the antenna.

When the animal is contracted and shrunk down into its sheath, the antennæ are closely applied to its club-shaped body; and as it rises they slowly rise also from their curved and recumbent position, while the action of the transverse muscles at the same time expands the animal, and (by forcing its perivisceral fluid into the tubes) pushes up the setæ-bearing knob till the antenna attains its full length. This too is almost always the signal for the unfolding of the trochal disk.

The internal organs are so like those of *M. ringens* that there is but little to say of them. One point however is worth noticing, and that is that I have no doubt that the lower stomach acts in this animal as the contractile vesicle.

The majority of rotifers, as is well known, have twisted tubes on each side of their bodies carrying ciliated tags, and these tubes generally open into a contractile vesicle which in its turn empties itself into the cloaca, or into the lower stomach. But some rotifers have no contractile vesicle; and some, as *Pterodina*, have apparently the ends of the tubes enlarged in lieu of it. It has been made out in one or two cases that where there is no contractile vesicle the tubes open directly into the cloaca; but in the case of *M. tyro* I distinctly saw on two occasions the empty lower stomach expand and become very transparent, just as the contractile vesicle does periodically; and after being slowly dilated to its utmost expansion, it shot out its perfectly fluid contents through the vent.

While this was going on the passage from the upper to the lower stomach appeared to be completely closed.

Of course it is impossible to *see* any fluid pass from the tortuous tubes into the lower stomach, nor indeed did I quite succeed in satisfying myself that I had seen their points of junction with it: but the tubes themselves are readily seen, and so with a little care in focussing are the ciliated tags, of which I counted five on either side.

My specimens all died before I had finished my investigations, but Mr. Bolton of Stourbridge most kindly came to the rescue and sent me a small piece of Anacharis on which there were nearly a score of the new rotifers, most of which had bred in his own tank. There were also many eggs attached to them, and I eagerly scrutinized all the specimens in the hope of finding eggs whose size and shape might denote males. While doing this I saw a young male rotifer circle round the female which I had under observation, and then dart off in their usual headlong fashion. This was too great a prize to be willingly surrendered, so I removed the zoophyte trough from the stage, and with eye-lens and pipette explored the vast Atlantic in which the little creature was disporting itself, till

I found and captured it. When it was transferred to a compressorium and gently held captive, it proved to be a newly hatched specimen barely the $\frac{1}{170}$ of an inch long, and with so granular and corrugated a skin that I could not clearly make out the whole of its internal structure. Not indeed that there is ever much in a male to make out; but it ought to have shown both the water vascular system and its muscles—and neither was to be seen owing to the unfavourable condition of the surface. The brain lying between the head and dorsal antenna was plainly visible, and so were the spermatic sac and penis—in the former of which could be seen the constant motion of the spermatozoa, though it was impossible to make out the spermatozoa distinctly.

As I did not see this animal hatched from one of the eggs of *M. tyro*, I cannot be certain that it is the male of that rotifer; but there was no other rotifer in the trough except *M. ringens* of which it could be the male.

I have however little doubt that it is the male of *M. tyro*, for I received soon afterwards from Mr. Wills, President of the Birmingham Microscopical Society, some excellent drawings of *M. tyro*, among which was figured a male animal very like the one I have just described, and which I have drawn at Fig. 6. Mr. Wills had very kindly assisted me to find the pool from which the new rotifer had originally been taken, and on examining the specimens which he had taken home for himself had observed a male whirling round one of the females, and had secured a characteristic drawing of it.

In both cases I believe *M. ringens* was also present;—so that this male might belong either to *M. ringens* or *M. tyro*, but in either case it adds the genus *Melicerta* to the diœcious rotifers.

It has been suggested to me that my new rotifer is possibly *Tubicolaria;* and if this suggestion be correct a good many of my remarks at the commencement of this paper would lose their point. Now the genus *Tubicolaria* was formed by Ehrenberg to receive a Melicertan that had no eyes at any time of its life, and lived in a gelatinous sheath. But I have seen the young female of *M. tyro* hatch from the egg, and have seen its two pink eyes when in the egg, and also when it has first escaped. Fig. 5 represents a very young one which I found just beginning to construct its sheath, and it too had two well-marked eyes: so that even if my rotifer is the same as that to which Ehrenberg gave the name of *Tubicolaria*, it ought not, according to Ehrenberg's system, to be placed in a different genus from *Melicerta ringens.* It is true that Ehrenberg says he is not sure about the eyes, and he never had seen but two solitary specimens; but without the distinction of its being eyeless there would remain only that of the gelatinous sheath, which is utterly insufficient to rest a new genus on.

There only remains the question as to whether or no it is a newly discovered animal. Now Ehrenberg's figure is egregiously unlike *M. tyro.* The antennæ of *Tubicolaria naias* are short, those of *M. tyro* are *very* long; and while the trochal disk of the former is barely more than the width of the body, that of *M. tyro* is at least three times as wide.

Moreover Ehrenberg states that his *Tubicolaria naias* is in his opinion the adult form of Dutrochet's *Rotifer albivestitus.* Now I have read Dutrochet's paper which appeared in 1812 in the 'Annales du Muséum d'Histoire Naturelle," and while his written description is very vague, his figures are so bad that it is impossible to determine anything from them. He however distinctly states that the antennæ of *Rotifer albivestitus* were much shorter than those of *Melicerta ringens,* while those of *M. tyro* are very much longer. To sum up then, I think it not unlikely that Ehrenberg would have called *M. tyro* a *Tubicolaria,* but I do not think that either he or Dutrochet ever saw it:—and it is clear that whether they did or no, it has no right to a different generic title from that of *Melicerta ringens;* for if the difference in the tube is to be held sufficient to constitute a new genus, then must *Melicerta pillula,* Mr. Cubitt's new Melicertan, have a new generic name.

I ought to mention that Mr. Bolton tells me he found this new rotifer some two years ago in his prolific pond at Stourbridge.

II.—*On the Identical Characters of Chromatic and Spherical Aberration.* By Dr. Royston-Pigott, M.A., F.R.S.

(*Read before the* Royal Microscopical Society, *October* 6, 1875.)

During the experience of the last twenty-five years I have had opportunities of hearing a number of persons express opinions about spherical and chromatic aberration as though they were totally different things.

To show how identical they are in the nature of things: Take an ordinary burning-glass and expose it, first, to blue solar light, and secondly, to red.*

Here, the spherical aberration of white light will now be changed into the spherical aberration of the blue and red rays. It will be found first of all with the *blue*, that the image of the sun is formed nearer to the lens; and secondly, that the red rays form an image farther from it.

This should be well known; but another point will strike the accurate observer, *that the size of the image differs in the two cases.*

Lastly, if the marginal rays of the burning-glass be compared with those of the centre the image will be formed at very different places, showing the spherical aberration of the blue ray; and the same experiment determines the spherical aberration of the red. This is for convenience called chromatic aberration, but all the time it is really and intrinsically the *chromatic spherical aberration.* For it is the aberration caused by the spherical forms of the lens, according as the light is blue or red.

I have noticed that in recent numbers of the 'M. M. Journal' a confusion has been made as regards spherical and chromatic aberration. Thus, one writer makes the somewhat startling statement that *all chromatic aberration does not involve spherical aberration.* It may be safely declared, in direct contradiction to this, that the aberration of every coloured ray passing through a glass lens is wholly due to the spherical curves of the lens. Chromatic aberration, in fact, being really the spherical aberration of the particular colour passing.

There is no other method of finding the chromatic aberration of a given coloured ray except by using the refractive index of the colour instead of that of mean rays in the expression for spherical aberration: mean rays meaning white light.

It is true that Parkinson says achromatism depends on focal length. But he also says that only as many lines of the spectrum can be united as there are systems of lenses: so that if there were eight lenses of different refractive powers, eight fixed lines of the spectrum only could be united.

* Using blue glass or red as a shade.

And speaking of irrationality of dispersion, he says, "if it had no existence we might simultaneously unite lights of all species. But since the colours are disproportionately dispersed in different media, the other colours will in such a case be very nearly but not exactly united. A pencil therefore of light refracted through an achromatic combination will illuminate a screen with light still slightly coloured, and give rise, as we have stated before (Art. 167), to a secondary spectrum. A combination of different media achromatic for all kinds of light being thus in general unattainable, it is customary to unite rays which are powerfully illuminating and also differ much in colour, the rest remaining partially united." *

Another familiar example of chromatic aberration being merely the spherical aberration of a coloured ray is given by Sir John Herschel's explanation of the achromatism of the Huyghenian eyepiece. †

Finally, to give examples, the spherical aberration of the blue ray is very different from that of the red ray.

Whenever spherical aberration exists, there also exists for monochromatic light a least circle of aberration or the smallest ring through which all the rays from a given lens pass.

For the same lens, aperture, and same conditions of focal distance or origin of light, considered as a point, the size of this ring is directly proportionate to the *dispersion* of the glass for that particular colour.

When photographs were taken by Dr. Woodward with blue light, the spherical aberration of the other coloured rays was entirely got rid of. Dr. Woodward would have found a much more blurred image with ordinary compound solar light.

A blurred image or indistinctness can be produced by residuary spherical aberration alone. This very aberration varies notably for different colours. It may be as well to state that every fundamental expression or calculated formula for determining aberration is used only for homogeneous light, that is, light of only one refractive index; but that may be any value found in nature. In this way the chromatic aberration of a given colour is found from the spherical: by substituting the refractive index of the said colour in the formula for the spherical aberration.

It is quite plain, to common sense, that the spherical curvature of a lens must produce spherical aberration whatever homogeneous coloured ray shall be transmitted through it. So that all chromatic aberration does involve spherical aberration: and is identical with it so far, that every coloured ray on passing through spherical surfaces of refraction (*as in an ordinary lens*) actually produces its own

* Page 160, Parkinson's 'Optics,' 2nd edition.

† See Herschel on the Telescope, page 56.

peculiar spherical aberration, dependent on the nature of the glass employed.

One other point should be stated, and that is, *aplanatism is usually calculated only to a first approximation*, though it is capable of being done to a second and third still nearer approximations: consequently the conventional expressions now loosely in use of aplanatism and achromatism are approximate terms: the greater the number of lenses of different kinds of glass, the greater number of the lines of the spectrum can be united in one focus. And for those lines or rays so united there exists no absolute achromatism and aplanatism, but a residuum more or less delicate must remain. Some of these ununited rays produce corresponding aberrations. Hence the extraordinary precision of definition produced by stopping out the refractory spectral lines.

III.—*Perforating Proboscis Moths.* By Henry J. Slack.

(*Read before the* Royal Microscopical Society, *Oct.* 6, 1875.)

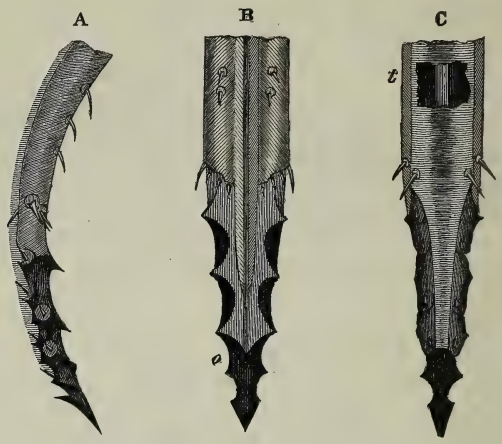
At the meeting of the Royal Microscopical Society, on the 6th October, Mr. Slack, Hon. Sec., called attention to a slide of the perforating proboscis of a moth given by Mr. McIntire to the Society in April, 1874, and described by him in a short paper of that date as having been purchased amongst damaged specimens, and said to have come from West Africa. Mr. McIntire appears to have been the first observer who described this object, and thus opened a new chapter in the history of lepidopters, none of which had been supposed to be furnished with such an apparatus. Mr. Slack read some portions of a paper on this subject by M. J. Künckel which appeared in 'Comptes Rendus' of the French Academy for August 30 of this year, and the following extracts are supplied by him.

M. Thozet, a French botanist, established in Australia, at Rockhampton, a little town on the Tropic of Capricorn, called M. Künckel's attention, in 1871, to a lepidopter of the genus *Ophideres* which he accused of perforating oranges to feed upon their juice. Convinced, as all naturalists were, that lepidopters without exception had flexible probosces destitute of rigidity, M. Künckel doubted M. Thozet's observation, and put aside the alleged devastators, intending to examine them at leisure.

Lately he received a copy of the 'Capricornian,' No. 9, 8th May, 1875, published at Rockhampton, and found in it an anonymous paper confirming M. Thozet's account, and establishing beyond doubt that *O. Fullonica* perforated the oranges, as stated before.

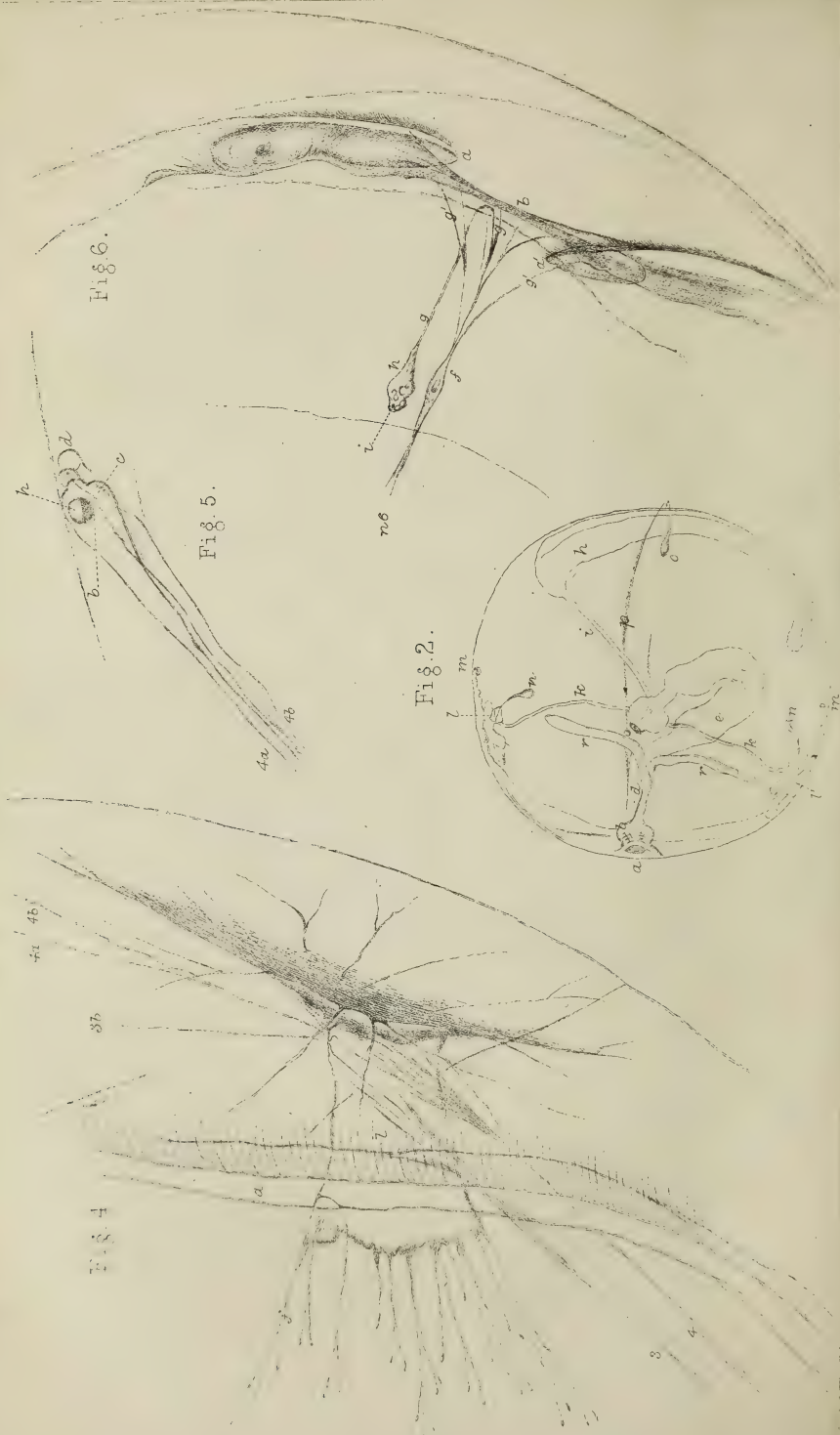
M. Künckel then examined his specimen, and says, "By a strange exception lepidopters of the genus *Ophideres Boisd.* have a rigid proboscis and a veritable auger;" and he describes it as capable of piercing the most resisting envelopes, and quite a model for an artisan's tool. It is incorrect to call the proboscis rigid, as it curls up in the usual way; but instead of a soft terminal portion, it has a hard one which he thus describes. "The two adpressed maxillæ terminate in a sharp triangular point furnished with two barbs. They then swell out and present on the lower surface three parts of the thread of a screw, while their sides on the upper surface are covered with short spines springing from a depression with sharp hard sides. These spines are to tear the cells and the pulp of the oranges, as a rasp opens those of beetroot, to extract the sugar. The upper portion of the proboscis is covered from below and on the sides with fine serrated striæ disposed in a half helix, which give it the qualities of a file. These striæ are from time to time

interrupted by small non-resisting spines which serve as tactile organs. The orifice of the canal by which the liquids ascend is situated on the lower face below the first thread of the screw. The figures beneath render sufficiently intelligible this short description.



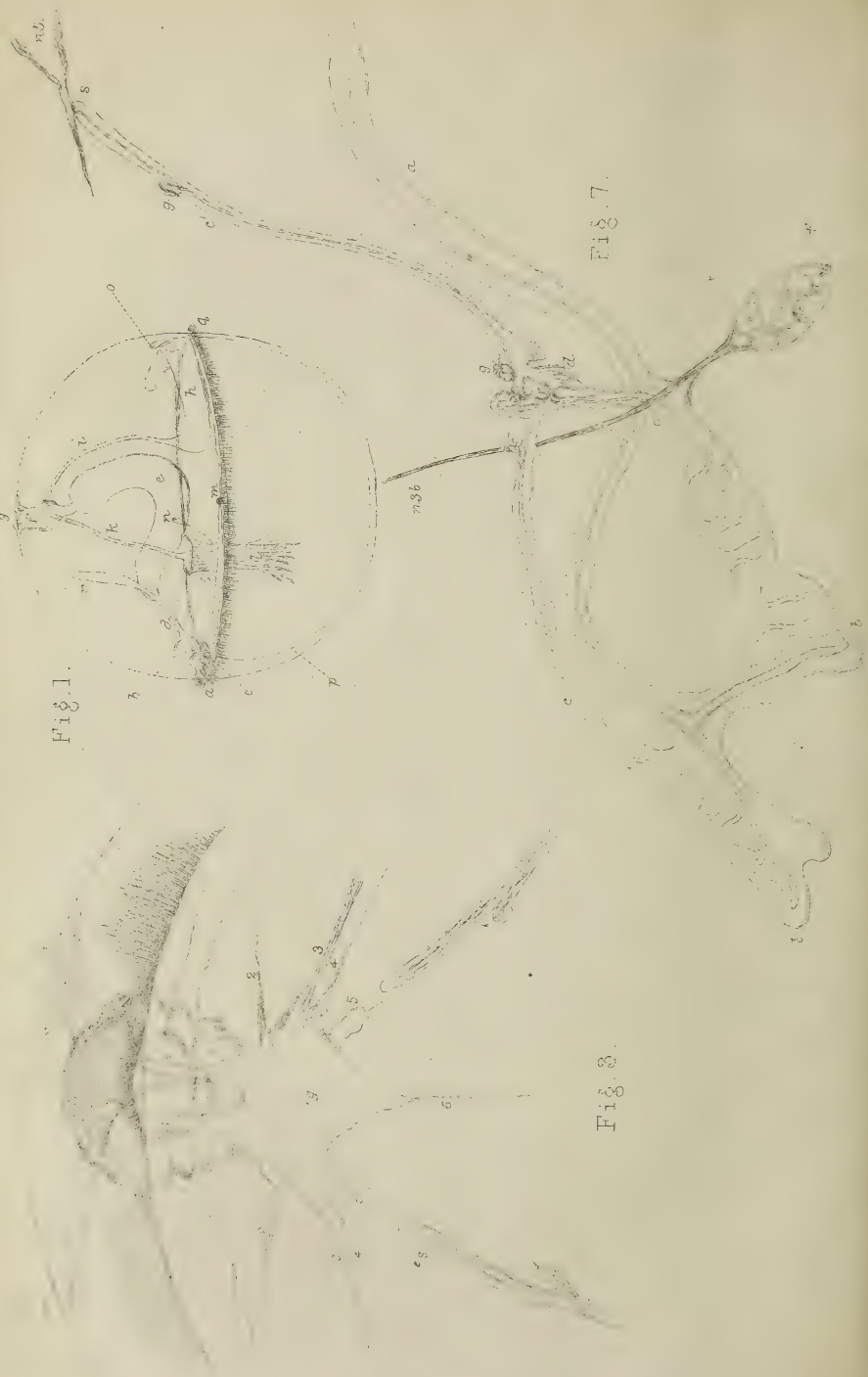
A, profile view; B, seen from beneath; C, from above; *t*, internal canal; *o*, aperture of canal.

"Not content with examining *O. Fullonica*, I studied *O. Salaminia*, *O. materna*, and *O. imperator*, which all had auger-like probosces. The structure of these maxillæ affords a generic character of great value; it moreover establishes a closer relation between the lepidopters, the hemipters, and certain dipters which have maxillæ adapted to pierce tissues."



Trochosphæra æquatorialis.

W. West & Son sc.



Trachosphæra

W. West del.

IV.—*Trochosphæra æquatorialis, a spherical Rotifer found in the Philippine Islands.* By Herr Semper.

Plates CXX., CXXI., and CXXII.

It is well known that the farther we descend in the scale of animal life a gradual abolition of all climatic contrasts becomes apparent; whereas among the higher animals these contrasts are exhibited in innumerable species and varieties. The Rotatoria on the whole follow this rule. My surprise was therefore considerably heightened when in a very limited space on the Philippine Islands I discovered a specimen differing from all its kin in the highest degree. The strange relations existing throughout its organization will perhaps justify a more minute description.

This creature is found among worms (for the most part naids), copepoda, other rotifers, infusoria, &c. (all of which resemble the European forms more or less), in the fresh water of the ditches which surround and intersect the rice-fields of the plain of Zam-

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EXPLANATION OF PLATES CXX., CXXI., AND CXXII.

Figs. 1 and 2.—Trochosphæra slightly magnified. Fig. 1 seen from the side. Fig. 2 as seen from the anal pole. *a*, mouth; *b*, mastax with jaws; *c*, brain-ganglion; *d*, pharynx (œsophagus) with the two glands *r*; *e*, stomach; *f*, cloaca; *g*, anus; *h*, ovary; *i*, oviduct; *k*, excretory duct; *l*, muscle-layer; *m*, eye; *n n*, the two problematical lateral sense-organs; *p*, middle nerve for the middle sense-organ *o*; *q*, equatorial circular fringe.

Fig. 3.—Section of mouth largely magnified. *o*, mouth; *m*, pharyngeal muscle; *c*, cilia of the equatorial fringe (not represented in front of the mouth to show the finer cilia of the buccal funnel); *g*, ganglion; 1 and 2, nerves to the pharynx and front part of the equator; 3 and 4, nerves to muscles, eyes, and sense-organ; 5, nerve of the excretory organ; 6, unmatched single nerve of the sense-organ *o*.

Fig. 4.—Muscle surface of one side. *a*, equatorial fringe; *b*, muscle-hollow along which the muscle surface *l* lies; *f*, its passage into the aboral hemisphere; 3 and 4, nerves to the muscles; 3 *b*, nerve to the lateral sense-organ; 4 *a* and *b*, nerves to the eye and another ganglion-shaped organ (cp. Fig. 5).

Fig. 5.—4 *a*, nervus opticus; *a*, lens of the eye; *b*, pigment covering of the eye; *c*, ganglion opticum; *d*, uncertain organ, final swelling of the nerve 4 *b*.

Fig. 6.—Part of the equatorial fringe, where it is broken at a point immediately opposite the mouth. *a a′*, blunt ends of the circular fringe; *b*, its connecting bridge not ciliated; *n* 6, middle nerve; *f*, ganglion-like swelling belonging to it; *g*, middle nerve to the sense-organ *h* with its final body *i*; *g g′*, nerves of the back part of the equatorial fringe.

Fig. 7.—Organ of excretion. *a*, duct; *b b*, the two glandular lobes; *c c*, the two ciliated channels, which cross (or unite?) at *d*; *c′*, extreme end of the ciliated channel to nerve 5; *n* 3 *b*, nerve to the problematical lateral sense-organ *e*; *f*, its extreme body.

Fig. 8.—Jaws of the mastax.

Fig. 9.—*a*, rectum; *b*, contractile bladder; *c*, cloaca; *d*, stomach; *l*, oviduct; *m*, muscular band of the ovary; *o o*, ovary; *n n′*, excretory ducts.

Fig. 10.—Cloaca and sexual organs with developed eggs. *a*, an egg in the cloaca; *b*, egg in oviduct; *c*, oviduct; *d*, point of junction with the cloaca; *l*, rectum; *f*, eggs in the ovary with star-shaped nuclei.

Fig. 11.—A developed young Trochosphæra found in the cloaca of another specimen.

boanga. In the months of October and November, 1859, it was not rare there. Of a perfectly spherical shape (Figs. 1, 2, and 11), without any perceptible front or back end, this animal, $\frac{1}{3}'''$ in diameter, moves with a continuous rolling and circular motion; no fixed axis of direction, however, can be distinguished. It is never at rest, and never fastens itself on any stone or plant, for it possesses no organ whatever, which serving such a purpose might be regarded as a homologue to the foot of rotifers. In fact, motion is caused simply by its strangely altered trochal disk. Just as the animal itself forms a perfect sphere, so with mathematical symmetry the rotatory organ surrounds the sphere in the shape of a fringed equator, and thus divides the creature into two hemispheres exactly equal in size. The remaining organs certainly somewhat disturb this symmetry. The greater part of the inner organs lie in the one hemisphere, whilst the other contains hardly any. The perfect transparency of the skin renders it possible to perceive their relative positions. In the following description I shall give the name of "oral" to the hemisphere containing the mouth and anus, of "aboral" to the other. The surface of both hemispheres is perfectly smooth, yet exhibits certain cavities leading to the mouth and anus (Figs. 1, 2 *a* and *g*) as well as certain places of junction of other organs, which, on more minute investigation, are found to be organs of sense and muscles. As almost all the internal organs are situated in the oral half, their orifices and places of junction of course belong to the same hemisphere. The equatorial line of fringe is not quite complete. If we call a meridian touching the mouth (Figs. 1, 2 *a*), which lies close by the equator, the first, then one diametrically opposite to it would mark the centre of a small interruption in the fringe, and also give the position of an organ (Figs. 1, 2 *o*) to which a nerve is joined (1, 2 *p*) that may be traced straight to the brain, and runs along the aboral hemisphere in a great circle. The anus, its circular edge slightly swollen (1, 2 *g*), is situated almost exactly at the oral pole; and the two simple eyes (1, 2 *m*) are so placed on the circular equatorial fringe that a great circle passing through them and the anus cuts the first meridian connecting the mouth and the problematical sense-organ at right angles. Thus the mouth and the eyes exactly divide the equatorial fringe into four quadrants.

The remaining organs, distinguished by their points of junction with the outer skin, are so situated that the mathematical symmetry becomes considerably disturbed by them. First of all on either side above the eye, and at the same time rather farther towards the mouth, we have two organs (1, 2 *n*), which both by reason of their connection with nerves as well as in form and appearance correspond exactly to the already mentioned problematical sense-organ. Farther on towards the mouth is a wide, ribbon-like

organ (1 *l*) taking its rise, with a broad edge, from a flat hollow on the oral hemisphere close under the skin, but free in the cavity of the body; it lies obliquely under the equator, then crosses into the aboral hemisphere, and here fastens itself to the skin by means of a number of thin filaments. Besides the nerve already mentioned, this is the only organ which in part lies in the aboral hemisphere. I now pass on to a particular description of the several organs.

As regards the histologic composition of the skin, I can give but little information, I am sorry to say. On the whole it is very thin, yet the cuticle is tolerably resistent, and can still be observed in animals preserved since 1859 in glycerine. It is only at the equatorial fringe that any noteworthy thickening appears (Fig. 6). Here I could easily detect two layers, which will no doubt allow of being taken for cutis and epidermis, although I have taken down no written notes on their connective or cellular nature. With the exception of the circular fringe and a second small fringe leading to the mouth, the skin is perfectly free from hair. A real layer of skin muscles is entirely wanting. The circular fringe itself is, as already said, interrupted at one place (6 *b*) diametrically opposite the mouth, yet only partially interrupted. Whilst the fringed arc on each side ends in a blunted point (6 *a*, *a'*), both these are joined by a narrow bridge (6 *b*), which shows the same two layers as the circular fringe itself, but is thinner and has no cilia. The cilia of the equator are tolerably long, and under the microscope exhibit the motion belonging to all snail larvæ, but no rotatory phenomenon. Still, through its relation to the second small fringe leading to the mouth (3 *o*), it is proved that this equatorial fringe really is the homologue of the rotatory organ of the Rotatoria. It further follows that the aboral hemisphere corresponds to the flat part situated between the creeks made by the fringe in Floscularia, i. e. to the forehead, whilst the oral corresponds to the real body of all other footed and footless Rotatoria. It thus becomes impossible to denote the parts of the body in a way that would apply to all the different forms of Rotatoria.

The muscular system is very meagrely developed. Besides those which, situated at the pharynx and at the head of the pharynx, belong to the tractus, undoubtedly developed muscles are only found at the mouth and anus. Two broad muscle-bands at the head of the pharynx, which have places of junction with skin in the oral hemisphere, have a forward pull (3 *m*, *m'*). A single (unmatched) muscle in the oral hemisphere acts as suspensory ligament to the oviduct and anus, both of which it joins simultaneously (9 *m*). The muscles are smooth, and of a yellow transparency. Probably the two broad ribbons mentioned above, which lie in the two quadrants nearest to the mouth yet connect the

oral hemisphere with the aboral (1 *l*, 4 *l*), belong to this point. With a broad end each joins to a hollow in the oral surface (4 *b*), then close under the skin, but yet suspended freely in the corporal cavity, it passes under the equatorial fringe and fastens itself (4 *f*) to the aboral hemisphere by a number of long, thin filaments, which in their course are divided up and join the skin with ends somewhat broader. I have never noticed contractions of these ribbons: yet in appearance they correspond to the pharyngeal muscles. Among them some nerves (which shall be described immediately) pass along, which are partly lost in them, in the part nearest to the hollow of junction.

The central nerve-system consists of a large brain-ganglion, lying over the pharynx (if the aboral hemisphere is regarded as the upper, i. e. as the forehead), with deep inlets in front and drawn out into large points towards the back in the middle as well as on the sides (3 *g*). Altogether five pairs of nerves leave the brain, and one single (unmatched) nerve from the middle point behind (6). The first two (3—1, 2) pass, the first to the opening of the mouth and the head of the pharynx, the second to the front part of the circular fringe. The third nerve with the fourth arise in common from a lengthening of the brain, still they separate so soon that they cannot be regarded as branches of the same nerve. At first they run almost parallel till they reach the problematical muscle-layer and its hollow (4 *b*, *c*). Here it seems they both send off branches into this organ, but both pass on under it in their main branch. Then they separate; the third nerve turns suddenly downwards at an obtuse angle, i. e. up towards the oral hemisphere (1 *n*; 4—3 *b*) and passes into a problematical organ (1 *n*), that will be more minutely described immediately. The nerve No. 4 divides into two branches immediately behind the muscle-band (4 *a* and *b*), which, passing on close by one another, end near the circular fringe in the eye and in a problematical organ lying close by this. The fifth nerve (3—5) begins at a short cylindrical appendage of the brain, passes across the cavity of the body, and joins the end of the organ of excretion, which will be described later on. Lastly, the sixth single nerve crosses the equator—differing in this from all the remaining nerves that lie in the oral hemisphere—then exactly at the meridian it passes close under the skin of the aboral hemisphere (1 *p*), again crosses the equator opposite the mouth (1 *q*)—at least in its main branch—after first (6 *f*) exhibiting a ganglion-like swelling of one cell, sends out on the sides symmetrical branches towards the encircling fringe which just here is interrupted (6 *g'*), and at last through the said main branch passes into a peculiar organ (6 *h*) lying on the oral surface.

Of the organs of sense the eye is easily found. It lies close by the equatorial fringe (1 *m*), and, in conjunction with the mouth and

the break in the fringe, divides it into four quadrants exactly. The optic nerve is the one branch of nerve No. 4 (5, 4 *a*); this swells into a very lengthened club-shaped ganglion (5 *c*); close upon it stands the eye furnished with a round lens (5 *h*) and with a semi-spherical covering of pigment closely enveloping it (5 *b*). The second branch of the same nerve also swells into a ganglion (5 *d*) situated near the eye-ganglion; in this, however, in spite of all my pains, I could find no particular final organs or elements, which might have explained the meaning of this organ.

Just as little do the other already mentioned final organs of nerves 3 and 6 allow of a certain explanation. The two nerves 3 bend round, as I have already said, behind the muscle-layer to the anal pole and end here (1, 4 3 *b*), crossing the canals of the organ of excretion (7), in an oval swelling (7 *e*) with a number of large ganglion-cells and a very small bright body of dark contour (7 *f*), which is situated on the rather pointed end of the ganglion, and rises close upon the skin. Whether a fine hair (for feeling or hearing perhaps?) is upon it, I could not distinguish, having no immersion lens. Quite similar too is the structure of the final organ of the single nerve No. 6 (6 *h*). The ganglion is here less large, deeply indented, and contains ganglion-cells both fewer and smaller; on the blunt end, rising close upon the skin, the bright extreme body is here too found, as in the two other organs. Even though a more minute specification of the nature of these bodies is impossible, yet there can be no doubt, after the description just given, that they belong to that series of sense-organs, still somewhat problematical, which have lately engaged such especial attention. One may well think of the pedunculated sense-organ situated behind the forehead in many Rotatoria, and, as is well known, more minutely described by Leydig; its position too allows it to be treated as homologous with these, for the three organs of the Trochosphæra all belong to the oral surface, and do not therefore lie on the forehead, but outside the circle of fringe: just as in the case of *Rotifer* (*vulgaris*), &c., the organ adduced does not lie on the surface of the rotatory organ—the forehead—but behind it. For the rest, the morphological comparison of sense-organs, at least in the case of animals without a vertebra, is always unsatisfactory, since it is well known that among them organs physiologically equivalent—eyes, ears, &c.—may often occur in the most various parts of the body, that can be by no means morphologically compared together. I will only instance here the most striking example, the occurrence of eyes on the belly of Euphausia; a discovery, however, which was not, as Gegenbaur in the second edition of his Comparative Anatomy wrongly states, first made by Claus, but by me.

Close under the equatorial fringe lies a small indented fringe of hair (3 *o*) which leads into the hollow of the mouth and serves only

for the introduction of food, whilst the first is only a rotatory organ.

Claparède, the excellent zoologist of Geneva, whose early death is much to be deplored, lately showed that this mouth fringe occurs in all other Rotatoria as well. The mouth is succeeded by a funnel-shaped thickly fringed cavity, joined by the mastax (1 *b*) furnished with the characteristic jaws of the Rotatoria (8). With this mastax is connected a longish, thin pharynx (1 *d*), which passes straight inwards, and at the place of passage into the stomach itself (1 *e*) gives rise to two gastric glands (1 *r*), such as occur in other Rotatoria in the same place. The thick hair in the pharynx is in the direction of the stomach. The latter is broad, cylindrical, and has a thick coating formed by large colourless cells; it stretches a little over the (1 *e*) middle of the oral hemisphere, turning a little towards the aboral, then bends sharply round and passes in a straight line towards the anus. The last part of the intestines, that can be perfectly separated from the small final intestines (9 *a*) by a muscular sphincter, must be considered as the cloaca (1 *f*, 9 *c*), since the excretory ducts (9 *n*, *n'*) as well as the simple oviduct (9 *e*) and a contractile vesicle are in connection with it. The final intestine is thickly covered with hair as well as the stomach; the direction of the hairs in the former is towards the latter.

Although the contractile vesicle at the cloaca certainly is no continuation of the two excretory ducts, yet I suppose it must be compared with the excretory bladder of the other Rotatoria. Its contents are always of a glassy brightness, it is free from cilia, and its contractions are not rhythmical, their duration being between five and fifteen seconds. The contraction takes place by jerks. Although the colourless contents of the bladder cannot be followed farther in its motions directly, yet from conditions of contraction or expansion of the cloaca and intestines connected with the narrowing of the bladder, we may conclude that the liquid coming from that bladder does not—or at any rate does not always—empty itself away, but passes straight into the stomach. For upon every contraction of the bladder an expansion of the cloaca immediately follows, and on the close of the latter immediately, with perfect regularity, an opening of the sphincter of the final intestine occurs, and an expansion of the latter. The expansion of the cloaca is not simultaneous in all its parts; but the part of it where the bladder leads into it is first expanded, and the expansion then is propagated in waves till the commencement of the intestines, and passes on into this in the same way. This always takes place whilst the anus is perfectly closed. The coats of the intestines and cloaca remain expanded a short time, that of the first for a longer time than that of the latter, which soon falls together again. Lastly, if

the intestines are contracted, the sphincter immediately closes, completely separating it from the cloaca.

Though I have never observed an opening of the anus after a contraction of the bladder, but, on the other hand, have noticed the contractions and expansions as described above in the case of many specimens, and in the same individual specimen for a long time, yet I will not assert that the contents of the bladder are not at times transmitted to the anus. Still, from what has been said above, I think it certain that it is far oftener driven back into the stomach instead of being emptied away. If then the liquid from the excretory organ really gets into this bladder—as is probable—it would follow that it cannot be considered pure excrement; it must, on the other hand, be regarded as in a certain way necessary or serviceable a second time for the nourishment of the creature, since in the manner described it is again driven back into the resorbent apparatus. The so-called excretory organ then is not what its name implies, at any rate not entirely such; and thus this organ—physiologically regarded—would be intimately connected with the kidneys of the molluscs, i. e. of those living in water, in the case of which this organ undoubtedly serves for passing water into the blood. The excretory organ is double, as usual, and exhibits the well-known formation of this organ in Rotatoria. At the cloaca begin the two ducts (1 *k*) in such a manner as to be situated pretty nearly in the meridian plane passing through the two eyes; they then bend forwards towards the mouth, and at the same time towards the equator, and pass under the muscle-band at the side into the glandular part. The duct has a coat of considerable thickness (7 *a*); beneath the muscle it divides off, and quickly swells to the two irregularly formed glandular flaps (7 *b*, *b*) in which the upper part of the canal winds. Each glandular flap passes into the two ciliated channels (7 *c*), which exhibit four or five of the characteristic ciliated tags rising upon them (7 *g*). At *d* the two channels *c* cross; here one of the two must end or they become joined; for from this place only a single channel *c′* can be traced up to the before-mentioned nerve 5; by this it is held fast. In this last section a single ciliated tag occurs.

Of the sexual organs I only recognized the female. They are exceedingly simple; from a single ovary a thin oviduct takes its rise (1 *h* and *i*). The ovary (1 *h*, 9) is a flat band, rather broad, which is situated close above the equator in the two back quadrants away from the mouth (2 *h*); with its flat surface it places itself along the skin through almost its whole length, only the free end bends away a little (2 *h*), as also does the end from which the oviduct originates (2 *i*). The whole band of the ovary is filled in its undeveloped state with small granular cells, three or four of

which make up the width of the band (9 *o o*). As the animal grows older this stretches itself lengthwise, the cells arrange themselves in a single row, so that the breadth of the ovary, i. e. at the end belonging to the oviduct, is always filled up by a single egg (10 *f*). Of the small nuclei (9) originally round, constituting the undeveloped eggs, which afterwards were no longer recognizable by their varying contours, the star-shaped ones (10 *f*) grew large; the eggs themselves, although without any covering, separated themselves sharply from one another, especially towards the oviduct. This is uncommonly delicate, but capable of stretching to an extraordinary extent; it is formed directly by the lengthening of the exceedingly thin tunica propria of the ovary, and appears not to possess an epithelium. It joins the cloaca (9) so as to be just in the middle (1) between the points of junction of the excretory ducts.

As soon as the egg passes into the oviduct, the nucleus contracts to a round bladder (10 *b*); but even here no egg-skin forms itself. Then it reaches the cloaca (10 *a*) without an egg-shell being formed even here, and here it develops into a young animal, which corresponds exactly to the mother in all particulars, even in the size and manifest separation of several inner organs (11). At the beginning of my investigations, October 14–16, 1859, I found a number of specimens with developed eggs in the ovary and oviduct, but only occasionally one with an egg in the cloaca. From October 18–20 all the larger specimens had eggs already developed, with generally one in the cloaca, sometimes two, in one case even three; then they were always in different degrees of development. Lastly, from October 25 to the beginning of November almost nothing but young. Development then seems to progress exceedingly rapidly, as was to be expected from the high and uniform temperature of the streams in the Philippine Islands.

My reason for calling these female was because the principle of the contrast formerly supposed between the real egg and the germ (ovum and pseudovum) cannot be maintained unswervingly according to the latest investigations on parthenogenesis. I certainly believe that I only discovered female Trochosphæra, which multiplied by parthenogenesis; for in all the hundreds of specimens which I observed, I never succeeded in discovering undoubted males nor zoosperms in the cloaca of specimens containing eggs. A short time after making these observations I was obliged to leave Zamboanga; and on returning three months later, and looking for my Trochosphæra in the same ditches, I could not find a single specimen. If we might suppose that in this interval sexual generation had begun, the succession of the two modes of generation there would correspond to ours here, i. e. in the heat of summer parthenogenetic generation, on the approach of the cold season

sexual, the germs of which, there as here quiescent in the mud, wait to be revivified by the increasing warmth of next year's sun. Unhappily I never succeeded in again discovering the Trochosphæra either at Manila or on Bohol, so that I must leave it to future visitors of Zamboanga to solve the questions here proposed, and to discover really sexual specimens of this the most interesting of all Rotatoria.—*Siebold and Kölliker's Zeitschrift*, drittes Heft.

V.—*Extracts from* Mr. H. E. Fripp's *Translation of* Professor Abbe's *Paper on the Microscope.*

(*Continued from p.* 201.)

The undulation theory of light demonstrates in the phenomena of diffraction a characteristic change which material particles, according to their minuteness, effect in transmitted rays of light. This change consists, generally, in the breaking up of an incident ray into a group of rays with increased angular dispersion within the range of which periodic maxima and minima of intensity (i. e. alternation of dark and light) occur. But these angular distances are for each colour proportional to its wave-length, and increase, therefore, in size from violet to red, and are also inversely proportioned to the distances between the particles in the object which cause diffraction.

This effect, which is not only such as might be theoretically predicted, but also capable of exact calculation, may be readily observed. Having placed some object of the kind in question under the microscope and got its detail in focus, the ocular must be removed and the image of the object in the open tube viewed with the naked eye, or a suitably arranged microscope of weak power ($\frac{1}{1}^{0}$ to $\frac{2}{1}^{0}$) which can be let down in the tube to the upper focal plane of the objective. The image of the mirror or whatever illuminating surface may be used will be seen as it is formed by the undiffracted rays, and surrounded by a greater or less number of secondary images in the form of impure coloured spectra, whose sequence of colours, reckoning from the primary image, is always from blue to red. Objects consisting of several systems of lines which cross each other show not only a series of diffraction images of each group in the direction of their perpendicular, but also other additional series in the angles between the perpendicular groups. Insect scales and diatom valves exhibit these phenomena in the greatest variety.

This method of direct observation of pencils of light coming from the object enables us to determine by experiment what part

is played by diffractive phenomena in forming the image of the structure in question. A suitable test-object being placed in focus, and the light being suitably regulated by diaphragms placed immediately *above* the objective, as closely as possible to its upper focal plane, for the purpose of excluding at will one or another portion of the groups of rays exhibiting diffractive effects, the image of the preparation, as formed by those rays only which were not so shut off, could be readily observed with the ordinary ocular. The immediate result of experiments carried out in this manner was as follows, it being first premised that every trial was made with very correct low-power objectives ($1\frac{1}{4}$ to $\frac{1}{4}$ inch) and corresponding weak amplification: Higher powers, an immersion lens of $\frac{1}{8}$ inch in particular being used only to control the results obtained already with coarse objects, by experiments on the finer diatoms. The preparations for all decisive trials were of such a kind that their structure was accurately known beforehand, system of lines scratched in glass, whose linear distance varied from $\frac{1}{800}$ inch to $\frac{1}{1200}$ inch; similar groups of lines ruled on silvered glass, the silver coating being immeasurably thin; groups of lines crossing each other without any difference of level were obtained by laying upon each other two glasses, the surface in contact being separately ruled.

The facts thus ascertained are—

(i.) When *all* light separated from the incident rays by diffraction was completely shut off by the diaphragm, so that the image of the preparation was formed solely by the remaining undiffracted rays, the sharpness of outline at the confines of the unequally transparent parts of the field was *not* affected, provided the opening of the diaphragm remained sufficiently large, so that no diffraction arising from the reduction of its opening should occasion any visible lowering of the "necessary amplification"; nor will the clear recognition of separate structural particles be sensibly hindered, provided that not more than 30 to 50 of such particles are found in $\frac{1}{25}$ inch.* But the more this number is exceeded, so much the more of detail disappears; so that when the fineness of detail reaches 100 parts to the millimeter (that is, when their interspace is only $\frac{1}{2500}$ inch) nothing remains visible except a homogeneous surface whatever magnifying power be used, or whatever mode of illuminating (direct or oblique). Even a couple of lines ruled on a glass will, under the circumstances above stated, be not otherwise distinguishable than as one broader line with sharp outlines. With the most powerful immersion lens nothing at all can be seen of the markings of *Pleurosigma angulatum*, and even the coarse lines of *Hipparchia Janeira* remain unrecognizable with a power of 200. In the case

* The definition of number is here uncertain, because the exclusion of diffracted rays, whose diffraction is slight, can only be obtained by using a diaphragm pierced with small openings.

of granular objects and other irregularly shaped particles, diffracted light cannot be completely separated from undiffracted light, and accordingly there is no absolute disappearance of all the particles; but such indefiniteness of image ensues that the finer particles of the preparation fuse into a homogeneous grey cloud.

(ii.) When all light is shut off, excepting a single pencil of diffracted rays, a *positive* image of the particles in the object which caused the diffraction is formed, and appears more or less brightly on the dark field, but without any detail. Ruled lines appear as uniformly clear flat stripes on a dark ground.

(iii.) But when not less than *two* separate pencils are admitted the image always shows sharply defined detail, whether it appears in the form of system of lines, or of separate fields; nor does it matter whether undiffracted light passes in with the incident cones or not: that is to say, whether the image appears on a bright or a dark field. If fresh pencils be set in operation fresh details appear, but always different, according to the degree of minuteness, or to the nature of the markings; *and this detail is not necessarily conformable either with that of the image as seen by ordinary illumination, or with the real structure of the object as known or ascertained in other ways.* In respect to this last point the following particulars are noteworthy.

(iv.) A simple series of lines will be always imaged as such when two or more illuminating pencils are set in operation, but the lines will appear doubly or trebly fine when, instead of the pencils being consecutive in order of position, one, two, or more intervening pencils are passed over. Thus a group of two lines only in the object appears as if composed of three or four separated sets. The phantom lines thus created cannot be distinguished by help of any magnification from the normal image of actual lines of double or treble fineness, either in respect to sharpness of definition or constancy of appearance, as may be shown by a conclusive experiment, in which namely, the falsely doubled image appears side by side with the image of an object actually ruled with lines of double fineness.

(v.) When two pieces of simple lattice cross each other in the same plane at any selected angle, the systems may, by suitable regulation of the admitted pencil of light, be rendered visible together or separately, and by varying the form of illumination numerous fresh systems of lines and variously figured fields which do not exist at all as such in the object may be made to appear with equal sharpness of delineation. These new systems of lines always correspond in position and distance from each other with the possible forms in which the points of intersection of the real lines of the object may arrange themselves in equidistant series.

With a network crossing at an angle of 60°, there appears, besides several smaller systems of lines, a third set marked just as

strongly as the real network of the object, and with equal distance between the lines, inclined also 60° to the others; and when the three sets are seen together there will be seen perfectly sharply defined six-sided spaces (fields) of the kind observed on *Pleurosigma angulatum*, instead of the rhombic fields. It may be added that all the appearances unconformable with the structure of known objects which are here described were observed with exactly the same focussing under which the normal image appeared well defined, and that they occurred under various combinations of objectives and oculars with regular constancy whenever the illumination was regulated in the same way.

The constant increase of *resolving* power resulting from oblique illumination, and the greater prominence of what was before visible with central illumination, is in every instance solely produced either by the entrance of diffracted rays into the larger aperture (with oblique illumination), which would otherwise not have entered into the objective on account of their greater divergence, or because diffraction pencils which were but imperfectly taken up when direct illumination was used now enter more completely and work with greater effect, whilst the direct rays are relatively less operative.

The facts here recounted appear sufficient, when taken in connection with incontestible laws of the theory of undulation, to warrant a series of most important conclusions which affect the doctrine of microscopic vision, as well as the composition and manipulation of the microscope.

Firstly, as respects the vision of objects under the microscope. Any part of a microscopic preparation which, either from its being isolated, or from its relatively large dimensions, produces no perceptible diffracted effect, is delineated in the field of the microscope as an image formed according to the usual dioptric laws of rays concentrating in a focal plane. Such an image is entirely *negative*, being dependent on an unequal transmission of light which partial *absorption* of the rays (e. g. coloured rays), or divergence of the rays (from refraction), or diffraction of the rays (produced by particles of internal structure), severally occasion. *The absorption image thus produced is an unquestionable similitude of the object itself, and if correctly interpreted according to stereometric rules, admits of perfectly safe inferences respecting its morphologic constitution.* On the other hand, *all minute structures whose elements lie so close together as to occasion noticeable diffraction phenomena will not be geometrically imaged*, that is to say, the image will not be formed, point for point, as usually described by the reunion in a focal point of pencils of light which, starting from the object, undergo various changes of direction in their entrance and passage through the objective.

Now to anyone who clearly realizes in his own mind what are the assumptions upon which a similitude between an object and its optical image is commonly accepted, the foregoing facts must suffice to lead to the conclusion that, under the circumstances above indicated, such acceptance is a purely arbitrary supposition. As a positive instance of the contrary stands the conclusion to which experiments lead by rigorous deduction, namely, that *different structures always yield the same microscopic images as soon as the difference of diffraction effect connected with them is artificially removed from the action of the microscope; and that similar structures as constantly yield different images when the diffractive effect taking place in the microscope is artificially rendered dissimilar. In other words, the images of structure arising from the operation of the diffractive process stand in no constant relation with the real constitution of the objects causing them, but rather with the diffraction phenomena themselves, which are the true causes of their formation.* As this is not the place to enter into a physical exposition of such phenomena, it may suffice to say in brief, that the conclusions here deduced from facts won by direct observation, are fully substantiated by the theory of undulation of light, which shows not only why microscopic structural detail is not imaged according to dioptric law, but also how a different process of image formation is actually brought about. It can be shown that the images of the illuminating surface, which appear in the upper focal plane of the objective (the direct image and the diffraction images), must each represent, at the point of correspondence, equal oscillation phases when each single colour is examined separately.

The delineation of structure seen in the field of the microscope is in all its characters,—those which are conformable with the real constitution of the object as well as those which are not so—nothing more than the result of this process of interference occurring where all the image-forming rays encounter each other. The relation existing between the linear distances from the axis of the microscope of constituent elements of the aperture image, and the various inclination of rays entering the objective, taken together with the dioptric analysis of the microscope, afford all the data necessary for complete demonstration of the above positions. From them may be deduced that in an achromatic objective the interference images, for all colours, coincide, and yield as a total effect achromatism, thus differing from all other known interference phenomena.

The final result of these researches may be thus stated:

Everything visible in the microscope picture which is not accounted for by the simple "absorption image," but for which the co-operation of groups of diffracted rays is needed—in fact all minute structural detail—is, as a rule, not imaged geometrically,

that is, conformably with the actual constituent detail of the object itself. However constant, strongly marked, and so to speak materially visible, such indications of structure may appear, they cannot be interpreted as morphological, but only as physical characters; not as *images* of material forms, but as *signs* of certain material differences of composition of the particles composing the object. *And nothing more can be safely inferred from the microscope revelation than the presence, in the object, of such structural peculiarities as are necessary and adequate to the production of the diffraction phenomena on which the images of minute details depend.*

From this point of view it must be evident that the attempt to determine the structure of the finer kinds of diatom valves by morphological interpretation of their microscopic appearances, is based on inadmissible premises. Whether, for example, *Pleurosigma angulatum* possesses two or three sets of striæ; whether striation exist at all; whether the visible delineation is caused by isolated prominences, or depressions, &c., no microscope however perfect, no amplification however magnified, can inform us. All that can be maintained is the mere presence of conditions optically necessary for the diffraction effect which accompanies the image-forming process. So far, however, as this effect is visible in any microscope (six symmetrically disposed spectra inclined at about 65° to the direction of the undiffracted rays, ordinary direct illumination being employed), it may proceed from any structure which contains in its substance, or on its surface, optically homogeneous elements arranged with some approach to a system of equilateral triangles of $0 \cdot 48\mu$ dimensions (= circa $\frac{1}{52000}$ inch). Whatever such elements may be—organized particles or mere differences of molecular aggregation (centres of condensed matter)—they will always present a delineation of the familiar form. All ground for assuming these elements to be depressions or prominences fails, after proof that neither the visibility of the markings nor their greater distinctions under oblique illumination has anything to do with shadow effects. The distribution of light and shade on the surface of the valve in the form of a system of hexagonal fields, is the mathematically necessary result of the interference of the seven isolated pencils of light which is caused by diffraction, whatever may be the physical condition of the object causing this diffraction: the position of the hexagonal fields, with two sides parallel to the middle ribs, has its sufficient reason in the visible disposition of the diffracted spectra towards the axis of this valve, and can be deduced by calculation without any necessity for knowing the actual structure of the object.

That the same state of things obtains in numerous instances of organic forms, the study of which belongs to the province of histology, we may learn from the instance of striated muscular

fibre. The manifold changes in the characters of the images which present themselves account, to a certain extent, for the notorious discordance between the representations of different observers.

In connection with the foregoing conclusions, which have an important bearing on the scientific application of the microscope, it appears, further, that the limits of "resolving" power are determinate for every objective and for the microscope as a whole.

No particles can be resolved when they are situated so closely together that not even the first of a series of diffraction pencils produced by them can enter the objective simultaneously with the undiffracted rays. As even with immersion objectives the angular aperture cannot, by any possible means, be increased beyond the degree which would correspond, in effect, to 180° in air, it follows that whatever improvement may be effected in regard to serviceable magnifying power, the limit of resolving power cannot be stretched sensibly beyond the figure denoting the wave-length of violet rays when direct illumination is used, nor beyond half that amount when extreme oblique illumination is used. The last limit is, in point of fact, already reached by the finest lines of the Nobert plate and the finest known markings on diatom valves, as far as *seeing* is concerned. Only in the photographic copy of microscope images can resolution of detail be carried any farther.

From these facts it appears that the microscope image—excluding two cases of a similar and exceptional kind—consists, as a general rule, of *two* superimposed images, each being equally distinct in origin and character, and also capable of being separated and examined apart from each other. Of these, one is a *negative* image, in which the several constituent parts of an object re-present themselves geometrically, by virtue of the unequal emergence of light which is caused by their mass affecting unequally the transmission of the incident rays. This image may, for shortness sake, be called the "*absorption image*," because partial absorption is the principal cause of the different amount of emergent light. It is the bearer of the "defining" power, whose amount is determined by the greater or less exactitude with which direct incident light is brought into perfect homofocal reunion. Consequently, it is always the *direct* light which "defines," no matter in what direction it arrives at the objective, i. e. whether the central or peripheral zones of the objective receive it. But, independently of the "*absorption image*," all such parts of the object as contain interior structure will be imaged a second time, and this time as a *positive* image, because these parts will appear as if self-luminous, in consequence of the diffraction phenomena which they cause. Now this "diffraction image" is manifestly the bearer of "resolving" power, that is, the discriminating or separating faculty of the microscope. Its development depends, therefore, in the first and chief place upon angular aperture, in so far

as this alone determines, according to rules above given, the *limits of its possible operation.* But its *actual* amount will, at the same time, depend upon the exactitude with which the partial images blend together: for it is through this last act that the detail which indicates the existence of positive structural elements in the object is rendered visible. Now, inasmuch as these isolated pencils, whose confocal reunion is the necessary condition of the formation of diffraction images, occupy different parts of the aperture, and vary constantly in position according to the character of the object and the mode of illumination: it is obvious that a perfect fusion, in *every* case, of the several diffraction images, and then an exact superposition of the resultant "diffraction image" upon the "absorption image," is only possible *when the objective is uniformly free from spherical aberration over the whole area of its aperture.*

In consequence of the onesidedness with which, in modern times, the improvement of the microscope has been directed towards the increase of angular aperture, the conditions under which abnormal appearances, and especially deceptive alterations of level are produced, occur abundantly in the new high-power objectives, as repeated experience has shown me, and I assuredly do not err in expressing my conviction that the consequences of this state of things affect to an unexpected extent the numerous questions in dispute amongst microscopists concerning the interpretation of minute structures.

Since everyone must admit that the first and most imperative claim which can be made, in the interest of scientific microscopy, upon the performing power of the instrument is *this*—that parts which belong together in the object shall also appear as belonging together in the microscopic image, it follows that uniform correction of spherical aberration throughout the whole area of aperture must be the absolute criterion and rule of guidance in the construction of a microscope. Now, it has been shown that with a dry objective an adequate compensation of spherical aberration is, as a matter of fact, impossible when the angular aperture exceeds 110°. Hence it must be concluded that a dry objective will be less suited for ordinary scientific use in proportion as it renders visible such finer systems of lines as exceed the limits of resolving power answering to that angle (namely, $0 \cdot 35\mu$ for oblique light). The greatest possible increase of resolving power can be obtained in a rational way only by means of immersion objectives, as these alone admit of the largest possible (i. e. technically practicable angular) aperture, without contravening the very first requirement of corrected spherical aberration.*

* The dry objectives made on Abbe's calculations, founded upon the principles before explained, have only 105° to 110° of angular aperture for the highest powers, and cannot pretend therefore to compete, in resolving diatoms, &c., with

A mode of testing which turns upon the determination of the utmost limit of "resolving power," whether tried upon a "Nobert" plate, a diatom, or an insect scale, brings into play a quite exceptional direction of rays of light into the microscope, such as is, indeed, required for this purpose by the physical condition of the problem. Theory and practice teach us that every objective which is not a total failure—however imperfect in respect to correction of spherical aberration—if its lenses be but moderately well centered, can always be made to work with *one* of its zones, e. g. the outermost, if during its construction it has been tried on a similar test.

The proof that an objective can resolve very minute striæ on a diatom or Nobert's test-plate, attests, strictly speaking, nothing more than that its angular aperture answers to the calculable angle of diffraction of the interlinear distance of the striæ on the test, and that it is not so badly constructed that a sufficient correction of its outer zone is impossible. A trial of this sort offers no means of ascertaining what conditions for the correct fusion of aperture images such an objective would present in the much more unfavourable case of the ordinary observing position. Nor can the result be considered as sufficiently characteristic even of the "resolving power" in its more general attributes.

Nor can the test of "resolving power" by direct light be estimated at a much higher value. In the neighbourhood of the limit of resolution corresponding to this form of illumination, all direct light passes through the central zone, and all diffracted light through the peripheral zone of the aperture.

From the point of view presented by the theory here propounded, another method offers itself, which, while employing the usual tests, brings directly into light the particular points which mainly influence the quality of performance during ordinary use of the microscope. If it be desired to test, in a most critical way, the conditions of exact co-operation of pencils of light which pass through every part of the aperture, there are truly no better means than those afforded by natural objects of the diatom class and insect scales, provided that the mere fact of accomplished "resolution" is not made the chief consideration, but that the exact constitution of the total image produced by the objective is studied.

The considerations adduced lead to certain rules respecting the

objectives of much higher angle. The immersion lens is constructed with a free aperture of about 100° in water, i. e. somewhat more than would correspond to 180° in air, because this is attainable without serious disadvantage. Professor Abbe is, however, convinced that even the immersion lens would not lose any of its value for ordinary scientific purposes, whilst it would be materially improved in many respects if its construction were based upon calculations for a smaller aperture, "but," he adds, "in view of this universally accepted standard of valuation, the practical optician can scarcely be expected to trouble himself about qualities of performance which would be very certainly ranked amongst those of a secondary order!"

right proportion between focal distance and angular aperture, which are opposed in many points to the hitherto prevalent practice.

Since theory demands a limitation of angular aperture of 110° for all *dry* combinations, the calculation of minutest detail accessible to such objective is readily made; and it may be shown that if "resolving" power be not unfairly exalted at the cost of the general excellence of the lens, there can be no question of detail which a practised eye would not recognize with a good amplification of from 4 to 500. Now according to the present standard of technical constructive means, such an amplification may be gained with an objective of 3 mm. ($\frac{1}{8}$ English inch), even if the attribute *good* be interpreted a little more strictly than is often done. With immersion lenses, the physical limit of "resolution," even where the angular aperture is the highest attainable, does not extend so far that an amplification of from 7 to 800 will not be fully equal to it; and this amplification would be gained with ease with a well-constructed objective of $\frac{1}{12}$ inch focal length. It may be admitted that an amplification exceeding the minimum here given as theoretically necessary, might greatly facilitate observation and render it more certain *if* the additional amplification be as correct as can be possibly made, although it would not occasion any new facts to be seen. Yet one can scarcely estimate the significance of this empty amplification far beyond the limits stated, and I therefore come to the conclusion that the scientific value of an objective whose focal length (if a dry system) is much shorter than $\frac{1}{12}$ inch, or if an immersion system, than $\frac{1}{25}$ inch, is altogether problematical.

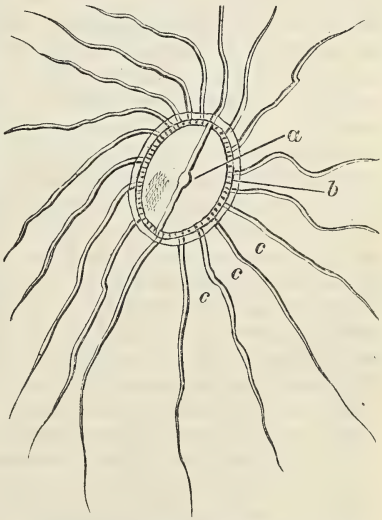
The actual powers of the microscope (in the strict sense of correct and useful power) are, in my opinion, exhausted at these limits, so long, that is, as no circumstances of moment are brought forward which change the bearing of present theory. There exists no microscope in which there has been seen, or will be seen, any structure which really exists in the object, and is inherent in its nature, that a normal eye cannot recognize with a sharply defining immersion lens magnifying 800 times. Reports of extraordinary performances (especially from England) of unusually high power ($\frac{1}{80}$ inch ?) are not of such a character as to induce me to change my opinion and lead me into similar error, for the superiority of such lenses is said to have been proved upon objects to which the results of my observations unreservedly apply, and which are said to appear under such amplification as everyone who can understand and give an account to himself of the optical conditions of such performances must know to be wholly illusory.—From *Proceedings of the Bristol Naturalists' Society*, New Series, vol. i., part 2.

Trochosphæra æquatorialis.

PROGRESS OF MICROSCOPICAL SCIENCE.

Dr. Hudson's Paper on the Rotifera.—Dr. Hudson's paper at the British Association gave an elaborate account of the structure and affinity of these animals. There was considerable discussion upon the author's views, some opposition being exhibited to his tendency to group them with the Entomostraca. However, Dr. Hudson's views are the result of a considerable study of the group, he himself being the discoverer of some very extraordinary novel forms.

An Animal-like Diatom.—The following letter has been addressed to 'Nature' (Oct. 14), by a gentleman who writes from Manila. The cut has been kindly lent to us by the editor. The writer, Mr. W. W. Wood, observes:—I have reason to think that I have made a discovery which may change the ideas of naturalists as to the nature of some diatoms. In collecting Diatomaceæ I have found a species of *Navicula* (?) which is invested with a gelatinous envelope, and from the edges of the frustule project a number of long processes or arms of the same soft nature. These vary much in number, in some specimens being eight or ten, and in others as many as twenty-five or even more. They are longer than the frustule, and radiate from it with much regularity. The diatoms when detected (upon a floating *fucus* common in the sea hereabout) were dead, and I was unable to detect any movements. I have examined so many individuals of this diatom that I think it hardly likely that I have been deceived, as they are by no means very minute. Dr. Carpenter, in the fifth edition of his admirable work on the Microscope, speaks of some observations by Mr. Stephenson on the genus Coscinodiscus, which hint at the possibility of some diatoms having appendages projected through apertures of the frustule. The highest power of my microscope is one of Messrs. R. and J. Beck's, $\frac{1}{5}$th, a very fine glass. I propose to forward as soon as possible the sticks, dry and in balsam, as well as the "gathering" in spirits, to a competent diatomist, who will confirm my observations if correct.



a, the frustule; *b*, the gelatinous envelope projecting beyond the margin; *c c c*, the processes, or pseudopodia.

The Origin of the Red Clay found by the 'Challenger.'—Dr. Carpenter made some remarks of interest on this subject at the Bristol Meeting of the British Association. He said that when it was first

found Dr. Wyville Thomson thought he could trace it to some great river, such as the Amazon, or to the fine deposit brought down by some great river; but he found on further research that he did not get it near the land, for where the water shallowed he did not get it, and he came to the conclusion that it could not be accounted for by any river action whatever. He then formed an idea that it might be what he called the ash of the shells of the Foraminifera, which was now believed to be diffused in enormous masses over the whole bottom of the deep Atlantic. There was first on the surface what Dr. Carpenter believes to be living Globigerinæ; then, below that, what was called ooze, in which not so much the shells as fragments of them were found, and a large quantity of white impalpable matter, forming a very fine white mud, which was certainly the result of the gradual disintegration of the shells. Then at further depths below that, Dr. Thomson, finding this red mud, formed the hypothesis that at great depths there was an excess of carbonic acid, and under great pressures with this excess of carbonic acid the calcareous portion of the shell was dissolved and any mineral matter that was not calcareous was left behind. This mud silicate of iron and alumina was analyzed, as also a portion of the ooze, when it was found by Dr. Thomson that after removing the calcareous portion and dissolving it by dilute acid, a residue of the same kind was obtained, namely, silicate of iron and alumina, and upon that basis he put forward the speculation that this red ash was simply the ash of the shells of these Globigerinæ and Foraminifera. It seemed to the lecturer that this was not the most likely explanation of it. In the first place he had no reason to believe that any ash at all was left behind when the pure shells were dissolved in dilute acid. He believed they would give as pure carbonate of lime mixed with animal matter as could be obtained anywhere. But then another source occurred to him. It was shown many years ago that most green sands occurring in all geological periods from the Silurian downwards, when examined by the microscope, were found to be really internal casts of Foraminifera. In consequence of this discovery, and of the discovery by the late Professor Bailey, of New England, that the same thing was found in recent Foraminifera, Dr. Carpenter and others associated with him found exactly the same thing with foraminiferous forms, namely, that by dissolving away the shell, they could get in some instances green silicates, and in other instances ochry silicates, giving the form of the animal. Chemists all agreed that this deposit took place by a process of chemical substitution, although they were not all agreed as to the precise mode in which it occurred. They all agreed, however, that it was through the decomposition of the animal that the silicates were precipitated from sea water, and that they took the place of the animal substances particle by particle, filling completely the cavities of these minute shells with green or ochry silicates, and thus giving perfect models of the animal. Among Admiral Spratt's dredgings in the 'Ægean' it happened that Dr. Carpenter had some internal casts which were a bright red, and also some which showed the transition from green to ochreous—a green core and a sort of ochreous efflores-

cence on the surface. On another of Admiral Spratt's specimens there was a transition from green to red—that is, in the very same cast he found red in one place and green in another. It occurred to him that it was not improbable that this was the real origin of the red clay. He thought he might say it pretty certainly—and he said it on the authority of Professor Hofmann, who had gone carefully through his series—that these three colours, the green, the ochreous, and the red, were simply dependent on the stages of the oxidation of the iron. They could not be analyzed, because they were so excessively minute. He might mention that the 'Challenger,' near the Cape of Good Hope, came upon a bed of green clay, which was found to be entirely composed of these internal casts, and when that came home materials might be expected for a very thorough chemical analysis of these very curious formations. His own suggestion was that this red clay was not the ash of the shell, but the result of the disintegration of internal casts. He thought it not at all improbable that what was originally green or ochreous was acted upon by carbonic acid, just in the same manner as every chemist knows that felspar was decomposed by carbonic acid; so that in the carbonic acid of the deep strata of the ocean, under the influence of pressure especially, there would be a decomposition or disintegration of these internal casts, and a higher oxidation of the iron giving it the red colour. It appeared to him that that was far more likely to be the origin of this red clay than its being left as an ash from the foraminiferous shells, which, as far as he had examined them, did not leave any ash at all. That Dr. Thomson should find a residue in the ooze was likely enough, because if some of these internal casts were formed in the Foraminifera layer, when all the decomposed shells were dissolved there would of course be the residue of these casts. It would be curious if it was referable to the Foraminifera at all. It was referable rather to the process by which the internal casts were formed, and the higher oxidation of the iron of them, and the disintegration formed by the action of the carbonic acid so as to form this deposit of red clay at the bottom. This seemed to him more likely than that it was formed by the solution of the foraminiferous shells themselves.

NOTES AND MEMORANDA.

Use of the Microscope in Mineralogy.—A paper on this subject was read before the San Francisco Microscopical Society, and it appears reported in full in the 'Cincinnati Medical News' (September). He uses the binocular instrument and a $7\frac{2}{3}$-inch objective, and he makes the following remarks on the subject of preparing and examining specimens:—My method of preparing them is as follows: The slide is placed on a card of the same size, upon which a dot of ink has been made to indicate the centre. A scale of shellac is held

in the flame of a spirit lamp; when nearly dropping it is touched to the glass over the dot. This ensures a central position for the object when mounted. The slide is then heated over the spirit lamp until the cement is liquid and the specimen placed on the melted shellac, with the best surface uppermost. It is then gently pressed down and the slide set aside to cool. It has then only to be labelled and placed in the cabinet. Pulverulent minerals, small crystals, concentrated washings, &c., are best mounted in cells and covered with thin glass in the usual manner. In some cases the dust from powdered minerals adheres to the glass cover if loosely mounted. In such cases I find it better to coat the slide within the turned circle with gum arabic, in rather thick solution. When dry, if breathed upon, the powdered mineral will attach itself to the gummed surface. The glass cover may then be replaced and cemented. Those minerals of which rocks are composed, as, for example, albite, orthoclase, mica, labradorite, epidote, quartz, &c., should not only be mounted as opaque objects, but also in thin sections, to admit of their being examined by polarized light. The study of rocks by the aid of the microscope is growing yearly in favour, and no geologist will now decide on the character of a specimen without first submitting it to the optical test. To fully understand the rocks, which are composite, the student must familiarize himself with the optical properties of the minerals composing them. He must learn to distinguish on the first turn of the polarizer the difference between quartz and felspar, and to decide as quickly if the felspar be orthoclase or albite. This can only be done by careful study of minerals. For opaque minerals I find that the best illumination to be that produced by the parabolic illuminator, although the large bull's-eye condenser will be found to produce good effects when the former apparatus is not at hand. The advantage of the illuminator is, that the light is thrown downward into the cavities of the specimen, which are often filled with beautiful crystals. In transparent sections in which opaque minerals are imbedded, the parabola is indispensable. The most beautiful effects are often obtained by its use. Thin sections should also be examined by polarized light; the details of the inner structure are by this means often brought out in an unexpected manner. The microscope can hardly be dispensed with in determinative mineralogy. If the mineralogist is called upon to decide upon the fusibility of a mineral and finds the result doubtful, he has only to strongly heat a thin fragment in the blowpipe flame, and place it in the field of his instrument. If at all fusible the thin edges will be seen to be rounded. A fragment of a mineral containing alumina, if wet with nitrate of cobalt, and strongly heated, will often show the prominent parts tinged blue under the microscope, when the unassisted eye fails to distinguish the reaction.

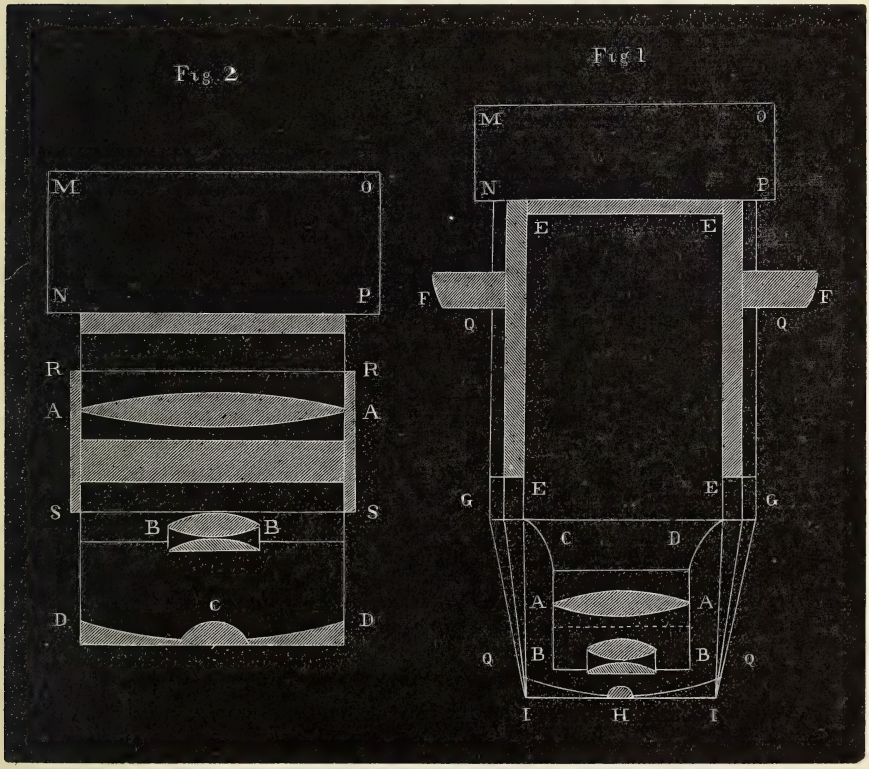
CORRESPONDENCE.*

On a New Form of Object-glass.

To the President of the London Microscopical Society.

Rue Mazarine, *Paris.*

Sir, — We have the honour (Mr. Thuét, ingenieur-opticien, of Paris, 19, Rue Mazarine, and I, microscopist, of 113, Chaussée du Maine) to offer you two new mechanical appliances for objectives corrected for immersion. These appliances have suggested themselves to us from our desire to facilitate the cleaning of the optical



arrangement. Even using the immersion front with glycerine, it is seldom that moisture does not pass the front lens into the correcting lenses, and thus the frequent cleaning of the objective. We submit our systems to the Society, and we will conform to the decision that may be passed upon this new idea.

* Several letters are "crushed out" of this number, but will appear in our next.

The above objectives are for various objects: the moisture has very little tendency to collect in them; but it is not the same in the immersion working in various fluids. Also, would it not be an advantage to get over this inconvenience by facilitating the cleaning of the optical arrangement without unscrewing the collar?

One of our systems is based upon the correction by Wenham, adopted by Nachet and others; and the other is based on the movement alone of the last lens, or second corrective. The first is more handy for quickness of adjustment, and the second offers greater facility for the centering of the optical arrangement.

Fig. 1 represents my objective corrected by Wenham. A cone fixed to the interior tube E E E E, into which are screwed the corrective lenses A B, A B. The collar E F advances or withdraws the front lens H from the two correctives. The front lens H is fixed in a ring, screwed at I I to the lower extremity of the exterior tube Q Q Q Q, in the part G G of which is screwed the tube of the front lens H. The correctives A A, B B screw into the cone of the interior tube E E E E. When it is necessary to clean these, this tube must be unscrewed as the piece G I, G I, then the two correctives A A, B B from the cone C D. To clean the front lens H, the ring of the tube G I, G I, in the centre of which it is fixed, must be unscrewed. This operation does not necessitate any undoing of the collar E F.

Fig. 2 represents my new objective, with movement by the second corrective. B B and D D represent the mounting of the front lens C, and of the first corrective B B; but the second corrective is mounted in a screwed tube A S, A S, put in motion by the collar R R. The optical arrangement is easily cleaned, without undoing the collar R R, by first unscrewing the two lenses, placed under the second corrective A A, the front and the first corrective.

This system appears to us more simple than the Wenham system.

Your obedient servant,

CLEMENT THUÉT.

[We insert the above as possibly containing an idea of practical value, but it is so obscurely described that we fear our readers will scarcely be able to comprehend it.—ED. 'M. M. J.']

PROFESSOR HASERT'S NEW OBJECTIVE.

To the Editor of the 'Monthly Microscopical Journal.'

DENSTONE, UTTOXETER, *October* 3, 1875.

SIR,—Professor Hasert of Eisenach, who, as the readers of this Journal will probably recollect, manufactured the lenses with which Dr. Schumann performed those truly wondrous feats recorded in his 'Diatomeen der hohen Tatra,' has been for a considerable time engaged in devising an altogether novel species of lens, which will, I expect, form an epoch in microscopy. The problem he proposed to

himself was to construct an objective which should completely satisfy the conflicting requirements of scientific microscopists and of genuine 'Naturforscher,'—which should be so absolutely perfect in its correction as to dispense with screw-collar adjustment for different thicknesses of cover, so as to work through a wide range of covering glass without any perceptible deterioration of the optical image, and combine the utmost beauty of definition with a depth of penetration amply sufficient for all medical purposes, and, while giving a perfect resolution of all known tests, including even those terrible puzzlers *Stauroneis spicula* * and *Amphipleura pellucida*, be capable of being worked up by deep eye-pieces to 3500 diameters without detriment to the definition.

In his correspondence he says: "I hope to bring my new lenses to such a point of perfection that they will not want any correction-screw for different thicknesses of glass cover. I tried a power of 2000 diameters on very fine organic objects with covering glass of $\frac{1}{5}$ and $\frac{1}{10}$ of a millimeter, side by side, without being able to find any difference in distinctness. I also tried *Grammatophora subtilissima* with cover of $\frac{1}{6}$ and $\frac{1}{10}$ without perceiving any difference." . . . . "My old dry lenses show longitudinal dotted lines on *Amphipleura pellucida*, and lateral of the same kind; and in very bright light the little corpusculi can be seen standing diagonally on the longitudinal lines, being of a rectangular form,† as you will find them on *P. balticum* and *P. attenuatum*. My *new immersion lenses* will resolve the object with ease, even with direct light, as they will also show the little corpusculi on the lines of *Gram. subtilissima* and *S. gemma*." . . . . "It [the new pattern immersion lens] will, even with direct light, resolve *Frustulia Saxonica* and the REAL *Grammatophora subtilissima*, which has never been resolved by any instrument which I compared with mine, neither English, German, nor French, all of which I compared at various times. But my slide of *Grammatophora* (in Canada balsam) was *never* resolved by any glass of the best makers. You will also see the puncta of *Amphipleura pellucida* with direct light."

And in his recent catalogue, which appeared within the last three weeks, this new species of lens is thus described: "Objectives of newest construction, on the immersion system, requiring no correction-adjustment for different thicknesses of glass, which, at a magnification of 2000 diameters, and on covering glass from $\frac{1}{11}$ to $\frac{1}{4}$ of a

* A veteran microscopist of great skill as a manipulator, in his letter to me dated Sept. 27, 1875, says: "I have this morning succeeded in resolving *Stauroneis spicula* to my satisfaction. I have had a great deal of trouble with it. It is undoubtedly the very hardest object I ever had to deal with,—*Amph. pellucida* not excepted. I could, however, do nothing with *St. spicula* by lamplight. I soon found morning was the only light that would enable me to deal with it; and I have now seen it resolved,—I may say 'brilliantly,'—all over the *lorica*." Compare also Mr. Leifchild's letter in the 'M. M. J.,' vol. xiii. p. 174.

† Without professing to follow the learned Professor in his views of the markings of *Amph. pellucida*, I may mention that a notable microscopist of my acquaintance sent him an especially difficult slide of that diatom, with a request that he would resolve it, and received in a few days a pencil drawing of it, corresponding in all its details with the above description. This drawing I have seen.

millimeter, exhibit equal distinctness, and bear a magnification of 3500 diameters with clearness and sharpness, and show the dots on *Amphipleura pellucida*, *Surirella gemma* and *Grammatophora subtilissima* even without oblique illumination, but with oblique light, with great prominence, and the most delicate organic objects superbly." So much for the man's own account of the matter. I may now state what I have done with it myself. I tried it with respect to the several points of screw-collar, resolution, penetration, depth of eye-piece, and definition, with the following results :

(1) *Screw-collar.*—So far as I have been able to examine it on this point,—and I tried it on covering glass ranging from ·003 to ·008,—the maker is fully justified in his statement. I found *no difference whatever.*

(2) *Resolution.*—It worked upwards, with comparative ease, through *S. gemma*, *P. macrum*, *Frustulia Saxonica*, and *Navicula crassinervis*, resolving them with great beauty and sharpness; but at *Stauroneis spicula* it STOPPED; and no coaxing could draw it onwards. On this occasion I used a C eye-piece with oblique light from an ordinary concave mirror. Next night, having substituted a peculiar arrangement of my Abraham's prism, and again using the C eye-piece, the resolution was absolutely perfect from end to end, and the object as colourless as water. In both cases I used a Bockett lamp. Its resolution, then, I must pronounce excellent.

(3) *Penetration.*—In this respect also it was perfectly satisfactory, showing layer after layer of tissue to a good depth; and I can only conceive its failure where the operator cuts his sections too thick.

(4) *Depth of Eye-piece.*—In this trial I had at command B and C eye-pieces by Baker, D by Powell and Lealand, and E and F by Ross. The third of these I was soon obliged to discard, as it gave much inferior images to those presented by the E and F eye-pieces.

On *P. angulatum*, with E eye-piece, the resolution was only *moderately* good, and there was a certain amount of unmistakable "fuzziness," which was not pleasant. On *S. gemma* with E and F eye-pieces, the result was simply nil, though it had but a minute before resolved this diatom beautifully with a C eye-piece.

In my own practice I should never think of using it with any higher eye-piece than C. I also came to the conclusion that there was ample room for improvement in our eye-pieces. It will be seen, then, that I have come far short of doing with it all that the maker promised it should do. This the reader may, if he likes, attribute to my want of manipulative skill. But there is something also in the fact that this glass,—confessedly one of a novel and singular construction,—had only been in my hands some thirty-six hours,—too short a time to enable one to become acquainted with all its little whims and peculiarities. For lenses too have their little whims as well as human beings; and these have to be studied, and *humoured*, if one is to succeed in making a lens do its very best.

(5) *Definition.*—*This* I conceive to be the distinctive feature and special excellence of Hasert's new system. Indeed, I seem to myself

never to have known what the word definition really meant till I saw this glass, so beautifully clear, sharp, and distinct were all the details. I certainly never saw any objective that even approached it!

I may add that, throughout the whole trial it was matched with my $\frac{1}{24}$th immersion, which is a glass of no mean capabilities.

On the other hand, this description of objective can never seriously compete with English manufacture, and for the three following reasons:

1. Its inordinately high price.
2. The strong prejudice in England against all objectives unprovided with correction-arrangement.
3. Its comparatively weak magnification.

I ought to have mentioned that, though nominally a $\frac{1}{16}$ inch, it is really only a $\frac{1}{10}$ inch; and that there is a great lack of neatness and finish in the brasswork.

Yours faithfully,

W. J. Hickie.

Mr. Mayall's Letter.

To the Editor of the 'Monthly Microscopical Journal.'

London, *October* 11, 1875.

Sir,—Mr. Mayall appears to have been afflicted with the desire that I should be wrong in the immersed angle of aperture question, and now relieves his mind by attempting to revive the controversy. In this I have said all that I consider necessary, or intend to say.

Mr. Mayall admits his inability to deal with the question, and consequently submits it to "one of the highest mathematical authorities in England." If the high mathematical authority, instead of taking his information second-hand from Mr. Mayall's version, had looked directly at my writings on the subject, he would have found at the root and foundation of it all, a paper dated more than twenty years ago, in the 'Quarterly Journal of Microscopical Science,' wherein I pointed out that any angle in an object-glass must necessarily be reduced by immersion in fluid or balsam, and showed how by means of a special frontal adaptation full aperture on balsam objects could be obtained. Therefore, however consoling to Mr. Mayall the opinion of the high mathematical authority may be, his anonymous verdict will not be considered important by others.

As Crito observed, Möller's Probe-Platte used with the reflex illuminator has so little to do with the question, that a slip of plain glass would answer the same purpose, supposing that there is anything important in the experiment. The diatoms in Möller's Probe-Platte are mounted in balsam, and whether they adhere to the cover, or slide, or float in the medium, practically there will be no difference; and

when fluid is introduced between the lens and cover not a feature in the principle of the reflex illuminator exists—all total reflexion has gone, and I should expect light to pass into the object-glass, and the field would no longer be a dark one.

Yours truly,

F. H. Wenham.

P.S.—Fix a very thick piece of plate glass on stage of microscope with body set horizontally. The upper side of the plate is polished, the under side fine ground. Focus the top surface with an immersion $\frac{1}{8}$th or $\frac{1}{10}$th having the highest available aperture. Place a lamp at eye-piece end, and a disk of light will appear on the under ground surface. The diameter of this is the base of a cone, the apex of which is on the upper surface, and the angle of the cone is the diminished aperture of the object-glass—the angle that the extra immersion partisans will have it is increased by a water connection between the front lens and object surface. Therefore look sharply while an assistant introduces the water intermedium. According to their notion the disk should at once increase in diameter, but not the slightest change is visible; all that the water has done is to straighten the sharp bend of the rays that before existed between the disunited surfaces. The angle in the plate of glass remains just as before.

I need scarcely say that I am wearied with describing these simple demonstrations. They are quite in vain, because the spirit of much of the opposition has shown more of a desire to have the satisfaction or credit of stating me to be wrong than to aid in establishing a scientific truth.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *October* 6, 1875.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

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

Mr. Slack drew attention to the turn-table which had been presented to the Society by Mr. Cox, and which was placed upon the table for the inspection of the Fellows. Also to a new form of microscope which had been kindly sent by Messrs. Beck for exhibition; it had a strong and firm stand, and a set of powers up to a dry $\frac{1}{20}$ inch, and packed in a moderate-sized case which contained all that was really necessary for a complete student's course. Also to a new form of pocket lens by Mr. Browning, and which seemed to be a great improvement upon those generally in use; it was an achromatic triplet, with an unusually large, flat field, and very fine definition. He

also reminded the meeting that in April, 1874, Mr. McIntire exhibited a slide of the proboscis of a moth which had a perforating organ appended to it, and which had been discovered amongst a collection of insects said to have come from West Africa. In 'Comptes Rendus' for last August a paper appeared on Lepidoptera with perforating trunks, which were said to do much damage to oranges in tropical countries. They belonged to the genus Ophideres of Australia. As the paper was of considerable interest, he would send a translation of it to the Journal. The credit of being the first European observer of a moth with such a proboscis certainly belonged to Mr. McIntire, and it would be very interesting if entomologists in this country would take the trouble to look through their specimens, and see if anything similar could be found amongst British Noctuidæ.

Mr. McIntire, after looking at the drawing, believed it represented precisely the same object as the one he had shown, except that the drawing gave a view of both sides of the proboscis, whereas his specimen only showed half.

The President hoped that the subject would not drop, and quite agreed with Mr. Slack that English entomologists should take the matter up.

The thanks of the meeting were voted to Mr. Slack for his communication.

Mr. Beck said he wished to present to the Society a specimen of the blood of the Amphiuma, which was remarkable for the great size of the blood-disks, being the largest known. The creature from which it was obtained was about 18 inches long, of a dark colour, something like an eel, with rudimentary legs. It was found in the Mississippi, and in times of flood they came up the gutters, and were said to bite and kill the negro children. There was, however, nothing in the anatomy of the creature to confirm this idea, and he had heard from naturalists that there was no foundation for the idea that these reptiles were venomous. He received his specimen from a friend in New Orleans. It seemed quite healthy on arrival here, and fed freely on worms, &c. After a time he cut off the tip of its tail, and got a quantity of blood from it, the same as mounted on the slide. After the creature was killed it was injected; and on dissection it was found to contain a great quantity of eggs. Many of the vessels were found to present interesting peculiarities, owing to the large size of the blood-corpuscles which had to pass through them; and he hoped at some future time to be able to present to the Society some slides of the ova, and also of the various organs.

The microscope which he had brought for exhibition was one further step in the direction of carrying the microscope within the reach of those whose means were limited. He had taken such points of the cheap foreign microscopes which commended themselves; he had also taken the best points of English instruments. The rack-and-pinion adjustment, though discarded by some foreign makers, had been retained. The stage was made particularly thin; and the diaphragm had an extremely small hole in it, and this was of great value in physiological study.

The thanks of the meeting were voted to Mr. Beck for his communication.

A paper by Dr. G. W. Royston-Pigott, "On the Identical Characters of Chromatic and Spherical Aberration," was read by the Secretary.

The President thought that some of the statements in the paper appeared to require a little limit; for he imagined it would be quite possible to produce a lens having the spherical aberration correct for each ray, and yet still having a different focus for each ray.

Mr. Slack suggested that the paper obviously opened up a very important question as to the finest possible point of correction for lenses; and this reminded him that Herr Hasert, of Eisenach, advertised a new objective, which he said magnified 2000 or 3000 diameters, required no corrections for covering glasses of different thicknesses, and with which, it was stated, *Amphipleura pellucida* could be seen "*without the use of oblique light.*" Now if he had done this, he had certainly done a most astounding thing; but whether it was all true or not, it showed the direction in which foreign makers were moving.

Mr. Beck said he had tried to understand Dr. Pigott's paper, but could not do so at all; perhaps when printed he might be able to comprehend the meaning of it.

Dr. Lawson said he had received a letter from Mr. Hickie respecting the objective referred to by Mr. Slack, to the effect that he had examined it, and had found it produced very remarkable effects; also that it was perfectly true that it did not matter within certain limits what covering glass was used with it.

Mr. Curties said that Mr. Hickie would be very happy to send this lens to the Society for examination.

Dr. Pigott said he had not much to say in reply. The matter seemed very simple. He had lately been studying Professor Litrow's remarks on the achromatism of lenses for telescopes, and he there shows that he has calculated the marginal, central, violet, and red rays, and carried them out to five places of decimals, and found that they all came to the same point.

Dr. C. T. Hudson gave a highly interesting communication "On a New Species of Melicerta," illustrating the subject by a large number of very beautiful diagrams in coloured chalk, and by drawings on the board.

The President felt sure that all would unite in passing a cordial vote of thanks to Dr. Hudson for his very interesting paper, and he could not himself refrain from complimenting him upon the very great artistic taste displayed in the drawings exhibited in the room. He had not studied that class of objects much, but felt there were many persons in the room who would be glad to say something on the subject.

Dr. Pigott said he should like to ask Dr. Hudson what power he used in making these investigations?

Dr. Hudson replied that he usually worked with a $\frac{1}{2}$ inch and a B eye-piece, but he preferred A. When looking at the objects with a $\frac{1}{2}$ inch through the side of a trough, he often found that he could

not reach them. He had seen it mentioned that by the introduction of a $\frac{1}{2}$-inch lens into the body of the microscope the focal length of the objective was increased without detriment to the power or the definition. He was glad to have the opportunity of asking Dr. Pigott if it was possible to do this, as it would be a very great advantage to him if he could.

Dr. Pigott said he had often got into similar difficulties himself, particularly with the $\frac{1}{50}$ inch; but by using lenses in the body of the tube he had obtained the advantage mentioned, and had saved the covering glasses of many valuable slides. With regard to Dr. Hudson's question, he presumed that the power employed would be about 200 diameters with a C eye-piece. In that case, if he would take another $\frac{1}{2}$-inch objective to pieces, and put the back lens of it between the objective and the eye-piece, he would find that it would have the effect which he desired to obtain.

Mr. Jas. Smith recommended the method of putting an object-glass below the stage and then working through it with a 3 inch above the stage in the ordinary manner. By this means a series of powers varying from 2 to 150 diameters could be obtained with a considerable length of focus, and with the advantage of the objects being seen erect. With a $\frac{1}{2}$ inch below and a 1 inch above a different range of powers would be obtained; they would in this way get not only a splendid range of objectives and definition, but could use it very well in tanks more than half an inch in depth.

Dr. Pigott said if good definition was desired, the lower the lens was put down in the tube the better.

Mr. Beck would be sorry to pass away from the very beautiful drawings before them to matters of detail without asking whether the variations from Ehrenberg's figure were more important than the fact that the creature did not build up a tube? Was not this tube building one of the characteristics of Melicerta? And if so, could they be right in classing equally as Melicerta a creature which did not build itself a tube?

Dr. Hudson said there was in all cases a gelatinous sheath, with foreign particles lying evenly or unevenly upon it.

Mr. Slack said it seemed to him that if the secreting organ described by Dr. Hudson had a structure like that of the brick-making organ, the creature might be put under the same genus as *Melicerta ringens;* but if it was not so, he thought this would be sufficient to make quite a generic distinction. He had suspected the existence of a Melicerta besides *M. ringens.* Ordinarily the pellets of *M. ringens* were round in shape, but by pressure together became hexagonal; but he had once found a number of pellets which were quite conical in form, and he thought that they must have been the work of another species. The identification of rotifers was often rendered extremely difficult by their peculiar behaviour. The Cephalosiphons sent by him to Mr. Gosse some years ago, and figured in the 'Intellectual Observer,' only partially expanded their disks, and looked like some other species. Other specimens sent to an excellent naturalist presented only misleading appearances.

The thanks of the meeting were then voted to Dr. Hudson for his paper.

The President announced that it was proposed to have another Scientific Evening on Wednesday, the 24th of November, provided the rooms could be obtained on that date for the purpose.* Due notice of this or of any other arrangement would of course be given.

Donations to the Library, &c., from June 2 to October 6:

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Proceedings of the Bristol Naturalists' Society, 1874–5 ..	*Ditto.*
Journal of the Quekett Club. No. 29	*Club.*
Transactions of the Watford Natural History Society. No. 1	*Society.*
Smithsonian Report for 1873	*Institution.*
Transactions of the Woolhope Club for 1871–3	*Club.*
List of Elevations West of the Mississippi River. 3rd Edit.	
Journal of the Linnean Society. No. 80	*Society.*
Bulletin de la Société de Botanique de France. Three parts	*Ditto.*
The Harveian Oration for 1875. By Dr. W. Guy	*Author.*
Popular Science Review	*Ditto.*
Quarterly Journal of the Geological Society. No. 123 ..	*Society.*
Report of the U.S. Geological Survey. Vol. VI. By F. V. Hayden	*U.S. Government.*
A Report of the Hygiene of the United States Army, &c.	*U.S. Surgeon-General.*
A Turn-table	*C. F. Cox, Esq.*
Two Slides of Diatoms	*F. Kitton, Esq.*
A Slide of Blood of *Amphiuma means*	*J. Beck, Esq.*

J. Birdsall Jones, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

* Permission has since been given.

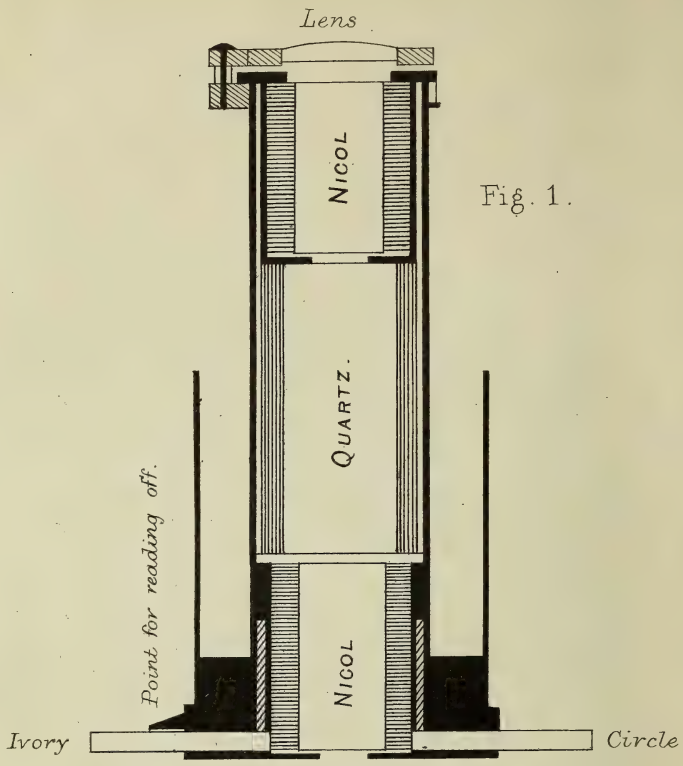


Fig. 1.

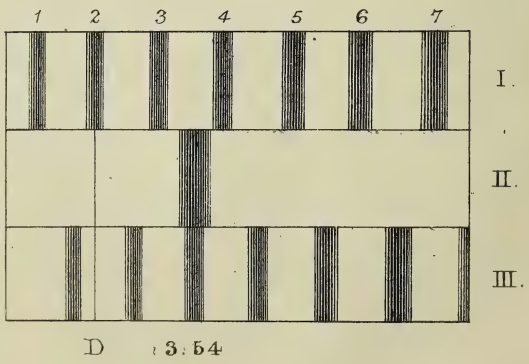


Fig. 2.

W West & Co lith.

THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1875.

I.—*On a New Method of Measuring the Position of the Bands in Spectra.* By H. C. Sorby, F.R.S., &c., Pres. R.M.S.

(*Read before the* Royal Microscopical Society, *November* 3, 1875.)

Plate CXXIII.

In various papers published during the last few years I have described a small piece of apparatus by means of which we can obtain a spectrum with a number of dark bands that can be used as a standard scale for measuring the position of those seen in any spectra compared with it side by side. For ordinary purposes, when very great accuracy is unnecessary, nothing could be more convenient; since the position of the bands can be read off at once. The only serious objection is that it is difficult to accurately appreciate the fractional parts of the intervals between the different bands of the scale. Thus, for example, it may be difficult to say whether the centre of a band were situated at $3\frac{5}{8}$ or $3\frac{3}{4}$. When there is any such difficulty I take the intermediate value, $3\frac{11}{16}$; but even with every care, and after having acquired all the skill that comes from long practice, it may be looked upon as impossible to measure the position of the centre of any absorption band to less than one-eighth of the interval between the bands of the scale. In many cases this is of very little importance, but when it is necessary to measure the position with sufficient accuracy to enable us to ascertain the true laws of the relative wave-lengths of a number of bands, such a source of uncertainty is very objectionable, and it is most desirable to adopt some method that may give the wave-lengths true to a millionth of a millimeter.

For some years past I have been fully alive to the importance of some plan which would enable us to obtain a spectrum in which a well-marked band could be made to travel up and down, and adjusted exactly in the line of the centre of any band in the spectrum under investigation. There is no difficulty whatever in seeing when one band is in exactly the same line as the other. It is a totally different thing to estimate by the eye alone the exact value of a fractional interval. This will be more easily understood by

referring to Pl. CXXIII., Fig. 2. I have constantly had this question before me, but for years felt that it was a thing more to be desired than hoped for, and I had almost come to the conclusion that it could not be accomplished, when all at once I hit upon a plan which is even better than anything I could have wished for.

In studying the spectra due to different kinds of interference in connection with the colour of feathers, insects, and minerals, I was led to examine the spectrum of crystals of quartz cut so that the light passes along the line of the principal axis. As is well known, the light then experiences no double refraction, but is circularly polarized. When viewed in a polariscope, a slice of about one-fourth of an inch in thickness gives fine colours, and when rotated no change whatever results; but if either the polarizer or analyzer be rotated, the colours vary much and return to the same tint at each half revolution. On studying the spectrum of this light, one or more well-marked black bands will be seen, and on rotating the polarizer or analyzer these move over the spectrum, and again occupy the original positions on the completion of each half revolution of the polarizer or analyzer. The number of these bands increases with the thickness of the plate of quartz, and their width diminishes, so that by using a very thick piece they may be made as numerous and narrow as may be desired. The question then is to choose such a thickness as may not be practically inconvenient, and yet give sufficiently numerous and well-defined black bands. The thickness which I have adopted is $1\frac{1}{2}$ inch. With this the whole visible spectrum is divided into eight spaces by seven well-defined bands, which in a prism spectrum are apparently at very uniform intervals, as shown by I of Pl. CXXIII., Fig. 2, but at a much less wave-length interval at the blue end than at the red end, as will be seen from the table given below.

In order to make use of these properties of quartz it is necessary to have it mounted between two Nicol's prisms, in the manner shown by Pl. CXXIII., Fig. 1. The upper can be rotated for accurate adjustment, and the lower is permanently fixed in a mounting with an ivory circle $2\frac{1}{2}$ inches in diameter, each half of which is divided into ten parts, and these again into five smaller divisions, so that there is no difficulty in reading off to $\frac{1}{100}$th of a half revolution. After placing this circle at the zero point, the upper Nicol is rotated so that the centre of the second dark band from the red end of the spectrum exactly coincides with the sodium line, or with the solar line D, as shown by I of Pl. CXXIII., Fig. 2. All the other dark bands are then of course in perfectly constant and definite positions, depending on the action of quartz on light of various wave-length. On rotating the ivory circle each band gradually passes from the red end towards the blue, until when the circle comes to the next zero point, commencing the next semicircle, the series of bands is

exactly the same as at first. Supposing then that when the circle is placed at zero an absorption band seen in some spectrum were situated between the bands 3 and 4 of the scale, as shown by II of Fig. 2, by rotating the circle the band 3 can be made to pass upwards until its centre exactly corresponds with that of the band whose position is being measured, as shown by III, and if this occurs when the point used for reading off is at ·54 of the circle, we at once know that the true position is 3·54. In a similar manner the situation of bands in any other part of the spectrum, can be referred to other bands of the scale.

If it were necessary only to compare one spectrum with another, this alone would suffice; but, as I pointed out in a paper read before this Society last April, it is very desirable to express everything in wave-lengths. In order to do this a table must be constructed, giving them in millionths of a millimeter for each tenth of a division between all the bands. For this purpose it is necessary to employ a diffraction spectroscope and strong illumination. I first made use of direct sunlight, but the movement of the light was found to be very inconvenient, and to cause some uncertainty in the measurements. I therefore finally adopted the results obtained by using a limelight, since it could be fixed in a proper position and kept immovable during all the measurements. The angle of the inclination (θ) of the telescope bearing the eye-piece with cross wires was read off to half-minutes of a degree in the case of all the bands. If two measurements differed by more than $\frac{1}{2}'$, other observations were made, and the mean of all adopted. This was repeated for each $\frac{1}{10}$th division of the ivory circle of the apparatus, since otherwise the irregular action of the quartz on light of different wave-lengths would have given rise to serious errors. All the calculations were based on the wave-length of the centre of the two principal sodium lines (D), which, according to both Angström, Huggins, Kirchhoff, and Thalén, is 589·2 millionths of a millimeter. The diffraction grating used was a photograph, for which I am indebted to the kindness of Lord Rayleigh, which by calculation must contain 6003 lines in a French inch, and the distance between each two lines must be ·004509 millimeter. The wave-lengths were calculated by means of the following equation:

$$\lambda = \cdot 004509 \sin. \theta,$$

since I made use of the spectrum of the first order. On the whole, I think the results may be looked upon as true to a millionth of a millimeter.

Of course it must be borne in mind that these measurements apply only to the quartz exactly $1\frac{1}{2}$ inch in thickness, cut and mounted exactly in the proper direction.

TABLE of the WAVE-LENGTHS in MILLIONTHS of a MILLIMETER corresponding to each $\frac{1}{10}$TH DIVISION between the VARIOUS BANDS, and of the Quantities to be deducted for each intermediate $\frac{1}{100}$th of the Half Revolution.

Readings of the Scale.	Wave-lengths for each $\frac{1}{10}$th.	Deduction for each $\frac{1}{100}$th.	Readings of the Scale.	Wave-lengths for each $\frac{1}{10}$th.	Deduction for each $\frac{1}{100}$th.
·7	687	·8	4·3	488	·3
·8	679	·9	4·4	485	·3
·9	670	·9	4·5	482	·3
1·0	661	·8	4·6	479	·3
1·1	653	·8	4·7	476	·3
1·2	645	·8	4·8	473	·3
1·3	637	·7	4·9	470	·2
1·4	630	·6	5·0	468	·4
1·5	624	·7	5·1	464	·3
1·6	617	·8	5·2	461	·2
1·7	609	·6	5·3	459	·3
1·8	603	·6	5·4	456	·3
1·9	597	·8	5·5	453	·2
2·0	589	·5	5·6	451	·2
2·1	584	·6	5·7	449	·2
2·2	578	·5	5·8	447	·2
2·3	573	·6	5·9	445	·2
2·4	567	·5	6·0	443	·2
2·5	562	·5	6·1	441	·3
2·6	557	·5	6·2	438	·3
2·7	552	·5	6·3	435	·3
2·8	547	·4	6·4	432	·2
2·9	543	·5	6·5	430	·1
3·0	538	·5	6·6	429	·1
3·1	533	·4	6·7	428	·3
3·2	529	·5	6·8	425	·2
3·3	524	·4	6·9	423	·1
3·4	520	·3	7·0	422	·3
3·5	517	·4	7·1	419	·1
3·6	513	·5	7·2	418	·1
3·7	508	·3	7·3	417	·1
3·8	505	·3	7·4	416	·3
3·9	502	·4	7·5	413	·1
4·0	498	·3	7·6	412	·2
4·1	495	·2	7·7	410	·2
4·2	493	·5	7·8	408	·1

The chief objections against this apparatus are, that it is somewhat difficult to obtain a suitable piece of quartz, and to cut it so that the light may pass exactly in the line of the principal axis of the crystal. Very often, though it may be perfectly clear and free from visible flaws, it may contain imperfections in the form of crystals arranged in a different optical position, easily seen with polarized light. Then again, though a tolerably well-formed crystal may be selected, much care is necessary to so cut and mount it that the two parallel faces are exactly perpendicular to the principal axis, along which there is no double refraction, but only true and perfect circular polarization. However, with care this can be done,

and the piece finished off true to less than $\frac{1}{100}$th of an inch. When once made, the only material objection is that since the quartz must be $1\frac{1}{2}$ inch thick and there must be Nicol's prisms at each end, the entire length becomes about $3\frac{1}{2}$ inches. For this reason it is the most convenient to place it in the position of the condenser under the stage of the microscope, and to make use of it with the binocular apparatus described in the paper alluded to above, care being taken to reflect the light directly through the axis, since otherwise there is a slight change in the position of the bands. By arranging a movable plano-convex lens over the upper Nicol's prism, as shown by Fig. 1, the light may be made equally bright in the two tubes of the microscope. The position of the bands seen in the spectrum of any object placed before the small reflecting prism can then be measured with great facility to something like the millionth of a millimeter, since with well-marked bands consecutive measurements do not differ more than $\frac{1}{100}$th of the half revolution of the circle, and when they do differ more the mean of several observations may be taken.

As an example, I give the measurements in the case of the bands seen in the spectra of oxidized hæmoglobin and of the same substance in which the oxygen is replaced by carbonic oxide.

	Measurements by Scale.	Wave-lengths.
Oxidized hæmoglobin	2·19	584 − 9 × ·6 = 579
	2·88	547 − 8 × ·4 = 544
Carbonic oxide hæmoglobin ..	2·28	578 − 8 × ·5 = 574
	2·96	543 − 6 × ·5 = 540

II.—*Note on the Markings of Frustulia Saxonica.*

By Assistant-Surgeon J. J. Woodward, U. S. Army.

(*Read before the* Royal Microscopical Society, *November* 3, 1875.)

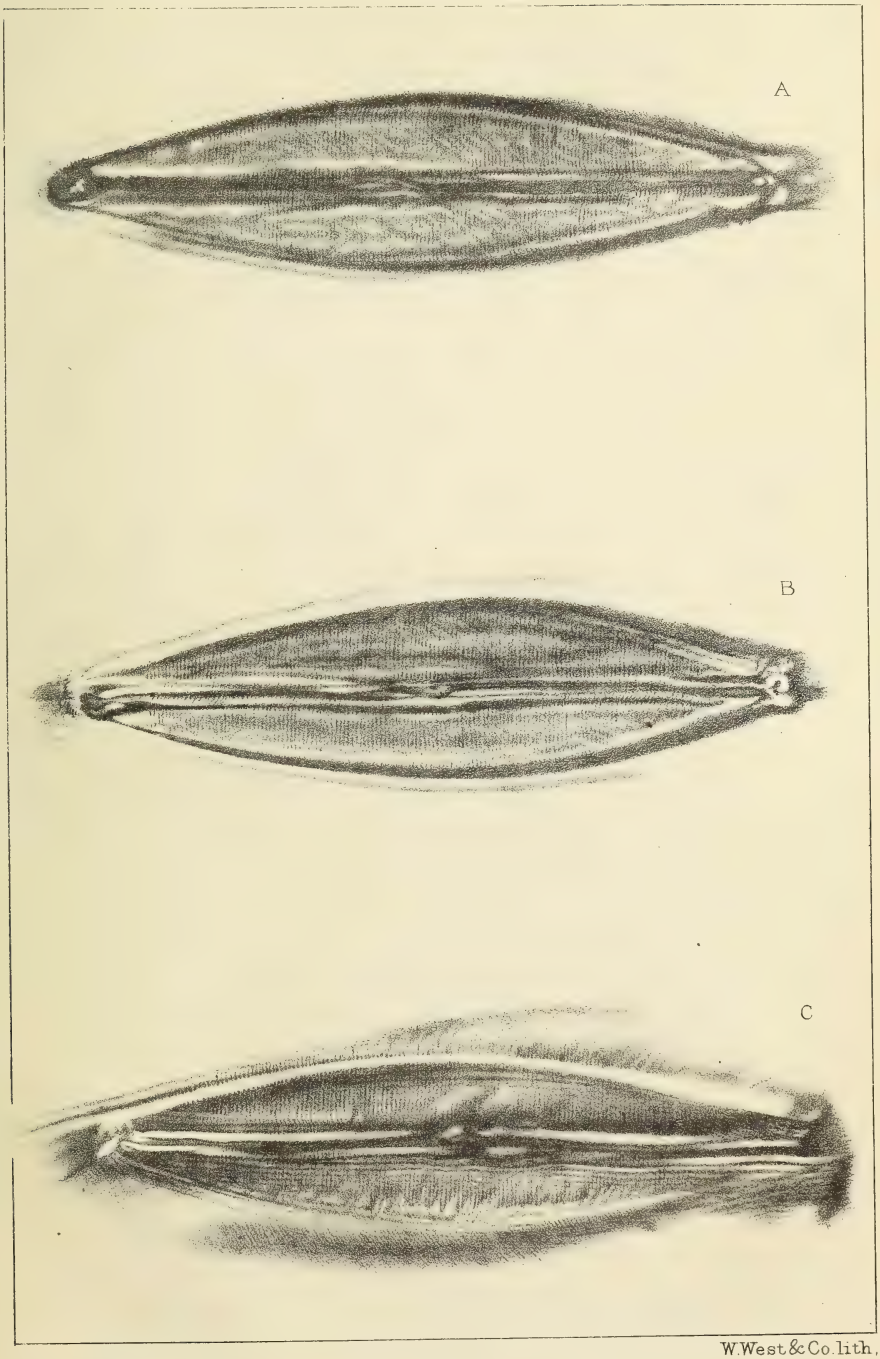
Plates CXXIV. and CXXV.

On returning from my last summer's vacation my attention was directed a few days ago, for the first time, to a letter by Mr. W. J. Hickie in the July number of the 'Monthly Microscopical Journal' (p. 32), on the subject of the markings of *Frustulia Saxonica*, which seems to call for some comments from me.

Mr. Hickie refers to a paragraph in the 'Monthly Microscopical Journal' (vol. ix., p. 86) headed "*Frustulia Saxonica* as a Definition Test," which he, however, thinks may possibly misrepresent my views, and remarks—"What is there said is by no means very clear; but it certainly does make him assert one of two things: either (1) that *Frustulia Saxonica* is a one-lined object (i. e. has transverse, but no longitudinal lines); or (2) that, though it undoubtedly has transverse, and may possibly have longitudinal lines as well, no one as yet has succeeded in seeing the latter, but that those who fancied they saw them, as Dippel and others, have been deceived by 'diffraction phenomena.'" I will frankly say that the second of these views very fairly represents my opinions on this subject, although it is not precisely what I said.

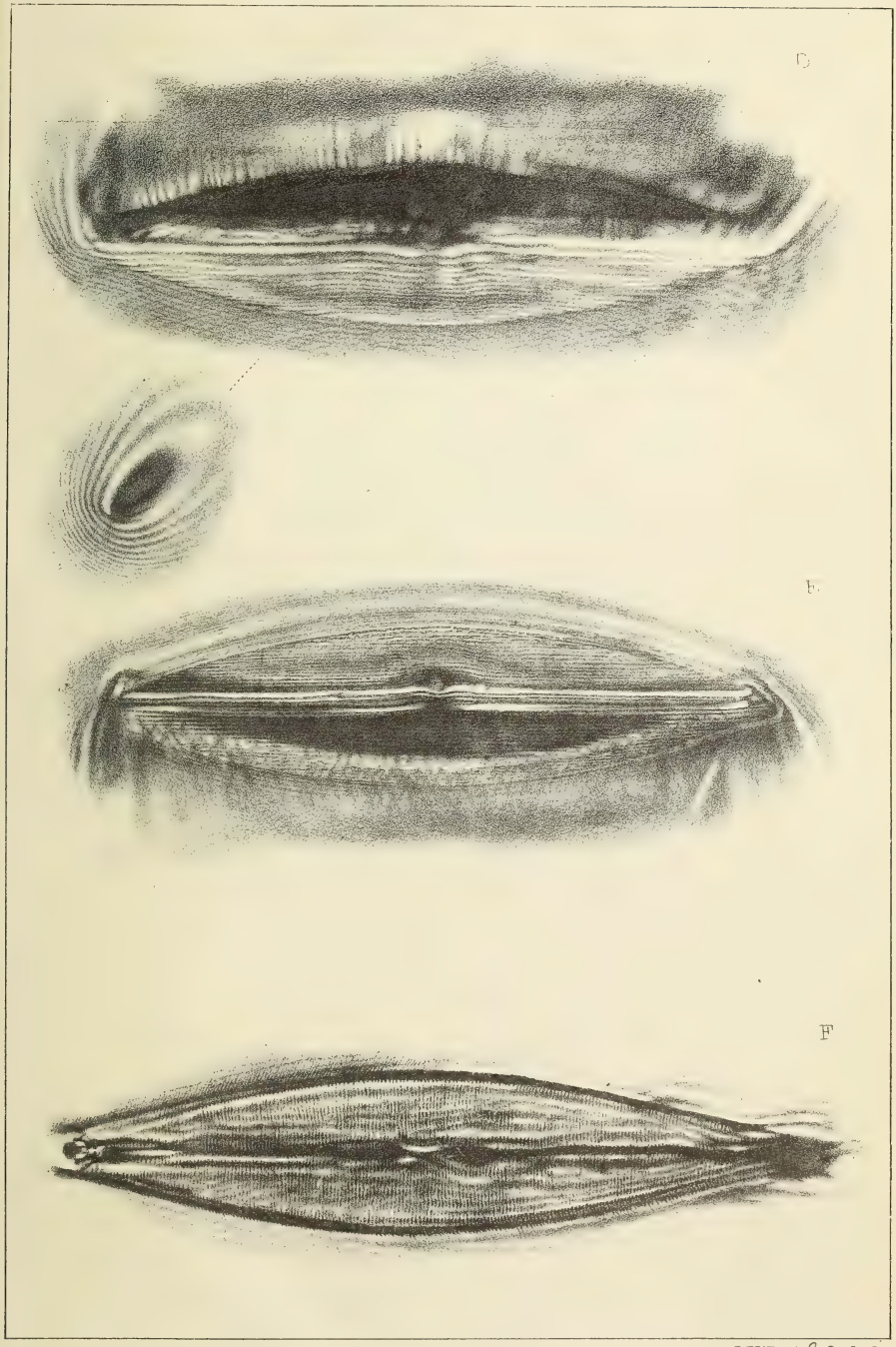
The paragraph quoted by Mr. Hickie is abridged from a "Note on the *Frustulia Saxonica* as a Test of High-Power Definition," which I published in the 'Lens' for October, 1872 (p. 233), and of which I send herewith two copies, with the request that one of them may be forwarded to Mr. Hickie. The original article was illustrated by a Woodburytype plate, of which I have now no spare copies; I send, however, two silver prints (marked A) from the negative used in its preparation. In this "Note" I quoted Dippel's description,* which attributes to *Frustulia Saxonica* both longitudinal and transverse striæ, and estimates the first at 18 to 20, the second at 34 to 35 in the one hundredth of a millimeter. I stated that I myself found the transverse striæ count 85 to 90 in the one thousandth of an inch, which agrees substantially with Dippel's figures, but said, "The longitudinal striæ of Dippel, however, I must regard as diffraction phenomena," and went on to assert that as I observed them "they varied too much in their distance apart, with varying obliquity of illumination, to bear any other interpretation." It will be observed that I did not, in my

* From 'Das Mikroskop und seine Anwendung.' Erster Theil. Braunschweig, 1867, s. 132.



W. West & Co. lith.

The markings on Frustulia Saxonica.



W. West & Co. lith.

The markings on Frustulia Saxonica.

"Note," speak generally, as Mr. Hickie does, of what "Dippel *and others*" fancied they saw, but specifically of the longitudinal striæ of Dippel. My reason for this limitation was, I confess, that I did not at the time know that any authority but Dippel had described longitudinal striæ on this diatom. So far as Dippel was concerned, I had not merely his description and his statement of the number of lines he observed to the inch, but the excellent and truthful woodcut in his book * on which to form an opinion. This woodcut, moreover, enabled me both then and now to know that the specimens I studied belonged to the same species as that which Dippel described.

In my "Note," then, I spoke only of the *longitudinal striæ of Dippel*, but now, in response to Mr. Hickie's letter, I willingly express my belief that the longitudinal lines which he describes are of the same character. At the same time I shall be very glad if he can convince me, by satisfactory evidence, that this belief is erroneous, for analogy inclines me towards the opinion that in both *Frustulia Saxonica* and *Amphipleura pellucida* the striæ are really rows of beads, as is so easily to be seen in *Navicula rhomboides*, and that, consequently, we ought to be able to see longitudinal striæ when the illuminating pencil has the proper direction, if only our glasses had the requisite defining power.

In favour of his opinion that he has actually seen these longitudinal striæ on *Frustulia Saxonica*, Mr. Hickie states, in the first place, that Herr Seibert showed him in the summer of 1872 "a couple of very beautiful photographs of that diatom, one of which exhibited the transverse, and the other the longitudinal lines, with far more clearness, sharpness, and distinctness than the printer will be able to reproduce the words I have here written." If I had these photographs before me, they might, perhaps, though I confess I do not expect it, show me that Herr Seibert has seen something I have been unable to see, and I must therefore express the hope that Mr. Hickie, in return for the package of photographs I send him herewith, will take the trouble to obtain copies of them for me to study. I hope also that he will obtain copies of them for the Royal Microscopical Society, to compare with those I send. In the absence of Herr Seibert's photographs, however, I would remark, that spurious lines, due to diffraction and interference, can be photographed quite as readily as real lines, from which they can only be distinguished by observing precautions which Mr. Hickie's letter does not show to have been considered either by himself or any of the distinguished gentlemen to whom he tells us he exhibited the longitudinal striæ in question.

Mr. Hickie tells us further, that "In Rhomboides also the transverse lines are much closer than the longitudinal, whereas in

* *Op. cit.*, fig. 103.

Frustulia Saxonica it is just the reverse." This statement directly contradicts the description of Dippel, whose measurements, quoted above, make the longitudinal lines nearly twice as coarse as the transverse instead of finer, and it might at first be thought that the two gentlemen were describing different things, but, in point of fact, as I stated in my "Note," these spurious longitudinal lines vary greatly "in their distances apart with varying obliquity of illumination," and also, I may now add, with varying positions of the fine adjustment, so that I have had no difficulty in photographing them, as seen on the same frustule, and with the same objective and distance, both finer than the transverse ones as described by Mr. Hickie, and coarser than the transverse ones as described by Dippel, and in both cases they appeared to me, as Mr. Hickie says they did to him, "as plainly and visibly as any of us are ever likely to see our own faces in our looking-glasses." The distinctness with which these lines can be seen has nothing to do with the question of their objective reality, for diffraction phenomena are often quite as distinctly visible as the optical images of actual objects.

The question of the real nature of these longitudinal lines appears to me to be one of considerable interest, because it brings up the matter of recognizing lines due to diffraction and interference, when observed in the field of the microscope, and because these phenomena have been a fruitful source of error in the interpretation of microscopic images. I have therefore thought it worth while to prepare a series of photographs of *Frustulia Saxonica* for the purpose of illustrating my meaning.

The first of these photographs (marked B) represents a frustule of *Frustulia Saxonica* adjusted to show the transverse striæ. This frustule measured $\frac{1}{500}$ of an inch in length, with eighty-six striæ to the $\frac{1}{1000}$ of an inch. The negative is magnified 1830 diameters very nearly, and to this of course the paper print closely approximates.*

The second photograph (marked C) represents the same frustule with its left end raised so as to bring it obliquely to the light. In its lower half, besides the transverse lines, a series of longitudinal lines can be seen, which give rise in places to a distinct appearance of dots at their intersections with the transverse striæ. The nature of these lines will be best understood after a study of the third and fourth photographs.

In the third photograph (marked D) the same frustule is shown with its left-hand angle still more elevated. The transverse striæ have disappeared; but in the left-hand half of the frustule we have a beautiful series of longitudinal lines which very closely resemble

* Silver prints after mounting are usually a trifle larger than the negatives from which they are printed.

those shown in Dippel's woodcut above referred to. Such longitudinal lines as these, I suppose, Mr. Hickie himself has observed and recognized as spurious, for he says in his letter—"I had also no difficulty in bringing into view those wide-spaced, spurious lines alluded to by Dr. Woodward."

Let us observe the character of these lines. They run parallel to the midrib, handsomely following its sweeping curves, and it will be seen that they become progressively closer and closer together from the midrib towards the margin of the frustule. It will also be seen that they do not terminate at the edges of the frustule, but sweep off into the open space outside, where they form a series of rhombic figures by crossing a fresh series of diffraction lines conditioned by the margin of the frustule, as the former series was by the midrib. These characters would be sufficient to show these lines to be spurious, but if, now, the fine adjustment be toyed with, or the illumination changed, or both, they pass, by the most insensible transitions, into new combinations, one of which is shown in the next photograph. I would also call the attention of those who examine this photograph to three shadows on the left of the lower part of the frustule, which are cast by three out-of-focus fragments of dirt adhering to the under surface of the glass cover of the preparation. Each of these shadows is surrounded by a series of lines due to diffraction and interference. The lower two sets of these lines especially very closely resemble the longitudinal lines on the frustule in number, distance apart, distinctness, and general character.

In the fourth photograph (marked E) the same frustule is shown standing vertically with the light coming from the right side of the picture. On both sides of the frustule, but especially on the right side—the left being in shadow—there are a series of fine longitudinal lines, which I suppose to be similar to those Mr. Hickie has described. They are rather closer together than the transverse striæ (counting eleven, on the negative, in the space occupied on the negative of the photograph marked B, by ten of the transverse striæ), and their distance apart is more equable than is the case with the longitudinal lines in the last photograph. Still, if the paper print be examined with a hand-lens, I think it will be plainly seen, as can readily be measured on the negative, that the lines become progressively closer towards the margin, those nearest the midrib being farthest apart. It will also be seen, on both sides of the frustule, and especially at its upper end, that these longitudinal lines are not limited to the surface of the frustule, but pass off into space outside, where they cross a fresh series of beautiful diffraction lines conditioned by the margin of the frustule. I would particularly invite attention to this last series of lines, especially as seen towards the top of the right-hand margin of the frustule. The

fine longitudinal lines shown on the frustule in this photograph, then, like the coarse ones shown in the last, have all the characters of diffraction fringes; but if any doubt remains, it is only necessary to toy with the fine adjustment, when the number of the lines and their distance apart will be found to vary continually, while in the case of the transverse striæ, or of any other real lines, the number remains constant as long as they can be seen at all.

Besides the principal frustule shown in these pictures, part of another frustule is shown in each, which affords still further illustrations of the longitudinal lines in question. I may add that, although I endeavoured to take the four pictures with the same power, trifling differences exist due to the necessary variations in the focal adjustment.

As a still further illustration of the spurious longitudinal lines of this diatom, I add a print (marked F) of a negative magnified 1600 diameters, made November 10, 1872, by Tolles's immersion $\frac{1}{18}$th, in which the central frustule shows the transverse striæ, while portions of two others exhibit longitudinal lines, similar, as I must suppose, to those Mr. Hickie sees. I would call attention on this print, and, indeed, on the two others (marked A and B) which show the transverse striæ, to a curious series of diffraction lines just outside of the margin of the frustule, which appear to be conditioned by the transverse striæ themselves. These spurious lines are at exactly the same distance apart as the transverse striæ, but form a sharp angle with them.

I send copies of all these photographs for Mr. Hickie, and also a set for the Royal Microscopical Society. If the longitudinal lines which Mr. Hickie sees are, as I suppose, similar to those which I have photographed, these pictures will enable those who examine them to decide whether his interpretation or mine is correct. If he thinks he sees something of a different nature, I shall be happy to consider the evidence on that head when he presents it.

These photographs will, moreover, enable those interested in the subject to decide another question raised by Mr. Hickie in his letter, viz. whether what I have photographed and described is really *Frustulia Saxonica*, and also, whether I have, as he suggests, "merely been wasting" my "time on a bad slide." It is quite true that I have been using Möller's slides; it is also quite true that, like Möller, I suppose *Frustulia Saxonica* to be identical with *Navicula crassinervis.* I do not pretend to any special personal knowledge of the proper classification of the diatoms, and derive my opinion entirely from my friend Professor Hamilton L. Smith, of Hobart College, Geneva, New York, who I suppose to be more thoroughly acquainted with the subject of the Diatomaceæ than anyone on this side of the Atlantic, and who wrote me, January 9, 1872: "*Navicula crassinervis* has long been recognized as =

Frustulia Saxonica." Mr. Hickie asserts that there is a difference, but does not make clear in what the difference consists. I should be happy to learn further from him on this head, if he has anything to teach. If, on examining my photographs, he thinks they represent something different from his slides of *Frustulia Saxonica*, I should be glad to receive one of these from him to study. I have the less hesitation in asking this favour, as he tells us his collection of these slides is a very extensive one.

I conclude these remarks with the hope that Mr. Hickie will find them as courteous as I acknowledge his own to be.

List of Photographic Prints accompanying Dr. Woodward's "Note on the Markings of Frustulia Saxonica."

A.—Print from the negative used to illustrate Dr. Woodward's paper in the 'Lens,' × 1750 diam.

B.—Frustule photographed for the present paper to show the transverse striæ, × 1830 diameters.

C.—Same frustule, same power, showing both transverse and longitudinal lines.

D.—Same frustule, same power, showing the longitudinal lines of Dippel.

E.—Same frustule, same power, showing longitudinal lines similar to those described by Mr. Hickie.

F.—Print from a negative made in 1872, showing the transverse striæ on one frustule, and longitudinal lines on parts of others.

EXPLANATION OF PLATES CXXIV. AND CXXV.

These Plates contain figures the same size as those in the several admirable photographs which Dr. Woodward transmitted to us. The various figures are given in the same order—A, B, C, &c.—as those above. Whereas, however, in the photographs more than one diatom is occasionally introduced, in the Plate but a single frustule is represented.

[The following remarks are contained in the 'Lens,' October, 1872.]

Note on the Frustulia Saxonica as a Test of High-Power Definition.

The genus Frustulia (Agardh) includes several species of diatoms which possess bacillar or navicular frustules, "immersed in an amorphous gelatinous substance." * The species are divided by Pritchard into two groups, the first with "evident striæ," while in the second the striæ are "wanting or very indistinct." In the second group Pritchard places the species Saxonica (Rabenhorst), so called from having first been noticed in Saxony, where it "forms dirty, olive-brown, tremulous jelly-like masses in little cavities of damp rocks." The 'Micrographic Dictionary' (second edition)

* 'A History of Infusoria.' By Andrew Pritchard. 4th edit., London, 1861, p. 924.

gives a brief description of this species, but does not mention the striæ.

The first description of the striæ with which I am acquainted was given by Professor Reinicke,* who saw fine transverse lines, but found no objective capable of bringing them out clearly enough to count them. (He used immersion objectives of Hartnack.)

The markings on the *Frustulia Saxonica* are described and figured by Dippel,† whose description is as follows: "The *Frustulia Saxonica* possesses longitudinal and transverse striæ, of which the first are tolerably far apart (from eighteen to twenty to the hundredth of a millimeter), the latter, on the contrary, somewhat closer than those of *Grammatophora subtilissima* (from thirty-four to thirty-five to the hundredth of a millimeter). Both systems of striæ are very pale (*schwach gezeichnet*), so they certainly require an excellent objective for their resolution. Nevertheless, with oblique light, and on bright days, they can be seen with objectives of the highest power almost as well as those of the Grammatophora, if the object is only properly prepared, the valves separated (*gespalten*), and mounted dry. In balsam, on the other hand, the transverse striæ are very difficult to see: nevertheless, I have recognized them, even in this case, with the System No. 10 of Hartnack."

During the summer of 1871, a slide of *Frustulia Saxonica*, mounted dry, was presented to the Museum by Dr. J. J. Higgins, of New York, and subsequently two other slides, also dry, were obtained from J. D. Möller, of Wedel, Holstein. I found no difficulty, on these slides, in seeing and counting the transverse striæ, both with monochromatic sunlight and with the light of a small coal-oil lamp. The longitudinal striæ of Dippel, however, I must regard as diffraction phenomena, similar in character to the longitudinal lines which some have described in the central portion of Grammatophora frustules; they varied too much in their distance apart, with varying obliquity of illumination, to bear any other interpretation. The transverse striæ, on the other hand, I found very definite in character. I counted on different frustules from eighty-five to ninety to the thousandth of an inch, which agrees substantially with the results of Dippel, whose figures correspond to from eighty-six to eighty-nine to the thousandth of an inch. The frustules themselves varied in length from ·0018 to ·0029 inch.

I subsequently removed the cover of one of the dry slides obtained from Möller with the diatoms adherent to it, and

* See a review of his "Beiträge zur neuern Mikroscopie," in the 'Quarterly Journal of Microscopical Science,' vol. ii., new series, 1862, p. 292.

† 'Das Mikroskop und seine Anwendung.' Erster Theil. Braunschweig, 1867, p. 132.

mounted the specimen in Canada balsam. The striæ were then paler than before, but I cannot say that I found them more difficult to resolve. Both in balsam and dry I could get resolution by the Tolles's immersion $\frac{1}{6}$th belonging to the Museum, and that by lamplight as well as by monochromatic sunlight. With immersion objectives of higher powers the lines were still more distinctly separated, and I obtained the finest results with the immersion front of the $\frac{1}{16}$th of Powell and Lealand, and with the new immersion $\frac{1}{18}$th recently made for the Museum by Mr. Tolles.

On the whole, the *Frustulia Saxonica* is an easier test than the *Amphipleura pellucida*, as may be inferred from the above measurement of its striæ, and the difference is especially marked by lamplight. Those therefore who work by lamplight only will find this test more extensively useful than the Amphipleura.

The Woodbury print [see Plate CXXIV., Fig. A] illustrating this article is copied from a negative made by the immersion front of the Powell and Lealand $\frac{1}{16}$th belonging to the Museum. The power used was 1750 diameters. It displays the transverse striæ as seen when the utmost pains are taken to avoid the longitudinal diffraction lines.

III.—*Appendix to the Paper on the Identical Characters of Spherical and Chromatic Aberration.*

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

(*Taken as read before the* Royal Microscopical Society, *Nov.* 3, 1875.)

The actual calculation of the difference between the spherical aberrations of the extreme red and violet rays may easily be obtained as follows, for an equi-convex lens.

For the spherical aberration of any ray whose refractive index is μ is

$$\frac{1}{\mu \,.\, \mu - 1} \cdot \left\{ \frac{\mu + 2}{\mu - 1} x^2 - 4 (\mu - 1) \alpha x + (3 \mu + 2) (\mu - 1) \,.\, \alpha^2 + \frac{\mu^3}{\mu - 1} \right\} \frac{y^2}{8 f^3}.$$

In the case of an equi-convex lens, $\alpha = 0$ and $x = 0$ in this expression and the spherical aberration becomes

$$\frac{1}{\mu (\mu - 1)} \cdot \frac{\mu^3}{\mu - 1} \cdot \frac{y^2}{8 f^3} = \frac{\mu^2}{(\mu - 1)^2} \cdot \frac{y^2}{8 f^3},$$

y being the semi-aperture and f the focal length of the equi-convex lens. In order therefore to get the spherical aberration for coloured rays, the refractive index of the particular ray is substituted for μ in this expression, viz.

$$\frac{\mu^2}{(\mu - 1)^2}.$$

Now take the rays B and H in the solar spectrum representing the extreme red and violet rays from Fraunhofer's celebrated table of refractive indices for crown and flint glass:

Kind of Glass.	Red Ray B.	Violet Ray H.
Crown glass, No. 9	$\mu = 1 \cdot 52583$	$1 \cdot 54657 = \mu$
Flint glass, No. 13	$\mu = 1 \cdot 62775$	$1 \cdot 67106 = \mu$

Hence the spherical aberration of the red ray for the crown is proportionate to the coefficient of $\frac{y^3}{f^3}$, viz.

$$\frac{\mu^2}{8 (\mu - 1)^2} = \frac{(1 \cdot 52583)^2}{8 (0 \cdot 52583)^2} = \frac{2 \cdot 328157}{2 \cdot 211976},$$

or red aberration in crown	=	1·05252
so violet " "	=	1·00082
Difference in aberration ..	=	·05170

Also the spherical aberration for flint for red ray is proportionate to

$$\frac{\mu^2}{8(\mu-1)^2} = \frac{(1\cdot62775)^2}{8(0\cdot62775)^2} = \frac{6\cdot72350}{8},$$

or red aberration in flint	.. .. =	0·840450
violet „ „	.. .. =	0·775124
Difference	.. .. =	0·065326

If these results be put in plain language, the spherical aberrations of the red and violet rays in crown glass No. 9 are as

105 to 100;

and in the flint glass as

84 to 77, nearly.

Entirely calculated, be it remarked, from the spherical aberration of the extreme red and violet rays in the two cases, which thus show the proportions of the chromatic spherical aberration.

Moreover, the focal lengths of the same equi-convex lens, if of crown, are

For the red and violet rays as

190 to 183,

and for the flint as

159 to 149, very nearly.

IV.—*The Slit as an Aid in Measuring Angular Aperture.*

By Professor R. Keith.

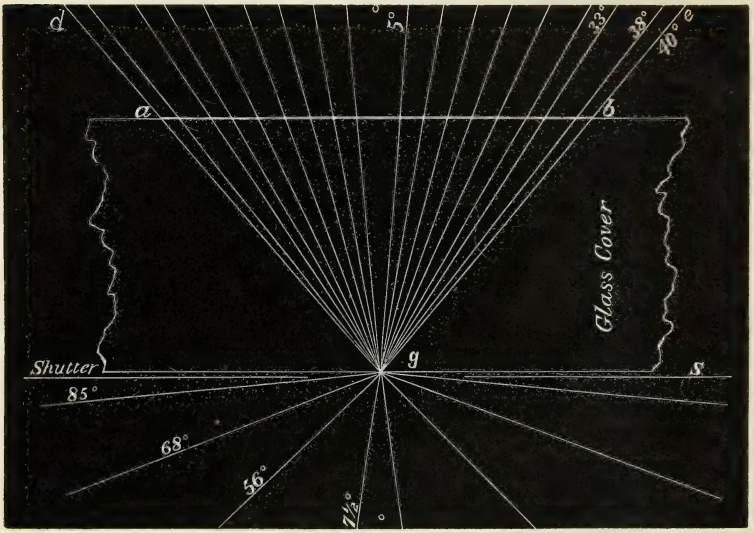
It has recently been proposed to use a very narrow slit, on the stage of the microscope, to prevent the supposed effect of stray light in measuring the aperture of objectives.

This device could hardly mislead anyone who keeps in mind the nature of spherical aberration. Most observers with the microscope, however, give but little attention to the abstract principles of the instrument, and therefore a graphic representation of the effect of aberration will probably be of interest.

I present two figures, which correctly show the direction of the light in the two cases specified: all the angles being calculated and measured.

The objective is adjusted in both cases so as to be free from aberration when the under surface of the glass cover is in focus, and the upper surface touches the objective. The lines marked 40°, 38°, &c., represent the direction of the rays of light, the figures indicating the angles that they make with the axis, and *a b* represents the face of the objective.

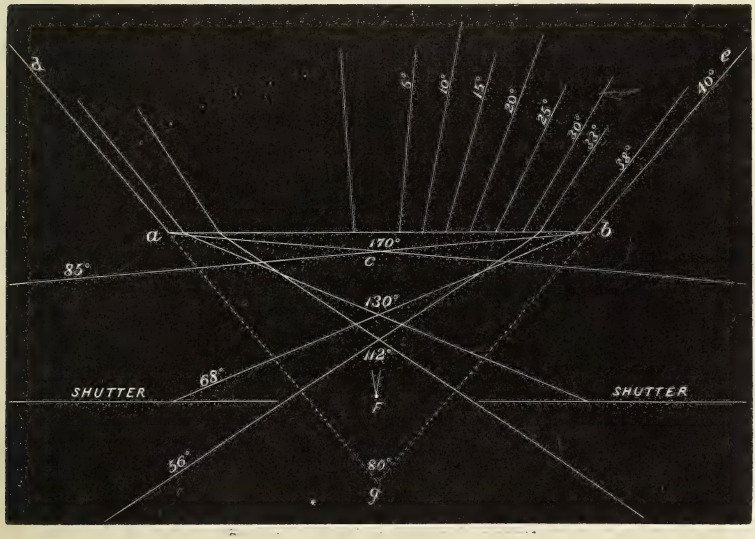
Fig. 1.



In the first figure, with the glass cover interposed, rays from a small luminous object near the eye end of the tube, will all be collected into a very small image at the point *g*. In this case there is no aberration, and no stray light, and any of the devices for mea-

suring agb, which is the balsam aperture, can be easily applied. If the shutter be now placed on the under surface of the glass cover, it is evident that, whether it be closed up more or less, it cannot affect in any way the measurement of agb, because *every point* in the image at g is the apex of a cone with ab for the diameter of its base.

Fig. 2.



In the second figure, the glass cover is removed, and the effect of aberration becomes strikingly apparent. The rays now, instead of all crossing at g, aberr all the way from F to C, and for every point along that distance the objective has a different aperture. The point at which the light is maximum does not coincide with the point at which the aperture is maximum. The shutter can now be placed so as to reduce the aperture, but can nowhere be placed so as to take it all in, except almost in contact with the objective and opened nearly as wide as its diameter.

It thus appears that, if the objective is corrected for aberration, the shutter placed in the plane of its focus has no effect whatever. And if the objective is not corrected, the shutter can only shut out light too much affected by aberration to pass through the slit, which is the very light of most importance, and that upon which the maximum aperture depends.

The extreme air angle (in this case 170°) is very properly given by the makers as indicating the extreme capacity of the objective. It is the only easily measured quantity that will answer the purpose.

It is further plain from the figure that if light about 1° outside of 40° could pass through *a b* the maximum air angle would be 180°.

Objectives are constructed allowing light making an angle of several degrees outside of 41° to pass through the front, if water or balsam is interposed between the objective and the cover. It is evident that for such objectives something more than a maximum air angle of 180° is required to indicate their extreme aperture. To simply state the maximum air angle at 180° does not do them justice because the balsam angle corresponding to that air angle is only 82°, and the objectives alluded to have a balsam angle of 100° and upwards.

ALEXANDRIA, VIRGINIA.

NEW BOOKS, WITH SHORT NOTICES.

Practical Hints on the Selection and Use of the Microscope. Intended for beginners. By John Phin, New York, U.S.A. The Industrial Publication Company, 1875.—The author of this little book, Mr. Phin, states that he has endeavoured to put together some remarks that may be useful solely for the beginner. He does not ambition anything higher than this, and we must certainly admire his modesty. We have gone carefully through the work, and we are glad to be able to say that in all respects it is a book which may be read by the junior student with pleasure and advantage. Almost the only fault we have to find with it is its very imperfect illustration. Indeed we might almost say its entire absence of illustration, for the few cuts widely scattered through its pages are out of all proportion to the wants of the reader. This is a serious disadvantage, and we trust it will be avoided in the next edition which this little book is sure to pass through. We must take exception to some of the author's observations on lenses. In the first place, it is exceedingly objectionable to divide all objectives into English and French varieties. His principle of classification is too old and exceptional. However, we quite agree with him in his remarks on the subject of naming lenses. The English mode is unquestionably the best, and the system of Hartnack and other foreign makers, of numbering them 1, 2, 3, and so forth, is most inconvenient, and the more so inasmuch as all Continental makers differ more or less from each other in numbering the object-glasses. The division he should have adopted is that of immersion and dry objectives. We must likewise call his attention to the observations on immersion lenses, which contain several misstatements in regard to their optical qualities. We observe also that some of our more recent and withal popular additions to microscopic apparatus are omitted. But on the whole we must give Mr. Phin very high praise for his excellent addition to our rudimentary works on the subject of the microscope.

Outlines of Practical Histology. Being Notes of the Histological Section of the Class of Practical Physiology held in the University of Edinburgh. By W. Rutherford, M.D., F.R.S.E., Professor of Physiology in the University of Edinburgh. London: Churchill, 1875.—While we are very glad to have such a book as the present one, and while it will save those of us who have classes an immense deal of trouble, still we cannot but utter our dissatisfaction with the general character of the volume, and we hope to see it altered if it should go through another edition. In the first place we must say that the author has been careful to admit every subject which the ordinary medical student—for it is for the medical student alone—requires. He gives him ample advice as to what animal is to be killed for the tissue to be obtained from; then he tells him what he is to mount it in, and how he is to look at it. But then, unfortunately, he has given no illustrations whatever of the tissues—a circumstance that we consider extremely unwise. Then, again, he has used Hartnack's glasses alone, and the

student is only informed whether it is No. 7 or No. 3 objective he is to use. This might have done very well for Dr. Rutherford's own class alone, but as not a few teachers prefer to employ the objectives of Ross, or Powell and Lealand, or Smith and Beck, as being better than Hartnack's, there is an omission which we fear will tell against the volume. Then, again, why does the author, in referring to the three unquestionably best makers in this country, omit Messrs. Smith and Beck? We may say, too, that his mode of teaching the student the estimation of the magnifying power he is employing is clumsy in the extreme. On this point he would have done well to have consulted Beale's excellent volume. We observe that some very valuable advice is given on the subject of epithelium, which shows that the writer is thoroughly practical: it is in reference to the question of killing the animal, which must not be done with chloroform. One passage on p. 23 is not quite clear, in which the author, who has all along been speaking of Hartnack's object-glasses, says that a tissue is "most evident with a $\frac{1}{25}$th lens." What does it mean? And again, what is the use of describing for the junior student what he admits can only be seen with a power of 1000 diameters? and even then his description is not quite clear as to direction of striæ in muscle, we mean, of course, clear to the young beginner. Further, Beale's glycerine and Prussian blue is referred to on p. 27, without any reference to where the description of its contents is to be found, though afterwards an account of it is given on p. 66. The best chapter in the volume is that in which the author's microtome is described and figured. In this he very fully explains his very useful instrument, which works so well that we quite agree with Dr. Rutherford in the opinion that the alterations proposed by others have been "but the reverse of improvements." In the author's observations on cements we think he has spoken wisely and well, and we think his last observations to the student most valuable advice. We like the plan he has adopted of interleaving the book with pages of plain paper, on which the reader can write any remarks he may have to make. On the whole then, with these few disparaging sentences we have already written, we think the book is a valuable addition to our literature, which we trust to see much improved after the next edition has gone to press.

PROGRESS OF MICROSCOPICAL SCIENCE.

Microscopic Work of the 'Challenger.' *—Professor Huxley writes as follows to 'Nature' (August 19), enclosing a letter from Professor Wyville Thomson:—The following extracts from a letter dated Yeddo, June 9, 1875, addressed to me by Professor Wyville Thomson, will, I think, interest the readers of 'Nature':

"In a note lately published in the 'Proceedings of the Royal

* This note has been "crushed out" of several successive numbers of the 'M. M. J.'

Society' on the nature of our soundings in the Southern Sea, I stated that up to that time we had never seen any trace of the pseudopodia of Globigerina. I have now to tell a different tale, for we have seen them very many times, and their condition and the entire appearance and behaviour of the sarcode are, in a high degree, characteristic and peculiar. When the living Globigerina is examined under very favourable circumstances, that is to say, when it can at once be transferred from the tow-net and placed under a tolerably high power in fresh, still sea-water, the sarcodic contents of the chambers may be seen to exude gradually through the pores of the shell and spread out until they form a gelatinous fringe or border round the shell, filling up the spaces among the roots of the spines, and rising up a little way along their length. This external coating of sarcode is rendered very visible by the oil-globules, which are oval and of considerable size, and filled with intensely coloured secondary globules; they are drawn along by the sarcode, and may be observed, with a little care, following its spreading or contracting movements. At the same time, an infinitely delicate sheath of sarcode containing minute transparent granules, but no oil-globules, rises on each of the spines to its extremity, and may be seen creeping up one side and down the other of the spine, with the peculiar flowing movement with which we are so familiar in the pseudopodia of Gromia and of the Radiolarians. If the cell in which the Globigerina is floating receive a sudden shock, or if a drop of some irritating liquid be added to the water, the whole mass of protoplasm retreats into the shell with great rapidity, drawing the oil-globules along with it, and the outline of the surface of the shell and of the hair-like spines is left as sharp as before the exodus of the sarcode. We are getting sketches carefully prepared of the details of this process, and either Mr. Murray or I will shortly describe it more in full. . . . . Our soundings in the Atlantic certainly gave us the impression that the silicious bodies, including the spicules of Sponges, the spicules and tests of Radiolarians, and the pustules of Diatoms which occur in appreciable proportions in Globigerina ooze, diminish in number, and that the more delicate of them disappear, in the transition from the calcareous ooze to the 'red clay'; and it is only by this light of later observations that we are now aware that this is by no means necessarily the case. On March 23, 1875, in the Pacific, in lat. 11° 24′ N., long. 143° 16′ E., between the Carolines and the Ladrones, we sounded in 4574 fathoms. The bottom was what might naturally have been marked on the chart 'red clay'; it was a fine deposit, reddish brown in colour, and it contained scarcely a trace of lime. It was different, however, from the ordinary 'red clay,'—more gritty—and the lower part of the contents of the sounding tube seemed to have been compacted into a somewhat coherent cake, as if already a stage towards hardening into stone. When placed under the microscope, it was found to contain so large a proportion of the tests of Radiolarians, that Murray proposes for it the name 'Radiolarian ooze.' This observation led to the reconsideration of the deposits from the deepest soundings, and Murray thinks that he has every reason to believe (and in this I entirely agree with him) that,

shortly after the 'red clay' has assumed its most characteristic form, by the removal of the calcareous matter of the shells of the Foraminifera, at a depth of say 3000 fathoms, the deposit begins gradually to alter again by the increasing proportion of the tests of Radiolarians, until, at such extreme depths as that of the sounding of the 23rd of March, it has once more assumed the character of an almost purely organic formation, the shells of which it is mainly composed being however in this case silicious, while in the former they were calcareous. The 'Radiolarian ooze,' although consisting chiefly of the tests of Radiolarians, contains, even in its present condition, a very considerable proportion of red clay. I believe that the explanation of this change, which was suggested by Murray, and was indeed almost a necessary sequence to his investigations, is the true one. We have every reason to believe, from a series of observations, as yet very incomplete, which have been made with the tow-net at different depths, that Radiolarians exist at all depths in the water of the ocean, while Foraminifera are confined to a comparatively superficial belt. At the surface and a little below it the tow-net yields certain species; when sunk to greater depths, additional species are constantly found, and in the deposits at the bottom new forms occur, which are met with neither at the surface nor at intermediate depths. It would seem also that the species increase in number, and that the individuals are of larger size as the depth becomes greater; but many more observations are required before this can be stated with certainty. Now, if the belt of Foraminifera which, by their decomposition, according to our view, yield the 'red clay,' be restricted and constant in thickness, and if the Radiolaria live from the surface to the bottom, it is clear that, if the depth be enormously increased, the accumulation of the Radiolarian tests must gain upon that of the 'red clay,' and finally swamp and mask it." Professor Wyville Thomson further informs me that the best efforts of the 'Challenger's' staff have failed to discover *Bathybius* in a fresh state, and that it is seriously suspected that the thing to which I gave that name is little more than sulphate of lime, precipitated in a flocculent state from the sea water by the strong alcohol in which the specimens of the deep-sea soundings which I examined were preserved. "The strange thing is, that this inorganic precipitated is scarcely to be distinguished from precipitated albumen, and it resembles, perhaps even more closely, the proligerous pellicle on the surface of a putrescent infusion (except in the absence of all moving particles), colouring irregularly but very fully with carmine, running into patches with defined edges, and in every way comporting itself like an organic thing." Professor Thomson speaks very guardedly, and does not consider the fate of *Bathybius* to be as yet absolutely decided. But since I am mainly responsible for the mistake, if it be one, of introducing this singular substance into the list of living things, I think I shall err on the right side in attaching even greater weight than he does to the view which he suggests.

NOTES AND MEMORANDA.

The Spinal Chord seen with the Polariscope.—In a recent number of the 'New York Medical Journal' is a report of one of this year's meetings of the Boston Society of Medical Science, in which it is stated that Dr. Webber had accidentally found, in using the polarizer with the microscope, that *granular corpuscles* from preparations of the spinal chord, hardened in chromic acid or bichromate of potassium and preserved in glycerine, react peculiarly to polarized light, taking on a crystalline appearance. Neither cholesterine nor any other tissues or substances to be found in a number of sections of the brain and chord affected the light in a similar way, and Dr. Webber thought that this characteristic might serve to distinguish the granular corpuscles in doubtful cases. Dr. Webber agreed with the suggestion of Dr. White that this behaviour of the granular bodies did not, necessarily, imply any physical change in them, since the preparations examined had been passed through one or more solutions of crystalline substances. Dr. Webber had also found, on examining sections of brain-substance, that the nerve-fibres affected the light differently, according to their direction.

Examination of Coal for Diatoms.—It will be remembered that some time ago we announced the discovery of these forms in coal by Count Castracane. As some of our readers may wish to follow up the subject, we give the following method, which is described by Count Castracane himself:—The course to pursue is decided by the flinty nature of the diatom valves, and in order to separate them from the mixture of calcareous or organic matter with which they are found united, it is usual to put the whole into a glass test-tube with hydrochloric acid, adding caustic potash from time to time, keeping all slowly dissolving by heat, in order to isolate the silex, destroying the remainder. But in unburnt coal it is too difficult to dislodge the carbon, and the acids have little effect upon it. I must, however, refer to the calcination I effected by grinding up the substance, and then, collecting it in a china vessel, placed upon a stove in a glass tube, subjecting the whole to the action of the heat, while, at the same time, a slight current of oxygen crossing the tube combined with the carbon in creating carbonic acid. Experience has taught me, however, the necessity of conducting this operation at a lower temperature, in order to prevent the alkaline or earthy bases and metallic oxides, which may be amongst the ashes, from forming vitreous silicates by melting and mixing with the valves of the Diatomaceæ. It is also well to leave the glass tube, in which the fusing is going on, uncovered, in order to watch its progress. The small residue obtained through this process is to be put into a clean test-tube, adding nitric acid and hydrochloric acid, and caustic potash, assisted by the heat of a lamp to eliminate any alkaline or earthy base, and every trace of metallic oxides. The last operation over, it only remains to wash repeatedly with distilled water the very light dust which is left

behind, letting it stand for some hours each time to settle, in order to be sure of not losing the smallest particle of it in pouring off the water. Those who follow this method exactly cannot fail to succeed. The object may then be mounted with Canada balsam, or in any other suitable medium; and steadily and closely watching it under the microscope, they will not be long before they see some valves of diatoms, entire or broken.

Examination of the Animal Tissues for Starch—Mr. T. Taylor, in the monthly Report of the Department of Agriculture to the American Minister, makes some very astonishing statements. Those who have been accustomed to examine the human tissues with powers of from 300 to 800 diameters will be somewhat surprised at the following assertion:—If about a cubic inch of liver, spleen, heart, brain or muscle of the higher animals be immersed in two fluid-ounces of caustic potash about twenty-four hours, at a temperature of about 80° Fahrenheit, it will dissolve completely. On the addition of acetic acid in excess, the potash will be neutralized, and a flocculent precipitate will fall, which, by ordinary filtration, may be separated from the liquid. Remove the filtrant by means of a sable-hair pencil, taking care not to remove any of the fibre of the paper with the animal matter. Place a small portion of the filtrant on a capsule, and add to it a drop of concentrated sulphuric acid, followed by one of the tincture of iodine. Then place a portion of the composition on a microscopic slide, covering it with a disk in the usual manner, and examine it with a power of *about* 100 *diameters* Under these conditions blue granules of animal starch and structural cellulose will sometimes be seen, combined with amber-coloured albuminous matter. Frequently starch and cellulose, although present, are not seen, but by subjecting the composition to friction, and adding a little more sulphuric acid and iodine, well-defined blue-coloured structural forms become apparent.

An Instrument for Cleaning Thin Covering Glass has been thus described by Mr. W. W. Jones to the Quekett Club. The paper is fully published in the 'Journal of the Quekett Club.' The following is the account of the instrument:—It consists of a small tube of brass or steel, of about an inch in diameter and the same in height, into which fits loosely a weighted plug. To the lower end of this plug is cemented a piece of chamois leather. Another piece of leather is stretched upon a flat piece of wood or plate glass to form a pad, which completes the apparatus. The mode of using it is this. You place the tube on the pad, breathe on the glass, drop it into the tube, put in the plug, and then holding the tube well down on the pad you can rub as much as you like with perfect safety, the weight of the plug giving sufficient pressure. With this simple arrangement you will find it almost as difficult to break the glass as many have hitherto found it easy.

CORRESPONDENCE.

Mr. Stodder's Defence.*

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *September* 20, 1875.

Sir,—Mr. Wenham having introduced my name into his letter in the September number of this Journal, with comments on some writing of mine, I claim the privilege of replying, not from any personal feeling in the matter, but that the history of the microscope object-glass in the third quarter of the nineteenth century may be understandingly read by our successors, say in the year 1925, which it could not be if statements of that letter remain uncontroverted.

Mr. Wenham writes that Mr. Stodder "refers to me with his characteristic rancour as claiming arrangements belonging to others" I had not before been informed that rancour was one of my characteristics. I certainly do not possess implacable personal malice, or stedfast hate, synonyms of rancour, to Mr. Wenham; on the contrary, I have great admiration of his ingenious mechanical and optical appliances for the microscope, and have given him full credit for them, and now feel under great obligations to him for originating the discussion or controversy, which has done so much to bring into notice both in America and Europe the merits of American workmanship. I am not aware that I have heretofore charged Mr. Wenham with "claiming arrangements belonging to others:" perhaps I shall find before I close something like that in his last paper. I have made that charge against other parties, and am ready to substantiate the charge at the proper time.

I have assuredly from time to time called attention to some of Mr. Wenham's mistakes, misrepresentations, and instances of forgetfulness, endeavouring always to do so in as courteous terms and phrases as Mr. Wenham uses himself. Surely he will not ask me to select a more accomplished model. I now propose to specify without "rancour" those in his last lucubration of September 1.

First, Mr. Wenham says his "attention has been called to a letter from Mr. Stodder, appearing in the 'Cincinnati Medical News' for July, 1875, wherein he attempts to claim pre-eminence for Mr. Tolles, and asserts that all recent improvements have been taken from him." There is no such letter from me in that issue of that journal! or in *any other* to which Mr. Wenham's remarks can truly apply. There is certainly a letter of mine in the May number of that periodical, written in consequence of some editorial observations in an earlier number (April, 1875), on the improvement of objectives in England, in which I use Mr. Tolles' name only as a consequence of its having

[* Our readers have by this time been fully informed on all points of this discussion—if so it may be termed—which Mr. Stodder has been involved in. We must therefore positively decline the insertion of any further correspondence on this subject.—Ed. 'M. M. J.']

been used by the editor. The paper cannot be fully understood except in connection with the prior editorial.

This paper in the May number is possibly the foundation for the text of Mr. Wenham's discourse; but why such a mistake in the date?

I will now take up *seriatim* Mr. Wenham's misrepresentations of that paper, supposing it to be the one subject to his criticisms, knowing of no other to which they can possibly apply.

"Mr. Tolles is there boldly put forth as the maker of the best objectives in the world." Nothing from me in the paper that will bear such an interpretation. The editor had said in the April article on object-glasses, "that since the superiority of R. B. Tolles', of Boston, new four-system lenses has been demonstrated, the distinguished English makers of objectives are abandoning their old formulas and instituting new ones." My paper used Mr. Tolles' name only, as a necessity after that remark.

When I put forth the claim attributed to me it must be supported by such evidence as will be satisfactory to Mr. Wenham himself, and to the next generation of microscopists.

"It is imputed that Messrs. Powell and Lealand have based their recent improvements on the fact of having seen Mr. Tolles' $\frac{1}{6}$th. . . . I am in a position to say that they have not copied."

I did NOT "impute" or "insinuate" that that highly respectable firm, whose work is the admiration of two continents, had "copied." * I did say that their new $\frac{1}{4}$-inch glass was made *after* seeing the Tolles' $\frac{1}{6}$th, and I knew nearly a year before it was exhibited at the exhibition of the Royal Microscopical Society that it was promised.

"And further . . . . that they also considered that much-vaunted object-glass not a subject for imitation." Neither do I! That $\frac{1}{6}$th possesses a peculiar characteristic, which has not been repeated in the same degree in any one of the kind that I have seen, made since, a characteristic in regard to which there is a difference of opinion among experts, one, however, that was specified by a late writer in England as proof positive of the surpassing excellence of a certain object-glass of Andrew Ross.

Messrs. Powell and Lealand can learn my estimation of their work by referring to the very paper in the May number of the 'Cincinnati Medical News.' If they thank Mr. Wenham for his championship, so be it.

"The four-system combination is claimed by Mr. Tolles as his invention." Never; Mr. Tolles has made no such claim. He was perfectly aware—perhaps before Mr. Wenham—that A. Ross had made four-system objectives. It does not follow, however, that he made the same combinations.

"That in which one single front lens works both wet and dry is not copied from America." Does Mr. Wenham make that denial of his own knowledge? I repeat and insist on the statement as made in

* It would be as impossible for them to copy that $\frac{1}{6}$th as it would be for Mr. Tolles to copy any one of the many objectives of their make which have been shown to him, without taking it entirely apart, dissecting it, i. e. destroying it.

my paper in the May number of the 'News' (will Mr. Wenham have that copied in this Journal, that his readers may know the whole story?). I there give the evidence that T. Ross saw Tolles' work before he put immersion fronts to his objectives. Will Mr. Wenham say that Andrew Ross ever made immersion objectives intentionally? Many dry objectives will accidentally work immersion; Mr. Wenham himself explains why. That is a different thing from making them so intentionally. So also many object-glasses may (as Mr. Wenham says of A. Ross's) work "equally well" both ways; but both ways may be very poor.

"Having shown that the four-combination system is no novelty, I must say the same of the doublet or duplex front now claimed by Mr. Tolles as the great improvement of his lenses." I challenge Mr. Wenham, or anyone else—I rather like that formula—to produce any evidence that Mr. Tolles has ever made any claim whatever about the duplex front. Mr. Wenham has committed the mistake of thinking that the duplex and the four-system are two different things, whereas they are but two terms, either correct, for the same thing, one being more euphonious than the other. Of course Mr. Tolles has not done the ludicrous thing of claiming a name as an improvement of his lenses!

But Mr. Wenham adds of the duplex, "This was suggested by myself years ago." Indeed! Who is "claiming arrangements belonging to another" now? I am authorized to say that the "duplex front" is unlike anything that Mr. Wenham has published; has a different optical function to perform, and must produce other results. So Mr. Wenham must reserve for some future time his "gratification" for learning that his suggestion is coming into practical use, by anything that Mr. Tolles has made.

So much for Mr. Wenham's observations on my paper. I have pointed out some eight or ten errors in his. I regret the necessity of doing so, but it is due to history that they should not pass unchallenged, for they would be accepted as true otherwise.

CHARLES STODDER.

MR. WENHAM'S REFLEX ILLUMINATOR.

To the Editor of the 'Monthly Microscopical Journal.'

BOSTON, MASS., *September* 23, 1875.

DEAR SIR,—As the reflex illuminator of Mr. Wenham promises to be one of the most valuable and useful of all the recently invented adjuncts of the microscope, destined, I believe, to receive more attention than it yet has, I wish to ask Mr. Wenham, through this Journal, a brief explanation, for the benefit of all microscopists, of his note, page 156 of the September number.

Mr. Wenham writes, "The total reflexion is the same irrespective of any aperture of object-glass, and the field equally dark whether

this is used with water between the front lens or not." Mr. Wenham is here referring to the effect produced when the object is mounted dry. Will he explain what will be the result if the object is mounted in balsam, or other fluid? Also, what is meant by "water between the front lens"?

Respectfully,

CHARLES STODDER.

BENÈCHÉ'S NO. 7 OBJECTIVE.*

To the Editor of the 'Monthly Microscopical Journal.'

BALTIMORE, *September* 27, 1875.

SIR,—The flattering notices of Benèché's No. 7 objective which appeared in the 'M. M. J.' induced me to order that objective from its maker, and also, about the same time, to determine the purchase of another by the Section of Microscopy and Biology of the Maryland Academy of Sciences. The following letter was read and delivered to Mr. L. Benèché by an agent of the Berlin bankers on the 16th August last, but Mr. B. refused to make any reply. The Academy of Sciences also has had no advices from the German optician, although, as in my case, a sight draft for the money accompanied the order.

"BALTIMORE, U.S.A., *July* 26, 1875,
"No. 82, FRANKLIN STREET.

"Sir,—On the 18th February, 1875, I sent you by mail my first of exchange, No. 6621, drawn by Messrs. Kümmer and Becker, of Baltimore, in my favour, on Messrs. Anhalt and Wagener, of Berlin, for the amount of forty-five rix marcs (45 R. mx.).

"The money was in prepayment of a first-rate Benèché No. 7 objective and an adapter to the Society screw, leaving a possible *balance*, which was to be returned to me.

"On the 6th March, current, you drew the money, as your receipt for it shows.

"Not hearing from you, I wrote you on the 26th May, and again on the 25th June; but up to present date I have had no advices from you, either about the money or the goods.

"I now write to ask an explanation of conduct apparently so dishonourable, and to say that if you thus deal with your would-be customers I will not fail to make the fact known. I wrote for the objective on account of recommendations I saw in the London 'Monthly Microscopical Journal,' and, trusting you, as I do all English

[* Mr. Johnston's note arrived in time for insertion in the November number. We, however, withheld it for a while, and in the meantime we wrote to Herr Benèché, thinking it right to give him an opportunity of replying to certain inquiries we made of him. Herr Benèché has not had the common politeness to acknowledge the receipt of our communication; in fact, he has treated us as he has dealt with Dr. Johnston, and therefore we consider the publication of Dr. Johnston's letter perfectly justifiable.—ED. 'M. M. J.']

tradesmen, I ordered from you, and also urged the Maryland Academy of Sciences, Microscopical Section, to do the same. But your conduct is alike in both cases, and reflects no credit on your house.

"I leave this letter open that the gentleman delivering it may know its contents and demand an answer.

"Respectfully, &c.,

"CHRISTOPHER JOHNSTON, M.D.

"MR. L. BENÈCHÉ, Berlin, Prussia."

THE PROPOSED MEDAL.

To the Editor of the 'Monthly Microscopical Journal.'

MONTPELIER PLACE, BRIGHTON, *October* 8, 1875.

SIR,—I am obliged to F.R.M.S., who, in your issue for the present month, has drawn attention to my proposal of a prize medal for objectives showing the best histological work. It is true we differ widely upon the subject as yet; nevertheless, I hope to bring him over to my way of thinking; for I am sure so good-humoured an opponent will be amenable to reason, and will not be influenced by any motives not in the true interests of the microscope.

The objections of F.R.M.S. to my suggestion seem to be chiefly directed to the practical difficulty of carrying it out, and fairly and impartially awarding such a distinction. He has, I regret to see, no confidence in the integrity of the opticians themselves, nor in that of those who would be selected as judges, and as little, I presume, in the efficacy of any safeguards that might be adopted to secure a real and unbiassed competition. Hence his opposition.

If, however, it can be shown that he is probably mistaken upon the first of these points, and certainly so upon the second and third, his hostility will, I am sure, cease; and in that case I even trust to claim him as a coadjutor in my scheme.

For my own part, I quite fail to see why the honour of the makers of objectives is to be impugned! I cannot believe that our celebrated opticians, the world-wide reputation of whose firms is the well-earned guarantee at once of the thoroughness of their work and the reliability of their word, would be at all likely to exhibit other than the most upright conduct in the matter. I am now, without disparagement of foreigners, speaking of English opticians, being naturally best acquainted with them. But if their brethren of repute in other countries resemble them, then I feel no doubt that all those sad, and let me say humiliating apprehensions of backstair interest and underground motives entertained by F.R.M.S. will be found to vanish when submitted to the test of actual trial.

If, however, my confidence in the independence and concurrent straightforwardness of the opticians were unhappily not justified by the facts, we should still have the security of the unimpeachable ability and fairness of the judges. These would naturally be not

mere amateurs with crotchets, but gentlemen professionally engaged in scientific histological work: men like Beale, Burdon Saunderson, Carpenter, Braxton Hicks, Klein, and others of the many competent teachers of histology in our medical schools and colleges. I apologize for using the names of these gentlemen without permission; but it will be understood that I do so because they are representative men, and belong to a class in whom all concerned would have the most complete confidence.

Further than this, in the interest of all parties, competitors, judges, and the scientific public, it would be well to observe the precautions adopted in judging prize essays. Thus the competing objectives should be sent in with a motto, accompanied by a sealed letter containing the maker's name and the same motto, but not to be opened until after the award. It would be an easy matter to make it a condition of competition, that all lenses sent in should be mounted in brasswork of a given pattern. Makers could adhere to such pattern afterwards or not, as they pleased.

F.R.M.S. hints that such a medal, if given at all, should be offered more frequently than once in three years. I have no objection to its being made an annual prize; but that is matter of detail, and does not touch the principle. It should be left for the consideration of the Society presenting it.

These are a few of the observations which occur to me as a fitting reply to the objections of F.R.M.S., and I hope he will not only think them *à propos*, but also that they fairly meet what he has said against the proposition.

With regard to Mr. C. Stodder's remarks, although I can make no abatement whatever from the weight of the authorities given in my former letter, I freely acknowledge that under the actual circumstances I have perhaps stated too broadly the view that objectives best calculated for the display of diatoms are not equally suited for histological work. On the other hand, it will be sufficient to remind him that what he claims to have settled, as I understand him with his own glasses, is exactly the point in dispute; and that no mere claim, however confidently made, can of itself settle anything. Epithets like that he has chosen to characterize my views, he will hardly expect to be complimented upon; they are inapplicable, and greatly to be deprecated as destructive of the mutual confidence and esteem which should exist amongst scientific men.

To return. Whether Mr. Stodder and those who still think with him, or those of the opposite school, are right, is of no consequence at all to the matter in hand. The gold medal should not be offered for objectives constructed upon any particular principles, but for such as are capable of doing the best histological work, according to a standard fixed by the Society. The aperture, and all other circumstances affecting the optical question, should be left entirely to the competing opticians. Those of them who believe that the highest angles are best under all circumstances, will no doubt send their most approved diatom lenses; while those who think that there is a material difference between the work which is confined to the examination of

surfaces, and that which deals with the interior substance of tissues, will no doubt construct glasses to meet that difference.

In common with all really interested in the perfection of microscope objectives, I cannot but greatly admire the beautiful performance of recent high-angled lenses in the display of diatoms. There cannot be a doubt we owe very much to their constructors, and it is to my mind probable we shall owe more still. On the other hand, as a practical histologist I confess with regret that these lenses, whether equal to others or not in histological work, are at least greatly inferior in this respect to the power they themselves exhibit in the case of diatoms. If this be so, and high-angled glasses have so to say already done their best for histology, it is imperative that other constructions be fairly tried, and the catalogue exhausted if need be, in the attempt to arrive at the greater excellence, which is still so much to be desired. I see no other way in which this is likely to be done than by the encouragement and reward of makers in such a competition as I suggest.

I am, Sir, obediently yours,
R. Branwell, M.R.C.S.E., F.R.M.S.

The Immersion Aperture Question: Reply to Mr. Wenham.

To the Editor of the 'Monthly Microscopical Journal.'

224, Regent Street, London, *November* 11, 1875.

Sir,—Mr. Wenham states that the root of the present immersion aperture question is to be found in his contributions to the Quarterly Journal of twenty years ago: I must express my dissent. The question then discussed had reference to the *illumination* of balsam-mounted objects,—had no reference whatever to the matter now in hand, which concerns immersion lenses *only*, in relation to their image-forming aperture capacity.

The argument maintained by Dr. Woodward is confirmed by the use of the Reflex Illuminator with those immersion lenses which claim from their construction to refract *viâ* water image-forming rays of greater obliquity than those corresponding to the maximum transmissible by a dry objective. In the experiment alluded to by me, the Illuminator is used as a means of providing rays incident on the object in balsam (and consequently on the field) at greater inclination than the angle of total internal reflexion between balsam and air; these rays emerge from the cover-glass conditionally on there being water contact between the cover and the objective front, but with *air* as the external medium they do not emerge. Mr. Wenham says that when fluid is introduced between the lens and cover ". . . . all total reflexion is gone;" it is this very fact which gives its value to the experiment. For what is the inclination of those rays which pass through the water and are so refracted by the objective as to produce a luminous field? Is it greater than corresponds to the maximum transmissible by a dry objective?

In describing the action of the Reflex Illuminator, Mr. Wenham said, ". . . . the most oblique [light] that we can obtain by ordinary

means, cannot strike on the object at an angle greater than 41° if it is either in balsam or on the slide, but on this principle we are dealing with rays entirely beyond this angle."*

If, by suitable adjustment of the mirror, we can ensure the illumination of the object solely with rays from the Illuminator of greater obliquity than 41°—which is admitted by the construction—the field rays must also be of greater obliquity than 41°; and if, with pneumo-lenses these rays are necessarily lost by total reflexion at the cover-glass, whereas with certain immersion lenses a portion is refracted into a luminous field; it follows that rays beyond the "critical" angle from balsam to air are "got through" the immersion lens, and Mr. Wenham's dictum that "no object-glass can collect image-forming rays beyond this limit" is confuted.

With reference to the "simple demonstration" cited in Mr. Wenham's postscript for my special benefit, I observe he had already favoured us with the method of procedure.† I try the experiment:—He says, "Focus the top surface of the plate glass with an immersion $\frac{1}{8}$th having the highest available aperture;" he must here mean to request me to focus the immersion lens *used as a dry lens*, because later on he asks me to look sharply while the water intermedium is applied. I take up Hartnack's No. 9 immersion having the highest available aperture, and find it will not focus the definition with *air* as the intermedium: the experiment fails when tried with such an immersion lens! But in order to meet him, I take Dallmeyer's new $\frac{1}{4}$ in which the immersion front can be used wet or dry, and, adjusting it to the "dry" point, focus sharply the image of the line on the surface of the plate glass. I remove the ocular and allow the rays from the lamp-flame to traverse the optical body and the objective, they cross at the focus and form a luminous disk on the ground side of the plate glass; the angular diameter of this disk is about 55°. I introduce the water film; but it occurs to me I shall no longer be measuring the angle of the image-forming cone of rays,—the focus is no longer on the glass surface,—the lens must be adjusted for "immersion." As I do this, the angular aperture increases, and when I reach the point at which the image-forming rays are sharply focussed, the disk of light has increased to 70°! With *air* as the intermedium, the image-forming aperture is 55° as shown by the disk of light; with water it is 70°. But Mr. Wenham asserts that whether there is water or air as the intermedium "not the slightest change is visible in the diameter of the disk"! and in his account of the experiment in No. lxiii., p. 116, he stated "It made no difference in the angle whether water is admitted in front lens or not . . . . *but in each case the object-glass must be focussed on the glass surface.*" I must leave him to explain the discrepancy between his result and mine.

When Dr. Woodward forwarded Professor Keith's diagram and trigonometrical computation to London in support of his position, he wrote: ‡ "I cannot be expected to pay attention hereafter to the assertion of anyone who may continue to hold that it is 'theoretically impossible' to construct immersion objectives with a balsam aperture

* 'M. M. J.,' No. xlii., p. 241. † Ibid., No. lxiii., p. 116.
‡ Ibid., No. lxix., p. 127.

greater than 82° of 'image-forming rays,' unless he can show some material error in Mr. Keith's computations."

In replying,* Mr. Wenham discovered no better ground on which to base his objections than to suggest that the data given by Mr. Tolles were uncertain. He said, "Mr. Tolles, from whom all measurements come, has repeatedly supplied diagrams to suit his purpose;" and again, "Of course Messrs. Woodward and Keith are not responsible for data, but the former gentleman tells us it is '*a diagram accurately constructed in accordance with the computed results*'; having therefore been drawn to suit the proposition, it may be dismissed"! This estimate of the value of his opponent's diagram differed from that he gave of his own with his "New formula," thus:† "Diagrams, however, are surprisingly accurate in their capability of indicating causes and results in the microscope and object-glass."

For my part, the question being put in the form of a mathematical demonstration, I considered no authority would be recognized as of value in criticising it except that of a professional mathematician; I therefore placed it in competent hands. Mr. Wenham says that "however consoling to Mr. Mayall the opinion of the high mathematical authority may be, his anonymous verdict will not be considered important by others." Let me assure Mr. Wenham the verdict is none the less consoling to me, nor will it be considered less important by Dr. Woodward, Professor Keith, and others, from the fact that it was given by Professor G. G. Stokes, Secretary of the Royal Society.

Your obedient servant,

JOHN MAYALL, jun.

THE APERTURE QUESTION.‡

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—Nothing can be easier than to dispose of Mr. John Mayall, jun.'s, claim to have discovered a "most elegant practical" refutation of Mr. Wenham's position on the aperture question.

His claim is based on the supposition that in the Reflex Illuminator none of the rays are transmitted when a dry objective is used on "Möller's Probe-Platte" (!); or, in other words, that all the rays fall within the critical angle, and are consequently *totally reflected* from the upper internal surface of the slide.

If your readers will refer to the seventh volume of the 'Monthly Microscopical Journal,' page 241, they will find in Mr. Wenham's own paper, when describing his new appliance, the most complete and conclusive refutation of this assumption; for after saying that he will anticipate a few objections to the arrangement, he adds:

"It may be said that the rays reflected from the lower end of the facet are just without the angle of total reflexion, and might enter true, and I had intended to stop off a small segment of the lens at this place, but found it so desirable, in many objects, to admit a little light, that I preferred it without alteration. It is easy to get a black field in all cases by mere mirror adjustment."

* 'M. M. J.,' No. lxxi., p. 221. † Ibid., No. lii., p. 164.

[‡ This letter has stood over from the last number.—ED. 'M. M. J.']

The answer to Mr. John Mayall, jun.'s, question, Whence comes the luminous field in the immersion lens if not from its having the power to collect rays which are *totally reflected* when the dry lens is used? is thus abundantly answered. It may come from the rays reflected from the lower end of the facet *intentionally* left to admit the light.

I must decline to argue with Mr. Carr whether when the image under a $\frac{1}{25}$th breaks up by the test of deep oculars its amplifying power is "about 1000 diameters" only? The answer is so obvious that the discussion is puerile: the initial power is greater. Equally unprofitable would it be to debate as to what Mr. Hogg did or did not say in his letter; at pages 97 and 98 of your Journal it will be found at full length, with the "non sequitur" at which he has arrived.

Your obedient servant,

CRITO.

CHROMATIC AND SPHERICAL ABERRATION.

To the Editor of the 'Monthly Microscopical Journal.'

1, BEDFORD SQUARE, *November* 12, 1875.

SIR,—Dr. Pigott's contribution on the question of chromatic and spherical aberration has at least the demerit of introducing confusion into this subject. Would he have us believe he has discovered that everyone up to the present time, save himself, has held erroneous views on the well-recognized distinctions between these aberrations? Dr. Pigott's attempt to persuade us that they "are identical in the nature of things," requires some explanation; for it appears to me he makes "confusion worse confounded," by calling two properties of lenses by the same name; whether by so doing this will add to the clearness with which the subject is usually apprehended is a matter demanding further consideration.

If, as he says, "all chromatic aberration involves spherical aberration," will he tell us how it happens that in the mathematical formula given by Herschel and other writers on optics, for the elimination of chromatic aberration, the expressions for *forms* and *order* are not found? And again, if these aberrations "are identical," why have they been discussed under separate and distinct propositions? and why do practical opticians continue to treat them as separate and distinct matters, knowing well that the conditions essential to the correction of chromatic aberration do *not necessarily involve* those of spherical aberration, and *vice versâ?* They may, as Professor Parkinson says, be corrected in one and the same combination; nevertheless, the aberrations are in themselves perfectly distinct phenomena.

I cannot well give Dr. Pigott a higher authority on this subject than the Astronomer Royal, whose words are, "The laws of spherical aberration and of chromatic aberration both in microscopes and telescopes are *totally different.*"

I remain yours, &c.,

JABEZ HOGG.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King' College, *November* 3, 1875.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

The subjoined list of donations to the Society was read, and the thanks of the meeting voted to the donors.

The President called attention to a series of photo-micrographs—included in the list of donations—presented by Dr. Rollins, and taken with a Tolles' objective. They represented a number of dental sections, and were handed round for the inspection of the Fellows.

The President gave a highly interesting account of a new method of measuring bands in spectra, illustrating the subject by drawings upon the black-board. (A paper upon the subject will be found at p. 269.)

A vote of thanks to the President for his communication was put to the meeting by Mr. Slack, and carried unanimously.

A paper by Dr. J. J. Woodward, of the United States Army Medical Department, was read by the Secretary; it was entitled, "Notes on the Markings of *Frustulia Saxonica*," and had reference to some observations in the 'Monthly Microscopical Journal,' vol. ix., p. 86; but more particularly to a paper by Mr. Hickie, published in the number for last July. Photographs in illustration accompanied the paper, the text of which will be found printed at p. 274.

Mr. Slack said that in the July number of the Journal there was a figure of this diatom copied from Dr. Schumann's work. The figure, from '*Die Diatomeen der Hohen Tatra*,' was rather broader in the centre than Dr. Woodward's photograph, and slightly constricted just before the nodules at each end. In his text he, Dr. Schumann, said, "*Frustulia Saxonica* occurs in the following forms: *Naviculia crassinervia*, Brébisson; *Navicula cuspidata*, Kützing. (*Sie tritt in folgenden Formen auf*), "one like the first form, but which showed two strong marginal border stripes on each side," is thus described: "In reference to the kind and number of the striæ (*Riefen*), all these forms agree in the chief characters, so that the formula which describes the first suits the others. With oblique light the walls of a single cross stripe appear like distinct striæ, and thus the number of the striæ is apparently doubled." After some remarks about focussing, he says, "I agree with Grunow as to the nodules." In Pritchard, *N. cuspidata*, Ktz., is given as identical with *N. fulva*, Em.

The President, in proposing a vote of thanks to the author of the paper, thought the subject was one of great interest, because it was certainly most desirable for all to know whether things which they saw really existed or not, because many things which were seen might under certain circumstances be due to interference. He thought that the photographs sent in illustration fully bore out Dr. Woodward's remarks. He had not himself paid very great atten-

tion to the study of diatoms, though quite enough to enable him to see the importance of Dr. Woodward's observations.

Mr. J. Mayall, jun., said: Dr. Woodward has made an exhaustive examination of one of the finest known test-diatoms by means of photography, and he has supplemented this by critical observations on the true definition of the diatom, which, coming from so practised an observer, must together go far to settle the question—so far as it can be settled with the optical means at present available. I am particularly interested by what appears to me an original observation of Dr. Woodward's,—I refer to his suggestion for readily distinguishing *diffraction* lines from *real* lines existing in the object. He observes the diffraction lines have this peculiarity, that they appear to increase or decrease in number as we approach to or recede from the focus of the real lines; whereas really existing lines in an object do not appear to vary in number when viewed slightly within or without the true focus. This is, I think, a highly important distinction, and if carefully mastered by microscopists will lead to greater certainty of interpretation. Dr. Woodward also calls our attention to the fact that photography gives as much apparent reality to the images of diffraction lines as to those of lines actually existing in the object: this is manifest in the photographs of *Frustulia Saxonica* which he has forwarded to us in illustration of his remarks. With regard to the distinction attempted to be drawn by Mr. Hickie between the definition of the diatom *Frustulia Saxonica* and what he alleges to be another diatom going by the name of *Navicula crassinervis*, I know the diatom well, and have always regarded the two names as applying to the same object. Möller uses either or both names in describing it, and I venture to think his opinion is far more valuable than Mr. Hickie's.

Mr. Slack said he did not know the object, but he thought that the fact mentioned by Dr. Schumann that there were two marginal edges was quite sufficient to account for the diffraction lines.

The President announced that arrangements had been made for carrying out the proposal to hold a scientific evening on the 24th instant; also, that at the next ordinary meeting a paper would be read by Professor T. Rupert Jones, entitled, "Remarks on the Foraminifera, with special reference to their Variability of Form, illustrated by the Cristellarians.

Donations to the Library from October 6, 1875:

	From
Nature. Weekly	*The Editor.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
Transactions of the Linnean Society. Five parts	*Society.*
Journal of the Linnean Society. No. 8	*Ditto.*
Transactions of the Royal Irish Academy. Fourteen parts	*Academy.*
Proceedings of the Royal Irish Academy. Five parts	*Ditto.*
Canadian Journal. No. 83.	
Bulletin de la Société Royale de Botanique de Belgique. Five parts	*Society.*
Ten Micro-photographs of Sections of Teeth, illustrative of Dental Surgery, by Dr. W. H. Rollins, of Boston, U.S.A., produced by Tolles' lenses	*Chas. Stodder, Esq.*

The following gentlemen were elected Fellows of the Society:—George Manners, Esq.; William Hadden Beeby, Esq.; George Hastings, Esq., M.D.

WALTER W. REEVES,
Assist.-Secretary.

MEDICAL MICROSCOPICAL SOCIETY.

Oct. 15, 1875.—Jabez Hogg, Esq., Vice-President, in the chair.

The Cochlea in Birds.—Dr. Pritchard explained and exhibited specimens illustrative of the structure of the cochlea in birds.

Artificial Fibrillation of Hyaline Cartilage.—Mr. Cresswell Baber exhibited specimens illustrating his paper in the 'Journal of Anatomy and Physiology' on the above subject. His observations were based upon a statement by Tillmanns, in Max Schultze's 'Archives,' to the effect that fresh hyaline cartilage can be fibrillated by macerating it for several days in a solution of permanganate of potash, or in 10 per cent. solution of chloride of sodium. Mr. Baber showed that fibrillation of the matrix can be produced by macerating sections of hyaline cartilage in solution of chloride of sodium (both 10 and ½ per cent.) in lime water, or in baryta water, and in each case after the maceration applying momentary pressure to the glass covering the section before examining it. The fluid that acted most rapidly was baryta water, which produced the fibrillation in half an hour; while permanganate of potash that Tillmanns prefers, he had found uncertain in its action. Mr. Baber had found the fibrillation of the cartilage matrix in all cases in which he had searched for it, and concluded therefore with Tillmanns, that the hyaline matrix is composed of fine fibres held together by an interfibrillar cement substance that can be dissolved by certain reagents. A discussion followed, and the meeting then resolved itself into a conversazione.

QUEKETT MICROSCOPICAL CLUB.

Ordinary Meeting, September 24.—Dr. John Matthews, F.R.M.S., President, in the chair.

A paper by Mr. James Fullagar, Hon. Assistant-Secretary to the East Kent Natural History Society, "On the Development of *Actinophrys Sol*," was read. In this paper the results of four months of continued observation upon a number of those organisms—kept carefully isolated—were recorded. The various changes which took place cannot be fully described without drawings, but may be shortly indicated as follows: A group of eight, which had become closely united, were isolated and examined. In this state they were seen to feed voraciously. The author considered the fusion of several specimens to be a preparation for encystment. When fully fed, they separated into their original number, assuming their well-known form, the pseudopodia becoming greatly extended. After some time the pseudopodia were gradually withdrawn, and in about six hours had disappeared, the centre of the body became darker, and the contractile vesicle ceased to pulsate. The central part of the body then divided into two equal

globular masses, surrounded by the original spherical casing, outside of which was a thin transparent film. In twelve hours this casing lost its spherical form, the central globes altered in shape, became gibbous, and separated from each other. After some time they again approached, and in an hour from the time of touching they again became fused into one globule, much smaller than the original size. To this point the same changes took place in each of the eight specimens. One of the group was then observed to move about among the others, gliding between and around them, and throwing out branched pseudopodia. This movement was continued, with short intervals of rest, for more than forty-eight hours. The other seven also exhibited slight movements from time to time, and protruded amœbiform pseudopodia, somewhat resembling *Amœba bilimbosa*. From some of the specimens a cloudy translucent matter exuded, from which issued minute globules and clear and transparent amœboid bodies, sometimes resembling *Amœba princeps*, and at others *A. porrecta*, *A. limax*, and *A. actinophora*.

In a note to his paper, Mr. Fullagar describes other changes, indicating the development of young *Actinophryans* from encysted specimens by segmentation, and a change from the amœboid form to that of an oval or pear-shaped body with one very thick and long pseudopodium. In some cases also the globular bodies in approaching each other became flattened, and again separated without coalition.

The paper was illustrated by a large number of drawings, showing every stage of development.

Reading Microscopical Society.*

October 12, 1875.—At this, the first meeting of the Society for the winter session, after the transaction of the usual business, and the reading of a letter from Major Lang, the following resolution, moved by Mr. J. G. Tatem, and seconded by Dr. Shettle, was unanimously passed:

"That this meeting receives with the deepest regret Major Lang's resignation of the office of President, which, from the very commencement of the Society, he has with so much courtesy, and profit to the members, kindly filled.

"That the members of the Society regret that the only return they can make for the frequent self-denial which their late President's regular attendance at the monthly meetings must have involved, and for his constant devotedness to the interests of the Society in every way, is to express their hearty and unanimous recognition of all the services he has so long rendered, for which they tender their best thanks; and they also indulge the hope that he will allow them to consider him honorary Vice-President and Corresponding member, which they trust they may for many years be permitted to do."

At the conversazione, which closed the meeting, many objects of interest were exhibited.

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

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