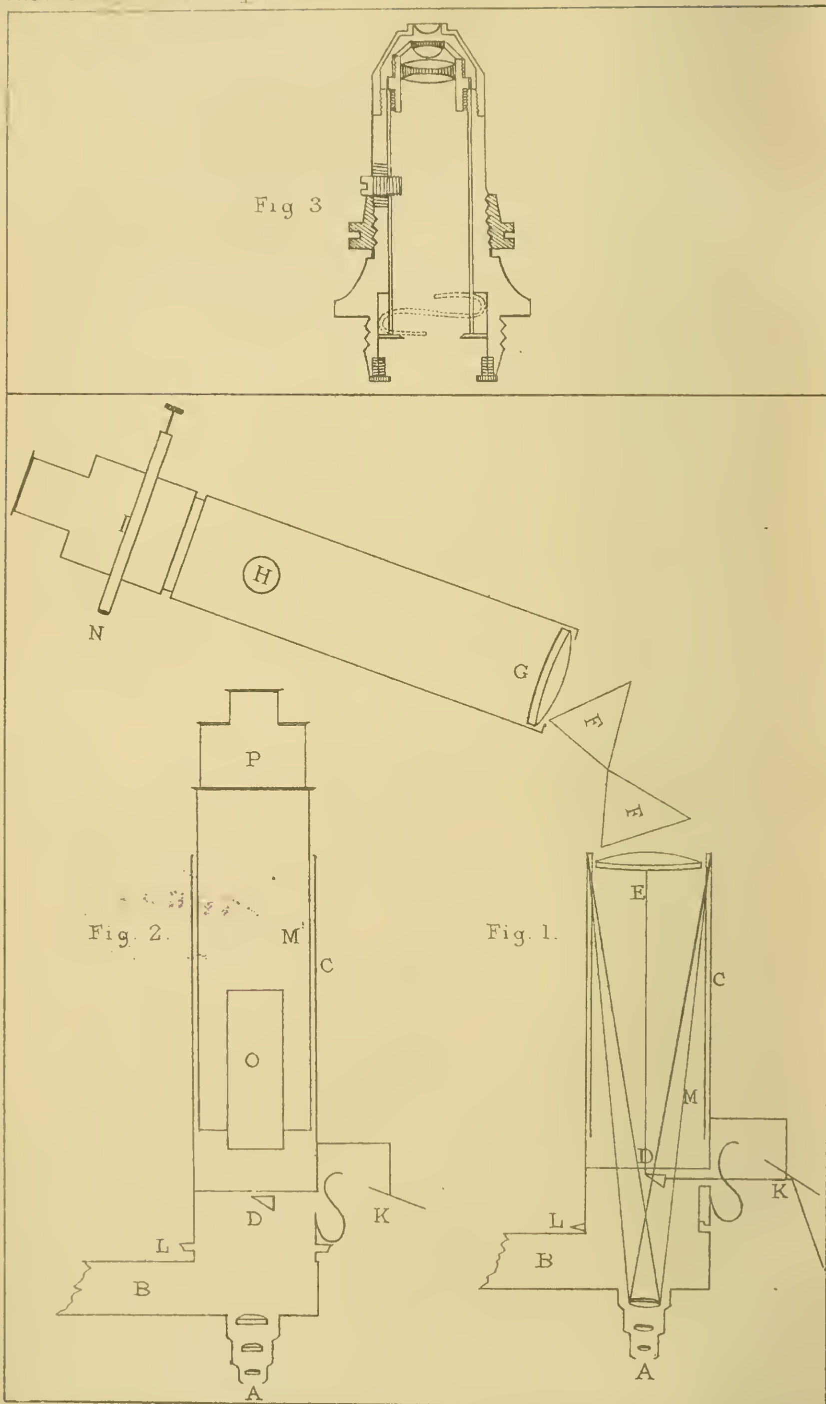

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THE MONTHLY

MICROSCOPICAL JOURNAL:

TRANSACTIONS

OF THE

ROYAL MICROSCOPICAL SOCIETY,

AND

RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

EDITED BY

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THE

MONTHLY MICROSCOPICAL JOURNAL.

JANUARY 1, 1873.

I.—*A New Form of Micro-spectroscope.*

By Edward J. Gayer, Surgeon H.M. Indian Army.

(*Read before the* Royal Microscopical Society, *Dec.* 4, 1872.)

Plate I. (Lower portion).

In the form at present in use, the Sorby-Browning direct vision micro-spectroscope, the divergent rays of light received from the object-glass are collected at the eye end of the microscope by a lens, placed about an inch in front of the slit, and after passing through it, are rendered parallel in the usual way by an achromatic collimating lens, before entering the compound direct-vision prism, through which the spectrum is viewed directly without a telescope.

In the new form, the divergent rays issuing from the object-glass of the microscope are at once passed through the slit, placed about an inch from the objective, without passing through any collecting lens; the prisms are not compound, and the spectrum is looked at through a telescope. The advantages of the new form are, that there is more light, more dispersive power, and that it is quite possible even with high powers to use a telescope for magnifying the spectrum, and collecting the whole of the light passing through the prisms; also no special form of micrometer is necessary because as there is a telescope any ordinary micrometer can be brought to focus and rendered visible. A reference to the annexed Plate will render the instrument more intelligible.

A is the objective; B, the arm of the stand of the microscope carrying the body C; D is the slit of the spectroscope; E, the collimating lens; F, F, two dense flint-glass prisms of 60°; G, the object-glass of the telescope; I, one of the eye-pieces belonging to the microscope; H, rack-work motion to the eye-piece of the telescope; K, the usual apparatus for the second spectrum; N, the micrometer. There are screw adjustments in the brass plate and tube in which the prisms are placed.

To use this micro-spectroscope, the tube of the microscope should be unscrewed at L, and replaced by the spectroscope, which should be made to fit the same screw. The angle, which the telescope makes with the body of the instrument, will be found a very convenient one, as the stage of the microscope will be horizontal, while the telescope will be inclined at a comfortable angle for the eye. It is obvious that more light passes through the slit of this spectroscope, because it is so much nearer the object-glass, and also that more light passes through the prisms, because they are fewer, and the collecting lens being done away with, the light has only one instead of two lenses to traverse. All this gives a greatly increased brilliancy to the spectrum, and enables objectives of high power to be used, as also a telescope. When it is required to examine the spectra of very minute objects, it is important to ascertain that the whole of the light which is used to form the spectrum passes through the substance which is being examined. In order to make certain that this is the case, the tube M (carrying the collimating lens and the spectroscopic apparatus and sliding in the outer tube C) should be removed, leaving the outer tube C carrying the slit and all its adjustments fixed to the microscope. The draw-tube of the microscope having the erector screwed into its place at the lower end of it, should now be inserted into the outer tube C, and an eye-piece should be placed in the usual position in the draw-tube; the open slit can now be easily and clearly focussed by sliding the draw-tube in and out until the slit is seen distinctly. The object on the stage of the microscope being now placed in focus in the usual way, both it and the slit will be in the same field of view, and it will therefore be easy to make certain that the material under examination occupies the proper position between the jaws of the slit.

Diagram No. 2 shows this arrangement. M′ is the draw-tube of the microscope; O, the erector; and P one of the negative eye-pieces of the microscope. The draw-tube being removed, the inner tube M carrying the spectroscope can now be replaced, and the observer will be certain that the slit corresponds to the proper radiant point.

II.—*On an Aërial Stage Micrometer: an improved form of engraved "Lens-Micrometer" for Huyghenian Eye-pieces, and on finding Micrometrically the Focal Length of Eye-pieces and Objectives.* By G. W. Royston-Pigott, M.A., M.D., &c.

(*Read before the* Royal Microscopical Society, *Dec.* 4, 1872.)

The writer wished to draw attention to a mode of using micrometers, which he thought was somewhat novel. He has found very great convenience from using an aërial image of micrometer spider-lines. The micrometer which he employed was one of Browning's best. The miniature image formed in the focus of the microscope on the stage was reduced six times exactly by means of an objective placed below it in a certain position within a sliding tube, adjusted precisely by a stud. He has thus brought the arrangement to this point, that, with an old ½-inch object-glass, the miniature aërial image of the micrometer spider-lines appeared precisely one-sixth of their natural size. In other words, the instrument is rendered six times more sensitive than it was before. There was not the slightest difficulty in moving the aërial image or spider-lines something like 80000th of an inch at a time in the plane of focal vision on the stage. When an observer had a very delicate object which he wished to measure, it was extremely inconvenient to disarrange the microscope. By this simple method, the micrometer was always in use; it formed part of the condensing apparatus, and by turning a screw the micrometer lines rose into view, which can then be traversed with the greatest nicety.

In measuring very minute quantities, it had been always a matter of difficulty to work beyond a certain point, in consequence of the vibration which was attendant on any movement of the instrument; but if you moved this micrometer placed below the stage, vibration would be avoided. On examining microscopically the image under a power of 200 by means of a stage micrometer, you are able at once to see, with the greatest precision and accuracy, that you really have got a certain proportion between the micrometer lines and the aërial image. He had chosen six times because it happened to suit the size of his instrument. If such as the ¼ be used the image is reduced twelve or fourteen times. As a general rule it is found the ½ inch of low angle gives sufficient light. It gave a sharply-defined miniature of the spider-lines; and the 100th of an inch interval between the aërial lines under a ½ inch was a very considerable quantity to look at. It could then be divided easily into 600 parts.

There was some interest displayed at the present moment in ascertaining magnifying power, and it was rather important to know what one was about. Very great difficulty is experienced in using the old-fashioned glass micrometer, usually placed at the stop within the eye-piece; there were disturbances arising from four surfaces of refraction, and from various changes in the Canada balsam, which underwent decomposition. The writer wishes now to communicate a plan which perhaps will be generally adopted, and it is this; instead of using a micrometer glass of that nature, after some little study he had had lines engraved upon a very long focus plano-convex lens.* The lens was then inserted into the stop. Its magnifying power was of course so slight at that position that it did not appreciably alter the effect of the eye-piece. By this means he got per-

* The weakest that is made for spectacles: nearly plano-convex.

fectly bright linear images without the errors of refraction from four surfaces and the changing film of Canada-balsam cement. We got a nearly perfect surface to engrave upon, the plano-convex lens being far easier of true workmanship than the formation of exactly parallel surfaces. The consequence was that he got a brilliant field, brilliantly marked, with an unchangeable micrometer. That was the first point.

The writer had just tried rather a curious experiment. If you place the micrometer in your different eye-pieces, say four; then view the stage micrometer, the Fellows would be surprised to hear that the reading of the micrometer as to the size of a 100th of an inch on the stage, was the same for all the four eye-pieces, very nearly indeed. It seemed very odd, for instance, that, under a $\frac{1}{4}$-inch objective, he read 37 hundredths: placed another eye-piece B he read 38 hundredths; in the D eye-piece he read 39 hundredths. The conclusion was this, that if you put the same micrometer into the four eye-pieces, A B C D, they all showed the same reading in the size of a 100th of an inch upon the stage, provided the principle of their construction and arrangement was precisely the same.*

The new micrometer, engraved on a very long focussed plano-convex instead of the old-fashioned cemented micrometer, contained one hundred divisions to the inch, the same as the stage micrometer. When the eye-lens of the eye-piece is exactly 10–11ths of an inch, then, and then only, did the reading of the new micrometer, as compared with a 100th on the stage, give the magnifying power of the whole instrument in use.† If anyone looked through the microscope by the use of this micrometer, he could read instanter the magnifying power, no matter what objective was employed. It seemed very strange at first sight that all the eye-pieces should read nearly the same. But only one of them precisely gave the correct magnifying power. If the divisions of the new lens engraved micrometer now exhibited were 200 to the inch, then only an eye-piece having a $\frac{1}{2}$-inch focal eye-lens would tell the power at sight, and so on.‡ He must compliment Mr. Acland upon the very beautiful manner in which the "lens-micrometer" now used in the eye-pieces had been engraved. He had often tried some experiments in reference to cutting fine lines upon glass, and had found that, by turning round the writing diamond upon its axis, there was generally some particular angle of rotation at which the diamond ploughed most beautiful grooves, scattering minute little glass curls or shavings, and then the grooves were as clear as if they had been planed a thousand times larger.

* This principle is in general that the field-lens shall have three times the focal length of the eye-glass, and that the interval between the glasses shall, to preserve the achromatism, be exactly one-half the sum of their focal lengths.

† Provided the distance of distinct vision is 10 inches.

‡ Accurately $\frac{5}{11}$ for 10 inches vision.

The micrometer eye-piece for displaying at once the magnifying power at a glance is thus constructed: focal length of field-glass, 3 inches; focal length of eye-glass, $\frac{10}{11}$ inch; distance between the lenses, 2 inches; number of divisions of lens-micrometer, 100 per inch.

OBSERVATION.—The aërial image of the spider-lines of Browning's micrometer placed under the stage was then shown in the focus of the microscope at about 200 diameters. Six full turns of the divided milled head of the Browning micrometer moved a spider-line image exactly 100th of an inch upon the stage micrometer, and as the milled head was divided into 100 parts, one of its divisions exactly corresponded to a movement of the aërial image $\frac{1}{100} \div \frac{1}{600}$th, or $\frac{1}{60000}$th of an inch, and half a division to the $\frac{1}{120000}$th.

Referring to the new "lens-micrometer" placed at the stop of the Huyghenian eye-piece, all objectives can at once be examined and their focal lengths assigned by simply remembering that all the makers construct their objectives on the following principle; if we divide 100 by the *assigned focal length*, we have the magnifying power of the microscope with "C eye-piece."

$$\text{Thus}\quad 1 \text{ inch power} = \frac{100}{1} = 1 \quad \text{with "C."}$$

$$\tfrac{1}{4} \quad ,, \quad ,, \quad = \frac{100}{\frac{1}{4}} = 400 \quad ,, \quad ,,$$

$$\tfrac{1}{8} \quad ,, \quad ,, \quad = \frac{100}{\frac{1}{8}} = 800 \quad ,, \quad ,,$$

$$\tfrac{1}{16} \quad ,, \quad ,, \quad = \frac{100}{\frac{1}{16}} = 1600 \quad ,, \quad ,,$$

$$\tfrac{1}{25} \quad ,, \quad ,, \quad = \frac{100}{\frac{1}{25}} = 2500 \quad ,, \quad ,,$$

Now in general the B eye-piece is half the power of the "C" and D double. Moreover the $\frac{1}{4}$ of Andrew Ross is really a $\frac{1}{5}$, for it magnifies 500 with C eye-piece instead of 400. In each case *the stop* of the eye-piece is supposed to be exactly 10 inches from the object on the stage.

The "Lens-Micrometer" is an improvement upon the Kratometer already described, p. 79, XXVI. of this Journal for 1870.

(*To be continued.*)

III.—*On the Development of the Skull in the Tit and Sparrow-Hawk.* By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *Dec*: 4, 1872.)

Part I.

Plate II.

In an embryo of one of our native Titmice (either *Parus cerulæus*, *P. fringillago*, or *P. ater*—one of the three), I found much to interest me: it had undergone about three-fourths of the incubating process. The head (Plate II., Fig. 1), the size of which makes it most probable that it belonged to *P. fringillago*—the Ox-eye Tit—had fine filamentous feather-tips streaming from it here and there: it had, already, the characteristically small neb, so that I should have guessed, if I had not known for a certainty, that it was a Titmouse embryo. It is difficult to say whether the *Histology* or the *Morphology* of this little cranium is most interesting. Already the cartilaginous skull was well formed (Fig. 2), but, like that of the *Lepidosiren*, and some of the *Tailed Amphibia*, it had very little bony matter belonging to the "endoskeleton": the "investing" or *borrowed* bony tracts were fairly on their way towards typical completion. *One* true internal bone was present, namely, the "basi-occipital" (Figs. 2 and 3, *b. o.*), a spear-head-shaped bony tract, formed round the notochord (*n. c.*) *in* its sheath, and already leavening the "investing mass," or basilar plate of cartilage (*i. v.*) on each side: this mass is the *undivided* counterpart of the blocks that unite at the mid-line to form the bodies of the vertebræ. The foramen for the hypoglossal nerve (9) and its near neighbour the "posterior condyloid foramen," are clearly seen, and also the larger holes farther forwards and outwards for the "vagus" (8). The "notochord" (Figs. 2 and 3, *n. c.*) is well seen; it scarcely reaches the fore-end of the "basi-occipital ossicle" (*b. o.*), and is seen running through the axis of the berry-shaped occipital condyle (*o. c.*). On each side, the occipital cartilage is of great size, and is flanked above by the investing "squamosals"—"squamæ temporis"; below, the

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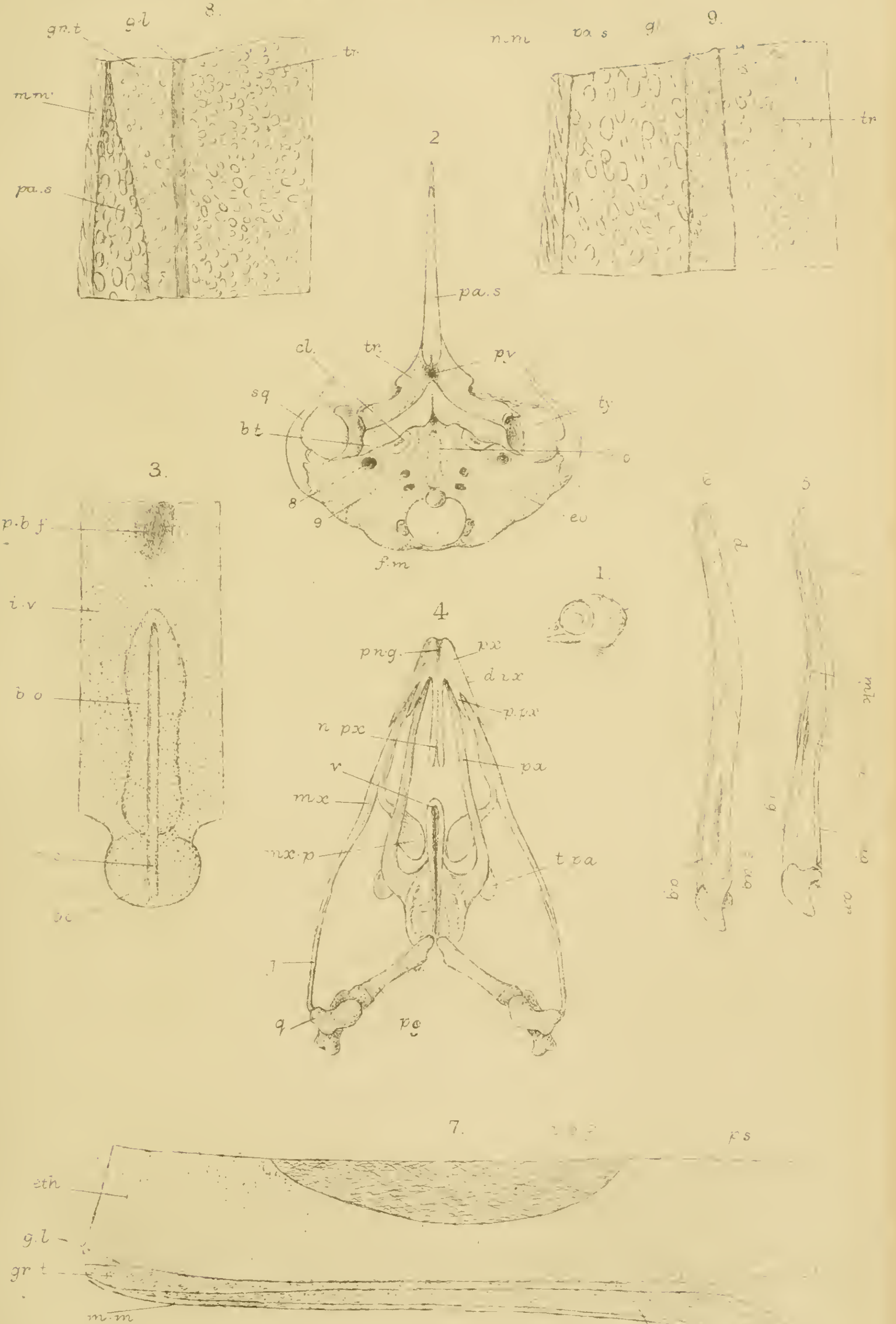
DESCRIPTION OF PLATE II.

Fig. 1.—Head of Tit (*Parus fringillago*?), natural size.
„ 2.—Lower view of skull of ditto, × 7.
„ 3.—Part of same, × 30.
„ 4.—Palate of same, × 7.
„ 5.—Mandible of ditto (inner view), × 7.
„ 6.— „ „ (outer view), × 7.
„ 7.—Part of orbital septum, × 30.
„ 8.—Fore-part of same object, × 150.
„ 9.—Hind-part of ditto, × 150.



W.K.P. del. ad nat. West on Stone

W. West & Co. imp.

Development of Tit's Skull

cartilage walls, in the tympanic cavity (*ty.*). On the side of the notochord, and in front of the vagus nerve passages the rudimentary cochlea are seen (*cl.*) lying imbedded, as in a burrow, in the substance of the basilar plate.

In front of these cavities the cartilage is still of great width, its outer edges bounding the drum-cavity, anteriorly. Much of the cartilage in this "basi-temporal" region is undergirt with a pair of bony rafters, that have become slightly fused together at their fore-end; these are the "basi-temporals" (*b. t.*). When these are removed (Fig. 3), we find the cartilage deficient at the mid-line; this space, which becomes very large, and is a great chink in the nestling, is the "posterior basi-cranial fontanelle" (*p. b. f.*) of Rathke; the "investing mass" meets in front of it, and in front of that commissure we have the space lying between the out-bowed apices of the trabeculæ —the "pituitary space" (*py.*). This space is floored with cartilage in Sharks, Frogs, and Mammals, and that floor is the *seat* of the "turkish saddle" (sella turcica) of the human skull. In the Bird, as in the Osseous Fish, this saddle has no cartilaginous seat, that is formed below, by secondary bony matter, the "parasphenoid." On each side of the "anterior basi-cranial fontanelle," or "pituitary space," we see projections of the much-narrowed cartilaginous mass; these are the tops of the "first præ-oral facial arch," or "trabeculæ cranii"; they have coalesced by their inner margin with the terminal part of the "investing mass"; the *face* is there grafted upon the *skull*, and from this point, the middle of the "basi-sphenoidal" region, the cranial cavity is up-tilted, its fore-part resting on the wall-plate of the great interorbital partition. Rapidly the trabecular bars run inwards, meeting each other at the mid-line, and becoming welded together in a long *commissure* to their very end. Hence, at this part, below the partition-wall of the eye-sockets, the *basi-facial* bar—it is no longer *basi-cranial*—is a rounded mass of cartilage with an ascending keel; it is a plank of cartilage strongly *beaded* below and set edgewise with the "bead" downwards. But this interorbital wall is underbuilt by another kind of material, as though a beam of soft wood should be strengthened by clamping along it a narrow, grooved plate of metal. In so small a creature as this germ of a Titmouse I found a good object for the examination of the tissues that compose this part of the face. So much of the "interorbital" wall as belongs to the trabeculæ is shown in Fig. 7; it was prepared by caustic soda and glycerine, and examined by an inch-focus lens with the lowest eye-piece. The middle of the upper part (*i. o. f.*) was found, like the "perichondrium" which had been removed, wholly of connective-tissue fibres,—long, delicate spindles. Down to near the base was all hyaline cartilage; then a tract of soft, much younger cells,—indifferent tissue; and then a form of tissue intermediate between hyaline cartilage and connective tissue. This was more fully formed, largely-granular, indifferent tissue. This part was underlaid by the palatal skin, or so-called mucous membrane, with its sub-mucous stroma. But very much of this solid tract had become bony; and this bone, the parasphenoid, it is which is seen as bony style, bifurcate behind, in the basal view (Fig. 2, *pa. s*). Various parts of this bar being brought under the quarter-inch lens, with the low eye-piece, I obtained such views as are given in Figs. 8 and 9. At the fore-part of the bony style (Fig. 8) the trabeculæ (*tr.*) and their common crest were already composed of solid, clear, true cartilage; here the intercellular spaces were of nearly the same width as the cells themselves, which were proliferating rapidly. Below the convex edge of this truly cartilaginous plate the tissue was gelatinous—"bioplasm," with interspersed granules (*g. t.*). Below this gelatinous stratum there is a very thick cushion of a very solid granular substance (*gr. l.*) running under the whole of the ethmoidal and sphenoidal regions. This is peculiarly the case in "Ganoid" and "Teleostean" Fishes, in the Dipnoi (*Lepidosiren*), and in all the Amphibia and the Serpents, but not in the Cartilaginous Fishes—Sharks, Rays, and Lampreys, nor in Lizards, Turtles, Crocodiles, and Mammalia. In front (Fig. 8) the granules form a thicker stratum, where the bony layer is thinning-out, than behind (Fig. 9); farther back it forms a large, outspread, thick mat, in shape like the double leaf of a *Banhinia* (see Fowl's Skull, Plate 82, Fig. 2, *b. t.*). In the hinder region (Fig. 9) the gelatinous layer (*g. l.*) is much thinned-out, and dies away in the solid præ-basi-sphenoidal region (Fig. 7).

The granules of the granular layer are only half the size of those in the cartilage, and are closely packed, the appearance of intercellular substance being due to the bioplasmic jelly in the interspaces of rather closely-packed ovoids. I have studied this part of Vertebrate Histology with painstaking care for many years, as it is at first undistinguishable from tracts that soon afterwards become hyaline cartilage. I have examined, with my friend Mr. Chas. Stewart, this substance in the early embryo of the Pig, and when it forms the nidus for the vomer of that animal. In very fine sections, stained with carmine, it has about *half* the transparency of the true, but very young cartilage of the "præ-sphenoid" and "ethmoid" above it. This is in embryos less than an inch and a half in length. But in *piglets* double that length it has been metamorphosed into solid bone, and no part of it, then unossified, shows any proper cartilaginous character. Yet in those earlier embryos it had a very much greater transparency than the surrounding mother-tissue, which was ready to become connective fibre.

This tissue has been called "simple cartilage," and still better, by Professor Huxley, "indifferent tissue," as it seems ready to be by metamorphosis for any duty that may be imposed upon it. These granules soon become osteoblasts in the basi-facial and basi-cranial regions of the bird, and the "parasphenoid" (*pa. s.*) and "basi-temporals" (*b. t.*) are the result of this histological change.

In the Lizard, which is extremely unlike the Bird in many respects, there is a rudimentary azygous "parasphenoid," but in place of the "basi-temporals" we have a delicate ectosteal lamina of bone immediately investing that part of the "basilar plate" from which the notochord has retired. The space between these two *true* "basi-sphenoids" in the Lizard is the "posterior basi-cranial fontanelle."

In the less magnified figure of the Tit's face (Fig. 7) below and behind the end of the gelatinous layer (*g. l.*) the "para-sphenoid" (*pa. s.*) is spreading, bifurcating, and grafting itself upon the basi-sphenoidal cartilage (*b. s.*). This extraneous bone undertakes the *bone-leavening* process for the anterior part of the "basi-sphenoid," and then runs outwards and backwards from the apices of the "trabeculæ," and grows into large temporal wings that wall-in each trumpet-shaped "anterior tympanic recess." The "basi-temporals" (Fig. 2, *b. t.*) undergird the skull where its floor is open—the "posterior fontanelle," and send their beautiful diploë-fibres round the internal carotid arteries and the tongue-shaped rudiment of the "cochlea." They thus form a lower floor beneath the true "occipito-sphenoidal synchondrosis."

It may be a very simple matter to take an adult bird's skull and with a fine saw cut it into sections that shall resemble ordinary vertebræ, but such easy *parlour-work* throws no light upon all that series of changes which laborious morphological work reveals. Some happily-constituted minds, anatomical *lotos eaters*, enjoy a soft and soothing sense of things in this delicious way. Having mentioned other vertebrate types, Fishes, Reptiles, Mammals, &c., and their likeness and unlikeness to the Bird in the modes of their development, I may remark that a sense of the real *Unity* of the whole sub-kingdom grows upon me. It makes me giddy *to look farther down*, and I turn my "deficient sight," for rest, to the exquisite fitness in the results of all those darkly-wise processes, all which "are placed in number, weight, and measure," and which, working together to one common end, in the upshot, produce the most charming of all living creatures.

Another addition to the first præ-oral arch is the vomer (Fig. 4, *v.*); this is composed of two delicate bony styles, formed by ossification of a small cartilaginous nucleus on each side, and afterwards grafted upon the vestibular part of the nasal sac; the two ossicles have already united at the mid-line in front. But the main *secondary* element of the first arch is the "præ-maxilla"—longest of the bird's facial bones; here, in the Tit's (Fig. 4, *px.*), it is less than in any other bird, with the exception of the Swift

(*Cypselus apus*). The moieties are already joined together at the mid-line, and are grooved below by the præ-nasal cartilage (*pn. g.*); each half is three-lined, and these divisions are the upper or nasal, outer or dentary, and lower or palatine pieces (*n. px.*, *d. px.*, *p. px.*).

The second præ-oral arch, or pterygo-palatine (Fig. 4, *pg. pa.*), has, in the young, stout rounded pterygoids (*pg.*), but little uncinate, and articulating with the palatines (*pa.*) by overlapping them; most of the overlapping portion is segmented-off, and ankyloses with the palatine. This latter bone (*pa.*) half embraces the posterior nasal canal behind by an upper and lower lamina; it then elbows out, and at the bend has an ear-shaped cartilage, the transpalatine (*t. pa.*). Towards the mid-line the two laminæ "ethmo- and interpalatine" are pointed forwards, the former, or upper of these spurs ankyloses with the corresponding leg of the vomer (*v.*). The "præ-palatine" bar is long and sinuous, ending in front close to the maxillary. This latter bone (*mx.*) is a slender stalk of bone, with an ear-shaped *leaf* growing out of it at its middle on the inner side. The bony leaf is the "maxillo-palatine" (*mx. p.*); it looks backwards, helps to join the imperfect nasal floor, and is separated from its fellow by the vomer. The narrow cheek-bone is the jugal (*j.*); it ties the fore-face to the "quadration" (*q.*)—the large *anvil-shaped* suspensorium of the mandibular arch—the mimetic *serial* homologue of the Mammalian incus. This bone articulates with *the back of the apex* of the pterygo-palatine arch, just as the "incus" of Man does with the back of the apex of the primary mandible—the head of the "malleus." The rest of Titmouse's mandible (Figs. 5 and 6) show long splints of bone ensheathing a large-headed rod of cartilage, articulo-Meckelian. On the outer side (Fig. 6), the dentary (*d*), the surangular (*s. ag.*), and the angular (*ag.*) are seen, and on the under side (Fig. 5) there is also seen the splenal (*sp.*). I did not in this case get a sight at this stage of the coronoid, which is, however, constant in the Passerines.

I hope soon to lay before the Zoological Society figures of the facial structure of the "*Paridæ*," or Tits. Amongst other related forms, one of these (*Cyclorhis*), from Bahia, Brazils, is as large as the Nuthatch (*Sitta Europæa*), and another sent me by the Consul of Formosa, Mr. Swinhoe, is smaller even than our *Blue* and *Coal* Tits (*Parus ceruleus* and *ater*). The Formosan species is evidently a creature of great energy; like our own native kinds, it has courage enough to "peck an Estridge." This form (*Suthora bulomachus*) carries in its specific name its own peculiar excellency of character, for the word, if I mistake not, is translatable into our English term *bully*. If any of our Fellows have the courage to innoculate themselves with a strong affection for bird-anatomy, the simplest and easiest plan is to prepare the skull of a Titmouse carefully by maceration, using afterwards the smallest modicum of chloride of lime for bleaching; this once accomplished, there would be an impetus given that would not soon die out. I verily believe that such a preparation, well made, is one of the most exquisite natural objects. The face is short, and sheathed in the living bird with strong horn, for this fierce little creature's carpentry; he is a great bark-chipper, doing this for the sake of entomological prey.

The cranium of a Tit is as large (relatively) and as well made as ours, and he is simply one of the most active persons in Europe, such as *Falstaff* would have been but for his redundancy of adipose tissue.

In the further development of the Tit's face the dentary and palatal ends of the præ-maxillaries become stunted, and so also does the fore-end of the maxillary; thus a hinge is formed between these bones on each side, as in Finches, Parrots, and other strong-cheeked birds.

IV.—*On a Simple Form of Mount for Microscope Objectives.* By R. L. Maddox, M.D., Hon. F.R.M.S.

Plate I. (Upper portion).

To others who, like myself, occasionally do a little glass grinding, the following form of mount may prove useful. It was made about two years ago. It consists of the usual outer tube (Fig. 3), but near the shoulder, on the outside, are turned a few threads of a coarse or fine screw, as desired, on which works the fine adjustment collar; a slot, below or in front of the threads, permits a steel pin, which screws into the inner core or tube, to slide up and down according as the collar is rotated, the inner tube being carried up by a couple of turns of steel wire (indicated by a dotted spiral line in the figure), forming a spring, which works in a small space near the neck of the mount, bearing against a shoulder in front and above, against a stop attached to the top of the inner or core tube, which carries, as usual, the back and middle cells. It works quickly, easily; has a considerable range, and no sensible slip; moreover, its construction is not difficult.

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

5. Sphagnum subsecundum Nees von Esenbeck.

Sturm's Deutschl. Fl. Crypt. Fasc. 17 (1820).

Plates III. and IV.

Syn.—Funk, Moos—Taschenherb. p. 4, T. 2 (1821). Nees and Hornsch. Bryol. Germ. I, p. 17, T. 3, fig. 7 (1823). Bridel, Bry. Univ. I, p. 8 (1826). Hübener, Musc. Germ. p. 26 (1833). C. Müller, Syn. I, p. 100 (1849). Schimp. Torf. p. 74, Tab. 22 (1858). Synop. p. 682 (1860). Lindb. Torf. No. 11 (1862). Hartm. Skand. Fl. 9th ed. II, p. 82 (1864). Russow, Torf. p. 71 (1865). Milde, Bry. Siles. p. 392 (1869). *Sph. contortum β subsecundum* Wilson Bry. Brit. p. 22, T. LX (1855).

Dioicous. Tall, slender, crowded in soft tufts of various colours, glaucous-green, yellowish-green, brownish or ochraceous. *Stem solid, brown or blackish, somewhat glossy, with a single thin layer of cortical cells.* Branches flagelliform, 2–3 arcuato-patulous, 1–2 pendent, less elongated, not appressed to stem; the retort cells perforated at the slightly recurved apex. Cauline leaves small, from a broad insertion, broadly ovate, minutely or distinctly auricled, cucullate at apex, finally flattened and very minutely fringed, narrowly bordered, upper hyaline cells fibrose and porose, the lower almost free from fibres. *Ramuline leaves* laxly incumbent or patent, *more or less subsecund, broadly acuminato-elliptic, very concave, with the margin involute in the upper half, narrowly bordered, the point* 3–5 *toothed; hyaline cells flexuose, elongated, very small,* with annular and spiral fibres generally forming a net, *pores very numerous at the back, minute, in two rows along the*

R. Braithwaite del. ad nat.

Sph[illegible] subsecundum

walls of the cells. Chlorophyll cells central, enclosed by the hyaline, strongly compressed. Male plants more slender, in distinct tufts, the amentula short, olive green, bracts broadly ovate, acute, with incurved, bordered margins.

Fruit usually seated in the capitulum, peduncular leaves laxly imbricated, elongate oblong, acuminate, fibrose and with a few pores in the upper part. Spores ferruginous.

Var. *β contortum.*

Sph. contortum Schultz, Sup. Fl. Stargard. p. 64 (1819). Nees and Hornsch. Bryol. Germ. I, p. 15, T. II, fig. 6 (1823). Bridel, Bry. Univ. I, p. 7 (1826). Wilson, Bry. Brit. p. 22, Pl. LX. (1855). Berkl. Handb. p. 308 (1863). *Sph. subsecundum β isophyllum*, Russow, Torfm. p. 73 (1865).

Robust, more or less immersed, ferruginous, blackish-green or olive. Ramuli crowded, terete, usually twisted or circinate, more densely leaved. Ramuline leaves much larger, broadly ovate, more or less densely imbricated and secund, somewhat glossy, the chlorophyll cells less compressed.

Var. *γ turgidum.*

C. Mül. Synop. I, p. 101 (1849). *Sph. contortum Var. γ obesum* Wilson, Bry. Brit. p. 22 (1855).

Stem paler, branches swollen, cuspidate or obtuse. Branch leaves large, very broad, truncate at apex, 5–toothed, with wide cells, stem leaves very large, ovate, fibrose at upper part or sometimes throughout.

Var. *δ auriculatum.*

Lindberg, Torfmos. in Obs. sub. No. 11 (1862). *Sph. auriculatum* Schimper, Torf. p. 77, T. XXIV (1858). Synop. p. 687 (1860).

Glaucous green, whitish below; the stem pale brown, cauline leaves large, lingulate acuminate, subhastate at base, with very large auricles composed of large fibrose utricular cells, perforated at the free apex; the point truncate and erose.

Var. *ε gracile.*

C. Müller, Syn. I, p. 101 (1849).

More slender in all its parts, the lateral branches remote, somewhat curved; those of the coma very dense.

Hab.—Turf-bogs and about springs in the moorlands. *β*, in deep bogs. *γ*, in ditches and at the edges of deep pools. *δ*, Hayward's Heath, Sussex (Mr. Mitten). *ε*, near Berlin, Mecklenburg and the Black Forest. Fr. July.

This most polymorphous of all the Sphagnums varies remarkably in size of leaf, habit and colour. *S. subsecundum* of Nees must be taken as the type of the species, and this is always more slender and attains a height of 12 inches or more; of all the forms it occurs in those localities where there is the least accumulation of moisture, and also where they assume a more subalpine character. The branch leaves are usually somewhat secund and a little curved in the direction to which they are inclined. Great diversity also exists in the quantity of threads present in the cells of the cauline leaves, some being almost free from them, while others have them more or less filled to the base.

Var. β is the commonest form in this country, and differs much in appearance from the typical state. The colour is often a fine ochraceous yellow (*S. contortum β rufescens*, Nees and Hsch. Bry. Germ., Tab. II, Fig. 6*), and not unfrequently the upper part is tinged with vinous-red. The woody layer is well developed, and thus the stem is conspicuous by its brownish-black colour; the stem leaves are not much larger than those of the typical form, but those of the branches are much broader and more or less glossy.

This variety occurs on Hampstead Heath, but it also attains a considerable elevation on the mountains, and is attached to deep bogs, where there is a constant supply of water.

Var. γ has generally more or less of a purple-brown tinge, and in its extreme form is remarkable for its short, thick clavate branches; it is, however, completely connected with the Var. *contortum* by intermediate states. I am indebted to Mr. Curnow for an extensive series of specimens collected near Penzance; some of these are 12 inches long, with the lower part of the stem naked and filiform, and others quite resemble *Sphagnum cymbifolium;* he never finds the fruit in the coma, but low down on the older stem, this is due apparently to the rapid growth of the innovations, and from one of these specimens the figure is taken. The woody layer of the stem is less devoloped, and thus imparts a paler hue, and the leaves both of the stem and branches are very large, with wide cells.

Var. δ is remarkable chiefly for the large auricles to the stem leaves, composed of loose inflated fibrose and porose cells; but this character alone is not sufficient to give it specific rank; and the colour, and presence or absence of fibres in the hyaline cells, are equally valueless for the purpose. It is to be feared that this interesting variety no longer exists at Hayward's Heath, but the figure is taken from an original specimen, for which I am indebted to my friend Mr. Smith of Brighton. *Sphagnum polyporum* Mitten in Herb. Mus. Brit., also from Hayward's Heath, is a form with more obtuse leaves, those of the stem having the cells fibrose and porose even to the base. The Lapland specimens collected by Ångstrom and distributed as *Sph. auriculatum* under No. 713 and 714 in Rabenhorst's Bryotheca, are very different from the English specimens, and belong, 713 to Var. *turgidum*, 714 to Var. *contortum.*

Var. ϵ I have not seen, but it is probably represented by the specimen 208*b* in the Bryotheca, collected at Kremsmünster by Dr. Pötsch. Schliephacke also finds it at Jeziorki in Galicia, and states

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

Sph. subsecundum Var^s.

that it resembles *S. tenellum.* Milde in Bryol. Siles. p. 393 describes another variety *simplicissimum,* "resembling *Hypnum turgescens,* the stem swollen, vermicular, quite simple and without branches," apparently some peculiar or imperfect local form.

EXPLANATION OF PLATES.

Plate III.

Sphagnum subsecundum.

a.—Female plant. *a* ♂.—Male plant.
1.—Part of stem with a single branch fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Fruit with its peduncle. 4.—Peduncular leaf.
5.—Stem leaf. 5 *a a.*—Areolation of apex of same. 5 *a b.*—Ditto of base.
6.—Leaf from middle of a divergent branch. 6 *x.*—Transverse section. 6 *p.*—Point of same. 6 *a a.*—Areolation of apex. 6 *a b.*—Ditto of base. 6 *c.*—Cell from middle × 200.
7.—Intermediate leaves from base of a divergent branch.
9 *x.*—Part of section of stem.

Plate IV.

Sphagnum subsecundum.

β.—Var. contortum. 5.—Stem leaf. 6.—Leaf from a divergent branch.
γ.—Var. turgidum. 6.—Leaf from a divergent branch.
δ.—Var. auriculatum. 5.—Stem leaf. 5 *a b.*—Basal wing of same × 200. 6.—Leaf from a divergent branch.

Note to Sphagnum neglectum.

I have just received a letter from Professor Lindberg, in which that great bryologist informs me that he has identified *Sphagnum neglectum* Ångst. with an original specimen of *Sph. laricinum* Spruce. This celebrated observer detected the plant in 1846, in Terrington Carr, Yorkshire, and since that time its place in the genus or its title to specific rank have never been settled; *Sph. neglectum* therefore drops into a synonym, and the species must stand as *Sph. laricinum* Spruce.

The Figure 6 *x* in my Plate, representing a section of the leaf, is erroneous, for the chlorophyllose cells are elliptic and central, just as in *Sph. subsecundum,* to which indeed *Sph. laricinum* appears to stand in the relation of a subspecies.

Ångstrom described both *Sph. laricinum* and *Sph. neglectum* as species in the Öfver. Vet. Ak. Förhandl. for 1865, but Professor Lindberg points out that the Lapland specimens collected by him and published under No. 712 in Rabenhorst's Bryotheca as *Sph. laricinum,* and also those of Austin's Musci Appalach. do not belong to the species but to *Sph. cuspidatum.*

Fine specimens of *Sph. laricinum* in fruit from the island of Åland and Stockholm accompanied the note.

VI.—*The Histology and Physiology of the Corpus Spongiosum and the Corpus Cavernosum, &c., &c.,* in Man.* By Alex. W. Stein, M.D., Attending Physician to Charity Hospital, Professor of Visceral Anatomy and Physiology in New York College of Dentistry and of Comparative Physiology, New York College of Veterinary Surgeons.

Histology.—The erectile tissue of the organ to which those parts belong may be said to consist of venous cavities or cells which freely communicate with each other, are continuous with the general venous system, and are lined with squamous epithelium. The direct connection of these cavities with the veins is clearly demonstrable in such preparations, in which the cavities are somewhat filled with blood (Fig. 8). In the corpus spongiosum these cells are quite large near the surface or immediately beneath the external fibrous investment, while toward the axis of the urethra they are short and narrow. In the bulb they are larger than at any part anterior to the same.

The interspaces between these cavities are occupied principally by non-striped muscles, which form, as it were, an external muscular tunic for these cavities. In these interspaces may also be recognized connective tissue forming the connecting medium of the muscles, blood-vessels, nerves, and lymphatics of the part.

The sheath of connective tissue, known as the albuginea of the corpus spongiosum, which lies beneath the subcutaneous areolar tissue, and which surrounds the corpus spongiosum in its entire length, is interspersed with innumerable fasciculi of organic muscles which are attached to or originate from the albuginea at innumerable and various points. From this origin they pass in a devious manner in the interspaces between the venous cells, inward toward the deeper portions of the spongy substance, verging suddenly from one direction into another, and by their manifold directions and connections with each other form the intricate trabecular structure or trellis-work of the corpus spongiosum.

Many of these fasciculi run horizontally internal to the albuginia, so that upon transverse section the periphery of the corpus spongiosum appears, at first glance, to be surrounded by a circular layer of organic muscles. Upon more precise examination, however, we find that this muscular layer does not form an absolute ring, but only an approximation to one. On the contrary, not a single fasciculus runs continuously around the spongy substance, but embraces only a small portion of its periphery in a horizontal direction,

* Illustrated by microscopical specimens prepared by my friend, John Busteed, M.D., and myself.

and that at those points where one fasciculus turns from one horizontal plane into another, others arise and continue in an analogous manner the horizontal course left by the first, and thus contributing to the formation of a more or less interrupted muscular ring upon the periphery of the corpus spongiosum.*

In the section of the corpus spongiosum before you can be seen the arteriæ profundæ corporis spongiosi, equidistant from the external fibrous covering and urethral canal in each lateral half of the corpus spongiosum. In this instance they are almost on a level with the urethra, though sometimes we find one artery a little below, the other a little above, the level of the urethra. At the bulbous portion we always find the arteries below the level of the urethra, because of the greater thickness of the parts below the canal at this part.

Fig. 1 (pp. 22 and 23).

Thin Transverse Section of the Corpus Spongiosum Urethra. Taken from the Penis of a man aged twenty-seven, midway between the Glans and Bulbous Portion. Prepared in Alcohol, and treated with Carmine. Magnified 20 Diameters.

The albuginea, or external connective-tissue sheath of the corpus spongiosum, is shown at A A′ A″ A‴. The upper half, which is connected with the corpus cavernosum, is seen at A A′ A″; the lower half at A, A‴, A″. It is abundantly interspersed with (dark) fasciculi of organic muscles, *a, a, a, a*, which do not run completely around the periphery of the corpus spongiosum, but form only fragments of a muscular ring. These fasciculi are continuous with those seen in the substance of the corpus spongiosum at B, B, B, B. At *A**, *A*† (between A and A′), may be seen such fasciculi, extending from the albuginea directly into the substance of the corpus spongiosum. At *b, b, b*, can be seen the cut surfaces of a large number of muscular bundles appearing as round, oval, or irregularly-shaped dark spots. At *c, c, c, c*, may be seen other muscular fibres running in continuity. The latter run in a parallel direction with the section, while the former run in a vertical direction, and are consequently cut by the section. Between these bundles of muscles can be seen, *d, d, d, d*, innumerable empty spaces or meshes of irregular shape. The walls of these spaces have become separated in preparing the specimen, while during life they are generally in immediate contact. These spaces are lined with pavement (Pflaster) epithelium, which can be distinctly seen, in suitable preparations, with a magnifying power of 60 to 200 diameters. Some of these meshes are quite near the mucous membrane, separated from it only by a thin layer of connective tissue, *i. e.* in the upper wall of the urethra in this figure. Transverse section of the corpus spongiosum will show as a rule two large arteries, *e e′*, the arteriæ profundæ corporis spongiosi. Each of these is surrounded by a number of organic longitudinal muscular bundles, *e**, *e**, which appear as round masses, in whose centre the artery passes. In the middle of the section is seen an empty space, C, the canal of the urethra. The epithelial covering of the mucous membrane is indicated at *f, f, f*, and is seen extending into its lacunæ. The existence of a real space in the accompanying figure is the result of the preparation. It does not exist during life. At *g, g′, g″, g*†, *g**, is seen the cut surface of a lacuna which extends deeply into the substance of the corpus spongiosum at *g**, and sends off several branches from its sides *g′, g″, g*†. The largest meshes are at the external portions of the corpus spongiosum; the smaller ones near the urethra. The substance of the corpus spongiosum near the urethra is more transparent than the rest, because it consists of finer fibres than the parts more external.

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* B. Stilling, 'Harnröhren-stricturen,' Fig. 1.

Fig. 2.

Longitudinal Section from the Superficial Parts of the Corpus Cavernosum (Man). Alcohol Preparation, treated with Carmine, Acetic Acid, and Glycerine. Magnified 15 Diameters.

This figure represents the trellis-work of the corpus spongiosum formed by the manifold directions and interlacing of its muscular fibres.

According to Müller, there are two sets of arteries, differing from one another in their size. The first are the rami nutritii, which are distributed upon the walls of the veins and throughout the spongy substance, differing in no respect from the nutritive arteries of other parts; they anastomose freely with each other, and terminate in capillaries. The second set he calls arteriæ helicinæ, from their supposed resemblance to the tendrils of the vine. They are given off from the larger branches as well as the smaller twigs of the arteries. They are especially to be seen in good longitudinal sections, sending out short branches somewhat like a ram's horn, several going off from one point in a stellate form, or as the arms of a chandelier, and terminating in an expanded or knob-like extremity, which project freely into the venous cavities (Fig. 5). They are not entirely naked, but are covered with pavement epithelium. These arteries are more easy of detection in the corpus

FIG. 3.

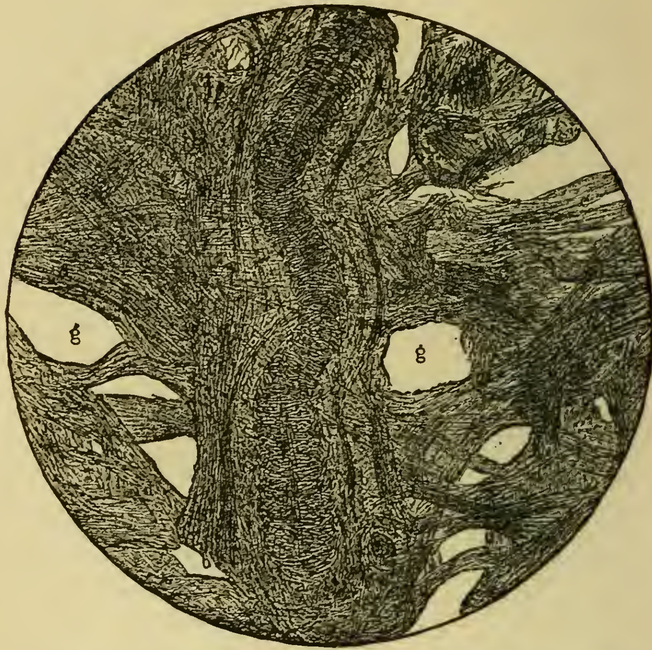
THIN TRANSVERSE SECTION OF THE CORPUS SPONGIOSUM (MAN), PREPARED IN ALCOHOL. TREATED FIRST WITH CARMINE, AND AFTERWARD WITH ACETIC ACID, AND PRESERVED IN GLYCERINE. MAGNIFIED 160 DIAMETERS.

In the centre of the picture at *a*, is seen a transverse section of the arteriæ profundæ corporis spongiosi. At *b*, the inner coat of the artery. At *c*, *c*, its very darkly-stained middle coat, with the nuclei of its individual muscular fibres. At *d*, *d*, its transparent external connective-tissue coat. The artery is surrounded by quite a number of organic muscular bundles, like a tube surrounded by a bundle of staves, *e*, *e*, *e*, *e*, *e*. At many points may be seen a portion of these fasciculi connecting and mixing with the muscular coat of the artery, as, for example, at *e**. The muscular bundles surrounding the arteries are so distinct from the other longitudinal muscular bundles that they must be recognized as a set belonging exclusively to the arteries. The meshes of the spongy substance, *g*, *g*, *g*, *g*, are seen to be lined by pavement epithelium, *h*.

cavernosum than in the corpus spongiosum. In the latter body they are most numerous in the bulbous and posterior portions.

In most of these terminal knobs can be seen a triple fissure, the form of a Y—like the three-horned figure in the crystalline lens of the human eye (Fig. 7). The smaller branches often present but a simple transverse fissure. Stilling is inclined to regard these fissures as the openings or mouths of the arteries, which are closed in the ordinary condition, but in the state of erection are opened and empty the arterial blood into the venous cavities or cells.

Fig. 4.



Thin Longitudinal Section of the Corpus Spongiosum (Man). Prepared in Alcohol, and treated with Carmine. Magnified 160 Diameters.

In the centre of the figure is seen a branch of the arteriæ profundæ corporis spongiosi, *a*, *a*. It is accompanied upon both sides by organic longitudinal bundles of muscles, *b*, *b*, *b*, *b*, which are seen running for some distance in continuity near the artery. Some of these fasciculi pass directly into the walls of the artery, as at *, *, *, and are inserted in the same. Transversely-running muscular fasciculi, *c*, *c*, *c*, as well as those which bend from the longitudinal to the transverse direction, *d*, *d*, *d*, *d*, are very distinct. Also the meshes between these fasciculi, *g*, *g*.

These arteries are accompanied by a special system of longitudinal bundles of muscles, whose fibres become inserted at various points into the middle coat of these vessels. Stilling, I believe, was first to call attention to this anatomical fact. These bundles may be seen in both transverse and longitudinal section (Figs. 3 and 4). The number and size of these bundles vary in different sections, in consequence of the changes which they undergo by the insertion of their fibres into the walls of the arteries on the one hand, and again by the addition of new fibres to these bundles from other planes.

The bundles accompanying the large arteries are comparatively numerous, while those accompanying the smaller ones are few, and not so distinct.

Fig. 5.

Thin Longitudinal Section from the Posterior Portion of the Corpus Cavernosum (Man). Magnified 15 Diameters. The Organ was injected through the Arteria Dorsalis with Beale's Blue Fluid, and then hardened in Alcohol. The Thin Section was afterward treated with Carmine, Acetic Acid, and Glycerine.

In the centre of the figure is seen the artery, *a*, *a*, running from below upward. The dark injection in its cavity is well shown. From the right and left of this artery are given off the arteriæ helicinæ, *b*, *b*, *b*, *b*. They assume different forms. Their terminal knobs lie free in the meshes, *b** *b**, and are covered with pavement epithelium, which by higher magnifying powers are very distinctly brought into view.

The knobby terminations of these arteries possess muscular fibres closely packed in concentric circles, which probably act as sphincters to the mouths of these vessels.

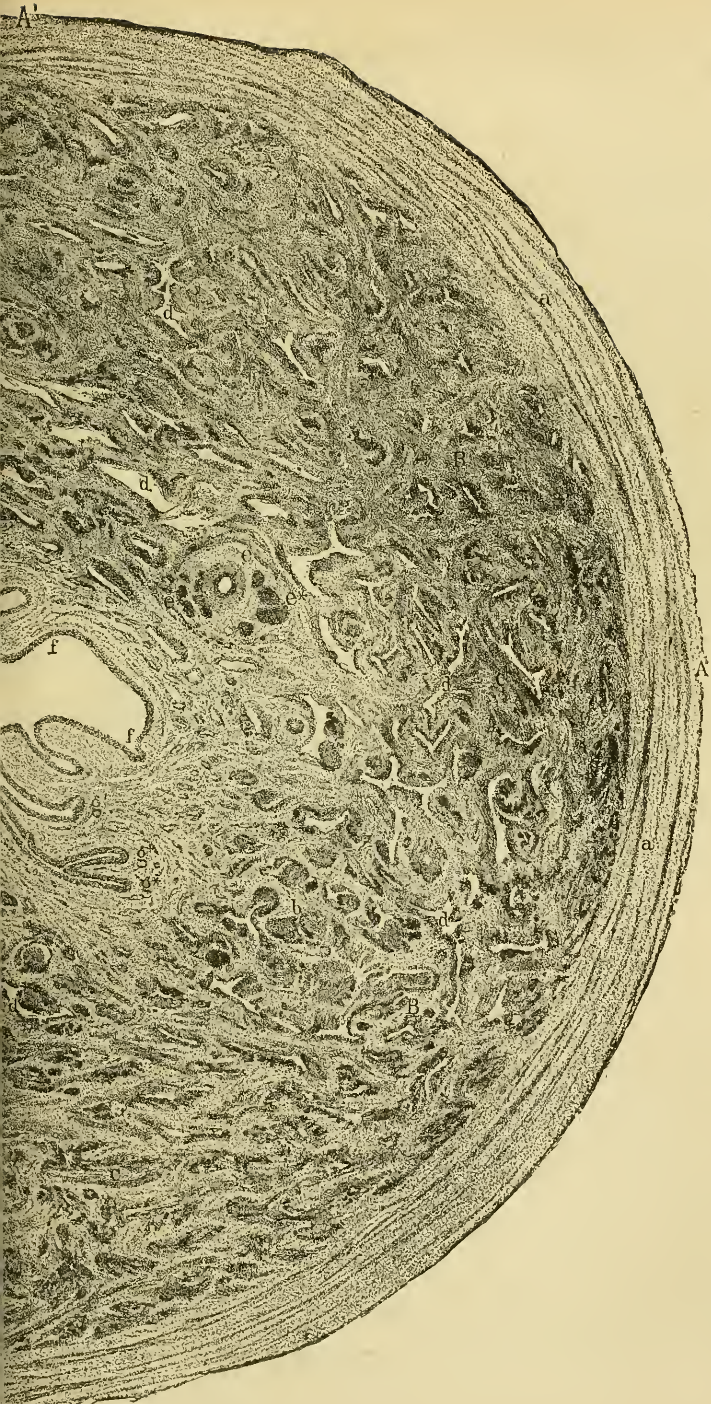
There is one other anatomical fact mentioned by Stilling, to which I will briefly refer, on account of its pathological bearing.* It is the intimate relationship of the epithelial elements of the urethra with sub-mucous tissues of the corpus spongiosum.

That certain epithelial cells possess prolongations by which they are in intimate connection with the underlying tissues, Stilling

* This I will reserve for another occasion when I shall have completed some observations now engaged in.

Fig. 1.

Thin Transverse Section of the Cor



…GIOSUM URETHRA. (See page 17.)

Fig. 6.

Part of a Fine Longitudinal Section from the Posterior Portion of the Corpus Cavernosum (Man). Prepared in Alcohol, and treated with Carmine. Magnified 160 Diameters.

Fig. 7.

The Knob-like End of an Arteria Helicinæ from a Fine Longitudinal Section of the Corpus Cavernosum (Man). Prepared in Alcohol, and treated with Carmine.

The terminal end *a* appears to lie free in one of the meshes. The mouth of the vessel is seen at *, which appears as a Y-shaped fissure.

In this section the artery *a*, *a*, is seen running along the entire length of the section, sending off a number of branches which terminate in thickened ends, *c*, *c*, *c*. The red knob-like ends of this artery are quite conspicuous in consequence of the imbibition of carmine. To the left of the artery is seen the course of a longitudinal muscular bundle, *d*, which is inserted in the upper third of the artery. Transverse and oblique muscular bundles are seen at *e*, *e*.

claims to have indicated as early as 1857, since which time his statements have become confirmed by the researches of others, who have represented the passage of nerve fibres to the epithelial layers of certain tissues, as for example in the cornea.* He affirms that the epithelial cells of the urethra are in manifold connections with the muscular fibres, nerves, and other tissues of the corpus spongiosum. "The most superficial cells," he says, "terminate in a pedicle or filiform prolongation, which often remain connected with the mucous membrane after the body of the cell has separated from its connections. A good longitudinal section will often show three or four of such cells, one above the other, hanging like pears by their stalks, the cells directed toward the bladder, the stalk toward the external meatus. To state the precise manner in which these connections occur is reserved for future research. Especially will it

* Frey, 'Handbuch der Histology und Histochemie,' 3. Aufl., Leipzig, 1870, p. 608.

have to be ascertained whether certain epithelial cells of the urethral mucous membrane are real terminations of nerves, and others real terminations of muscular fibres. If we consider the extreme sensitiveness of the epithelial layer of the healthy cornea to the contact of ever so fine a foreign body; if we consider that the most superficial contact of the point of a needle with the cornea produces the most severe pain and reflex movements; while, after destruction of the most superficial layer at one point, the needle produces at that point only an inconsiderable amount of pain—facts which every physician has opportunity to observe in removing foreign bodies (dust, iron filings, particles of coal, &c.) from the cornea—the conclusion is almost irresistible that the epithelial covering of the cornea consists of cells which are to be regarded as nerve terminations.

Physiology.—The cause and mechanism of erection may be said to depend upon two phenomena occurring simultaneously:—

1. Upon an increased influx of blood through the arteries which empty by the arterial helicinæ into the venous cavities.

2. Upon mechanical pressure affecting the veins which convey the blood from the penis, whereby a retardation of the venous circulation is induced.

In the passive condition of the penis the same quantity of blood flows to and from the organ, but during erection the entire arterial system becomes distended, especially those known as the arteriæ helicinæ become actively dilated, and empty themselves into the venous cavities. The dilatation of these vessels is supposed to be effected through the agency of the special system of longitudinal bundles of muscles which accompany, and whose fibres are inserted into the walls of these arteries.

The muscles chiefly concerned in arresting the efflux of blood, or at least preventing it from being as great as the influx, are the acceleratores urinæ and erectores penis.

The contraction of the acceleratores urinæ muscles impedes the return of blood through the vena corporis spongiosi, by pressure upon the bulbous portion of the corpus spongiosum.

The erectores penis act as erectors, by compressing the crura and vena dorsalis. They embrace the root of the penis and compress it. The pressure upon the vena dorsalis impedes the return of blood by this vessel, and the pressure upon the crura produces the same effect upon the veins of the corpus cavernosum.

The transversus perinei probably assists the posterior fasciculi of the acceleratores urinæ in producing turgescence of the corpus spongiosum, by virtue of its insertion into the fibrous tunic of the bulb.

These muscles are supplied with nerves by the pudic branch of the sacral plexus, and it is an interesting fact, capable of demonstration upon the lower animals, that after division of this nerve the penis is incapable of erection.

FIG. 8.

TRANSVERSE SECTION OF THE CORPUS CAVERNOSUM (MAN). PREPARED IN ALCOHOL. TREATED AT FIRST WITH CARMINE, AND AFTERWARD WITH ACETIC ACID AND GLYCERINE, AND PRESERVED IN FARRANT'S LIQUOR. MAGNIFIED 160 DIAMETERS.

The object of this figure is to represent the communication of the veins with the meshes of the corpus cavernosum (and spongiosum). The dark portion of the picture indicates the corpus cavernosum, *a*, *a*, the lighter portion the albuginea of the corpus cavernosum, *b*, *b*, *b*. The meshes are all filled with blood, in consequence of which the structure of this part appears considerably more distinct than when these cavities are empty. Internal to the albuginea of the corpus cavernosum are seen a large number of veins, *c*, *c*, *c*, forming a plexus. The direct connection of some of these veins with the meshes of the corpus cavernosum can be seen at *, *.

In the passive condition the natural tonicity of the muscular trellis-work of the penis is sufficient to maintain the walls of the venous cells in apposition; and they, together with the sphinctoric action of the circular fibres around the mouths of the arterial helicinæ, prevent the flow of blood into these cells. But, when the parts are stimulated to erection, the muscular bands are obliged to yield to the distending force of the blood (according to Müller, it appears that the blood accumulating in the penis during erection is subjected to a pressure equal to that of a column of water six feet in height). The meshes become filled, and remain so until the stimulus to erec-

Fig. 8.

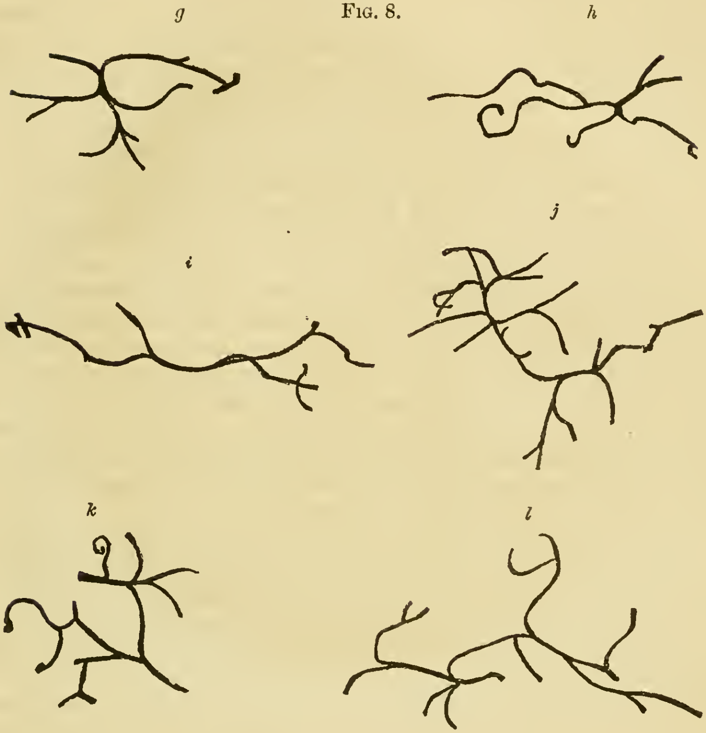


Fig. 8—*g*, *h*, *i*, *j*, *k*, *l*—represents the various shapes of the arteriæ helicinæ, which are frequently found in injected preparations of the corpus cavernosum. Magnified 15 diameters.

The description of the plates is taken from 'Die Rationelle Behandlung der Harnröhren-stricturen,' by Dr. B. Stilling.

tion subsides, when the acceleratores urinæ and erectores penis muscles relax, and remove the pressure from the veins. The trabeculæ (non-striped muscles) of the penis now contract and expel the blood from the dilated venous cells.

The contractile force of the corpus spongiosum is well displayed in persons who, for the first time, submit to the introduction of a catheter or sound into the urethra; the entrance of the instrument is often sensibly opposed, and during withdrawal it is forcibly expelled.

This phenomenon cannot be attributed entirely to the action of the acceleratores urinæ and compressor urethræ muscles, for it is manifest even within an inch of the external meatus.

The action of the muscular trellis-work of the corpus spongiosum, as it affects micturition and the ejaculation of the spermatic fluid, is of interest. During micturition the entire corpus spongiosum, as well as the urethra, becomes somewhat stretched. At the end of micturition the tonicity of the trellis-work is sufficient to coaptate the walls of the urethra, and to expel the last drops of urine which may remain in the anterior portion of the canal. This pressure is strongest at those parts where the urethra and its surrounding meshes are narrow; and weakest where these cavities are wide, and consequently more dilatable, as at the bulb.

As the external fibrous investment of the corpus cavernosum consists of broad, thick bands of connective tissue, it becomes firmer, harder, and more unyielding during erection, than the corpus spongiosum. The latter being smaller in circumference, with its external fibrous investment interspersed with non-striped muscular bands, it remains even during the most complete erection, and during the ejaculation of the sperm, sufficiently dilatable or distensible to permit the seminal fluid to flow through the canal. Should the corpus spongiosum attain that degree of hardness which the corpus cavernosum acquires, the canal would not yield to the pressure of the sperm, and ejaculation would be obstructed.

The pressure which the corpus spongiosum is capable of exerting upon the urethra becomes quite considerable, when to the natural tonicity of its muscular trellis-work above mentioned there is added a special stimulus to contraction, which obtains when the penis is brought to a state of erection. Each new pulsation increases the quantity of blood in the meshes or venous cavities, and this—as it were—supplementary body within these cavities stimulates the muscular bands to increased activity. The pressure now brought to bear by these bands very materially assists the acceleratores urinæ and compressor urethræ muscles in communicating an expulsive impetus to the outflowing stream of urine or seminal fluid.

The greater the special force exerted by the contraction of the acceleratores urinæ during ejaculation, the more is the erection of the penis augmented, and the concentrical pressure of the muscular trellis-work upon the spermatic fluid increased.

For this reason micturition is so difficult during complete erection, and *immediately after* ejaculation.

Thus I have endeavoured to lay before you, in a somewhat condensed form, a synopsis of some views at present entertained upon the histology and physiology of the penis. This subject is but imperfectly understood; its literature is meagre, and much of that which has been written is vague and unsatisfactory, while many points have remained entirely unexplored. Yet there are few subjects which present a more fruitful field for histological investigation than the one to which I have now had the honour of calling your attention.—*Read before the New York Dermatological Society.*

VII.—*Apertures of Object-glasses.* By F. H. Wenham.

On the 14th day of November, 1872, the dry and immersed apertures of Mr. Tolles' $\frac{1}{10}$th were tested in presence of the undersigned.

The angle in air (taken at the best adjustment for a Podura scale) measured 145°.

With the front in water, the angle became reduced to 91°. And lastly in balsam the result was 79°.

Chas. Brooke, F.R.S., V.P.R.M.S.
H. Lawson, M.D., F.R.M.S.
W. J. Gray, M.D., F.R.M.S.
S. J. M'Intire, Esq., F.R.M.S.

For measuring the immersed apertures the sector was fixed vertically. The object-glass was set with its surface level to the centre of the arc. A small circular tank was firmly clamped below, into which the end of the object-glass passed. A wax candle was placed exactly in a line on the floor three feet beneath.

After the air aperture had been taken, the tank was filled with water so as to cover the end of the object-glass. At each extreme of the aperture a pencil line was drawn along the straight edge of the bar that carried the microscope body, leaving a permanent record of the trial. The water was then turned out and replaced by fluid balsam, and the diminished aperture again marked with pencil, the object-glass having been radiated in the fluid as before.

A trial in the presence of such competent judges might well end the question, and establish the infallible law by which such a result could be foretold.

From the result of the water trial I asserted that there would be a further reduction of 15° in balsam or an aperture of 76°, but 79° was indicated. These extra *three* degrees beyond my theoretical limit may be accounted for in this way. I had assumed the index of refraction for hard balsam. That used was very fluid, and therefore contained turpentine, which of course diminished the refractive power.

Dr. Josiah Curtis has witnessed an experiment conducted by Mr. Tolles, and speaking in a somewhat jubilant tone of his assumed triumph, remarks, "Equally gratified probably will Mr. Wenham be when he shall see for himself that an angle of more than 82° can be attained through balsam." *

I may say that I certainly was surprised at seeing a result so strictly in accordance with my argument; for after all this

* See 'M. M. J.,' Nov. 1872, p. 243.

ceremony of sending a glass across the Atlantic for trial, I expected, *in this particular instance,* to find some light beyond the theoretical angle, and hoped to investigate and explain the cause in order to furnish some incidental proof of fallacy in the measurement of extreme apertures; but the limits of the immersed angles in this case were more decided than in the air, and there was nothing beyond the mere record left for me to do. Dr. Curtis has given his faith to the trial without proof that he has paid any attention to the principles of refraction involved in the experiment, and as Mr. Tolles has shown that he will not be led by any theory, and therefore is quite unprejudiced thereby, I can only infer that he has conducted the experiment in some way without regard to the laws that should guide the rays through the media, and that a false indication has resulted.

It needs but a very limited knowledge of optical theory to demonstrate that the utmost angle of possible transmission or conversely of emergence from the first surface of ordinary crown glass with a refractive index of 1·531 does not exceed 40° 43′, and therefore with still lighter crown, the angle behind the first surface cannot get beyond 82°; and supposing the other lenses to be of such a form as to bring to a posterior conjugate focus the rays of even such an improbable angle, this cannot be increased either with water or balsam immersion, without destroying that focus and giving a negative result with *no* image in the eye-piece; therefore this 82° must continue straight in the balsam medium if of a similar refraction to the glass.

I demonstrated this seventeen years ago, and till now no one has disputed the position and at the same time tried the plans for obtaining full aperture both of object-glass and illuminator, that are *now* revived as new facts. I make the following extract from the 'Quart. Journal of Mic. Science,' No. XII., July 1855, by which it will be seen that having succeeded with the hemispherical lenses, I felt somewhat diffident about encysting objects in Mr. Tolles' "Pilluloe."

"The sharpness and beauty with which *some* test-objects are displayed under the diminished aperture consequent upon balsam mounting, is, on first consideration, rather surprising, and tends to show analogically the very great increase of distinctness that would be obtained if the object could be seen in the same medium, with the *full aperture* of the object-glass. Having been rather curious to know if objects in balsam could be observed under such an advantage, I have tried a few experiments which were successful in their results.

"I first took a small *hemispherical lens* of about $\frac{1}{50}$th of an inch radius, and cemented it over a selected specimen of one of the *Diatomaceæ* (N. Sigma) with Canada balsam, in the manner represented by the annexed diagram . . . . . It will be seen from the position of the object, that each ray of light passing from that point through the surface of the hemisphere will be transmitted in straight lines in a radial direction without undergoing any refraction, the consequence of which is that the full and undiminished aperture of the object-glass is made to bear upon the object.

"If an object is already covered with thin glass it may be surmounted with a lens, so far short of a hemisphere as the thickness of the cover, which of course amounts to the same in effect as if the lens were hemispherical. I have a specimen of *P. formosum* mounted in this manner, by which the markings are remarkably well displayed. A more simple method of obtaining a similar result, is by the following course of proceeding:—Spread some of the desired forms of *Diatomaceæ* upon a slip of glass while in a moist state, and when dry, scatter a number of small fragments of hard Canada balsam upon the same surface. Apply heat *very gradually* and these will run into the form of a spherule; they will next slowly sink into a shape approaching to that of a hemisphere. Before the figure is quite completed, place the slide under the microscope, and ascertain if any one of the nodules of balsam exactly covers a fair specimen; if not, the trial must be repeated with a fresh slide. Having found an object properly situated under the particle of balsam, the next step is to bring the latter down to the form of a hemisphere, by the further aid of heat very cautiously applied. The nodule of balsam having been too spherical in the first instance, will now gradually sink, and must be repeatedly tested under the microscope, till the perfect hemisphere is obtained without any refraction being produced on the rays from the object in the centre. The criteria for knowing this are:—first, the object under the balsam must be *in the same plane of focus* as similar dry objects outside; secondly, the balsam object must not appear *more magnified* than its uncovered fellows; and thirdly, the balsam-covered object should not require a different adjustment from the dry ones on the same slide. The existence of these combined conditions indicates a perfect hemisphere.

"When an object is seen under these circumstances it at once shows the great increase of distinctness that is to be obtained in the structure of the more difficult diatomaceous tests; when they are thus viewed in Canada balsam with the full aperture of the object-glass, markings which in the neighbouring dry objects of the same character are scarcely discernible, are sharply and distinctly visible under the balsam hemisphere with the same illumination.

"The luminosity of the field of view around the balsam object is many shades darker than in the uncovered portion of the slide, which appearance is caused by the diminished angle or cone of rays of the illuminating pencil. From this fact it is evident that the theoretical perfection of mounting objects in this manner would be to enclose them exactly in the *centre* of a minute sphere of balsam. In this case the pencil of rays, both from the achromatic condenser and object-glass, would pass directly to the object without refraction or diminution ; but I must confess that I have not been successful in effecting this; it is very easy to form a sphere of balsam at the end of a needle point of any degree of minuteness, but very difficult to coax an object into the centre of such a spherule."

I have been engaged in countless comparisons of object-glasses of different makers brought together by their respective owners, and each held his private opinion of the result. I have avoided any public mention of comparative merit as an assumed dictatorship both odious and uncalled for; and in this object-glass so confidingly forwarded by Mr. Tolles, I would not sanction a trial against glasses of any English manufacturer, as it was sent for a different purpose. But as Mr. Tolles has stated in allusion to my $\frac{1}{8}$th described in my paper "On the Construction of Object-glasses" (published in the commencing numbers of this Journal) as being composed of curves that he would not use on any account, and that it would not give a large angle in balsam (beyond that assigned by theory) because it had "no collecting power"!! I did therefore venture to make the comparison with this, of curves now obsolete, made twenty-two years ago. It proved far superior to Mr. Tolles' (made three years ago) on every object on which it was tested. I have sent my glass to Dr. Lawson in order that he may also make the comparison. I value it as a matter of history, as the first successful glass with a triple back and single front, and showing Podura markings of a light ruby tint on a green ground, then a novel appearance, but now recognized as a criterion of perfect correction.

I do not wish this to be taken invidiously, as Mr. Tolles may have made a great advance since then, and may profit by the hint. His glass appeared to have a compound front. The single front may be used with advantage in all the powers from the $\frac{1}{2}$ upwards.

I am now weary of urging these reiterated demonstrations both theoretical and practical of the angles of immersed objectives, and must drop the question, believing that my views are accepted by a discriminating majority, therefore no reply must be expected from me; Mr. Tolles' sect may set me down as an infidel having faith in what, to them, is an unknown creed, or as a serpent deaf to the voice of the charmer, but even this shall not provoke an answer. As I before stated, I had no wish to discuss the question further with Mr. Tolles, and the necessity of testing the aperture of his glass sent for the purpose is an apology for my reappearance.

F. H. Wenham.

PROGRESS OF MICROSCOPICAL SCIENCE.

Microscopic Life at the Bed of the Mediterranean.—We would merely allude to this subject for the purpose of referring our readers to the splendid paper of researches on ocean currents generally, and vitality at different depths in the Mediterranean and other seas, by Dr. B. W. Carpenter, F.R.S., which occupies a whole number of the Proceedings of the Royal Society, extending to about 110 pages. It is of special importance, from the justification it affords to the hypotheses of the late Edward Forbes, F.R.S., who made most of his researches in the Mediterranean. Dr. Carpenter states, in reference to this subject,—" I am disposed to believe, therefore, that in the Mediterranean basin, the existence of animal life in any abundance at a depth greater than 200 fathoms will be found quite exceptional; and that, without pronouncing its depths to be absolutely *azoic*, we may safely assert them to present a most striking contrast, in respect of animal life, to those marine Paradises* which we continually met with in the Eastern and Northern Atlantic at depths between 500 and 1200 fathoms. And I have the satisfaction of finding that my conclusion on this point is entirely borne out by the results of the dredgings carried on in the Adriatic by Dr. Oscar Schmidt; who found the like barrenness at depths below 150 fathoms, except as regards *Foraminifera*, *Bathybius*, and *Coccoliths*. After a most careful microscopic examination of the mud obtained from the depths of the Mediterranean, I feel justified in saying that even of these lowest organisms scarcely any traces are to be found.—Thus it appears that Edward Forbes was quite justified in the conclusion he drew *as regards the particular locality he had investigated;* and that his only mistake lay in supposing that the same conditions would prevail in the open ocean "

A Mite in the Ear of an Ox.—At a meeting of the 'Academy of Natural Sciences of Philadelphia,' whose report we have only now received, Professor Leidy said he had received a letter from Dr. Charles S. Turnbull, in which he stated that while studying the anatomy of the ear he had discovered in several heads of steers, at the bottom of the external auditory meatus, a number of small living parasites. They were found attached to the surface of the membrana tympani. Specimens of the parasite preserved in glycerine, and a petrosal bone with the membrana tympani, to which several of the parasites were clinging, were also sent for examination. These prove to be a mite or acarus, apparently of the genus *Gamasus*. The body is ovoid, translucent white, about $\frac{3}{5}$ of a line long, and $\frac{2}{5}$ of a line wide. The limbs, jaws, and their appendages, are brown and bristled. The body is smooth or devoid of bristles. The limbs are from $\frac{2}{5}$ to $\frac{1}{2}$ a line long. The feet are terminated by a five-lobed disk and a pair of claws. The palpi are six-jointed. The mandibles end in pincers or chelæ, resembling lobster claws. The movable joint of the chelæ has two teeth at the end. The opposed extremity of the fixed joint of the

* This word is used in the sense familiar to the Greek scholar.

chelæ is narrow, and ends in a hook. Whether this mite is a true parasite of the ear of the living ox, or whether it obtained access to the position in which it was found after the death of the ox in the slaughter-house, has not yet been determined. Dr. Turnbull observed it only in the position indicated.

Tea and Cotton Blights.—'Grevillea' for December contains an interesting paper on the above, by the editor, Mr. M. C. Cooke, M.A. He says that the tea-planters of Cachar have been complaining of late that the leaves of the tea-plants have become blighted, so as to interfere seriously with the production of tea. Two or three of the diseased leaves have been sent us for examination. They were not in good condition for the purpose, but on one we detected some punctures of an insect, and on two of the others a parasitic fungus. The leaves are blistered, deformed, and stunted; the fungus appearing on both surfaces like minute black points. The following is a description drawn up from the dry specimens:—*Hendersonia theæcola.* sp. nov.—Perithecia globose, black, prominent, pierced at the apex, scattered over both surfaces, or sub-gregarious; spores cylindrical, rounded at the ends, triseptate, pale brown, on long hyaline pedicels (·0004–·0005 in.), 01–·0125 mm. long, without the pedicels. On leaves of *Thea.* Cachar, India. The ultimate cells have sometimes a more hyaline appearance, but we could detect no terminal cilia, otherwise it reminds us of such species of *Pestalozzia* as *P. Guepini*, which occurs on *Camellia* leaves. The only remedy we can suggest is to pick off the diseased leaves and burn them. What portion of the destruction is also due to the insect we have no material for determination, but both are probably culpable. From Dharwar we have also received samples of "Black-blight" on naturalized American cotton. The cotton presents but little external indication of disease so long as the seeds remain entire, but, on crushing the seed, the cotton becomes covered with a sooty powder, which at first we were disposed to regard as the spores of a species of *Ustilago*, which entirely fills the seed. After a closer examination, however, we became satisfied that the spores are concatenate, being produced in chains, or jointed threads, in the interior of the seed, and afterwards break up into subglobose spores. This is rather an anomalous habitat for a *Torula*, but such, nevertheless, we are disposed to regard it, and append its description. *Torula incarcerata.* sp. nov.—Produced within the seeds of *Gossypium.* Threads simple, or slightly branched, breaking up into minute, subglobose, fuliginous spores. Within cotton seed. Dharwar, India. It is rather to be presumed that the *Torula* makes its appearance after the commencement of decay in the seed, stimulated by moisture, than that it should be the cause of disease in the plant. The species of *Torula* with which we are acquainted are produced upon decaying substances, and we have no experience of any one causing disease in living plants. Had this proved to have been a species of *Ustilago*, the case would have been different, but we believe that, notwithstanding its habitat, we are justified in placing it with *Torula*.

NOTES AND MEMORANDA.

A Microscopical Life-slide has been described in an American Journal,* which will not appear novel to some of our readers, though it is described as new. It is constructed to retain the greatest quantity of material under the smallest cover glass, and is designed to be used with the highest powers of the microscope for studying the bacteria, vibriones, and other very low forms of life. The slide consists of a central polished cavity, about which is a similar polished bevel; and from the bevel outward extends a small cut, the object of which is to afford an abundance of fresh air to the living things within, as well as to relieve the pressure which shortly would become so great, from the evaporation of the liquid within, as to cause the destruction of the cover glass. No special dimensions are stated for the central cavity. The bevel is usually $\frac{1}{8}$ inch in diameter; the small canal is cut through the inner edge of the bevel or annular space, outward, for the purpose named above. It is found upon enclosing the animalculæ, &c., that they will invariably seek the edge of the pool in which they are confined, and the bevelled edge permits the observer to take advantage of this disposition; for when beneath it, the objects are within range of the high-power glasses. Another very important feature in the device is the fact that a preparation may be kept within it for days or weeks together, without losing vitality, owing to the simple arrangement for supplying fresh air. "We," says the writer, "have repeatedly had the opportunity of witnessing the use of this slide, and are convinced that nothing of the kind has yet been devised which can equal it in excellence either for observing or generating the lower forms of life."

A Review of the "New Conspectus of the Families and Genera of Diatomaceæ," by Professor H. L. Smith, has been published in 'Grevillea,' by Mr. F. Kitton, in which he says that the Professor "has applied the pruning knife most unsparingly, doubtless to the great disgust of the 'species mongers.' Some of the genera might, he thinks, have been retained with advantage; for example, the Campylodisci, which have been relegated to the Surirellæ. This genus has two unvarying characteristics, *viz.* the circular form of the valves, and the median space of the two valves of the frustule are always at right angles to each other; consequently the valve must be truly circular. Professor W. Smith, the author of the 'Synopsis,' has erred in placing *Campylodiscus spiralis* in that genus. Kützing was right in making it a species of Surirella (*S. spiralis*). The union of the genera Triceratium and Amphitetras with Biddulphia we think will not be generally accepted; to do so necessitates the enlargement of the generic characters of the last to too great an extent. The number of species will also be inconveniently large. The genus Triceratium might, we think, be united to Amphitetras without

* 'Journal of the Franklin Institute.'

much alteration of the generic character. The author is, no doubt, right in abolishing the conditions of stipitate, tubular, &c., as being of no value. He remarks, 'The conditions *frondose, stipitate, filamentous, tubular*, &c., I have not considered sufficient to warrant the formation of new genera. A long study of living forms has convinced me that these characters are fleeting—not to be relied on.'"

A Latin Work on the Desmidiæ, which contains 100 pages and five plates, has been issued by the Royal Society of Sciences, at Upsala. It is under the authorship of *M.* P. M. Lundel, and is said to be a very good book.

CORRESPONDENCE.

The Mistake about Objectives used Binocularly.

To the Editor of the 'Monthly Microscopical Journal.'

Clifton, Bristol, *Dec.* 11, 1872.

Sir, — Immediately I observed Mr. Wenham's letter in your December number, in reply to mine of the 19th Oct., I wrote to the gentleman who furnished me with the information, and he replied to this effect — that he decidedly objected to his name being mentioned, and added, "*you* misunderstood what *I* said, and Mr. Wenham has misunderstood what *you* wrote."

Now I must repeat that what I stated in my letter to the 'Monthly Microscopical Journal' was precisely what he did say in our conversation on the subject, and also that the statement relative to the matter was made during a casual conversation at the meeting of the Society, several members standing near, any one of whom might have heard what was said, and, that it was made without the slightest reservation as to its publicity, so that at the time I neither doubted its correctness, nor imagined anything to the contrary of its being commonly known.

Although I do not enjoy the pleasure of Mr. Wenham's acquaintance personally, I very much regret that he should have been annoyed by the publication of what appears to him to be an absolute untruth.

I am, Sir, yours truly,

Samuel Smith,
Surgeon.

A Microscopical Puff.

To the Editor of the 'Monthly Microscopical Journal.'

King's College, *Dec.* 21, 1872.

Sir,—On the 18th instant a paragraph, headed "Royal Microscopical Society," and purporting to give an account of the "Annual Soirée," held on the 11th, appeared in 'The Times,' and I believe in some other papers.

No "Annual Soirée" was held on that occasion, but a private Scientific Evening, as reported in this number of the Journal.

The writer of the paragraph in question evidently intended to convey an idea that an objective by Hartnack was of such unusual merit as to deserve to be singled out for honourable mention beyond all others.

In case anyone should suppose that this paragraph is sanctioned by the Council of the Society, I beg you will allow me to state that they entirely repudiate it, and have the strongest objection to the abuse of the Society's name for any purpose of trade puffing.

There is no member of the Council who would refuse to recognize the merit of Mr. Hartnack's work; but justice to other eminent makers would entirely preclude the sort of notice the objectionable paragraph contains.

Dr. Carpenter's object can be shown distinctly with a good $\frac{1}{4}$th or $\frac{1}{5}$th, and specimens of Rhomboides have frequently been displayed with $\frac{1}{2}$ inch.

I remain, Sir, your obedient servant,

HENRY J. SLACK,
Sec. R. M. S.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *Dec.* 4, 1872.

W. Kitchen Parker, F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

The Secretary, Mr. Hogg, announced that some beautiful photographs of the 19th band of Nobert's lines had been received from Dr. Woodward, of the U. S. Army. Dr. W. had also forwarded a photograph of a diatom, which he called *Frustulia Saxonica.* He (Mr. Hogg) thought it very much like *Navicula rhomboides.* He wished some of the Fellows would carefully examine these objects and name them more definitely and correctly. He might mention, with reference to *Rhomboides*, he was able to resolve it very satisfactorily with an old $\frac{1}{4}$ and Wenham's new illuminator. The $\frac{1}{4}$ objective was one made by Andrew Ross twenty-five years ago.

Mr. Slack called attention to the fact that in the diatom photographs there were spurious lines shown outside the picture of the object itself.

Mr. Slack then said, the Fellows would perhaps remember the observations made by Mr. Stewart at the last meeting of the Society, which tended to show that the apparent hexagons in certain diatoms were real. Apart from the special optical considerations advanced by Mr. Stewart, the question very much turned upon the appearance displayed along lines of fracture. He had sought out a good many fractured diatoms, and in such forms as *Coscinodiscus oculus Iridis*, and *Triceratium favus*, the lines of fracture showed the lines bounding the hexagons to be the strongest parts, and led to the belief the hexagonal markings on such diatoms were real; but in the pleurosigmas, and in many others, including several circular ones, the lines of fracture passed between the bead rows, as long since shown by Mr. Wenham. The structure of the hexagonal diatoms did not, however, vary in principle from the others, as there was evidence that the whole floor of the hexagons was made up of minute spherules. He hoped to publish some specimens of fracture where the hexagonal markings were real, so that the Fellows might institute a comparison between them and those which corresponded with simple beaded structures.

Mr. Gayer then read a paper "On a New Form of Micro-spectroscope."

Mr. Wenham thought the arrangement described by Mr. Gayer remedied one very great objection to the use of the ordinary form of micro-spectroscope. In the usual form the slit is placed in the eye-piece, and the view is often drowned by diffraction lines. His impression was that putting the slit in the position indicated by Mr. Gayer would obviate the objection alluded to altogether.

Mr. Hogg said, there could be no doubt that the slit was placed by Mr. Gayer in the proper position. But he thought Mr. Gayer had laid too much stress on the dispersive power of his instrument. It had been the object of Mr. Browning to get rid of dispersion: his object was not to get dispersion, but a perfect image, with as little dispersion as possible. It was not, as in the telescope, an object to divide the sodium band, but it was a thing of great importance to get a sharp, definite band under the microscope, and to be quite sure that the substance under examination always gave bands in the same part of the spectrum. Mr. Gayer said, that by using a deeper eye-piece it was possible to remedy a defect in the instrument he was using. He (Mr. Hogg) asked Mr. Gayer if he had examined a difficult test, the chloride of calcium and cobalt, which was a very good one?

Mr. Ingpen said, the form of Browning's spectroscope is that in which the intermediate lenses are dispensed with, and the only thing between the objective and the eye was the collimating lens. That gave the sharpest spectra of anything he had ever seen. There was the collimating lens just in front of a series of prisms which were viewed through a small aperture in the other end of the instrument. No eye-piece or intermediate lens of any kind was used. The whole instrument was put in instead of the eye-piece, after having focussed the object with the ordinary eye-piece, and a sharp, beautiful short spectrum was obtained. Mr. Browning got rid of stray rays by slipping a cap on the end of the objective, and then bringing that cap down so as to touch the object.

Mr. Slack would mention that the form desirable for a spectroscope depended necessarily on the purpose for which it was to be used. The micro-spectroscope had been usually employed to examine objects which give cloudy bands. It is a peculiarity of these bands to be like astronomical nebulæ in the telescope. Consequently, if there was too much dispersion, the bands were thinned out so much that it could not be seen where they ended. So if absorption bands were over magnified, failure would again be the result. For absorption bands enough dispersion was wanted to render them easily separable. There should not be more dispersion, as it rendered the band difficult to be measured. With respect to the size of objects, he had shown about $\frac{1}{3}$rd or $\frac{1}{4}$th of the human blood globule with a 20th objective, and the Sorby-Browning spectroscope, and had brought out the characteristic bands. The blood globule as to size was about the 4000th of an inch, and a $\frac{1}{4}$ of that was an excessively small object.

Dr. Lawson inquired whether the ordinary system of measurement could be adopted in Mr. Gayer's instrument.

Mr. Gayer replied he believed it could. With regard to the dispersive power of the instrument he had described, there were two prisms in the tube, and it would be optional for anyone to have either of them.

Mr. Wenham said he thought the micro-spectroscope should be made more universally to bring out Fraunhöfer's lines fully. The arrangement Mr. Gayer had introduced would effect that object far more perfectly than the form of spectroscope generally used.

Mr. Ingpen said he could bring out Fraunhöfer's lines beautifully with Browning's small spectroscope.

A vote of thanks was given to Mr. Gayer.

Dr. Royston-Pigott read a paper " On a New Method of Using a Micrometer."

Mr. Wenham said he thought it was very desirable to get an aërial image with a distant micrometer in the same plane with the object; because in that case you can dispense with extra surfaces in the eye-piece. If he understood Dr. Pigott correctly, he has attained that end.

Dr. Pigott assented; a vote of thanks was unanimously accorded to him.

The President then read a paper " On the Histology and Growth of the Skull of Tit and Sparrow-Hawk."

A vote of thanks was passed to the President for his interesting paper.

Donations to the Library, from Nov. 6th to Dec. 4th, 1872:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Quarterly Journal of the Geological Society, No. 112	*Ditto.*
The Microscopical Theatre of Seeds. By James Parsons. Vol. 1, 1745	*Dr. Millar.*
Royal Society's Catalogue of Scientific Papers. Vol. 6	*Royal Society.*
On the Natural System of Botany. By Benjamin Clarke, F.L.S., 1866	*Author.*
On New British Graptolites. By John Hopkinson, F.G.S... ..	*Ditto.*

Walter W. Reeves,
Assist.-Secretary.

Scientific Evening of the Royal Microscopical Society, Dec. 11, 1872.

On the above evening a numerous company of Fellows assembled in the great hall of King's College, kindly lent for the purpose, for inspection of, and for conversation upon, the numerous objects of special interest which were exhibited. Many of the physiological preparations —such as Mr. Stewart's, Mr. Loy's, Mr. Needham's, &c.—were extremely fine. The *Appendicularia*, exhibited by Mr. Alfred Sanders, and a preparation, showing striped muscular fibre in the larva of a tape-worm, attracted great attention. Mr. Renny, of Hereford, sent, and Mr. Reeves exhibited, a microscopic fungus, *Helicomyces roseus*, new to this country.

An opportunity, as will be seen from the subjoined list, was afforded of comparing new objectives by eminent English and Continental makers. Messrs. Powell and Lealand exhibited an $\frac{1}{80}$th and $\frac{1}{50}$th; Mr. Ross showed glasses of Mr. Wenham's recent construction; while Mr. Hogg and Mr. Mayall showed the works of Hartnack and Schieck.

Dr. Pigott and Mr. Ingpen exhibited their methods of ascertaining magnifying power; and Mr. Stephenson showed with his binocular arrangement fine specimens of fractured valves of *coscinodisci*, &c., mounted in *bisulphide of carbon*, and viewed with an $\frac{1}{8}$th. The effect of this highly refractive fluid was remarkable for the distinctness with which it displayed details not seen when balsam is employed.

Mr. Browning showed a set of diffraction bands, ruled by Nobert, to exhibit a series of prismatic colours; and a section of opals in the matrix, from South America.

Mr. Curteis exhibited a chigoe *in situ;* and Mr. Baker an elegant slide of butterfly scales and diatoms, arranged to form a floral device.

Mr. Ackland exhibited a slide of diatoms, grown on a glacier of the Rhone.

Mr. Swift exhibited a new and very small pocket microscope, described by Professor Brown; and Mr. How sent specimens of Dr. Guy's hand microscope.

The Council have to thank Mr. Baker, Mr. How, Messrs. Horne and Thornthwaite, and Mr. Norman, for their kindness in lending lamps.

The subjoined list is not as complete as could be wished, as several exhibitors omitted to supply names and descriptions of their objects.

The Council observed, with great satisfaction, that many Fellows came from long distances to be present on this occasion, and much gratification was expressed by all who attended this second Scientific Meeting of the present session.

Objects and Apparatus Exhibited.

Mr. John Mayall, jun.: A new immersion No. 8 objective, made by M. Prazmowski, of the firm of Hartnack and Co. Of this, Mr. Hogg observes,—" This objective has a focal length of about one-sixth of an inch, and has been specially made for physiological investigations. Its defining power and clearness of image were admirably displayed on the 'Rhomboides' with an eye-piece that gave a magnification of upwards of 1200 ×. One of Dr. Carpenter's spiral thread-cells (*Ammodiscus Lindahli*) was shown with most satisfactory results."

Mr. Jabez Hogg: An old $\frac{1}{4}$ of Andrew Ross, and the new "Reflex Illuminator" of Wenham. *P. angulatum* and *Rhomboides* were brought out in lines and dots, in a perfectly clear and crisp manner, and without difficulty. He also showed several objectives by F. W. Schieck, of Berlin. As these only came into his possession an hour before the time of meeting, it is almost impossible to speak of their performance. He found that a $\frac{1}{12}$th immersion tested on "Rhomboides" and on Dr. Carpenter's test "thread-cells." gave fine definition, with good penetration.

Mr. Browning: A set of Nobert's lines. A section of opal in matrix, from South America.

Mr. Loy: Preparation, showing anatomy of leech, and illustrations of his method of dissecting.

Mr. F. H. Ward: Rectangular crystals of uric acid.

Mr. S. J. M'Intire: Podura, *Templetonia nitida* alive, hatched and bred in captivity.

Messrs. Ross: Their new patent $\frac{1}{2}$ inch and $\frac{1}{10}$ object-glasses.

Dr. Royston-Pigott: A new aërial micrometer for the stage, measuring to the 100,000th of an inch; and a new lens micrometer for estimating power to supersede the Canada-balsam micrometer.

Mr. Thomas Curteis: A chigoe *in situ*.

Mr. Charles Baker: A slide, with 400 butterfly scales and diatoms, arranged as a vase of flowers.

Dr. W. J. Gray: Parasitic growths in Closterium.

Mr. Charles Stewart: Vertical section of the compound eye of blow-fly, &c.

Dr. Millar: Stephenson's new binocular as a dissecting microscope.

Mr. J. W. Stephenson: Diatoms mounted in bisulphide of carbon, with a Ross' $\frac{1}{8}$th, on his new binocular microscope; and the Test Podura scale.

Mr. H. J. Slack: Sepal of gum cistus, with polarized light. A fractured valve of *Coscinodiscus oculus Iridis*, showing the reality o the hexagonal markings.

Mr. Thomas C. White: Hippuric acid crystals recrystallized over fuming sulphur.

Mr. John Ingpen: Photograph of Fraünhofer's metzotint of the solar spectrum.

Mr. James How: Dr. Guy's "Illuminator hand microscope."

Mr. J. C. Sigsworth: Transverse section of *Acrocladia mammillata*, dry, with combined reflected and transmitted oblique (white cloud) illumination, by means of one light.

Messrs. Powell and Lealand: Scale of Podura, with $\frac{1}{50}$th object-glass, 2500 diameters; and *Pleurosigma angulatum*, with $\frac{1}{80}$th object-glass, 4000 diameters.

Dr. Rutherford: An injecting apparatus. The pressure obtained from a column of water. This appears to be the cheapest and best form of injecting apparatus.

Mr. H. F. Hailes: Some new forms of Foraminifera.

Mr. Alfred Sanders: The larva of an ascidian; and striated muscular fibre from the larva of a tape-worm.

Mr. Moginie: Some selections of Diatomaceæ from Montery earth, &c.

Mr. James Swift: Professor Brown's pocket microscope, &c.

Mr. Amos Topping: Injected bone of kitten, and section of cartilage injected.

Mr. William Ackland: Diatoms from the glacier of the Rhone.

Messrs. Beck: Some injected preparations.

Mr. Walter W. Reeves: *Helicomyces roseus* (Link), a fungus, new to this country, and some acari, supposed to be undescribed.

Medical Microscopical Society.

At the second General Meeting of this Society, held at St. Bartholomew's Hospital on Friday, Dec. 6, W. Morrant Baker, Esq., in the chair, the minutes of the previous meeting were read and confirmed. Mr. Jabez Hogg gave some account of the proceedings of the Provisional Committee, of which he was Chairman, and then the Secretary (Mr. Groves) read a code of rules, which it was proposed to adopt, and which was passed, with but few amendments.

The following gentlemen were elected as officers:—

President	Mr. Jabez Hogg.
Vice-Presidents	Dr. H. Lawson. Dr. F. Payne.
Treasurer	Mr. T. C. White.
Hon. Secretaries	Mr. C. H. Golding Bird. Mr. J. W. Groves.
Committee:—	
St. Bartholomew's ..	Mr. H. E. Symons.
Charing Cross	P. M. Bruce.
St. George's	Mr. E. C. Baber.
Guy's	Mr. A. E. Durham.
King's College	Dr. U. Pritchard.
London	Mr. J. Needham.
St. Mary's	Mr. Giles.
Middlesex	Mr. B. T. Lowne.
St. Thomas's	
University College ..	Mr. E. A. Schafer.
Westminster	Mr. Geo. Cowell.
Cabinet and Exchange Committee:—	Mr. E. C. Baber. Mr. J. Needham. Dr. Urban Pritchard. Mr. F. H. Ward.

The place of meeting was not decided upon, but the meetings will take place on the third Friday in each month, from October to July inclusive, and the subscription will be 10*s*. per annum, without any entrance fee.

Intending members are requested to forward their names, qualifications or medical school, and addresses, to Mr. T. C. White, 32, Belgrave Road, Pimlico, S.W., or to Mr. J. W. Groves, St. Bartholomew's Hospital, Smithfield, E.C.

The next meeting, of which due notice will be given, will take place on the third Friday in January.

Brighton and Sussex Natural History Society.

October 24th.—Microscopical Meeting. Mr. G. Scott, President, in the chair. Dr. Hallifax on "The Invertebrate Eye."

When asked to introduce the subject of the invertebrate eye, between which and the vertebrate were great points of dissimilarity, he felt, from having made sections for the microscope of the eyes of insects and crustaceans, he might be able to direct the minds of some of his hearers to, not simply an admiration of a beautiful object, but the establishing a principle, which should guide all inquiry, *viz.* the tracing out a unity of plan where there appeared to be diversity of structure. Whatever organ we investigated in any class of the animal creation should be compared with the same organ in other classes, in order to show their connection by some general plan of unity. As an example of what might be deduced by comparison, he might mention what Mr. Wonfor had proved. He found certain scales, called battledores and plumules, only on the males of certain butterflies, and pursuing the plan of comparison, he had arrived at a general law that any butterfly on which such scales were found was invariably a male.

Newton, who brought the seemingly chaotic mass of stellar and planetary matter not only into order, but simplified their movements under mathematical laws, believed that the seeming chaos of the animal and vegetable kingdom would in time be reduced to harmony. This was before Cuvier, Linnæus, and others had brought about our present classification. It was always a great incentive to inquiry, when we saw any organ devoted to the same evident purpose but differing in structure, to endeavour to bring it in harmony with some general law of unity of plan.

In the invertebrates, taking the eye of the dragon-fly as a type, was an apparent divergence, as wide as possible from the highest type of the vertebrates, the human eye. Comparing them, we found in the first a great mass of optic ganglia, proceeding from the cephalic ganglia (the equivalent of brain in the vertebrates), subdivided and covered with pigment, giving off nervous matter, covered also with dark pigment, changing into a transparent substance terminating in a curved surface, which abutted on a cornea composed of numerous facets, 4, 5, and 6 sided, but each consisting of a lens, convex externally and internally. Each of these facets, or convex lenses, was capable of bringing the rays of light to a focus upon the transparent pigment-covered substance, consisting of transformed nerve matter. In the vertebrate eye we had a globe filled with vitreous matter, a crystalline lens and aqueous matter, refracting the rays of light and causing them to fall on the nervous matter, called the retina, lining the interior of the globe. The nervous expansion of the retina was the only part of the eye cognizant of external impressions, the rest of the eye being merely a physical apparatus.

The sensitive retina, only one hundredth of an inch thick in its thickest part, consisted of at least seven layers, all consisting of nervous matter: the first composed of rods and cones; then four layers of granular matter; next nerve cells; and then the optic nerve. It was believed that the rods and cones were the percipient part of the retina, and for a long time it was held that no similar structure could be traced in the invertebrate eye. Müller, who investigated the eyes of both, came to the conclusion that they were constructed on a totally different plan, and that there were two types of eye in nature.

Recently, Leydig and others had come to the conclusion that there was a unity of type after all. It was a common thing in nature, while preserving the unity of plan, to modify the structure, sometimes transforming or suppressing unimportant parts, but retaining all the essential ones, in accordance with the wants and habits of the creatures. Thus in the invertebrate eye the dioptric apparatus was not necessary, but the nervous mass, with its essential part, the baccillary layer, was retained. It would be seen that each convex facet, hemispherical in the Crustacea, abutted on the layer of transparent pigment-covered rods and cones, which were allowed to be the essential parts of the vertebrate eye. Thus it was seen the essential parts were retained, *viz.* the baccillary layer, which was a modification of nerve structure, abutting on the corneal facets, as the rods and cones in the human eye abutted on the optic apparatus, thus tracing out a unity of plan with diversity of structure.

He had made these observations for the purpose of introducing to their notice certain slides, more especially one lent him by Mr. T. Curties, of Holborn, which cut through the eye of the death's head moth, showed the several parts *in situ*, especially the cones in connection with the corneal facets. Seeing the eyes and antennæ of insects were then instruments of sensation, it was not wonderful these apparatus were highly elaborated.

In proposing a vote of thanks, the President remarked that Dr. Hallifax had promised them a few introductory words, but had, without notes of any kind, given them an elaborate lecture.

Mr. Wonfor said Dr. Hallifax modestly attributed the views of the connection of the vertebrate and invertebrate eye to others, whereas, whatever others had done, to his knowledge he had at least six years ago worked out the views he had enunciated to them, and, moreover, pioneered the way to making sections of eyes prior to that time. Some years since he explained his method of making sections of insects and their parts to the "Quekett Microscopical Club," and a section of insect's eye on the stage of his own microscope, showing the parts *in situ*, was made by Dr. Hallifax at least five years ago.

The meeting then became a conversazione, when the slide above mentioned, together with sections of the eyes of prawn, shrimp, crayfish, crab, lobster, moths, flies, &c., made by Dr. Hallifax, were shown by him, and by Messrs. Hennah, R. Glaisyer, and Wonfor.

Development of Sparrow Hawk's Skull

THE

MONTHLY MICROSCOPICAL JOURNAL.

FEBRUARY 1, 1873.

I.—*On the Development of the Skull in the Tit and Sparrow-Hawk.* By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *Dec.* 4, 1872.)

Part II.

Plates V. and VI.

My next instance of cranio-facial growth is the representative of quite another "Family," or rather "Order" of Birds; this is the Sparrow-Hawk (*Nisus vulgaris*), a good type of a huge group which begins, in extant forms, with the Cariama (*Dicholophus*), and culminates in the Falcon and Eagle.

My illustrations are of a half-fledged nestling of this robber, "a speckled bird among the birds of the forest," the dreaded enemy of the Warbler tribe.

This kind of bird, in this its immature state, is excellent for comparison with the Reptile's skull, especially that of the Turtles and Tortoises ("*Chelonia*"). It would seem at first sight that the frowning brow and the arched and knife-edged beak were special modifications of this form of bird, in thorough harmony with its character and its habits, and having no other or *wider* meaning.

But the permanent down-bend of the beak and the bony roof to the brow are both retentions of the Chelonian type of structure, not derived from the Turtle, and yet pointing to a probably common origin for both. Yet with all these evident reptilian marks upon them, in many respects these birds are to their own class what Tigers and Lions are to the Mammalia, namely, amongst the most

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EXPLANATION OF PLATES V. AND VI.

Plate V., Fig. 1.—Skull of Nestling Sparrow-Hawk (*Nisus vulgaris*) (side view), × 2.
" " 2.—Ditto, ditto (lower view), × 2.
" " 3.—Ditto, ditto (upper view), × 2.
" " 4.—Part of Fig. 2, × 3.
Plate VI., " 1.—Skull of same (end view), × 2.
" " 2.—Fore-view of skull, with face cut away, × 3.
" " 3.—Part of palate of ditto, × 3.
" " 4.—Os hyoides of same, × 2.

highly specialized; being indeed, as to skeletal mechanism, the most wonderfully endowed of all the Birds. Not only their whole body, fitly joined and compacted together, but every several limb and every hinge and joint is in itself a paragon of form, strength, and elegant lightness. Still, my proper business is not with these final results, but with those *still-life* vegetative processes by which these special ends are brought to consummation.

Looking at the side view of the skull (Plate V., Fig. 1), it is at once seen how the fierce face of an aquiline bird "is modelled on a skull," which differs from that of other birds only by the gentlest modifications. Indeed, it were not an impossible task to make a diagramatic figure of a Carinate bird's skull that should serve as a general illustration for the whole of the feathered nations. Amongst these *Plunderers* there is one bird, the South American Cariama, which combines the characters, otherwise seen separately, of Owl and Vulture, Eagle, Falcon, Hawk, and Secretary Bird, whilst it is also unmistakably related to the Cranes and Fowls.

Looking at the skull from beneath (Plate V., Fig. 2), we see that already the process of ankylosis has greatly effaced the occipital sutures, and the basi-sphenoidal and basi-temporal tracts have melted into each other. The occipital condyle (*o. c.*) is transversely largest, having more of the Lacertian form than in the Singing-birds. The "ex-occipital tympanic wings" (*t. eo.*) are still edged with cartilage. The parasphenoid has formed large trumpet-shaped recesses to the tympanic cavity (*a. t. r.*), and on the lips of these tubes, behind, I have seen, in older specimens, as many as three "tympanics" on one side. In still older specimens these have coalesced together, and with the trumpet lip. Good, stout basi-pterygoids appear on the base of the "rostrum" (*b. pg.*), and these, although pointed, and scarcely functional, are permanent. The "parasphenoidal rostrum" (Figs. 1 and 2, *pa. s.*) is thick behind, and comes upwards to a fine point in front, behind the great "cranio-facial notch," which is slowly pushed upwards in the adult, the cartilage becoming absorbed between the ethmoidal and septal ossifications. The former of these (Plate V., Fig. 1, *p. e.*) is now a huge plate of bone, running forwards to the "notch" (hidden in the figure by *p. p.*) and backwards towards the "præ-sphenoid" *above* and the basi-sphenoid below (*p. s.*, *b. s.*); its posterior crescentic margin embraces the front of the pear-shaped "interorbital fenestra" (*i. o. f.*). The basi-sphenoid (*b. s.*) is ossified, and is continuous behind with the huge grafting wings of the "parasphenoid," and has coalesced by its lower surface with the basi-temporals (*b. t.*). *Within* the skull it is being fixed into the margins of the "periotic" masses and basi-occipital (*b. o.*). Seen from behind (Plate VI., Fig. 4), the large, once double, supra-occipital (*s. o.*) is still partly traceable as distinct from the ex-occipitals (*e. o.*), and in this view the "epiotics" and squamosals (*ep.*, *sq.*) are seen all surmounted by the squared parietals (*p.*). The peculiarly *ornithic* condition of the great sphenoidal wings is best seen in a skull which has had the whole face cut away through the orbits (Plate VI., Fig. 2, *al. s.*). They are placed far more from without inwards than longitudinally, and are attached to the orbital plate of the frontal (*o. f.*). A subvertical view of the post-orbital plane (Plate VI., Fig. 2) helps much to the understanding of this very complex and most instructive skull. The narrow, superorbital plates of the frontals (*f.*) are cut through, and these large orbital plates (*o. f.*) are seen from their scooped front aspect. They are sending bony spiculæ into the up-tilted cranial floor towards the "anterior sphenoid," which lies in the middle. The upper plate of each frontal is grooved for the "olfactory nerve" (1). Propping up these plates, another bony bar is seen; this is an azygous "ectosteal" orbito-sphenoid; it has three points below, and rests upon the præ-sphenoid and the narrowest foremost part of the "orbito-sphenoidal" cartilaginous alæ, which are here unusually large for a bird; these together are pyriform, and on each side there is a largish "endosteal" orbito-sphenoid; they are unequal. Between them there is a large vertical plate of endosteal bone (Plate V., Fig. 1, and Plate VI., Fig. 2, *p. s.*); this is the "præ-sphenoid"; it lies above and in front of the opotic foramen (2). An irregular "fenestra," or rather "fontanelle," of the unhardened membranous cranium extends upwards on each side between the orbito-sphenoids mesially and the orbital plates of the frontals and "ali-sphenoids" laterally. The large squamosals are scarcely seen in this view (Plate V., Fig. 1, *sq.*); but in the lateral view they show nearly all their relationships. The large cartilaginous wings of the posterior sphenoid ("*al. s.*") had a fenestra in their centre; here the bone is still very thin, and I suppose, as in most birds, the upper and lower regions were separately ossified. A very small additional centre is seen on the near margin of each bone. Between the lower edge of the "ali-sphenoid" and the upper edge of the huge complex "basi-sphenoid" there is a suture, and in this suture two holes; the outer, or posterior, is the "foramen ovale" and the inner the "foramen rotundum."

The sectional view of the median part below is the "præ-pituitary" part of the basi-sphenoid cut through close to the "basi-pterygoid processes." Seen in a shadowy manner, at a distance, we have, below, the occipital condyle at the mid-line (*o. c.*) on each side, and in front of that the swellings caused by the cochleæ where the underlapping "basi-temporal" ankyloses with the basi-occipital (*b. t.*). The outermost swellings are the "basi-temporal floors" of the tympanic cavity. The two "anvils" that look towards each other are the "quadrate bones." They must be considered soon.

The huge "internal ears" only come fairly into view at two points in the figure; the antero-superior angle of the "periotic" mass breaks out between the "squamosal" and "ali-sphenoid" (*sq.*, *al. s.*), and a lateral element of this organ is seen below the head of the quadrate; this latter bone (*pto.*) is the "pterotic," and the former the "sphenotic" (P.), the symmorph of the "post-frontal" of the Osseous Fish. Laterally (Plate V., Fig. 1) and behind (Plate VI., Fig. 1) the "epiotic" (*ep.*) is well seen as an oblong obliquely-placed tract of bone overlying the "posterior semi-circular canal." The main part of the mass is only to be seen from within, but a little of the chief bone, the "prootic" (Plate VI., Fig. 2, *pro.*) is seen below and outside the "foramen ovale." The opisthotic or fifth osseous element of the ear-mass has already coalesced with the "ex-occipital," and is becoming ankylosed to the prootic. In a paper which I hope will soon appear in the 'Philosophical Transactions,' I have gone into this matter thoroughly, and have shown why I have had the audacity to add two new elements to the "periotic mass," namely, the "pterotic" and the "sphenotic." Professor Huxley is responsible for the terms "prootic," "opisthotic," and "epiotic." My terms are in imitation of his. With regard to the bone which I have called "pterotic" (See Fowl's Skull, Plate 85, Figs. 1 and 4, *pto.*), it is a bony centre with which I have long been familiar, but its unusually fine development in the Sparrow-Hawk has at last thrown an unexpected light upon certain bones in the Lizard's skull. *By the light of a most patient and long-continued study of the tissues microscopically*—having never been satisfied with rough observations, although I have not used extravagantly high powers—I now (Dec. 3, 1872) can master and masticate things that have been as gravel to my teeth for years past. When Professor Huxley was working out his "Malleus and Incus" paper, for which all posterity shall bless him, he wrote to me (Jan. 29, 1869) a letter with a sketch of the condition of the facial arches in "*Hatheria* alias *Sphenodon*."

In his graphic pen-sketch the hyoid arch is seen to be attached to the side of the "auditory mass," and a piece of cartilage at the end of the "par-occipital bones" is pointed to, with this description, namely, "a bit of cartilage with endostosis, which *some* call pterotic." This granular epiphysis anyone can see in a Lizard's skull, and outside it a strongly binding falcate bone (an "ectostosis") which is wedged-in between the similarly *falcate* squamosal and the out-turned *parietal* horn. The two falcate bones thus overtop the "quadrate," binding the cartilage to which that bone hinges, like metallic clamps. The *some* body who had called that endosteal patch "pterotic" has often foregathered with his friend as to the nature of the *post-squamosal* "sickle"; it stood out, an enigma. This is one of those points in the histological study of the metamorphosis of a bird, in its ascent above a Lizard, which is so very instructive. In the bird the "pterotic" (see Plate VI.,

Fig. 4, *ptc.*) is *one* bone made up of an endosteal metamorphosis of the cartilage, and the ectosteal transformation of its perichondrium. In the Lizard these parts, like the *clavicle* and *præ-coronoid*, are permanently distinct; other instances in the Lizard's skeleton could be shown. Hence we may see that any hasty interpretation of homologous parts is worse than useless, and that the most delicate microscopic research must go hand in hand with morphology.

Going forwards to the nasal capsule, we find each "pars plana" (Plate V., Fig. 1, *p. p.*) a large sub-quadrate flap of cartilage, undergoing endostosis at its inner edge. The bridge by which this flap passes into the "ali-ethmoid" above is separately ossified, at least it is in *Buteo*, and the nasal branch of the fifth nerve runs outside it, not through the same passage as the first. The swollen ali-ethmoid (rudimentary upper turbinal) has its own bony tract in old birds. The septum-nasi (Plate V., Fig. 2, *s. n.*) is well ossified, and is very large; it ankyloses with the præ-maxillaries and the maxillo-palatine plates in the adult; in the young the latter union has not taken place: this bird is typically "desmognathous."

The two-horned "nasals" (Plate V., Figs. 1 and 2) and the lachrymals (*l.*) are very characteristic; the lachrymals, besides their long superorbital *process*, have in the adult a square superorbital *bone* at its extremity; this was fibrous in my young specimen. In the Monitor Lizards the whole upper part is *super*-orbital, and the descending crus or *præ*-orbital is the true lachrymal.

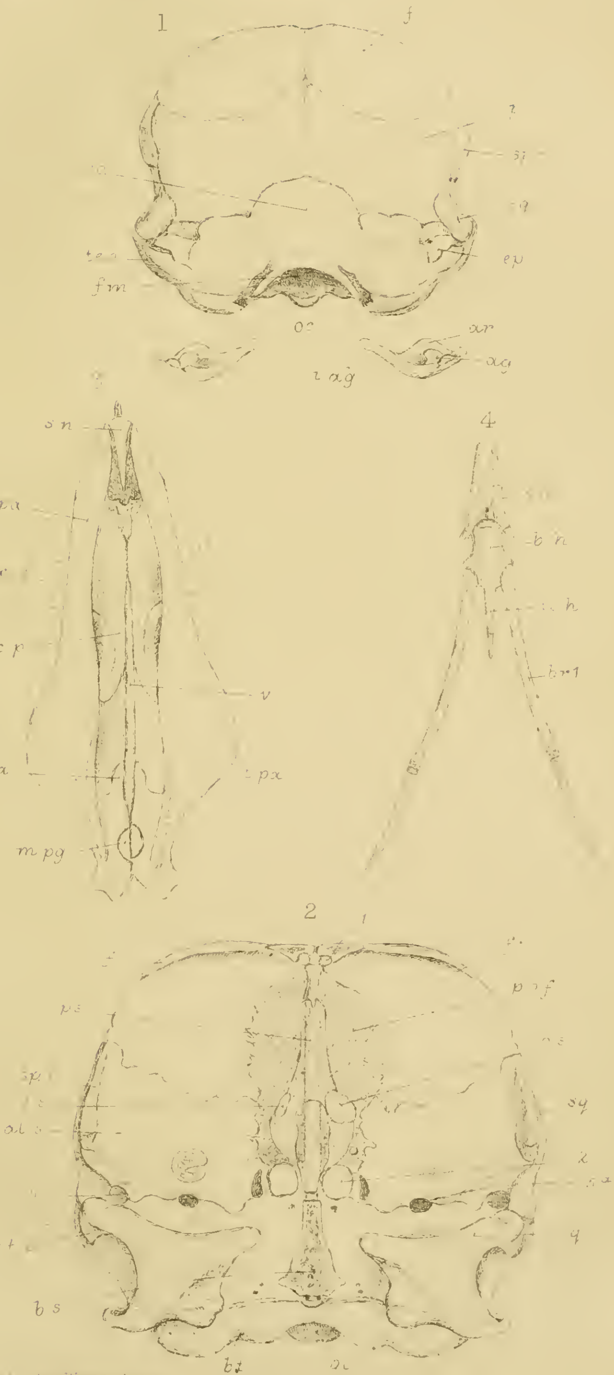
The "vomer" (Plate V., Fig. 2, *v.*, and Plate VI., Fig. 3, *v.*) is a long knife-like bar of bone, narrowest in front; it is wedged-in between the ethmo-palatines (*e. pa.*). In the adult the vomer is very long and downturned, and enlarged in front, as in *Falco* and *Dicholophus*, but to a less degree. In *Ulula uluco* (the Hooting Owl), and in two forms nearly related to this Hawk, namely, *Haliastur Indus* and *Circus cyaneus*, there is a "median septo-maxillary" above the vomer. The vomer is a membrane-bone, and enters into no union with the nasal walls or turbinals. In all Rapacious birds I have found the vomer azygous. Only in *Dicholophus* is there an "os uncinatum"; it is a rod-like ossicle underlapping the lachrymal, and standing on the zygoma.

The palatines (Plate V., Figs. 1 and 2, and Plate VI., Fig. 3, *pa.*) have a somewhat outspread form. I have not seen a separate "transpalatine" or any cartilage in that region, yet in the European Vulture (*Gyps fulvus*) the bone is partly separated by a suture from the body of the palatine. The præ-palatine bar is long and lathy; the "interpalatine" spurs aborted; the "ethmo-palatines" are rounded lobes, and the groove between the two inferior ridges behind is of moderate depth. Not in this species, but in several, there is a junction, or "commissure" of the "ethmo-palatines," namely, in *Dicholophus*; the Owls; *Helotarsus ecaudatus*; *Sarcoramphus papa*; *Neophron percnopterus*; *Falco tinnunculus*;

and in this part there is a separate bone, the "medio-palatine." The short pterygoids (*pg.*) are flat in front and rounded behind; they grow *towards the* basi-pterygoids, but by a *point*, not by a facet. The meso-pterygoids (Plate V., Figs. 1 and 2, *m. pg.*) are large and spongy, and divide the palatines above the posterior nasal canal (Plate VI., Fig. 3). The maxillaries (*mx.*) are in themselves small, but their "maxillo-palatine processes" are large, spongy, and hugely developed, fore-and-aft. As in *Dicholophus*, they unite by *harmony* in the young; but in the old bird they are thoroughly ankylosed, both with each other and with the nasal septum. The jugal process of the maxillary, the jugal and quadrato-jugal bones (*j. q. j.*), are all long and slender styles. The large quadrate (*q.*), with its small, blunt-pointed orbital process, is best seen from the front (Plate VI., Fig. 2, *q.*), and from the side (Plate V., Fig. 2, *q.*); it has two articular heads above; the outer articulates with the "squamosal" and "pterotic," and the inner with the "prootic" and "opisthotic." Below (Plate VI., Fig. 2) it articulates in the usual manner with the mandible. This latter part (Plate V., Fig. 1, *ar. d.*, *s. ag.*, *ag.*) is composed of an endosteal "articulare," and of the ordinary splints, namely, dentary, splenial, coronoid, surangular, and angular. The posterior angular process is blunt; the internal long and pneumatic; the dentaries (*d.*) are well ankylosed by a short, strong symphysis; they are deflected in front. The "os hyoides" (second and third post-oral arches, Plate VI., Fig. 4) is composed of two rod-like "cerato-hyals" (*c. h.*) which partly ossify and afterwards unite at the mid-line; a broad "basi-hyal" continuous with a slender "uro-hyal"; and the elements of the third arch (*br.* 1) are of the usual form, but are very straight in the Raptores; the "stapes" has a bony rod with three cartilaginous forks, the "supra- extra-" and "infra-stapedials."

In conclusion, I may remark that this last-described type, the Raptorial, is full of interest, as full as the forms that have already come under consideration, namely, the "Passerines." The working-out of these types has been *microscopic* from first to last; but the power to see the image of one type reflected in another—to trace the same touch—the same *habit* or fashion, and to be able to deduce arguments for the absolute unity of morphological law—these seem to me to be precious results of painstaking labour, in which "the hand of the diligent maketh rich." Only he who, counting the cost, is willing to become foot-sore and weary, not once nor twice, can have the serene satisfaction of beholding Nature in her broader features of beauty and never-cloying variety.

Intellectually, out of even these foul eaters of flesh there comes forth meat; and out of these strong robbers there comes forth the sweetness of new light, shed on the old paths of Creation.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II.—*On an Aërial Stage Micrometer: an improved form of engraved "Lens-Micrometer" for Huyghenian Eye-pieces, and on finding Micrometrically the Focal Length of Eye-pieces and Objectives.* By G. W. Royston-Pigott, M.A., M.D., &c.

(*Read before the* Royal Microscopical Society, *Dec.* 4, 1872.)

(*Continued from p.* 5.)

In the present article it is intended to give an easy method of determining the focal lengths of either eye-pieces or objectives; and also their magnifying power micrometrically.

It is indeed generally known that the focal length of an eye-piece may be found by multiplying the focal lengths of each component lens, and dividing the product by their sum when diminished by the interval separating the lenses,

$$\text{or} \quad F = f \times f' \div (f + f' - a).$$

But then the observer is landed in the double difficulty of finding these focal lengths and the interval. The method I now propose obviates these measurements altogether, and it entirely resolves itself into micrometric measurements.

In the paper given at page 266 (No. XLVIII.) the writer gave methods for establishing the focal lengths of a single lens by means of ascertaining the magnifying power when a certain distance intervenes between the object and image.

For deeper glasses, as $\frac{1}{2}$ inch, $\frac{1}{4}$, &c., *it will be found sufficient to divide the distance by the magnifying power when increased by the number two.*

It has been for a very long time a custom with opticians to place a rule, divided into inches and tenths, on the stage besides a glass micrometer divided into hundredths; when, by observing with *the left eye* how many inches and parts are subdivided by a magnified hundredth in the field of view observed with the right, a tolerable estimate can be roughly formed. Besides, however, the fact that both eyes frequently have different distances of distinct vision, and that several experiments give different results, it can lay no claim to the accuracy of a scientific determination.

The new method may thus be described:—To find focus of an eye-piece, measure by a stage micrometer, divided into 100ths and 1000ths, the image of a *ten-inch* circular white disk placed on a black board 100 inches from the stage of the microscope, as miniatured by the given eye-piece used as a condenser.*

Let N be the size of the image in number of thousandths—so that m is the proportionate size of object to image, or $10 \div N$—

Then there is a beautiful relation between the focal length and the

* The eye-piece is arranged with its field-glass towards the disk.

proportion of the miniature to its object at a given distance, expressed by the formula given by the writer ('Phil. Tr.,' vol. ii., 1870), *viz.*—

Focal length of equivalent single lens

$$= \frac{\textit{distance between object and image}\,*}{\textit{diminution of image} + 2 + 1 \div \textit{diminution}}$$

$$\text{or} \quad = \frac{d}{m + 2 + \frac{1}{m}} \quad \text{which in our case is}$$

$$= \frac{100}{m + 2 + \frac{1}{m}}$$

Remark.—When the miniature is extremely small compared with object, $\frac{1}{m}$ may be neglected.

Ex.—A Huyghenian eye-piece is placed like a condenser, and forms an image of a ten-inch disk, 100 inches distant from the stage micrometer; and with a low power the miniature is found to measure 147 thousandths (0·147 inch) by stage micrometer, required the focal length. Here the miniature is reduced in the proportion of 10 inches to 0·147 or 68 times nearly; so $m = 68$.

$$\text{Then} \quad F = \frac{100 \text{ inches}}{68 + 2 + \frac{1}{68}} = \frac{100}{70 \cdot 015}$$

$$= 1\tfrac{4}{10} \text{ very nearly.}$$

The next question arises, if such be the sidereal† focal length of the eye-piece, what is the magnifying power?

The writer has for this purpose worked out the following simple problem:—

If D be the distance at which an observer can see an object distinctly and the distance at which he sees really the field of view in the microscope, F the focal length of a single lens, then the magnifying power, or the proportion of the size of the image to the object is *one less than the distance of distinct vision divided by focal length,*

$$\text{or} \quad m = \frac{D}{F} - 1.$$

If the observer's eye adopts 10 inches,

$$\text{Then} \quad m = \frac{10}{F} - 1.$$

Ex. 2.—Find the magnifying power of the eye-piece of last example at 10 inches' distance for distinct vision.

$$m = \frac{10}{1\frac{4}{10}} - 1 = 6\tfrac{1}{7} \text{ times.}$$

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* This formula will be found extremely useful to photographers.

† "Sidereal and solar focal lengths" are terms indifferently used for expressing the principal focus or focal point formed by parallel rays.

Ex. 3.—Magnifying power of E eye-piece, whose focal length is $\frac{1}{8}$, is for 10 inches,

$$m = \frac{10}{\frac{1}{8}} - 1 = 80 - 1 = 79.$$

Ex. 4.—Focal length of an eye-piece E when the object at 100 inches is diminished 798 times by measurement on the stage of the miniature of disk 10 inches diameter can be similarly found as follows:—

$$\text{Neglecting } \frac{1}{m},$$

$$F = \frac{100}{798 + 2 + \frac{1}{798}} = \frac{100}{798 + 2} \text{ nearly.}$$

$$= \frac{100}{800} = \tfrac{1}{8} \text{ nearly.}$$

Magnifying power at 10 inches,

$$= \frac{10}{F} - 1 = \frac{10}{\frac{1}{8}} - 1 = 80 - 1 = 79.$$

Focal lengths of objectives and their magnifying powers can be similarly found.

III.—*Note on the Scalp of a Negro.*

By Charles Stewart, M.R.C.S., F.L.S.

(*Read before the* Royal Microscopical Society, *Jan.* 1, 1873.)

Plate VII.

Besides the dark colour which is seen in the skin of the Negro, and which is due to extra pigmentation of the deep layer of the epidermis, there are some peculiarities which I believe have not been noticed, or at all events are not generally known. These features may be seen in vertical sections of the scalp; but before describing them perhaps I may be excused for saying a word on the principal structures displayed in a similar section of the scalp of a European.

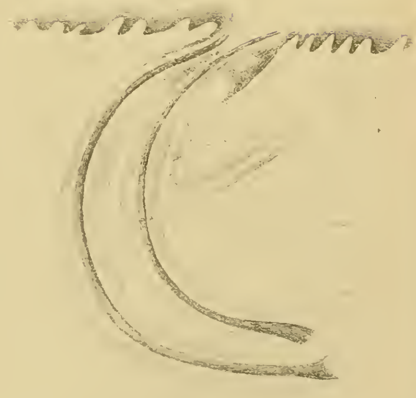
The free surface of the epidermis will be seen to be nearly level, whilst the deeper layers, or rete mucosum, will be raised by the papillæ, or conical elevations of the subjacent dermis. Bundles consisting of five or six hairs surrounded by their follicles will pursue a straight but oblique course through the greater part of the thickness of the dermis, each follicle having at its base the papilla upon which the hair is seated, and attached about a third of the way up on its under surface the bundle of involuntary muscular fibres which moves the hair. In the angle between this and the hair itself will be situated the sebaceous gland. Besides these, a few scattered sudoriparous glands may be noticed.

In the Negro the scalp is altogether thinner, and of course will show the pigmentation of the rete mucosum already alluded to. In addition to this familiar peculiarity the hairs and follicles present the following remarkable differences, *viz.* the portion of the hair and follicle imbedded in the skin is much longer, and is also remarkably curved, so that it commonly describes a half circle. The papilla at the base of the follicle consequently either lies horizontally or even becomes directed obliquely inwards towards the subjacent bone. In other respects there is no great difference, but perhaps the sebaceous gland is somewhat smaller.

It may be suggested that some of these conditions were produced by the mode of preparation, but this can hardly be the case, as the Negro and European scalps were prepared at the same time in the same way.

Proximal surface of Rosette (Podophora)
— opaque —

Ring (Podophora)
— transparent —



Vertical Section of Negro's scalp

IV.—*Note on the Calcareous Parts of the Sucking Feet of an Echinus* (*Podophora atrata*).

By Charles Stewart, M.R.C.S., F.L.S.

(*Read before the* Royal Microscopical Society, *Jan.* 1, 1873.)

Plate VII.

In addition to the spines and pedicellaria which cover the outer surface of an Echinus, or Sea-urchin, five rows of tubular muscular cylinders may readily be seen; these are called the ambulacral tubes, and are the sucking feet which assist the animal in locomotion. The feet are most developed on the under surface of the Echinus, those on the upper surface being commonly modified, and used either as organs of touch or for respiration.

In all known Echini, these feet are strengthened by a calcareous framework, which consists always of two, and usually of three, distinct parts, each composed of many separate pieces. Two of these, called the rosette and ring, are apparently constructed for keeping the sucking extremity expanded; the third exists in the form of numerous spicula, which vary greatly in shape in the different genera, and afford valuable aids for their distinction.

The rosette is usually figured and described as a flat circular disk composed of separate pieces, varying in number from three to six or seven, five, however, being the most usual; its margins are ornamented by spine-like projections, and its centre is perforated by a large hole. The ring is described as a single polygonal plate, having a large hole in the centre, whose sides are equal in number to the plates of the rosette.

This description, however, is not quite true, either for the rosette or ring.

Although the general outline of the rosette is correct, the marginal spines do not stand out horizontally, but project forwards from the flat anterior surface; this arrangement seems to enable them to grasp the body to which the sucker is applied when its centre is pulled towards the animal. The part of the rosette nearest the animal is very complex, each plate showing a depression of great depth, so that the inner as well as the outer margin of the rosette is thin, the greatest thickness being found a little to the inner side of the centre of each plate. Just inside the outer margin of the depressed area may be seen a small extremely transparent tubercle; it is upon this tubercle that the angles of the polygonal ring rest, and to the constant motion of the ring upon the rosette must be attributed the solidity and consequent transparency of the tubercle. The posterior contiguous edges of the plates of the rosette are bevelled off, so that their anterior diameter is greater than their posterior; the result of this is, that when the posterior margins of all the plates are brought together the anterior surface of the entire sucker is rendered convex, and so becomes detached from the body to which it was fixed, whereas, when the posterior margins are separated, the front surface of the sucker becomes concave, and the marginal spines grasp like claws any inequalities of the body to which they are applied.

The ring, which is described as a single plate, I have always found to consist of separate plates, equal in number to the segments of the rosette; they are, however, most frequently arranged in many corresponding sets, one beneath the other. In all cases the extremities of the plates overlap one another.

V.—*Oblique Illumination for the Binocular.*

By W. K. Bridgman, L.D.S.

The management of *oblique* light with the *Mo*nocular instrument is comparatively an easy matter, but when it comes to the *Bi*nocular, it is altogether a very different affair; in fact, with the ordinary mirror turned out of the axial line as commonly practised, it is scarcely possible to obtain an equal degree of illumination in both tubes simultaneously, and why it should be so will be very apparent on consideration of the conditions produced. When the reflector is turned to either side of the stage, the light falling obliquely upon the surface of glass necessarily becomes graduated in intensity from one side to the other, and as the prism bisects the field in the same line of gradation, it divides the illumination from A to B, Fig. 1, unequally, by giving the lighter half to one tube and the darker half to the other. It will, hence, be obvious that if the prism could be turned one quarter round and made to cross the aperture in the direction from C to D, Fig. 2, it would divide the light fairly, so that each half would be a complete counterpart to the other, and both tubes could thus become similarly lighted up at the same time; but as this is an impracticability, the only alternative will be to alter the direction of the light itself instead. Oblique illumination can of course be obtained by the prism, the parabola, the spot lens, or the "kettledrum," in any direction; but these appliances are more adapted for special purposes than available for general use. The want is for some plan of illumination that will serve for all kinds of work and be as little trouble as an ordinary condenser. It is quite a mistake to suppose that *oblique light* is only of service for bringing out the markings upon diatoms and other tests; it is in reality the most suitable mode of treatment for every kind of object that can be seen by transmitted light from beneath the stage.

Fig. 1.

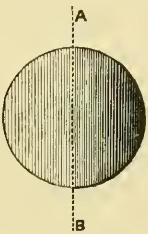
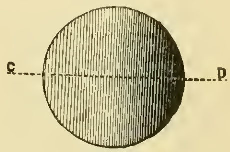


Fig. 2.



With *direct* light thrown up from below and a transparent or translucent substance, it is little better than mere shadow-work; as the thicker or darker portions then only become visible by stopping out more or less of the direct rays, while the illumined surface, *or that part which ought to be seen*, is the under-portion presented to the mirror and away from the object-glass. In obtaining a good defining illumination the practised observer is well aware that it is desirable to exclude as many as possible of the *direct* rays which suffer little or no refraction in their course upwards, but tend to produce an uncomfortable and confusing glare, and to accomplish this, the mirror is generally thrown a little out of axis, so as to give a slight degree of obliquity to these portions of the light, as well as to cast the shadow more or less to one side of the object. Thus, although we speak of *direct* illumination, it is not so in reality; a slight degree of obliquity is found to be a practical necessity, and I have been endeavouring ever since the introduction of the binocular to find some means of obtaining a more satisfactory arrangement for accomplishing this, and have at length been rewarded with sufficient success to warrant its being offered to the attention of others. It has always appeared that as the outside edge of the picture of a flame is found to be the most effective, there is something more than the mere *obliquity* of the rays that is the desideratum, and I have been inclined to attribute it to *inflection*, or *diffraction*, and this idea has been kept in view throughout.

The first point has been to stop out all the useless central rays. The next to throw up a good body of light in a suitable direction to produce the necessary shade and shadows, and then to let in a sufficient amount of light from another point in order *to render the shadows transparent* and thus enable us to distinguish the detail within them. As a foundation to commence upon, a hemispherical lens of about one inch in diameter was found to be most advantageous, but it was also found that placing the diaphragm in contact with the upper flat surface of the lens, as recommended by the late Rev. J. B. Reade, did not produce the same effect (which I attribute to diffraction) as placing it beneath the convex surface and at a very slight distance below it. After innumerable trials it was found that the form (accurately obtained by tracing) represented in Fig. 3 gave the best results. This is fixed by the tongue A being bent up, and then inserted into the doubled end of the flat ring at B, Fig. 4. This ring is left as a fixture within the setting of the lens, and so admits of the stop being removed at pleasure or changed for any other form. The stop is cut out with a pair of scissors from sheet tin about the thickness of thin writing paper, and is then curved at the transverse edge to something less than the curvature of the lens,

Fig. 3.

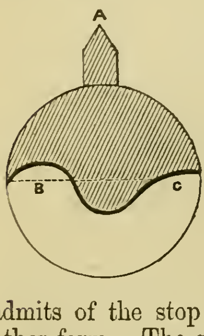
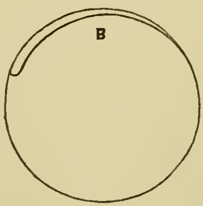


Fig. 4.



and placed at a slight inclination downwards as well. It will thus appear that the central portion of the lens is stopped out for the direct rays, while a greater portion of light is admitted from one side than the other, and being placed transversely to the edge of the prism, or from C to D of Fig. 2, and with the larger space B, Fig. 3, on the left-hand side, the bulk of the light is sent across towards the prism and helps to compensate for the loss by reflexions within it. By running the condenser up and down it will be seen that the quality of the light varies very much in its different parts, and by slightly turning the mirror to one or the other side the most varied effects will be produced; but at the edge where the effects of diffraction would be found, the greatest distinctness will be seen to occur. Then, again, the stop being of polished tin and slightly inclined downwards, a portion of the refracted rays will probably be reflected downwards by the plane side of the lens, and being met by the reflecting surface of the tin, will be thrown upwards again, helping to break the gloom of the otherwise darkened part of the circle. In addition to this, there would seem to be a considerable amount of light reflected downwards by the surface of the covering glass or the under surface of the object lens, for objects thus illumined have the appearance of being lighted up with both transmitted and reflected light, so that the most delicate surface markings and the most transparent tissues become remarkably and most beautifully distinct, and indeed I have never before seen the more delicate membranes of Infusoria, Zoophytes, &c., brought out with such exquisite delicacy and clearness. It should be observed that a piece of finely-ground glass should be placed *beneath the slide,* and with artificial light this glass should be of a pale neutral tint and the ground side upwards, and only just sufficiently distant to be quite out of the focus.

VI.—*On the Spherules which compose the Ribs of the Scales of the Red Admiral Butterfly* (*Vanessa Atalanta*), *and the Lepisma Saccharina.* By G. W. Royston-Pigott, M.A., &c.

The scales of this splendid insect exhibit by reflected light fine shades of red, brown, and yellow. Some of them are intensely black, resembling the dead black of the finest velvet. It is upon these apparently black scales I now propose to make a few observations with a succession of object-glasses.

1. By transmitted light with a low-angled "inch," the particular scale now under notice is of a fine Indian-ink colour, approaching burnt sienna; daylight without sunshine. Under a fine half-inch

and a "C eye-piece" magnifying about 200 diameters, the dark colour is seen to be owing to very dark ribs, with bright intercostal spaces—brighter as they approach the quill; and these spaces appear filled with some kind of structure upon drawing out the tube 4 inches, so as to give about 300 diameters.

Some of the scales adhere to the under side of the cover, and these are better defined. Inch and a half condenser (direct light), without stops (a very fine Ross objective).

2. Fine Ross Quarter "1851." The ribs assume a pale bluish colour, and the pale intercostal spaces a pale rosy hue. This appearance is best seen towards the quill end, and is easily brought out with a delicate fine adjustment.* Aperture of $\frac{1}{4}$ 70°.

An antique $\frac{1}{8}$th ($\frac{1}{7}$th) of Powell's make (Aperture 60°), with A eye-piece shows the ribs much darker, but clearly *beaded.* No appearance of rose colour, but a faint bluishness. The scale has been crushed in places, and in the wide gaps of the openings appear very dark clusters of minute bodies, resembling granules, in strong relievo. It is noon, and a cloudy grey day.

3. Powell and Lealand's fine 1862 dry $\frac{1}{8}$th, A eye-piece. The ribs are indistinctly beaded, and very dark; the intercostal spaces light and translucent; the granules indistinct in the broken up spaces. The edges of the quill are somewhat thick and blurred. A few isolated spherules lie in the open spaces, surrounded by a white halo. With the "C eye-piece" and a power of 800, *above the bead appears a bright focal* point (which demonstrates its convexity); in concave objects the bright focal point lies below the surface in "dry mounted" objects.

It appears probable the bead can be measured by finer definition with a good light and Powell and Lealand's new immersion $\frac{1}{8}$th.

I now substitute a very fine half-inch objective for the $1\frac{1}{2}$-inch condenser in order to get more light.

The ribs appear narrower, and the intercostal spaces full of some kind of structure, apparently granular, which is still more distinct with condenser formed by the "Ross 1851" Quarter.

4. B eye-piece; Powell and Lealand's new immersion $\frac{1}{8}$th, $\frac{1}{4}$ condenser, dull daylight as before. The ribs appear darker, Indian-ink colour, *thinner*, sharper, cleaner; between the ribs (and crossways like the rounds of a ladder, only thickly set together) small dark beads are arranged in rows of *three spherules* in contact. In the broken spaces I perceive a set of trios isolated, still arranged nearly in straight transverse lines, and these spherules appear nearly half the diameter of a rib. At the quill end of the scale the spherules appear fainter.

* I cannot too highly praise the *extreme steadiness* and delicacy of the new stand Messrs. Powell and Lealand have specially constructed for me, with a silver-rimmed stage divided to minutes of arc of rotation.

A remarkable deterioration in definition occurs with scales attached to the *slide* and laying below those adherent to the cover.

In order to measure the diameter of these cross trios of spherules, an eye-piece micrometer was now used to examine the thousandths of an inch on a stage micrometer. The divisions in the eye-piece* were 200 to the inch. One stage thousandth exactly measured 13·5 divisions; the power employed was $675 = \overline{1350} \div 2$. The spherules appeared to be just four to the width of one division of eye-piece. The diameter of one spherule therefore = ¼th of these divisions.

$$1 \text{ division} = \frac{1}{1000} \div 13\tfrac{1}{2} = \frac{1}{13500}.$$

$$\tfrac{1}{4}\text{th division} = \frac{1}{4} \times \frac{1}{13500} = \frac{1}{44000}.$$

The spherules are therefore about 1–44000th of an inch, set close together in contact. Now it is singular that although Nobert's Band VII. on the new plate contains about

45,000 divisions to the inch,

they are very much easier to be seen than these spherules separated from centre to centre by almost the very same interval. Indeed, in the case of the spherules in contact there are only minute notches ○○○○, whilst in Nobert's bands there is a clear space between the lines; and this notch is much less than the spaces of Nobert's lines of Band VII.†

I think that those of our Fellows who will take the trouble to examine the spherules of the Red Admiral scales, will quickly discover that (with an inch and a half object-glass condenser) on a cloudy day it is only by their very best glasses they can detect the cross rows of beads in the intercostal spaces. Perhaps the *rationale* of the easier visibility of Nobert's lines 50,000 to the inch, as compared with equivalent spherules in close contact, will be more apparent by means of a diagram.‡

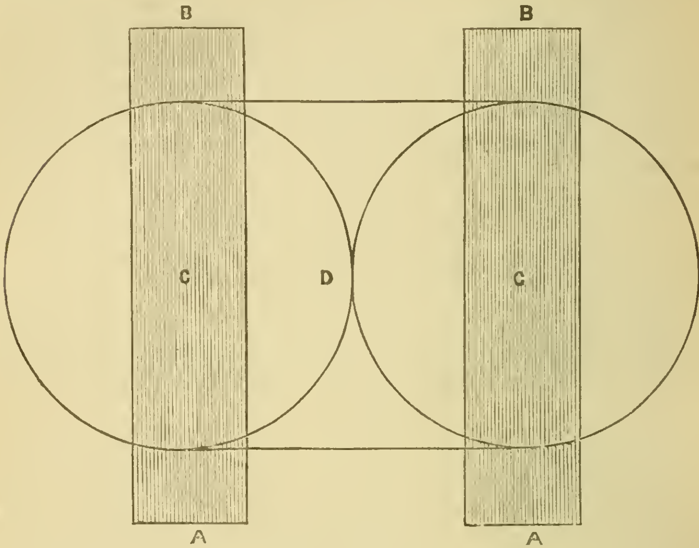
Let A B, A B be a pair of Nobert's lines of Band VIII., about 50,000 per inch; C C, the centres of two spherules, whose diameter is the same, *viz.* 1–50000th. Now the visibility of these spherules

* This eye-piece is described at page 5 of the last number of Journal. When the "lens-micrometer" *is divided* 100 *per inch*, the power 675 is indicated at sight by the stage micrometer, but *at* 200 *per inch*, it would read, of course, 1375 instead of 675.

† Tolles' immersion $\frac{1}{10}$th with a power of 800 showed the 8th band with central light, which rates at about 50,000 per inch. Wales' ⅕th with a power of 475 showed them with oblique sunlight: a fine performance.

‡ The letters A A, B B in the diagram should have been engraved at points close to the lines touching the circles; moreover, *the fine lines drawn by the engraver are merely intended as a shading, A C B in each case representing two consecutive single lines of a band.*

depends chiefly on the curved areas, B D B, A D A, formed by completing the square, A B, B A, and in the difference between the areas of the two inscribed semicircles and the square, *i. e.*



between the area of a square and its inscribed circle, *i. e.* between 1·0000 and 0·7854 for a square of diameter 1, or 0·2146, *i. e.* one-fifth nearly; so that the combined areas of these curved notches are only about $\frac{1}{5}$th of the corresponding square, or, allowing for the breadth of a Nobert's line, we may at least say *one-fourth.* The double notches therefore which really enable the eye in such minute objects to discriminate the beaded form, are four times smaller in area than the space corresponding to Nobert's lines ruled at a similar rate.

On precisely the same principle, when first diatoms began to be studied, the lines formed by shadows disposed in lines were much more easily made out than the beading projecting the shadows. It would be interesting if some of our Fellows would work at this question of the comparative easiness of distinguishing lines drawn at the same rate of closeness, or of defining rows of spherules in close contact. The retina repeats, as it were, the sensation all along the line which is seen; and a much fainter *line* can be distinguished than dots in close proximity, which if dotted on paper and removed to a distance quickly run into a line, whilst finer dark lines drawn at the same intervals as the dots still remain visible. Of course the case is quite altered if brilliant dots be used, as these, if widely separated and very brilliant, can be seen almost as easily as lines, especially if the eye glances near them rather than at them directly, as well known to astronomers.

Nobert's New Bands are indicated to be from $\frac{1}{1000}$ to $\frac{1}{10000}$th of a Paris Line. Now, according to Babbage, the French foot is equal to 1·0657654 English foot, and the *line* is the 1–12th of a *pouce*, which is the 1–12th of a French foot. By these data I find the French line is 0·088813783 English inch, and not 0·088815, as generally given. This makes some difference in the *assigned* English divisions per inch, and for those who may feel interested in comparing the visibility of Nobert's Bands with rows of spherules in contact of the same category, *viz.* so many to the inch, I now add the result of some calculations accurately verified. (The decimals are given merely to show the care taken.)

Band.	No. of Spaces per inch.	Band.	No. of Spaces per inch.
I.	11,259·51358	XI.	67,557·08148
III.	22,519·02716	XIII.	78,816·59506
IV.	33,778·54074	XV.	90,076·10864
VII.	45,038·05432	XVII.	101,335·62222
IX.	56,297·56790	XIX.	112,595·13580

A very careful examination of the ribs of the *Atalanta Vanessa* (Red Admiral) has enabled me to distinguish the molecules of which they are composed, *viz.* minute beads of the same size as the transverse trios, crowded thickly together. Two rows of beads to a rib would not exactly represent the appearance, as they are arranged on a grooved surface, which, from the action of oil-drops running along, seems to resemble a corrugated galvanized roof. In many cases, when a little pressure has been applied to the dry slide, the natural oil of the scale exudes. Under *a Sixteenth*, touching the glass, the play of this oil is remarkable, and in my glasses greatly improves the definition when it spreads between the scales and the cover. I have seen these component beads, or molecules, with a quarter condenser and an uncovered moderator lamp.

On the Resolution of the Lepisma Saccharina Scale.

The *Lepisma Saccharina* scale is far more difficult.

1. The same arrangement with Powell and Lealand's new $\frac{1}{8}$th immersion reduces the size of the ribs, making them finer than inferior glasses, but with direct light they appear structureless; between them, in the intercostal spaces, a faint irregular structure is visible. Power 1200.

With slight obliquity of illumination, these clear spaces began to appear lumpy, broken up here and there with irregular bodies. Removing the "quarter" condenser, I replaced it by Powell's achr. condenser, one half of which was covered with green paper. After searching among the scales, and rotating them with the axis towards the light, I was rewarded with the appearance between the second, third, and fourth spaces at the round end of a large scale, of two rows of beads of a red colour taking directions exactly radiating from the quill.

At first the ribs are bluish, the obliquely radiating rows of beads are reddish, and much more numerous than the upper parallel ribs, which show also a beaded structure. More fortunate resolution shows the reddish beads of the radiating class intervened with whitish, which are best seen at the sides of the scale when the radiating ribs cross the parallel at about an angle of 35°. In this position about *five* beads can be counted crossing between the upper parallel ribs. As before, those scales are the easiest to resolve which lie adherent to the cover. Those with a film of air between require a different correction for spherical observation.

In some cases a good definition shows black ribs and bright radiating ribs of the same size, with red interspaces. At this degree of fine definition the edging of the scale at the round end is a clean black line, and the ribs project slightly, like black points.

2. A eye-piece; Powell and Lealand's $\frac{1}{16}$th dry; same illumination. Upper ribs show very small and clear, shaded with two black lines very sharp. At the places where the lower radiations cross them, the black lines are obliterated, so as to give a peculiar shaded *twisted* appearance, alternating with blue and red shading. On rotating the scale so that its axis forms an angle of 15° with the light, the lower radiating ribs, where they cross the upper at an angle of 40°, also show black lines precisely similar to the others; both sets of shadows, of a sharp, clear, decisive character, being seen at once, forming an elegant black lattice-work, the red beaded rib below colouring the upper ribs at the parts where they intersect.

A very fine phenomenon is exhibited of the effect of the ribs crossing obliquely, which appears to me a perfectly satisfactory proof of the nature of the structure, *viz.* obliteration of the black line double shadows of both sets of ribbing at the places of intersection; also the colours of the lower ribs glistening there through the upper at graduated intervals, just as are seen when a fine colourless rod of glass is obliquely crossed upon a pink one; still more so when a series of such rods are crossed in radiating positions.

I have the more pleasure in describing this interesting scale, because this resolution demonstrates the very great advance in microscopic definition since the time when the ribs of the scale were described as translucent and merely an appearance of a dot at the intersection of the lines, whereas there are scores of beads between the ribs in a very small space. At the same time, I may be excused for making a few animadversions on the subject of spurious beads and ghost beads of which so much has lately been said.

It has been roundly stated that, *when striæ intersect, spurious beads are produced or seen at their intersections. And that this is especially the case with the crossing striæ of the Lepisma.*

To this I reply, a finer glass dissipates these ghost beads entirely, and replaces them by others very much smaller and at least ten times more numerous. These ghost beads demonstrate (not the existence of the smaller reality but) the absurd correction and errors of the microscope itself.

The lovers of these ghosts may stick to their spurious showings and prefer them, whilst a wealth of fascinating beauty lies hid from their gaze. Residuary aberration still hovers about our best glasses. Perfect achromatism and perfect correction of spherical aberration are at present almost incongruous. Correct the one, then the other appears. Get rid of the colour totally and you will see woolly and blurred outlines, thick edges, and spurious beads or ghosts. What is the pleasure or advantage of seeing for instance say forty spurious beads in the Lepisma, when these forty should vanish and make room for four hundred realities—rows upon rows of tiny, shiny gems instead of a few non-existent spurious images? The intercostal spaces of the Lepisma, like the Podura curvicollis, teem with closely-packed rows of beading: admirers of Podura nails can see nothing in the blank spaces: and champions of the Lepisma ghosts can see nothing there between the ribs, but ghostly shams of intersecting striæ. In each, these blank spaces are full; but not till the aberrating obnoxious rays are controlled can these tiny strings of pearls start into view. A greenish-blue sky above a setting sun, for instance, gives comparatively few aberrating rays in my 1-50th immersion. The more colours the light contains, the more difficult becomes the destruction of the aberration. The finest effects in Colonel Woodward's photography could only be produced when the number of the colours was reduced to the blue ray which now alone exactly suits his photography. But we use all mixed together in our daily researches, and involve ourselves in the mists of residuary aberration.

It is when the ribs appear sharper, thinner, and the intercostal spaces become broader; when sharp, keen black lines take the place of blurred thick outlines; when the quill is keenly portrayed and the serrations finely pronounced,—it is then only that the aberration approaches a minimum, and then that we may expect to see with a little obliquity of light the gratifying sight of that inner structure which has hitherto eluded the eyes of the most diligent microscopists of modern times.

VII.—*The History of the Micro-spectroscope.*

By John Browning, F.R.A.S.

Dr. Gayer has kindly afforded me an opportunity of trying the micro-spectroscope he has contrived and described in the last number of the 'Monthly Microscopical Journal,' the performance of which is very satisfactory. It appears to me only just that I should recall to the members of the Society the fact, that in consequence of a suggestion I received from Dr. Huggins, I have made a micro-spectroscope exactly similar in optical construction several years since. The principal difference in the mechanical details of the instrument I used was that my spectroscope could be inserted in the body of a microscope, and removed as in the case of an ordinary eye-piece, thus avoiding the necessity of another body for the microscope, and reducing considerably the cost of the apparatus. There is an engraving of a contrivance such as I allude to, on page 120, figure 70, in the 6th edition of Mr. Hogg's work on the Microscope.

My method of finding the object in this micro-spectroscope was as follows: I had a small slot in the body of the microscope and a projecting pin on the adapting tube of the micro-spectroscope. I had an eye-piece with a point or indicator in the field of view. This eye-piece had a steady pin on the side of the tube, corresponding to that on the spectroscope. On finding and focussing any small object, bringing it to coincide with this point, removing the eye-piece and substituting the spectroscope, the spectrum of the object was visible at once.

NEW BOOKS, WITH SHORT NOTICES.

A Manual of Microscopic Mounting, with Notes on the Collection and Examination of Objects. By John H. Martin. London: J. and A. Churchill, 1872.—There can be very little doubt that the general microscopical student requires a new work or a new edition of some of the older books on the subject of the preparation and mounting of microscopic objects. The old books, though admirable each in its own special department, are, nevertheless, devoid of all that has been done in the way of work and apparatus for the past few years. It is not therefore surprising that persons like the author of the book under notice should—thinking themselves the ones who are chosen for the task—give us the results of their experience. But we are sorry to say that Mr. J. H. Martin is about the very last who should be selected for so important a duty, and that his book, though it may possess some three or four points of interest, is the very worst possible companion that can be placed in the hands of a student. For ourselves, we had hopes that a new edition of a recent work would have been all that could be desired, but, unfortunately, that is not the case. So we are left to the desire that Dr. Carpenter may, either by himself or some one perfectly skilled in the subject, bring out a new edition of his excellent treatise on the microscope; for assuredly, as regards the wants of the general student, it is the very best and most abundantly illustrated work on the subject, that any language possesses. In the meantime we cannot accept books like the present one as offering at all what we require, for, in addition to the fact that the present treatise is most limited in outline and heterogeneous in plan, it is certainly almost the worst illustrated book we have ever beheld. It contains in the commencement some useful remarks on some of the apparatus to be employed by the worker, and a few good cuts accompanying the text, but beyond this it is really unworthy of any serious criticism. The plates are simply horrible, and the alphabetical arrangement of the subject is an absurd one. We are sorry to have to speak so distinctly, but we fear we should be but pretending to discharge our task were we to commend the essay to our readers' notice.

The Micrographic Dictionary. A Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects. Third Edition. By J. W. Griffith, M.D.; the Rev. M. J. Berkeley, M.A., F.L.S.; and T. Rupert Jones, F.R.S. London, Van Voorst, 1872.—Parts VIII., IX., and X. of this work have lately appeared, and they bring the work down to *Equisetum*, thus showing how much more remains to be done. It is to be regretted that the publisher cannot produce the book more rapidly, for behindhand as the earlier numbers unquestionably are, they will become doubly so if it is not to be completed till four years after they have been issued. We trust, therefore, that he will use his best endeavours in this direction, and further, that he will insist on another editor being added to the list which is already published. We speak in this matter from the most perfectly disinterested motives, but we cannot see a really capital work spoiled through the proprietor's neglect of modern work and modern writers. On the whole, it strikes us that the numbers now upon our table are better than their predecessors. There has been more effort to bring new light upon the subject than was shown in the commencement, and we can only rejoice that it is so. We notice, for example, that the subject of *Diatomaci* has been more fully dealt with than we had expected. A good deal of space has been given to this subject, but, in our opinion, not enough, so important is it if only regarded in the light of the test of microscopic power. It appears to us, too, as if the subject were dealt with by one who was deeply in love with the old notions, and could only give the more modern views as other ideas, but by no means the correct ones. Still, this section is evidently written by one who is a master, and we are glad that he has even stated the views of more recent authorities. A good "diatom" plate accompanies the tenth portion of the Dictionary, in which, however, it must be observed that the writer's views of structure are distinctly adhered to. The allied subject of Desmids is not so fairly dealt with. A good deal of foreign work has been done upon this branch of late years, yet we find nothing whatever of it in the Dictionary. Elastic tissue is by no means satisfactory. The illustrations are all old, and the idea of treating of the branches of the subject as though distinct, is manifestly antique. Endosmosis, too, is a subject which requires to be much more fully dealt with than it has been under the present heading. *Entomostraca* is wonderfully well treated, the remarks being terse, to the point, and modern; whilst the references to work done at the subject by others is both full and modern. Entozoa, too, is not bad; but, in our opinion, it will be regretted if it is not dealt with in the succeeding parts of the Dictionary more fully. *Eozoon Canadense* is, of course, a new paragraph. It is, however, extremely short, and gives no very full description of this interesting fossil. We should have wished to have seen a plate with some of Dr. Carpenter's admirable drawings of this remarkable *Rhizopod*, but as yet none has appeared. The cuts throughout the pages are too manifestly those only of two or three groups; why, we cannot say, but clearly it is a somewhat prejudicial and unfair distribution. The plates, too, are immensely too crowded; in most instances they contain some of Tuffen West's best drawing, and these, of course, are in most cases excellent, but in other instances they are not so well executed; and further, they in some degree but very poorly represent the progress which has been made. With these several defects—and of course we have dwelt upon them—the book is, nevertheless, an improvement on the old edition, and we doubt not that the succeeding parts will be even better far than those which have already been published.

Various foreign memoirs:—'Nuove Ricerche sull' interna tessitura dei Tendini del Prof. G. V. Ciaccio.' Bologna, 1872.—In this paper, which the author has been kind enough to send us, there is given a very full account of all the modern work that has been done upon the histology of tendon and other varieties of the more distinct connective tissues; that is, upon those forms of the tissue which essentially consist of the white or inelastic structure. Besides giving the views of many other workers on this subject, he enters upon the distinctions between the views held by Herr Boll, who published a paper in the 'Archiv für Mikroscopische Anatomie' (one of the numbers for 1871), and M. Ranville, who published his opinion in the 'Archives de Physiologie' (Part II., 1869). After discussing these views he then sums up his own, and gives an admirable plate in illustration of his opinions.

When the two following reached us last year, we gave them to a friend to notice. He, however, has been since out of the country, and hence they have been forgotten. We now can only give the titles, and express our regret that they were not noticed earlier:—

'Su la illuminazione monochromatica del microscopio e la fotomicrografia e loro utilità Nota del Sig. Conte Abate Francesco Castracane.' Roma, 1871. And also 'Esame microscopico e note critiche su un campione di fango atlantico ottennto nella spedizione del "Porcupine" nell' anno 1869; Memoria dell' Ab. F. C. Castracane.' The first memoir is accompanied by an admirable photographic plate, the best we think we have ever seen, even Colonel Woodward's being, if anything, inferior. It represents three species of Diatoms, *Arachnodiscus ornatus*, *Cocconeis punctatissima*, and a species of *Eupodiscus*. They are reproduced by the well-known Albert-type, and exhibit the elevations on *Cocconeis* admirably. The second memoir relates particularly to the various diatoms collected by the expedition.

[We regret that owing to the late date at which we received Dr. Bastian's book from Messrs. Macmillan, we are compelled to postpone our notice to the next number.]

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of the White Blood Corpuscle.—Dr. Richardson, of America, has recently published a very able paper on this subject, from which the following may be taken as a summary:—The white blood corpuscle is a cell composed of, in the first place, a nucleus (or nuclei) which possesses the power of independent amœboid movement, and is insoluble in water, but capable of slowly imbibing that fluid until swollen to nearly double its normal size. The cell wall of the corpuscle is a membranous envelope, insoluble in water even when boiling, too thin to exhibit a double contour with a magnifying power of 1200 diameters, but firm enough to restrict the movement of its contained granules within its limits. Its exterior is adhesive, so that surfaces or particles coming in contact are liable to become attached thereto. Some phenomena observed lend countenance to a theory that this membrane is dotted with minute pores which permit delicate threads of the soft protoplasm it encloses to be extruded, and that the edges of these foramina, if the projection still continues, are carried outwards during the amœboid movement, forming a sheath to all except the extreme point of the tongue-like process. The material occupying the space between the capsule and the nucleus, denominated the protoplasm of the cell (the fibrino-plastin of Prof. Heynsius), is a soft jelly-like matter in which chiefly resides the capacity of amœboid motion. This protoplasm appears to be soluble in water and saline solutions in all proportions, and when freely diluted loses its amœboid power, which, however, is regained in a majority of cases, when the excess of fluid is removed. The laws by which leucocytes take up and part with fluid seem to be simply those of the dialysis of liquids through animal membranes by endosmosis and exosmosis, as investigated on a larger scale by Graham in 1855; the rapid inward current from the rare solution of high diffusive power, through the cell wall, distending that membrane and diluting the contained fluid, until an equilibrium of the endosmotic and exosmotic flow is attained, or the capsule is burst by the centrifugal pressure of accumulated liquid. "The structure of the particles which exist in the protoplasm and exhibit dancing motions when the latter undergoes dilution is yet undetermined, although sundry facts indicate that their movement is not dependent on 'vital' causes, but is merely a molecular one; also that some of them, at least, are minute granules of fatty matter which after a time may coalesce into visible oil globules, as in the older pus corpuscles. In regard to any difference of their motion in the salivary bodies, my experiments as above detailed so fully and uniformly corroborate each other, that, reluctant as I feel to dispute the assertions of such celebrated histologists as Stricker and Pflüger, I cannot but call in question the general correctness of their statement upon this point; for it is manifestly inaccurate to affirm that a half to one per cent. salt solution still permits the 'dancing' movements of fresh pus or lymph corpuscles to continue, when the fact is that the motion ceases in nineteen out of every twenty globules under its action, just as it would be erroneous to maintain that quinine does not stop the course of ague, because in one case out of twenty it fails to prevent a recurrence of the chill. I therefore am induced to think, from the above investigations, which I trust any critic will do me the justice to repeat before disputing, that, contrary to the views of these histologists, no essential difference exists in the effects of salt solutions of various strengths upon the salivary, pus, and white blood corpuscles; and from this circumstance, in conjunction with the interesting fact discovered during one of my experiments, that the salivary globules, when acted upon by the denser saline liquid, *contract to the size of the blood leucocytes, and manifest amœboid movements*, I conclude my theory, that the corpuscles of the saliva are 'migrating' white blood globules, which, 'wandering out' into the oral cavity, have become distended by the endosmosis of the rarer fluid in which they float, may now be considered established upon a firm experimental basis."—*Transactions of the American Medical Association.*

Diatoms in Hot Water.—It is not a novel discovery to find diatoms in hot water. Still Dr. Blake's recent paper, read before the 'California Academy of Sciences,' is not without interest. He says that the forms were very abundant in water of a temperature of 163° Fahr.,* a small portion of the mud not larger than a pin's head containing many hundred individuals, amongst which could be recognized more than fifty different species. The most interesting point connected with the discovery of these diatoms was their almost perfect identity with the species found in beds of infusorial earth in Utah territory. Most of them were identical with the fossil species described by Ehrenberg, from near Salt Lake, but many were new. He found about fifty-two species, of which thirty are the same as Ehrenberg's, who mentioned about sixty-eight. So close was their identity, that there can be but little doubt that the Utah beds must have been formed in an inland sea, whose temperature was probably about the same as that of the water of the Pueblo spring. The fact that these diatoms can grow in such abundance in water of so high a temperature, affords an explanation of the total absence of every other form of organized beings in the infusorial beds, as such an elevated temperature would be totally incompatible with the existence of every other form of living beings. The time at which these infusorial beds were deposited was probably, in the author's opinion, during the Miocene period, as we have evidence that at that period Spitzbergen and other Arctic regions had a temperature of some fifty or sixty degrees above that of their present climate.

Columnar Dolerite under the Microscope.—At the same meeting of the California Academy as that above referred to, Dr. Blake showed the advantage of the microscope to the mineralogist. Not being satisfied with the opinion of Mr. Durand regarding a mineral which he found near Black Rock, Nevada, he says:—"I prepared a thin section of one of the crystals, or crystalline prisms, and looked at it through the microscope. This section I now present to the Academy, and even an examination of it with the naked eye suffices to prove that it is a compound rock, made up of heterogeneous substances, imbedded in a dark greenish matrix. With the microscope we detect crystals of augite, nephiline or labradorite, and titanite. I believe the transparent crystals are nephiline; so perfect is their transparency, that at first I concluded that they were holes where the rock had been ground out; but on using polarized light, I discovered they were doubly refracting crystals. I am not aware that any of these compound rocks have been found in such small prisms. Some of the smaller prisms measure not more than 0·10 inch across, and in the specimens I have some three to four inches long. There can be no doubt but that originally they were aggregated into masses, and that their separation is the result of weathering; in fact, amongst my specimens are some in which many prisms are still attached to each other."

* A hot spring in Pueblo Valley.

The Microscopic Examination of Cotton.—We have received a copy of a paper which was read before the Cotton Brokers' Association of Liverpool, by the Rev. H. H. Higgins, M.A., on this subject, which is admirably illustrated with a series of twelve photographic microscopic views of the fibre. Although there is not very much novelty in the paper, still it is interesting, and to many instructive. The following quotation which he gives from Barnes' work on 'The History of the Cotton Manufacture of Great Britain' is not without interest:—"Rouelle, in the 'Memoirs of the French Academy of Sciences' in 1750, and Dr. Hadley, in the 'Philosophical Transactions' in 1764, had contended that the mummy cloth of Egypt was cotton; and so it was esteemed to be till, in 1836, James Thomson, F.R.S., having obtained from Belzoni various specimens of mummy cloth, determined to renew the investigation. All other methods failing to afford a satisfactory solution of the difficulty, he bethought himself of the microscope. He was not, however, possessed of such an instrument, nor was he accustomed to its use. Mr. Thomson therefore applied to his friend Mr. Childern, who undertook to secure the good offices of Sir Everard Home in prevailing on Mr. Bauer, a microscopist, to make the requisite examination. Thus a Fellow of the Royal Society, less than forty years ago, found himself three removes from an authority competent to resolve a question capable of being decided in a few moments by the aid of a very ordinary microscope." The author sums up the advantages of the microscope in the examination of cotton thus:—"(1.) It may probably be found useful chiefly in deciding questions of some difficulty: for example, in comparing various kinds of Surat, or North American cottons. Where two samples are apparently equal in value and suitability, the microscope may give a decided preference to one of them; and a decision thus formed would probably be justified by the manufacturer. It is not altogether improbable that the microscope may lead to a more correct appreciation of some kinds of cotton which may hitherto have been under-rated or over-rated. (2.) The microscope may greatly facilitate and generalize the power of judging cotton."

Larvæ in the Human Ear.—The 'Lancet' of Dec. 14, 1872, contains an article on this subject which is of interest in connection with the subject mentioned in the last 'H. M. J.' It appears that in a recent number of the 'Archives of Ophthalmology and Otology'—a publication brought out simultaneously in English and German—Dr. Blake, of Boston, describes four cases of the occurrence of dipterous larvæ in the human ear. In one of these cases the larva was that of *Muscida sarcophaga*, and in another case that of *M. lucilia*, which was probably also present in the fourth case. The presence of these larvæ is always associated with an otitis media purulenta, which can hardly be wondered at if we bear in mind the nasty predilections of the ordinary blow-fly. *M. lucilia* is oviparous; from which may be explained the fact that in the cases in which it was present only a single larva was found, most of the eggs having been presumably washed away by the fetid secretions from the ear. *M. sarcophaga*, on the other hand, whose larvæ were more numerous, five and four being respectively present in the cases recorded, and being visible as "a white undulating mass," is viviparous, the larva being seen at birth to protrude from the abdomen of the fly, and move its head as if in search of some point of attachment; and, should a piece of meat be presented to it, "the mandibles are driven into it, and the larva withdraws itself from the body of the mother, and is immediately followed by another and another, until several have been delivered." The intense deep-seated pain and the streaks of blood which accompanied the purulent discharge were easily accounted for when a larva was examined under the microscope, after being included between the bottom of a glass beaker and a piece of meat; the animal being seen to be armed with a pair of strong mandibular hooks, articulated with a kind of chitinous endo-skeleton lying within the anterior rings of the body. When the larva strikes its prey, the integument of the rings is made tense, the mandibles being thereby extended and fixed firmly in that position on the framework; the anterior rings are then retracted as a whole, "and thrust quickly forwards with a force often sufficient to bury the claws up to their roots." This manœuvre is rapidly repeated, while the body is simultaneously dilated, the sharp backwardly-directed papillæ which clothe it giving it an additional hold. "We can hardly, then," says the 'Lancet,' "be surprised that these pests cannot be dislodged merely by the process of syringing, but that they must be picked out singly with a forceps, and that after their removal the intense pain ceases and the blood-streaks in the discharge disappear."

The Anatomy of the Cerebellum.—One of the best articles that have ever appeared on this subject is one which is given * in the 'British Medical Journal' for November 30, 1872. We congratulate the 'Journal' on its appearance, as we hope that this article is but one of a series which the editor proposes to give. It states that, according to M. Luys, the apparatus of cerebellar innervation constitutes a subsystem clearly isolated, in spite of its close connection, on the one hand, with the cerebral system properly so-called, and, on the other, with the spinal system. It is a true central apparatus, engendering and distributing the cerebellar influence by a special conductive system to an apparatus of peripheral reception. He compares this disposition to that of the circulating system. The cerebellum represents the heart as the central apparatus; the peduncles which emerge from it are the analogues of the arteries; and, finally, the capillary network of arteries can alone give an idea of the diffusion of the plexuses of grey peripheral matter, in which the terminations of the peduncular fibres are lost. The disposition of the cerebellar hemispheres recalls and reproduces that of the cerebral hemispheres. From the periphery of the cerebellum (cerebellar convolutions) start converging fibres, which terminate in a grey central nucleus (rhomboidal body of the cerebellum). From this nucleus spring three bundles of efferent fibres, constituting the superior, middle, and inferior cerebellar peduncles. These peduncles all pass forwards and mingle with the elements of the nervous system, which appear to have

* It is No. II. in order.

exclusive relations with the motor functions. All three intercross at the median line with their congeners, and pass then into masses of nerve cells, constituting nuclei spread out over the whole of the height of the encephalic isthmus. Finally, from these nuclei start grey fibres, which throw themselves into a special nervous substance, described for the first time by M. Luys under the name of peripheral cerebellar substance. In this way the inferior cerebellar peduncles terminate, after decussation, in the grey masses of the olivary body (anterior and inferior olive of M. Luys). This anatomist has found in the thickness of the posterior pyramid an analogous nucleus of grey matter, which enters similarly into connection with the inferior cerebellar peduncles. This is the inferior and posterior olive. The median peduncular fibres cross each other also in the median line, and pass into the grey matter of the protuberance. Finally, the superior cerebellar peduncles, after a similar intercrossing, terminate in a grey mass, already described by Stilling as the red nucleus, and which M. Luys proposes to call superior olive, seeing its striking analogy with the grey nucleus of the bulbar olive. From these different nuclei start grey fibrils, the ultimate termination of the peduncular fibres, which put themselves in connection with the fibres of the anterior spinal system, by the medium of the peripheral cerebellar substance. The latter extends on the neck of the bulb as far as the corpus striatum in a continuous layer, presenting here and there swellings, the most apparent of which is the *locus niger* of Soemmering. Histologically, this grey substance is constituted by a network of polygonal cells, provided with numerous prolongations. It is probably by the medium of these branching and anastomosing cells that the connections between the fibres of the cerebellar apparatus and those of the spinal apparatus are established. So far as concerns the latter, M. Luys is almost entirely in accordance with accepted opinions. It should be mentioned, however, that the posterior sensitive fibres of that apparatus are shown in his drawings as all terminating in one ganglion. From this ganglion start two orders of fibres. Those of one order connect themselves with the grey centre of the spinal marrow, ganglio-spinal fibres. These are properly the reflex motor fibres. The others take an ascending direction, cross each other at the level of the bulb, and, according to all appearances, terminate in the optic thalamus (ganglio-cerebral fibres). As to the anterior or motor fibres, they emerge from the large asteroid cells of the grey matter of the anterior horns. Towards the bulb and the protuberance, these cells are agglomerated, and unite in masses to form the grey nuclei whence spring the fibres of origin of the cranial motor nerves, hypoglossal, facial, external oculo-motor, masticatory, &c. Finally, the cells of the anterior horns unite together, and with the nuclei of the corpus striatum, by fibres of which the *ensemble* constitutes the antero-lateral column, and, after decussation at the level of the bulb, the cerebral peduncles. Combining the whole of these details, the following general conception is arrived at by this author. In respect to the sensitive fibres, a part of them, after having traversed the spinal ganglion, take their course directly into the large cells of the marrow. The impression in this case is not felt; the activity of the spinal cord only is called into play, and only a simple reflexion results. The other sensitive fibres, on the contrary, pass directly into the optic thalamus; there the impressions undergo a first relay; then they are radiated by the converging cerebral fibres towards the cells of the convolutions, when they become the materials of the ulterior operations of the intelligence. The will, thus awakened, enters into activity, and transmits, in its turn, its determinations along the path of the converging fibres, especially the cortico-striate fibres. The voluntary impulse thus arrives at the striated body; it is there reinforced; thence sets out again along the cerebral peduncles and the grey axis of the cord, and finally proceeds to excite the roots of the motor nerves, and to determine movement. As to the cerebellar apparatus placed at the confluence of the cerebral and spinal systems, it constitutes a sort of reservoir of nervous influence—an influence which appears to spread itself incessantly along the anterior spinal system, and to expend itself in answer to each appeal that reaches it from the superior voluntary centres. In the normal state, the distribution of the cerebellar influx in each half of the body maintains the physiological equilibrium. If this uniform distribution be disturbed, there appear immediately incoordinated movements, blind and irresistible influence, vertigo, and a veritable titubation.

Is the Source of Ague discovered?—Dr. Salisbury, of the United States, thought it was proved by him some years ago,* by his remarkable discovery of the malarial essence in the cells of certain Palmelloid plants. Desiring to investigate the subject, Dr. John Bartlet says that he sought for the plants described by Dr. Salisbury, in the ague bottom of the Mississippi River, opposite Keokuk, Iowa, lat. 10° 25′. Not being provided with a suitable microscope, he was unable to discover the microscopic algæ described by the Doctor. He was pleased, however, to find the fungi, samples of which he has sent to Mr. Cooke, the editor of the Journal to which he addresses his letter. Generally it answers Salisbury's description. It does not correspond in these important particulars: Salisbury's plants are so minute that it requires a powerful lens to render them visible. A single specimen of plant may be discovered as you stand. Salisbury's plants were not less (?). These have roots $\frac{1}{8}$ or $\frac{3}{16}$ of an inch in length. They grow on the flat moist alluvium of the slough and river margins and their drying beds; in the vicinity of such localities they may be found on ordinary soil in damp places, even at some elevation. "The specimens sent you are green; I have observed them slate-coloured, pink, and black. They vary in size from a mere point to $\frac{3}{32}$ of an inch in diameter. When in natural state they are globular in shape and of a fresh colour; when covered with water they swell and present a gelatinous appearance. They discharge their spores when ripe by slitting open at the top, and a falling in, collapsing of the upper circumference: so that a discharged plant appears cup-shaped, and to the naked eye it seems to have lost the upper half of its circumference. So far as I have been able to determine with the imperfect means of observation at my command,

* 'American Journal of Medical Science,' 1866.

the cells are composed of two walls, the outer green (or otherwise coloured), composed of laminated cells, the inner white and structureless. Upon puncturing the plants a liquid is forcibly ejected. I have never been able to discover the contained cells for want of a good microscope. By placing the cake of earth sent you in a plate, and adding water enough to make it of about the consistence of potter's clay, and keeping it at a temperature about 60°, you will find a fresh crop of the plant to develop, and you will thus have an opportunity of studying them."—*Grevillea*, Dec. 1872.

The Structure of the Nerve Fibres.—In the 'Centralblatt,' * Dr. Tamamschef gives the results of his researches on this point, which have been recently translated in the 'Lancet,' † which we may mention usually contains, of late, something of interest to the microscopist. Dr. Tamamschef's specimens have chiefly been taken from the lumbar, sciatic, and brachial plexuses of man and of the mouse. Those from the latter, immediately after their removal from the living body, were plunged either into distilled water or into serum or the aqueous humour, and examined with high powers. Many nerve tubules, he finds, are united together and enclosed by a common sheath composed of flat shells, which may be brought into view by a solution of nitrate of silver. This sheath probably belongs to the lymphatic system. The nerve tubules are composed of an external sheath or neurilemma, the white substance of Schwann, and the axis cylinder. On the careful addition of ammonia and then of acetic acid to the nerves on the stage of the microscope, it may be shown that the cylinder axis is composed of a completely homogeneous matrix, which dissolves readily in ammonia, and in which spheroidal bodies gradually in about three-quarters of an hour make their appearance, which he proposes to term nerve corpuscles—corpuscula nervea. The corpuscles are in contact with one another throughout the whole length of the tube, and are capable of spontaneous movement; their size is nearly equal to that of the red corpuscles of the blood, and they may with cautious manipulation be obtained altogether detached from the nerve tubules under the influence of various reagents; they break up into minute granules, which exhibit Brunonian movements in oil of turpentine. In order to determine whether the cylinder axis belongs—as is usually thought—to the albuminous compounds, M. Tamamschef undertook a comparative series of micro-chemical investigations between fresh albumen and these cylinder axes. He finds that pure albumen, in the course of two or three days, exhibits the same kind of resolution into spheroidal elements, more or less nearly approximating in size those of the cylinder axis. After the addition of alkalies and acids, however, the similarity is no longer perceptible. Alkalies, and especially ammonia, cause the corpuscles to swell up, and several granules make their appearance, which are capable of moving in a concentric manner; sometimes a triple zone appears, of which the internal appears reddish, the middle greenish blue, and the external dark-violet. Acids cause the muscles to shrivel, and finally to break up into numerous

* No. 38, 1872.

† December 14, 1872.

granules. The following tables give M. Tamamschef's results in a condensed form:—

	Axis-cylinder Matrix.	Albumen Matrix.
1. Ammonia	Dissolves	Dissolves.
2. Weak alkalies (potash).	Dissolves	Dissolves.
3. Carmine	Uncoloured	Uncoloured.

	Corpuscles.	Corpuscles.
1. Ammonia	Action slow, distensive ..	Action slow, distensive; prismatic colours.
2. Concentrated alkalies (potash).	Rapid distension; gradual disappearance.	Rapid distension; sudden disappearance.
3. Acids: chromic, acetic.	Gradual shrivelling; appearance of granules, and their disintegration.	Rapid shrivelling, the corpuscles becoming irregularly angular; disintegration of granules.
4. Turpentine ..	Persistent oscillation of granules.	Persistent oscillation of granules.
5. Carmine	Feeble coloration	Very slow and feeble coloration.

The Surface of Botrydium granulatum.—Mr. E. Pigott has an important paper on this plant in 'Grevillea' for January, 1873, giving it the specific name of *granulatum*. He says that, "as Dr. Greville and others have said, its surface seems minutely granular. Now this I have ascertained by careful microscopic examination to be not external, although the effect is seen on the surface of the vesicles, but it results from the pressure of the protoplasm and grains of chlorophyll on the inside. The membranes composing the walls of the vesicles, for there are two, an outer and an inner membrane, although this cannot be ascertained with certainty, except at the base of the vesicles and where the inner membrane begins to dry up when it shows in folds, by carrying, and the breaking up of the endochrome, into folds with it, and in the underground stems, where they are distinctly visible, they appear to me to be perfectly structureless, that is, they are thin transparent membranes only, without any cellular structure, and when the plant is alive they remain distended to their very utmost from the pressure of the fluid within. The young vesicle which, as will be observed, only the swollen apex of a branch of the creeping or underground stem, when it emerges from the ground it is frequently only a clear transparent sac filled almost to bursting with a watery fluid; after a time minute green spherical grains will be seen, mostly adhering in little groups to each other, and at length they take up their position on the wall of the inner membrane, until the whole vesicle appears to be filled with them; but the vesicle being filled with them is only in appearance, as it is only the walls that are covered, with a few exceptions of granules floating in the fluid. When a full-grown plant is pressed between slips of glass and examined, the membranes composing the vesicle will be seen to shrink up into folds, on which are seen the adhering granules. When the plants are full grown the epidermis is furfuraceous, or having a number of minute scale-like processes attached to it, as if it were a very thin outer membrane broken up." A very able note is appended to the paper by Mr. W. Archer, of Dublin, in which this authority criticizes some of the author's statements.

Delhi Ulcers.—We have received from the author, Dr. Fleming, a short note on these ulcers, in which he says that last year a partial microscopical examination of the ulcers, which affect the nose of most dogs in Delhi, was made in connection with a few experiments to ascertain their nature. The result of this investigation showed that they were similar to those which occur on the human subject, and also proved that dogs, as well as men, can easily be inoculated by the cellular substance from an undoubted Delhi sore. Delhi ulcers have been proved to be contagious.

The Bailey Diatoms at the Boston Society of Natural History.—Prof. H. L. Smith writes in the 'Lens' (Nov., 1872) with regard to these. He states that they are not at all equal to what Mr. Bailey possessed when alive. Hence he thinks that they have been stolen or taken away. He says it "is well known that Prof. J. W. Bailey bequeathed his microscopical collection, his collection of Algæ, his books on botany and microscopy, his memoranda and scientific correspondence, to the Boston Society of Natural History. The Algæ, books, and memoranda are, I believe, still about as Prof. Bailey left them; but the slides of Diatomaceæ, and more especially the crude materials left by him, have not been so fortunate in escaping the grasp of greedy collectors. Perhaps I am mistaken, but either the collection of Prof. Bailey, which he gave to the Society, was much more meagre than that I had seen at his own rooms at West Point, or it has suffered since its deposit. I believe it was the wish of Prof. Bailey that the crude material should be distributed, and this indeed was publicly announced. I myself, shortly after the announcement, made application, and received about a dozen specimens of fossil earths, and a few soundings, but in very small quantity, as was proper; most of which I now have. For a long time the collection was inaccessible, during the period when the Society was about moving from its very confined rooms in the Walker House, to the new and elegant building now occupied. Just previous to this time, and perhaps just after, there is reason to believe that the collection was very seriously depleted; not, however, by any fault or connivance of the officers, but rather from perhaps too much confidence in allowing free access of those who professed to be especially interested in the study of the Diatomaceæ. It is reasonable to suppose that Prof. Bailey left mounted specimens (if not the material) illustrating the new genera and species described by him, and especially those which had been engraved. Besides these, I know he had a considerable collection, exchanges, &c., from abroad."

American Dredging Results.—A valuable paper, more zoological than microscopical, on this subject appears in 'Silliman's Journal' for

January, 1873, by Professor A. E. Verrill. The paper records the results obtained by the dredging parties who sailed in the 'Mosswood' and 'Bache' steamers, but it is impossible to abstract it.

The Red Blood Cells in Mammals, Birds, and Fish.—In the new medical journal, the 'Medical Record' (January 8th), which we hope will prove a successful undertaking, Dr. Ferrier gives an abstract of M. Malassez' paper on the above subject in the 'Comptes Rendus.' It is stated that according to the method recommended by M. Potam, a drop of blood is mixed in exact proportion with some preservative liquid, and introduced into an *artificial capillary*, which consists of a flattened glass tube, in which the volume is calculated for each unit of length. By means of a microscope, the eye-piece of which is divided into squares, the number of corpuscles comprised within a certain number of squares can be counted. Knowing the length of tube corresponding to the squares and the corresponding volume, one can easily calculate the number of corpuscles in the cubic millimètre. Among the mammals the number varies from 3,500,000 to 18,000,000 in the cubic millimètre. The average number in man is 4,000,000. Camels possess from 10,000,000 to 10,400,000. In the goat the number amounts to 18,000,000. The porpoise has 3,600,000—a number exceeding that found in fishes. Birds have fewer than mammals. The maximum is 4,000,000, the minimum 1,600,000, the mean being about 3,000,000. Fishes have still fewer, and there is a difference between osseous and cartilaginous fishes. Osseous fishes possess 700,000 to 2,000,000. Cartilaginous fishes have 140,000 to 230,000. Thus the number of corpuscles diminishes as one descends the animal series. But the richness of the blood depends not on the mere number, but also on the surface, volume, and weight of the globule in the cubic millimètre, and also on the amount of hæmoglobin in each corpuscle. The author has not been able to resolve these questions, but compares the number of the corpuscles with their dimensions. The corpuscles increase in size as we descend the animal scale, so that there is an inverse proportion between the size and number of the corpuscles. This proportion is not, however, altogether constant, for man has fewer than the dromedary or llama, and at the same time smaller corpuscles. The consequence of this inverse proportion is, that the diminution in the number is compensated by the increase in volume. This is not always exact, however, as birds gain more by the augmentation in volume than they lose by the diminution in number, the weight of the bird's being greater than that of the mammalian corpuscle.

The Rabbit's Ovum, its Fecundation and Development.—In the same journal, the 'Medical Record' (January 8th), Dr. E. Klein has an admirable paper on the above subject. He gives a valuable abstract of Dr. Carl Weil's researches on the subject. He states that since the discovery of spermatozoa in the ovum of the rabbit by Barry,* it has been held by all embryologists that the spermatozoa form the most important part of the sperma as regards the fecundation.

* 'Philosoph. Trans.,' 1838–40.

Barry's observation has been confirmed by a great number of embryologists, amongst whom we may mention Bischoff, Lehmann, and Meissner. Spermatozoa have been found by those observers in the albuminous envelopment of the ovum of different mammals, during its passage through the oviduct, farther in the zona pellucida, and between the latter and the germ (yolk) itself. They have been seen in the latter place, not only previously to the cleavage process—when the germ (yolk) had retracted itself from the zona pellucida—but also in a later period, when the germ was already divided into a number of cleavage globules. In all these instances, however, the spermatozoa were motionless. Dr. Weil has observed in a number of ova, taken from the oviduct between the 17th and 46th hours after fecundation, spermatozoa in very lively movement in the albuminous envelopment as well as within the zona pellucida. Weil gives an account of four instances where he has seen unchanged spermatozoa *in the substance of the germ* itself, besides numbers of moving spermatozoa between the germ and the zona pellucida. There were to be found in these and other instances filaments either isolated or in bundles inside the germ, which Weil regards as the tails of spermatozoa. In later periods, when the ovum had already reached the uterus, no spermatozoa were to be found, neither outside nor inside the germ. From these facts, Weil takes it as probable that the spermatozoa, after having penetrated the germ, vanish completely, and that this intimate union of the spermatozoa with the germ forms the most material part of the fertilization of the ovum. Consequently, the inheritance of faculties from the father may be in this way explained. Weil confirms the assertion of Bischoff that the coitus in rabbits is not to be regarded as the chief cause of the extrusion of the ova from the ovary; but that, if there exist a relation between the coitus and the extrusion of the ova, it is only in so far as the former takes place a few hours before or after the latter. According to Weil, each ovum possesses two vesicular nuclei before the cleavage process commences. As to the earliest changes of the ova on their way through the oviduct, Dr. Weil's observations confirm those of Bischoff, fully described in his great work on the development of the rabbit's ovum (1842), which may be briefly described as follows:—The germ is first closely surrounded by the zona pellucida; then the germ retracts itself from that membrane; farther, the germ divides itself into two halves, each of these again into two halves; then the germ consists of eight cleavage globules, and finally of sixteen. In ova taken from the uterus (four days), the germ is already transformed into a vesicle (*vésicule blastodermique* of Coste), which exhibits on its surface a mosaic of cells, and on one place a mass of opaque elements, projecting into the cavity of the vesicle. In a later stage (five days), this vesicle consists only of one layer of cells; that is to say, the elements which result from the cleavage of the germ have arranged themselves in one layer, which encloses the cavity of the vesicle. In a still later stage (seven days), the vesicle shows a circular opaque spot, which consists of two layers of cells, whereas all other parts of the vesicle have only one layer. Not being able to find the above-mentioned mass of opaque elements at the stage when the germ vesicle was seen to consist of only one layer of cells, Weil takes it as improbable that this mass of elements participates in the formation of the *area germinativa*, and is therefore in agreement with the earlier assertion of Bischoff and that of Remak, and against the later assertion of Bischoff and that of Coste. Weil does not give any explanation of the spherical finely-granular bodies that are to be found between the germ and the zona pellucida previously to, as well as in, the earlier stages of the cleavage process. He does not think it necessary to conclude that they stand in a relation to the germinal vesicle (*viz.* the nucleus of the unfertilized germ), which, according to some authors, leaves the germ before the cleavage process commences. The method employed by Weil in his researches is as follows:—Within the first twelve hours after littering, the female is coupled with the male; from twelve until about eighteen hours after coition, the oviduct and ovary of one side are excised from the living animal, which is then allowed to live, the wound being treated according to the ordinary rules; the other oviduct may be made use of at a later period. For observation of the ovum from eighteen hours to seven days after coition, the cornua uteri are excised. In the first instances the oviduct is freed by the aid of forceps and scissors from the surrounding fat and peritoneum, and opened on a glass slide with fine scissors inch by inch. Whether ova have left the ovary can easily be recognized by the presence of blood-stained specks on the ovary—the openings of ruptured Graafian follicles. The folds of the mucous membrane being stretched with a pair of needles, ova can be discerned under a lens as spherical bright bodies.

The Nerves of the Cornea.—Dr. Woodward has, in the American journal the 'Lens,' complimented Dr. E. Klein upon his paper in this Journal. He says that the "London 'Monthly Microscopical Journal' for April, 1872, contains an admirable paper, by Dr. E. Klein, of the Brown Institution, 'On the Finer Nerves of the Cornea,' which substantially corroborates Cohnheim's observations, while some new and important points are added. This paper is illustrated by two admirable plates, to the accuracy of which I bear my humble testimony. The chief point of difference between Cohnheim and Klein is, that while the former thinks the sensitive nerves of the cornea terminate in various animals in the substance of the anterior layer of epithelium, or on its surface, in free extremities, the latter holds that the peripheral termination is a network. The difference of opinion appears to result from the circumstance that Cohnheim used perpendicular, Klein oblique sections. At the Museum both have been made, and both correspond to the descriptions of the distinguished investigators named. After careful comparison, I am disposed to approve the modification of Cohnheim's views adopted by Klein as best covering all the facts of the case."

Dr. Beale's Theories from an American point of view.—Dr. Danforth, who is pathologist to St. Luke's Hospital, Chicago, in a recent paper says that to "Professor Lionel S. Beale, of London, the world is more indebted, it seems to me, than it has yet seen fit to acknowledge. Nor is this at all strange. Clad in an impenetrable garment of good stiff English egotism; firmly convinced of the absolute correctness of his own views, and the absolute incorrectness of the views of everybody else; ready at all times to fancy himself 'hit,' and more than ready to strike back; the author of two or three very useful, and a far larger number of very useless, books; a writer of ordinary ability, but of extraordinary productiveness so far as pages of octavo are concerned; these qualities, either singly or combined, are not calculated to win the esteem or command the confidence of the great brotherhood of scientists. And yet Professor Beale deserves the highest commendation. He is an indefatigable worker; as a microscopist he has few equals, and probably no superiors, and he is largely endowed with that quality of persistency which always means results. His cell theory is attractive and plausible, and, so far as the function of the nucleus is concerned, has been accepted by a considerable number of observers, and is likely to increase rather than diminish its adherents. The cell, as a whole, he calls the 'elementary part'; the nucleus, 'germinal matter' (more recently, 'bioplast'); and all outside the nucleus, 'formed material.' The 'formed material' is solely the product of the action of the 'germinal matter'; through the agency of the 'formed material' the various functions of the body are carried on; and yet, by a strange paradox, this very 'formed material' is, according to Dr. Beale, *dead*, or, to employ a softer phrase, 'non-living.'"

Dr. Bastian's Experiments on the Beginnings of Life.—Under this heading Dr. Sanderson, F.R.S., contributes the following valuable information to 'Nature,' January 9th, 1873:—"In every experimental science it is of great importance that the methods by which leading facts can be best demonstrated should be as clearly defined and as widely known as possible. This is particularly true as regards physiology, a science of which the experimental basis is as yet imperfect. All experiments by which a certainty can be shown to exist where there was before a doubt, serve as foundation stones. It is well worth while taking some pains to lay them properly. Your readers are aware that Dr. Bastian, in his work on the Beginnings of Life, has asserted that in certain infusions the 'lower organisms' come into existence under conditions which have been generally admitted to exclude the possibility of the pre-existence of living germs. It is also well known that these experimental results are disputed. Not long ago I witnessed the opening of a number of experimental flasks charged many months ago by a friend of mine with infusions supposed to be similar to those recommended by Dr. Bastian. The flasks had been boiled and closed hermetically, according to Dr. Bastian's method. Finding on careful microscopical examination that the contents of the flasks contained no living organisms, I charged calcined tubes with the liquids, sealed them hermetically, and forwarded them to Dr. Bastian. When I next saw him he pointed out that two of the three liquids used were not those which he had recommended, that if the infusions had been properly prepared there would not have been any necessity for keeping them many months before examination, that his results with organic infusions were obtained after a few days, and that they were generally of a most unmistakable nature. To satisfy my doubts on the subject he most kindly offered to repeat his experiments relating to the production of living organisms in infusions of hay and turnip in my presence. To this proposal (although I have hitherto taken no part in the controversy relating to spontaneous generation, and do not intend to take any) I gladly acceded, at the same time engaging to publish the results without delay. Fifteen experiments were made. They were in three series, the dates of which were respectively, Dec. 14, Dec. 20, and Dec. 27," of which we publish only the third, referring our readers to 'Nature' for the more full results, which are of the highest interest. Dr. Sanderson says that even after two series of experiments, which quite bore out Dr. Bastian's conclusions, "it appeared to me desirable to ascertain whether the condition of the internal surface of the glass vessels exercised any influence on the result. I therefore heated two retorts to 250° C., keeping them at that temperature for half an hour, and closed them while hot in the blow-pipe flame. These Dr. Bastian charged by breaking off their points under the surface of a neutral infusion of turnip with cheese, freshly prepared for the purpose, without employing any of the rind. The retorts were boiled and sealed in the same way as before, excepting that whereas one was boiled only five minutes the other was boiled ten minutes. The specific gravity of the infusion used was 1013. A third uncalcined retort was charged with some of the same infusion containing no cheese. This was also boiled for ten minutes. I was out of town from the 28th to the 30th, and therefore did not examine the retorts until the 31st. Dr. Bastian informed me that on the 28th, twenty-one hours after preparation, the liquids in both the calcined retorts were distinctly turbid, the temperature of the water-bath being 32° C.; and that sixty-six hours after preparation, whilst the turbidity was much more marked, each flask also contained what appeared to be a 'pellicle,' which had formed and sunk. At this period the fluid in the third flask had also become very decidedly turbid. (*a.*) *Neutral turnip infusion with cheese in calcined retort, boiled ten minutes.*—The retort having been tested in the way previously described, was opened on the 31st. The liquid was very fœtid, had an acid reaction, and contained much scum. It was found to be full of Bacteria, whilst Leptothrix existed in abundance in portions of the scum, together with granules of various sizes which refracted light strongly. (*b.*) *The same boiled five minutes.*—The state of the liquid was the same as that just described. (*c.*) *Neutral infusion without cheese, boiled ten minutes; retort not calcined.*—In this liquid the rods and filaments were much less numerous. In other respects its characters were the same. In each case before opening the retort it was observed that a portion of its neck became drawn in when exposed to the blow-pipe flame. As regards the results of the foregoing experiments, it is unnecessary for me to say anything as to their bearing on the question of heterogenesis. The subject has already been frequently discussed in your columns. The accuracy of Dr. Bastian's statements of fact with reference to the particular experiments now under consideration, has been publicly questioned. I myself doubted it, and expressed my doubts, if not publicly, at least in conversation. I am content to have established—at all events to my own satisfaction—that, by following Dr. Bastian's directions, infusions can be prepared which are not deprived, by an ebullition of from five to ten minutes, of the faculty of undergoing those chemical changes which are characterized by the presence of swarms of Bacteria, and that the development of these organisms can proceed with the greatest activity in hermetically-sealed glass vessels, from which almost the whole of the air has been expelled by boiling."

NOTES AND MEMORANDA.

Monochromatic Sunlight, by means of Glass Plates. — In a recent number of the 'American Naturalist' it is stated that Mr. J. Edwards Smith, of Ashtabula, Ohio, has obtained light with which he is perfectly satisfied, by means of a light sky-blue and darker green glasses. He prefers to use one blue glass combined with two or three green ones, the best shades being ascertained by trial. Several such sets, of different depths of colour, may be mounted in a series, like magic-lantern pictures, so that either set can be brought easily over the hole in the shutter. By sunlight transmitted through such a combination of glasses, and without condenser or apparatus of any other kind, he "resolves" all the shells of the Probe Platte with perfect ease. He considers the light thus modified as good as the more nearly monochromatic light of the troublesome ammonia-sulphate cells.

An interesting paper on the Thysanuradæ is contributed by one of our Fellows, Mr. S. J. M'Intire, to 'Science Gossip' for December. We refer those of our readers who are anxious to know where they may obtain *scales,* to the paper itself, which describes many species of these animals.

Mr. Powell's $\frac{1}{80}$ Objective.—This is described by a contributor to 'Science Gossip' (December) in the following terms, which we think a little too flattering, though the glass is certainly a wonderful piece of mechanism and the definition remarkably good:—It appeared to perform well, defining the Podura test sharply and without colour, and having plenty of light. Its magnifying power is 4000 diameters with the A eye-piece, with an angular aperture of 160°; it bears the B and C eye-pieces, with no other detriment than some loss of light, and works well through covering glass ·003 thick.

The Use of Glycerine in Microscopy. — Dr. Woodward replies to the remarks of Dr. Beale in the last number of the 'Lens.' He says (among other things) that as the climate of England is not subject to the high summer temperatures which so often prevail in the United States, it is very possible that glycerine may behave somewhat " more kindly " as a preservative in that country than it will here ; though from what he has been able to learn from the conversation of friends who have been abroad, and from the letters of correspondents, he has been led to the conclusion that the experience of the majority of histologists, both on the Continent and in England, has been, in this matter, essentially the same as mine. Nor does he understand Dr. Beale's language, either in the article before him or in his former publications, as claiming any constant or uniform permanency for glycerine preparations. Dr. Beale does not tell us that all, or even the majority, or even a large minority, of the thousands of preparations he has immersed in glycerine, retained their pristine beauty and usefulness for any considerable time. Is it not a fact, he would ask him, that even in his own skilful hands it is only a few fortunate preparations out of many made, which by some happy chance survive the common ruin of the first summer? " Now, as to just what I have been able to do myself, or to have done at various times by my assistants, I have not a great deal to add to my former article. Dr. Beale proposes that I should get some friend in whom I have 'implicit confidence,' to cross the ocean and wait on him with some of the Museum specimens, for comparison with his. If he really desires that any such comparison should be made, would it not be much simpler for him to send me by mail, or otherwise, one or two glycerine preparations of tissues which he thinks it quite impossible to display in balsam? I would take pleasure in sending back what I could, in the same way. Meanwhile, I cordially invite any English or American microscopists who have seen Dr. Beale's preparations in his own hands, to call at the Museum in Washington, and see what we have been able to do in the way of making a collection of histological preparations which are likely to be permanent."

Draw-tubes versus Deep Eye-pieces.—Mr. Stodder says in the 'Lens' for November, in reference to a paper which had previously appeared in that journal, that the writer omitted the whole question by not specifying *any* length of tube. " With what length of tube has good definition been obtained? Thousands are using English and American instruments with tubes eight to ten inches long, giving the conjugate focus ten to twelve inches from the optical centre of the objective, and to that adding some inches of draw-tube. Now, all these will undoubtedly agree that the 'perfection, clearness, and brightness' of the image depend upon the skill of the maker in correcting 'the spherical and chromatic aberration,' and not on the length of tube or power of eye-piece entirely. He has by him photographs of *Amphipleura pellucida*, taken by Col. Dr. Woodward, of Washington, with a Tolles' objective (a little less than one-fifth inch focus) at 48 inches distance from the objective : this is equal to a pretty long tube. Now, this photograph bears enlarging twenty diameters, and still shows 'clear and bright.' But he has other photographs by Dr. Woodward of the same object, taken with Powell and Lealand's $\frac{1}{16}$th (so-called) and with Tolles' $\frac{1}{16}$th at 7 feet 6 inches distance, still giving good definition, and bearing a low-power eye-piece well. It is to be supposed that these are at least not 'short tubes,' and are good evidence that the definition is obtained by quality of the objective."

How to pick out Diatoms in Mounting.—Dr. C. Johnston in a paper on this subject ('Lens,' November), says that nothing is easier than to seize particular diatoms and transfer them to a bottle for future use, or to a slide, provided the field from which we select be rich and clean. Difficulty, however, occurs when forms in any gathering are few and far between. Let such prepared material be spread upon a large slide, covering a space of one inch by two, and let it be filliped as it is set away to dry spontaneously. With a $\frac{2}{3}$-inch objective, search the white field for any diatoms whatever, and, upon finding, encircle each one with a line, made with a point of a match sharpened and moistened, adding near the circle a dot, or cross, or other sign, always appropriated to the same diatom, and of which a tallying record is kept on paper. At leisure one may, without trouble, single out any desired object, pick it off with a fine dampened point of cane (reed), not including the silicious cuticle, and deposit it, free from injury, in a small drop of distilled water placed in the centre of the slide.

The Use of Osmic Acid.—Osmic acid when obtained pure is a yellow crystalline solid, which is volatile at ordinary temperatures, has a peculiar stinking odour, and rapidly excites a severe and persistent catarrh in those exposed to its slightest influence. It is better, therefore, says Col. Woodward ('Lens,' November), for the microscopist to procure it from the dealer already made into a one-per-cent. solution, which he can dilute at pleasure for use. Even dilute solutions, however, have an unpleasant smell, and excite catarrh unless great care is taken to avoid exposure. A box outside the window of the working-room is the proper place to set capsules containing portions of tissue during the action of the reagent, which should always be handled near an open window. The preparation to be acted upon should be as fresh as possible, and laid in a small quantity of the solution for several hours, when it may be washed with water, and is ready for examination in water or glycerine. According to the intensity of action desired, the solution may vary in strength from one-half to one-tenth of one per cent. It will be found to have a particularly energetic action on the medullary sheath of the nerves, which acquires a peculiar purplish-brown colour passing into black if the action is very intense, while the surrounding tissues are but little stained or remain quite uncoloured. Fatty matters of all kinds are also blackened by the acid, the use of which, therefore, is undesirable if the part to be investigated contains much adipose tissue.

Frustulia Saxonica as a Definition Test.—Dr. Woodward does not appear to set so high a value on this test as the Germans do. He states ('Lens,' November), that having been lately presented with an opportunity of examining specimens of the diatom, he found no difficulty 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, he regards 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, he found very definite in character. He 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. He subsequently removed the cover of one of the dry slides obtained from Möller with the diatoms adherent to it, and mounted the specimen in Canada balsam. The striæ were then paler than before, but he is unable to say that he found them more difficult to resolve. Both in balsam and dry he got resolution by the Tolles' 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 he 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, he thinks the *Frustulia Saxonica* 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 photograph, or rather the Woodbury print which accompanies the paper, is a very good one. It shows the transverse striæ most distinctly; but, of course, parts of the diatom, which is magnified 1750 diameters, are somewhat indistinct.

Shall Microscopic Specimens be Photographed, or Drawn by Hand?—On this question a note appears in the 'American Naturalist' (December, 1872). In the September number of that journal there appears an article entitled "Photo-mechanical Printing," giving, says C. S. [Mr. Charles Stodder, we believe], "some of Dr. Woodward's ideas, and an editorial dissent from them. Now this difference of opinion relates to a point that ought to be settled by the judgment of microscopists, and I write this for the purpose of calling for their views of the question. I quote from the article:—'Even the microscopist himself, being unable to represent all that he sees, is obliged to select what he conceives to be of importance, and thus represents his own theories rather than severe facts' (*Dr. Woodward*). The comment is ['If, however, his theories are correct, and his delineation skilful, this very power of selection and construction enables him to give a distinctness and completeness which is lacked by the photographic camera.'] Here are two almost opposite principles of illustration in question. Which should be the governing one? What is the object of the pictures? Obviously there are two—one for explanation of the observer's theories; the other, that other observers may, in repeating the observation, be guided by and recognize what the first one had seen, and this I consider the all-important object of 'figures.' If the observer draws only what he thinks important, he must almost invariably make a picture quite different from the one seen in the microscope—he has omitted what he deemed the unimportant parts—and the pupil trying to follow him finds the actual appearance so different that he does not recognize it as the same. No doubt many of the misunderstandings or differences of opinions among microscopists have originated from this very defect of published figures, which have been taken to be what they purported to be, representations of what was actually seen—'if his theories are correct'; but if his theories are wrong, then his skilful delineation has only misled his readers. But if the draughtsman publishes his figure as explicitly as his theory, not as the representation of the 'severe fact,' then he will be understood. On the other hand, the camera represents exactly what may be seen by any other observer *using the same appliances* (which should in all cases be described), and the student can draw his own conclusions from the picture as to the soundness of the theories advocated. But then it must be remembered that a photograph can represent only one view of an object, while the observer, by changing the focus of his instrument, obtains a new view at each movement of the screw. With the high-power lenses now in use, these differing views are all important for correctly understanding almost any object. Therefore scarcely anything can be properly illustrated by one photograph. Many objects must require several." To which remarks the editors reply:—This inflexible limitation of the photographic view to one section or plane of the object is evidently one of the points referred to in the criticism quoted above, which, without referring to photography as a means of proof of alleged observations, or of submitting observations to investigators for criticism or deduction, only suggested that for communicating well-ascertained facts a skilful delineation may contain more information than any available number of photographic representations. A good drawing, as intimated by Dr. Beale, may often supply the place of a long and unread verbal description.

CORRESPONDENCE.

Micro-spectroscope.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Mr. Gayer was probably not aware that the micro-spectroscope described by him is the same in optical construction as the original form which Mr. Huggins explained, and figured in a paper before the Society, May 18th, 1865. In this two equilateral prisms of dense glass were used. I was present when the first investigations were made with this instrument, which gave good results with the highest powers and also on opaque objects. This was the first arrangement in which the light from a microscopic object was analyzed by the prism itself.

In the previous experiments of Mr. Sorby the prism was set below the stage, and the object placed in the spectrum. I write this note without any wish to detract in the smallest degree from Mr. Gayer's merit in the construction of the efficient instrument described by him, but merely in the cause of scientific history.

F. H. WENHAM.

THE NOTICE OF HARTNACK'S OBJECTIVE.

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—In correcting proof of the notice of objects exhibited on the 11th ult., by inadvertence I put Mr. Hogg's name to the remarks concerning Hartnack's objective. This I did because they were in his handwriting. He explained to me afterwards that the description of the lens was supplied to him by the exhibitor in the usual manner.

I remain, yours obediently,

HENRY J. SLACK,
Sec. R.M.S.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *Jan.* 1, 1873.

W. Kitchen Parker, Esq., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

The Secretary stated that the Society had received a very handsome present from Mrs. Farrants. The gift had been made in order to commemorate her late husband's long connection with the Society, and his interest in its welfare. The present consisted of a cabinet, containing about 1000 slides, and many specimens of Mr. Farrants' works with Dr. Peter's machine. An early opportunity would be taken of bringing some of the most interesting specimens prominently before the notice of the Fellows.

A special vote of thanks was accorded to Mrs. Farrants.

The Fellows then proceeded to the election of auditors for the purpose of examining the accounts to be presented at the Annual Meeting of the Society in February next, and it was resolved "that Messrs. Loy and Hilton be requested to act as auditors."

The Secretary stated in reference to a paragraph which had appeared in some of the daily papers, relating to the late Scientific Meeting of the Society, that, after advising with several members of the Council, he had taken upon himself the responsibility of writing to the 'Times' and 'Daily News' to say it did not emanate from the Council, but his letters had not yet been published. The whole of the Council present that evening had approved of the course he had taken. He regretted he had been misled at first into supposing that the paragraphs in question had been written by Mr. Hogg, through their containing phrases similar to a description of Mr. Mayall's exhibition on the 11th ult., in Mr. Hogg's writing. Upon inquiry it appeared that Mr. Hogg had copied that description from a statement written by Mr. Mayall. The newspaper paragraphs were disclaimed and disapproved by Mr. Hogg. He (the Secretary) then thought it right to give Mr. Mayall an opportunity of offering an explanation to the Society, which resulted in Mr. Mayall's stating that the notices in the papers were written by a reporter, the technical information having been furnished by him. The Council thought the paragraphs were very unfair to the eminent makers who came down to exhibit lenses on the night of the meeting, and they (the Council) had taken the best means of correcting any erroneous impression that might have been formed.

The following list of officers for the ensuing year, proposed by the Council for election at the Annual General Meeting in February, was read by the Secretary, who stated that in the absence of Mr. Hogg, who had not been able to attend that evening, his nomination must be regarded as to some extent provisional.

Proposed as President.—*Charles Brooke, M.A., F.R.S. *As Vice-Presidents.*—William Benjamin Carpenter, M.D., F.R.S., &c.; Sir John Lubbock, Bart., M.P., F.R.S., &c.; *William Kitchen Parker, F.R.S.; *Francis H. Wenham, C.E. *As Treasurer.* — John Ware Stephenson, F.R.A.S. *As Secretaries.*—Henry J. Slack, F.G.S.; Jabez Hogg, M.R.C.S. *As Council.*—*James Bell, F.C.S.; Robert Braithwaite, M.D., F.L.S.; John Berney, Esq.; William John Gray, M.D.; Henry Lawson, M.D.; *John Millar, L.R.C.P. Ed., F.L.S.; Samuel John McIntire, Esq.; Henry Perigal, F.R.A.S.; *Alfred Sanders, M.R.C.S.; Charles Stewart, M.R.C.S., F.L.S.; Thomas Charters White, M.R.C.S.; *Charles Tyler, F.L.S.

Mr. Joseph Beck thought a change in the Secretariat desirable, and nominated Mr. Charles Stewart. Mr. Coppock and Mr. McIntire joined in this nomination, as provided by the by-laws. It was announced on behalf of the Council that should there be a vacancy caused by Mr. Stewart's election as Secretary, they would suggest the name of Mr. B. T. Lowne to replace him on the Council.

Mr. Stewart then read a paper "On some of the Characteristics of the Negro, as revealed by the Microscope."

The same gentleman also described the structure of the calcareous framework which is connected with the locomotive apparatus of the Echinus.

A vote of thanks was given to Mr. Stewart.

The meeting was then adjourned until the 5th of February.

* Those with the asterisk placed before their names are proposed as new members.

Donations to the Library and Cabinet, from Dec. 4th to Jan. 1st, 1873:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Journal of the Linnean Society, No. 68	*Ditto.*
On the Nomenclature of the Foraminifera. By W. K. Parker, F.R.S., and Prof. R. Jones	*Authors.*
The U. S. War Department Weather Map. 3 copies ..	*U. S. War Department.*
The Daily Bulletin of the U. S. War Department. 3 copies	*Ditto.*
24 Slides of Starches	*Mr. Waldron Griffiths.*
Cabinet and 980 Slides	*Mrs. Farrants.*

The Rev. John George S. Nichol was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

BRIGHTON AND SUSSEX NATURAL HISTORY SOCIETY.

November 14th.—Ordinary Meeting. Mr. G. Scott, President, in the chair. Dr. Ormerod, F.R.S., and Mr. W. Puttick, were elected ordinary members.

Mr. Wonfor read a paper "On Certain Wingless Insects."

After briefly sketching the changes through which insects passed from the egg to the so-called perfect state, he showed that with the exception of the *pediculi* and *Thysanuridæ* all insects possessed either four wings or their modifications; the *halteres*, or poisers, of the *diptera* being, in his opinion, only modified wings, while the fleas in the place of wings had four scales. He then pointed out that among moths, which were characterized by four large wings, were some in which they were either so small as to be quite useless as organs of flight, or altogether absent.

One group of moths, the *Liparidæ*, closely allied to very swift and broad-winged moths, contained two species, called "Vaporers," from their peculiar flight, in which while the males were good fliers, the females were nearly wingless, and seldom crawled beyond their cocoons, on which they laid their eggs and died. Wingless moths were also found among the "geometers," earth-measurers, so named from the mode of progression, and from the circumstance of looping their bodies while walking called also loopers. Several examples of moths among these were mentioned, and exhibited, in which it was shown the wings of the females were either too short for the purpose of flight, or were altogether wanting, some presenting the appearance of spiders, for which they might well be taken. Another family, the *Psychidæ*, whose caterpillars made cases of pieces of grass, &c., which they carried about with them, and which afterwards served the purpose of cocoons, exhibited examples of females not only wingless, but in one case footless, and without antennæ. In this particular instance the female laid her eggs inside the cocoon, and died; while the first experience of life on the part of the caterpillars was to eat up their dead mother's body! It might be asked why some females should be so different, not only from the males of the same species, but from the other females of the same family? At present no satisfactory answer could be given, and examination of the larvæ did not show any difference between those which produced winged males and wingless females. There were other examples of wingless females among other groups of insects. Thus the cochineal females were wingless, the summer broods of aphides, some cockroaches, stick insects, and a very notable case, the glowworms. In the last-named the females were not only wingless, but alone were luminous, while the males, different in appearance, flew well.

It was strange that, in some cases, the sex to which wings would seem more important, should not only be destitute of them, but that as far as decoration, power of locomotion, and the possession of certain organs went, the so-called perfect insect should fall short of the immature or larval form.

November 28th.—Microscopical Meeting. Mr. G. Scott, President, in the chair.

Mr. Wonfor, after announcing the receipt for the cabinet of four slides from Mr. W. H. Smith, and one from Mr. Gwatkin, stated that as the next Microscopical Meeting would fall on Boxing Day, it was determined not to meet on that evening. He also announced that Mr. T. Curties had sent down for exhibition some slides, designated a "Microscopical Novelty," in which birds, flowers, and insects had been built up from the scales of butterflies and moths. Though to the microscopist they were only toys, yet they were marvels of what patience and skill could do.

Mr. W. H. Smith then read a paper "On the Ingredients of the Unfermented Drinks—Tea, Coffee, and Cocoa."

The meeting then became a conversazione, when the ingredients of tea, coffee, and cocoa, with their adulterations, prepared by Mr. Smith, were exhibited under the microscope by Mr. W. H. Smith, Drs. Badcock and Hallifax, and Messrs. Wonfor, R. Glaisyer, and T. Glaisyer. Later in the evening Mr. Wonfor exhibited the microscopical marvels made out of insect scales, which were pronounced to be very beautiful and ingenious. It was mentioned that they could only be obtained at Baker's, Holborn.

Reading Microscopical Society.*

November 5th.—Captain Lang presided.

Mr. Austin read a paper "On the Structure of the Floating-bladders of the Common Bladderwort (*Utricularia vulgaris*)," describing more particularly the microscopic structure of the air vessels, and the several forms of hairs peculiar to them. The paper was illustrated by mounted sections and sketches.

Mr. Tatem exhibited mounts of *Necrophorus vespertilio*, *Aphis aceris*, and of the vulvar appendages of *Epeira diadema*.

* Report supplied by Mr. B. J. Austin, Devonshire House, Bath Road, Reading.

Captain Lang exhibited slides of arranged Polycystina and Diatoms; not only for their intrinsic beauty, but to show his mode of finishing off slides with a ring of tenacious white cement, on which are described circles of red or white varnish.

December 3rd.—The President read a paper "On the Eyes of Insects." This was specially illustrated by a beautiful mount of a section of the eye of the Death's-head Moth, prepared by Mr. H. Mozeley, according to a method described in the October number of the 'Quarterly Journal of Microscopical Science.' The paper described the general structure of the compound eye of a typical insect; but more particularly presented a comparison between the diagrams and descriptions in Dr. Hicks' work on the Honey Bee, and the appearances presented by the section. The possible difference between the eye of an insect flying by day, and of a nocturnal insect, was naturally referred to. The section seemed to afford no proof of the corneal lens being made up of *two* conjoined plano-convex lenses, though it was clearly seen to be doubly convex; nor did the section exhibit the narrow constriction behind that lens of which Dr. Hicks speaks; nor did the conical lens, behind the corneal lens, appear strictly to impinge upon the bulbous expansion of the optic nerve. The writer was also disposed to regard the compound eyes of insects as more microscopical in their action than telescopical, in which latter aspect Dr. Hicks regarded them.

January 7th, 1873.—Dr. Moses read a paper "On *Picus auratus* and *Picus tridactylus*," two North American woodpeckers; describing also the structural adaptations of woodpeckers, generally, for their mode of life. Specimens of the two somewhat rare birds were exhibited.

Captain Lang brought before the Society the first three numbers of the 'Lens,' a new American Quarterly Journal of Microscopical Science, calling the attention of the members to Professor H. L. Smith's conspectus of the Diatomaceæ, and to Dr. Arnold's views (as contained in the 3rd number) of the nature of the so-called "exclamation-marks" of the *Podura* scale, which he regards as simply minute epithelial scales on the surface of the principal scale, from which they may be removed by electrical discharge, or by mechanical means. He also exhibited slides of selected diatoms, from a gathering from the Sandwich Islands, kindly sent to him by Professor H. L. Smith, of Hobart College, Geneva, N.Y.

Mr. Tatem reported an addition to the local fauna of some interest, having recently found a male specimen of *Salticus formicarius*,* hybernating in an empty shell of *Cyclostoma elegans*, the mouth of which it had closed with a compact web. This spider is admitted to be British on the authority of Dr. Leach, who states that it is found, though rarely, in Scotland.

The Secretary exhibited a section of the human scalp, and also sections of the stipes of exotic ferns, to show the differing arrangements of the vascular tissue.

* Blackwell's 'British Spiders,' plate iii., fig. 36.

The Liverpool Microscopical Society.

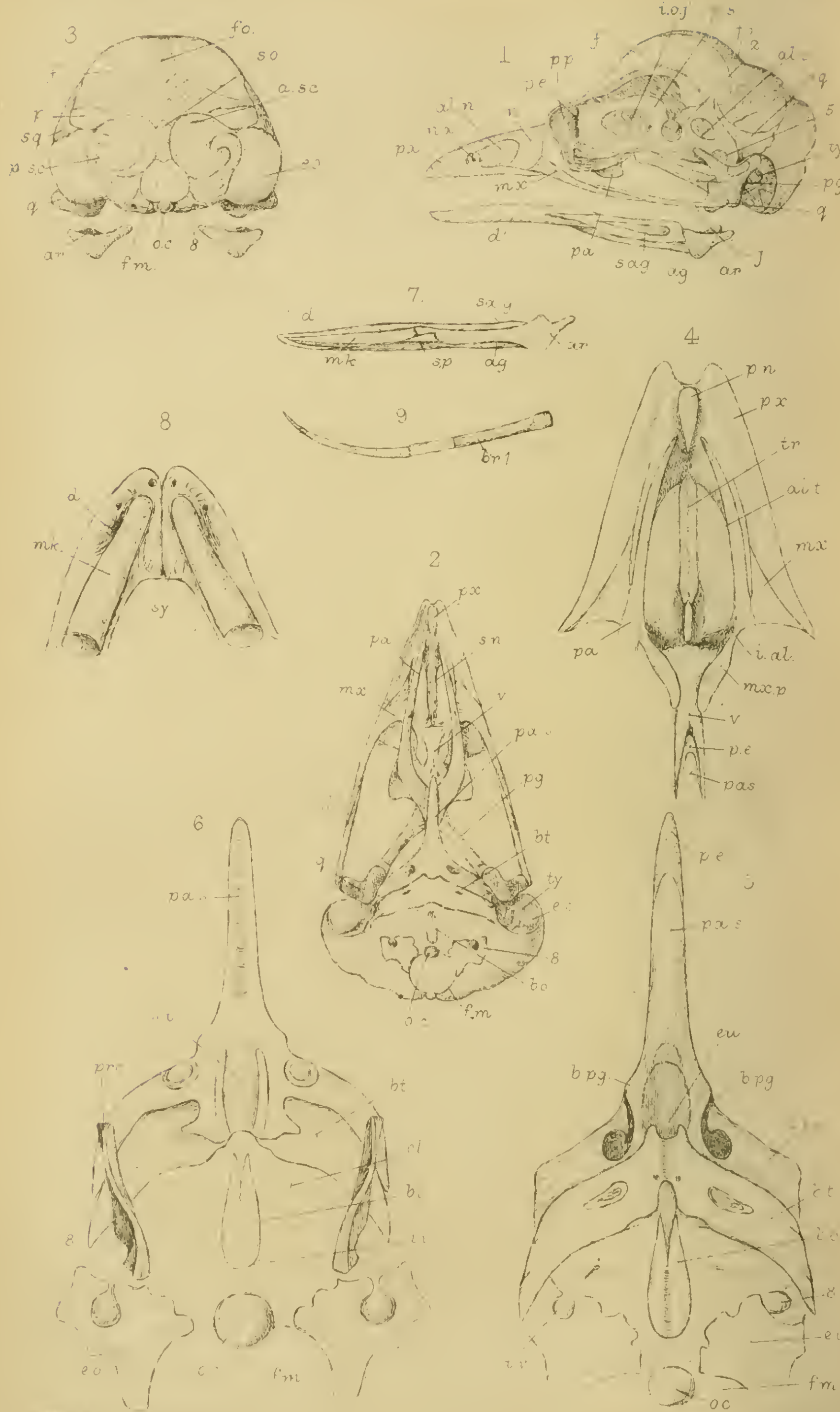
Ninth Meeting of the Session.

The adjourned discussion on Mr. Newton's paper "On Spontaneous Generation," and on Dr. Bastian's recent work on the 'Beginnings of Life,' was resumed by Mr. Hamilton, F.R.C.S., who read a paper on the subject, of which the following is an abstract:—After mentioning Dr. Bastian's view, that life arises *de novo* in animal and vegetable infusions, he went on to say — The smallness of the vital material, if we could trace it to its ultimate form, cannot be conceived. All the experiments that have been hitherto made seem only to point to latent forms, of which the earliest outgrowths which are perceptible to the eye are monads and bacteria. The way in which they first come into view, their rapid increase, the changes they undergo, seem to indicate that life exists in forms so infinitesimal as to be far removed from view. This being the case, what can heat do, carried to its utmost limit, with the ultimate forms of matter? It tells upon its matured development, and that, too, in an almost regulated proportion, destroying the higher forms of life at a temperature which would not affect simpler forms. The writer next went on to question the truth of Dr. Bastian's statement that all organic matter, in the shape of food taken into the stomach, was dead matter. He proceeded to show, on the contrary, that because it was living material from the first which was worked up into chyme and chyle, that it took an active part in the process, not as a stone or a piece of metal, either of which if taken into the system will be ejected unchanged, because it is really dead inorganic matter. In conclusion, Mr. Hamilton pointed out the possibility of two forms of life being present in every living animal and vegetable—an embryonic life and a molecular or cell life.

The Rev. W. H. Dallinger said he considered a question of that sort too large for profitable discussion of the kind which they could afford it. The Society knew that he had been, for the past four years, patiently working and accumulating facts on this really great question. But at the end of the time he was less prepared to speak positively than at the beginning. "Facts" of the most diverse complexion could be produced on both sides; and inferences diametrically opposite could be drawn from them. Eventually, he should have much to say on this subject; at present he confined himself to a general glance at Dr. Bastian's book. In the first place, he submitted that it opened with an assumption. Life, it was argued, was evolved from the physical forces: life, in fact, was correlated with the known forces of matter. Because "heat" is a "mode of motion," which, although we have every reason to believe, is not proved, therefore life is a mode of motion! He protested that there were truly no scientific grounds for such a dogma. There is something in life, even in its lowliest forms, which distinguishes it from all the known powers of chemistry and physics. The microscopic sting of the Tsetze fly inserts an infinitesimal portion of fluid into the skin of a huge brute on the African plain, and strikes it down to death. Can either chemistry or physics explain its actions? Dr. Frazer has just completed a series of exquisitely accurate experiments on the antagonistic action of the poison of the Calabar bean and atropia on each other in the living subject. These poisons have no chemical and no physical action on each other. Yet physiologically the one destroys the destructive action of the other; proving that vital action, whatever be the cause of its difference, differs from mere chemicro-physical action. To say that life and all its phenomena are produced by mere chemistry and physics, is equal to saying that when an ounce and a ton of gunpowder are exploded by the same spark, that the work done by the explosion was in each case done by the spark. The spark merely determined — set free — the working power—chemical affinity; and chemical and physical forces simply excite or determine vital action. Even if Bastian's supposition be true, that life was at first evolved from physical force, it does not follow that it is so now. Because there has been one carboniferous period on the globe, it does not follow that there must be another. And to suppose that the lowly organisms we see must be physically evolved, inasmuch as otherwise they would have taken by the laws of evolution a higher grade in the scale of being, is to forget that there are definite groups of animal forms almost as lowly as the Foraminifera that have remained the same through all geological epochs. To talk of the monads as unstable is to know nothing about them. It requires the patience of years, and powers from 1–16 to 1–50th to truly study them. He had worked out *Bodo saltans* after three years of work with Powell and Lealand powers; had traced it through seven metamorphoses, but these were as constantly repeated as the metamorphoses of the frog or the crab. There is no doubt that bacteria develop where there is a comparative optical purity when nothing is seen. But does it follow that they develop where nothing is? Ferns were fertilized a hundred years ago as they are now, in spite of our inability to discover the process. The author had developed spores by reagents where none were visible otherwise; and the invisibility of bacteria spores, or whatever else they might be called, was only natural with our present powers of analysis. Where an experiment was made by Bastian and Sanderson, it is not difficult to predicate whose results the scientific world would sooner receive; and in a similar experiment the results of the former were positive, those of the latter negative. It is easy to get positive results. Proof such as true science should demand is wanting, that the issues of Dr. Bastian's experiments with saline solutions were vital; while the same want of delicate manipulation which permitted a fragment of *sphagnum* to enter a sealed tube, and to be discovered in Huxley's presence and mistaken for a vital product of the infusion until the fallacy was dispelled by Huxley,* would have allowed him to introduce the *penicillium*, which is a common adherent to crystals of *ammoniac tartarate*. The assumption that prolonged boiling destroys the possibility of life from germs Mr. Dallinger gave strong evidence for suspecting, and wholly questioned the production of one form of life from the ova or spore of another. He had often seen rotifers emerge from cells of

* 'Nature,' vol. i., p. 475.

Vallisneria, and seen them living and swimming in the cells of Chara; and Mr. Chantrell had given them demonstration of the hatching of Tardigradia inside the body of another form. But to suppose that because they emerged from a certain cell or seed the vitality of that cell or seed had been transformed into the new form is certainly to draw largely on our faith. No chemist supposes that a solution capable of crystallizing into sulphate of iron will by some hidden process produce chloride of sodium. Why, then, should a conferva spore produce—say, a *Rotifer vulgaris?* Finally, the "areas" of supposed germination, as seen in the proligerous pellicle of an infusion, the author said had yielded, with a Powell and Lealand $\frac{1}{25}$th, entirely divergent results to him from those which Dr. Bastian had seen.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THE

MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1873.

I.—ANNUAL ADDRESS FROM THE PRESIDENT.

(*Read before the* Royal Microscopical Society, *Feb.* 5, 1873.)

Gentlemen,—I am permitted by the Council to speak to you to-night of things within my own measure; for, working within a very limited territory, I have no power or leisure for expatiating on other men's labours. So far as I become acquainted with the results produced by the working of a thousand excellent labourers, I rejoice; but my limitations are too strict for me to become an annalist even in my own department.

Besides this, long-continued labour in one particular direction tends to give the mind somewhat of a narrow and exclusive character, from the habit of obliviousness which is acquired by any man who keeps for year after year one train of ideas in his mind. Besides this I have the misfortune to have a cat-like nature—I must scent out my game for myself, and do not readily receive hints from others. The creature I have referred to, as you all know, escheweth change of place; so also do I, and thus lose many excellent opportunities of learning from my fellow-workers. In this hermit-like scientific life, I have lost much time and labour; digging over acre after acre to find what would have turned up much sooner had I possessed the dog-like qualities of more ready men, who take the least hint, and catch at suggestions by instinct. After all, I suppose, this is as it should be; for the human species is, within its own boundary, an epitome of the whole animal world beneath it, and nature, abhorrent of uniformity, has evidently intended man to break up into ten thousand varieties and sub-varieties. My own idiosyncrasy is merely mentioned as an apology for the restricted character of this Address, which is the result of the necessary limitation of my researches; he who will observe every flower bordering his footpath, will miss much of the extended scenery around him. My own line of research is the growth, as of a plant or flower, of those animal forms which, like us, possess a vertebral column; and I search in them, even in the lowest, for the pattern and type of our own form, which is the highest of all. But, as the field is far too wide for any single worker—the labourers are few, but there is work for thousands—I am fain to restrict my own work to one part of the vertebrated animal, its *head*. And, indeed, the head is far too large a territory for one worker, so I have selected merely its framework; the box that contains the brain; the basket-work of the face; and the wondrous passages of the ear and nose. All the forms that compose the great sub-kingdom of the vertebrata may be considered as branchings from one stock: the whole group is in one sense a unity. All the forms lead up by many a winding path to man; they all foreshadow him, and their very existence has no meaning but as seen in the light of him who is the head and chief of all. Therefore, when carefully studying the development of newt and blindworm, spotted snake or thorny hedgehog, we are ever searching for the development of the form of man. I cannot be restricted to the mere animal form; I must attain to a vision of the highest; for, by other methods, I am seeking the same delight in the contemplation of human beauty as the sculptor or the painter.

> "Be it ounce, or cat, or bear,
> Pard, or boar with bristled hair,"

—or "the clamorous owl that nightly hoots at our quaint" featherless forms; or the cold and silent fish—in all these leaves of the book of nature I find my members written. If there is one idea which impresses the mind more than another in these researches, it is that these unnumbered forms, all in manifold ways foreshadow and give glimpses of their rightful king. No Fellow of this Society will ask, "To what purpose is all this scientific labour?" For even setting aside the higher æsthetic and intellectual end, it has the highest value in relation to the healing art. The meaning of life, as life, in all its forms, is the one thing needful to the art and science of medicine; and all knowledge leading up to light in that division of human work, to us, being what we are, and suffering what we do suffer, must be precious. Yet I hold that all these researches into nature have a still higher value in the education and development of those excellent faculties which are placed within us. They yield immediate pleasure of a refined and a refining kind; this is an *end* in itself, and need not be dragged into the category of a *means* to something further; yet it is a means to something further, notwithstanding. In making these remarks, I am thinking, not merely of my own narrow headland, which I have endeavoured to make into a dainty bit of garden; but of the work of this, and of all other societies established for the furtherance of biological research. Our own most excellent and beautiful bodies will be slowly but distinctly revealed to us, as to their structure and functions, as we learn more and more of vegetable and animal life. Gradually the hidden meanings will become apparent, and we shall see ourselves to be "parts and proportions of one wondrous whole." I therefore bid God speed to every worker in this vineyard: To him who improves the invaluable instrument with which we, especially, work—the microscope; and to him, to anyone indeed, who adds a single fact to the science of life. It has been cast at us, as Fellows of this Society, that we do nothing but improve our tools, or measure the markings on the frustule of a diatom, or count the bristles on a flea's nose. We need care but little for these criticisms; whatever we have done, if so be it has been done well, will have a recognized and permanent value. However, it is possible and even desirable that we should improve; and this one thing we will do, and our monthly journal shall witness for us that we have not been faggoted together into a society in vain. I am afraid you are saying this is a long *introduction;* it is; but the *discourse* shall be short. My proper duty for this evening was not to go rambling over all creation, "rounding about," as Bacon would have said, but to ask your attention to some plain details concerning my own work. This work is on one special subject, *viz.* the formation of the skull and face of one of the ordinary mammalia. It was my purpose to have taken the guinea-pig as a fresh subject of research, but I was led to change it through the kindness of my friend Mr. Charles Stewart, who put into my hands about seventy embryos of the common pig.

These are of six or seven different stages, and range from the size of a bee's grub to that of a new-born kitten. I have supplemented these by specimens of the sucking-pig, and the carefully prepared skull of one that died in an emaciated condition at six months old. This specimen, being absolutely free from fat, is as pleasant an object as if it were composed of ivory. For my work, as many young elephants and hippopotamuses, or even *hairy* rhinoceroses, would not have served my purpose better than these germinal pigs. Indeed, I very much question if a more *central* type could be found. It is commonly believed that the pig comes very near to man in its internal structure; happily for us, zoology places it very far from *our* group. Yet, when my plates are finished, they will show the marvellous conformity between the highest and the medium type of the mammalia. I am satisfied that all those who are not in the secret will suspect me of having laboured at the primordial form of my own species. The size of the smallest of these embryos is $\frac{7}{12}$ths of an inch in length, then an inch, then an inch and a third, and so on. But the complete bony framework of the skull and face is best seen in the skull of the half-grown individual, where all the bones are well developed, but are, to a large extent, distinct from each other; in old individuals, they are largely coalesced. The higher types of vertebrated animals undergo as real a transformation as the lower, such as the newt and the frog; so that we do not watch merely the expansion of the parts, but also their metamorphic changes. This metamorphosis is exactly like what is seen in the lower forms, but it runs to a higher pitch, and takes place more rapidly; whilst hidden from ordinary view, like the concoction of gems in the lower parts of the earth, unvisited by rays from sun or moon. As you may suppose, all the parts in the youngest specimens are very soft; much of their tissue being a sort of jelly, having little cells, or masses of "bioplasm" scattered through it. When a more solid structure is about to be formed, the cells breed rapidly, and thus clouds of more crowded granules are found gathering together, as the vapours gather together in the sky. In fine sections, that have been stained with carmine, these cell-clouds are extremely beautiful; perhaps the most elegant object of this sort is the nascent tooth-pulp, which has the appearance of a granular fruit, such as a mulberry. As for the blood-vessels, streaks of cells are formed, which run into each other, to make a network; the outer cells form the wall of the vessel, and the inner ones, proliferating rapidly, fill the tube with blood disks, which float free, and circulate at a very early period through the canals. The cartilage is at first merely a cloud of granules, distinguished from the surrounding tissues by their closely-packed condition. They breed at this stage with extreme rapidity. Gradually these masses acquire a clear margin, and then a pith and a bark can be seen in sections; the pith is cartilage, the bark is its investing perichondrium. And so for tissue after tissue; for if a cavity has to be formed, the cells vacate a certain region, and then the new-born cells, standing on end, and closely packed together, become its epithelium, or lining skin. The first formation of the cranium is not an easy process to observe. In its simplest part, at the roof, it is merely the innermost part of the skin, subdivided again into a dense membrane,—the dura mater, and the cranial roof bones external to this. But the floor and side-walls are pre-formed in cartilage, the morphology of which it is not easy to make out. All the specimens from which my objects are made, are preserved in alcohol; nothing can take the place of this old-fashioned way of keeping moist tissues. Beginning with the youngest, these, for sections, are dried on blotting-paper, and imbedded in paraffin; a sharp razor being used for slicing them as they lie in the cheesy mass. The slices are one by one transferred into alcohol again, by the use of a small bent slip of tinfoil; they are then stained with an ammoniacal solution of carmine, and are mounted in glycerine, to which a small quantity of muriatic acid has been added. The sections thus prepared are very beautiful, the protoplasmic masses taking up the colour very rapidly. As soon as cartilage begins to be formed, the intercellular substance not taking up the colouring matter, a very different appearance is presented, and the tracts of this tissue are well marked. For making solid sections, and for dissection of the early embryos, I prefer to put them for awhile into a weak solution of chromic acid; they can then be divided vertically or horizontally, being held between the finger and thumb. Dissection must be done in water, on a black substance; paraffin and lampblack make the best cake of this kind, and the object can be fastened on to it with pins. The dissection is made by fine needles, mounted in small holders. It is anxious work, and is done with the help of a pocket glass. Of the smallest embryos, portraits have to be taken of very perfect specimens which have not become shrunken in the spirit. These are of great value, as they show the form and relations of the principal masses of the skull and face. They also serve as a *platform* on which the mind can place what it has discovered of the structure of the parts as displayed by sections. After the embryos have grown somewhat, and bone begins to appear, they can still be treated like the smallest, for the first traces of bone do not turn the razor. These early traces of bone are of a rich crimson colour in specimens that have been coloured with carmine. In larger specimens, the heads must be placed in a weak solution of chromic and nitric acids; this acid must be much stronger, and be used for a much greater time, because of the solidity of the bone. These larger specimens make very valuable thick sections to be used as opaque objects, and with a low power; and each face of a slice, $\frac{1}{8}$th or $\frac{1}{10}$th of an inch in thickness, shows something fresh, so that each object is practically double. Perfect sections of the heads of embryos taken from snout to occiput vertically, are of great value; a very fine saw has to be used for this purpose on the older ones. The section should be made a little to the left, so as not to injure the septum of the nose. Bird's-eye views of the skull-floor are taken from unroofed preparations; these are very valuable, but they require extreme care in dissection, and all the *landmarks* have to be observed and drawn, the nerves, arteries, veins, muscles, and the like. Then there are lower and lateral dissections to be made, so that no stone be left unturned in the elucidation of the problem of the growth of the skull. Now, it will be clearly seen that such a thorough root-and-branch sort of work as this gives the worker no easy task; it is such as does not admit of being hurried; but once done, it serves as a felled opening in a large wood; and when such a space is connected with similar cleared spaces, a survey can be made of what was tangled and dark enough at first, but which makes fine fields when well tilled.

I cannot give you an abstract of *results;* everything is in progress, and nothing finished; moreover, it is natural to us to delight in unfinished works, but the objects at which we labour are all perfect; completeness, beauty, and unity are stamped upon them all.

How the *unity* has arisen I do not pretend to say; if all organic forms have become evolved from one common parental protoplasm, the *planting* and *stocking* of this planet has been a slow business: I more than suspect that there has been an overruling *Will:* and that the whole was foreordained.

II.—*On the Development of the Skull in the Genus Turdus—the Thrushes.* By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *May* 1, 1872.*)

Plates VIII., IX., and X.

The Singing-birds, both as living creatures and as *organisms*, have for many years exercised a sort of witchery over me, so that I am always ready to give them my attention; and they perhaps have engrossed an undue share to themselves. There are plenty of them; the types of Passerine birds almost rival the fishes of the sea in multitude; and as to individuals, they are a prolific sort of bird, and abound everywhere. Yet they are the highest kind of birds, notwithstanding; and in them the great Oviparous group, called "Sauropsida," culminates. Amongst the cold-blooded groups the Lizards come in most naturally for comparison with "carinate" birds; yet, morphologically considered, these are mere *pupæ* as compared with them. The particulars of conformity and of non-conformity between these two great groups are of great interest, and the genus here to be considered is important, in that, in one

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EXPLANATION OF PLATES VIII., IX., AND X.

Plate VIII., Fig. 1.—Lateral view of skull of *Turdus viscivorus*, 3 or 4 days before hatching, × 3 diam.
" " 2.—Lower view of ditto, × 3 diam.
" " 3.—End view of ditto, × 3 diam.
" " 4.—Fore-part of palate of same, × 9 diam.
" " 5.—Hinder-part of lower view, × 9 diam.
" " 6.—Same object, upper view, × 9 diam.
" " 7.—Inner view of mandible of same, × 3 diam.
" " 8.—Mandibular symphysis of same, seen from above, × 9 diam.
" " 9.—"Cornu major" of "os hyoides" of same, × 3 diam.

Plate IX., " 1.—Lateral view of skull of *Turdus merula*, 1 day old, × 3 diam.
" " 2.—Lower view of ditto, × 3 diam.
" " 3.—Upper view of ditto, × 3 diam.
" " 4.—End view of ditto, × 3 diam.
" " 5.—Posterior part of basal view of ditto, × 6 diam.
" " 6.—Floor of cranium of same, seen from above, × 3 diam.
" " 7.—Part of end view of skull of same, unroofed, × 6 diam.
" " 8.—Mandible of same, inner view, × 3 diam.
" " 9.—"Os hyoides" of same, × 3 diam.

Plate X., " 1.—Side view of skull (with mandible) of *Turdus merula*, 1 week old, × 2 diam.
" " 2.—Lower view of ditto, × 2 diam.
" " 3.—Upper view of ditto, × 2 diam.
" " 4.—End view of ditto, × 2 diam.
" " 5.—Section of skull (with inner view of mandible) of the same × 2 diam.
" " 6.—"Os hyoides" of same, × 2 diam.

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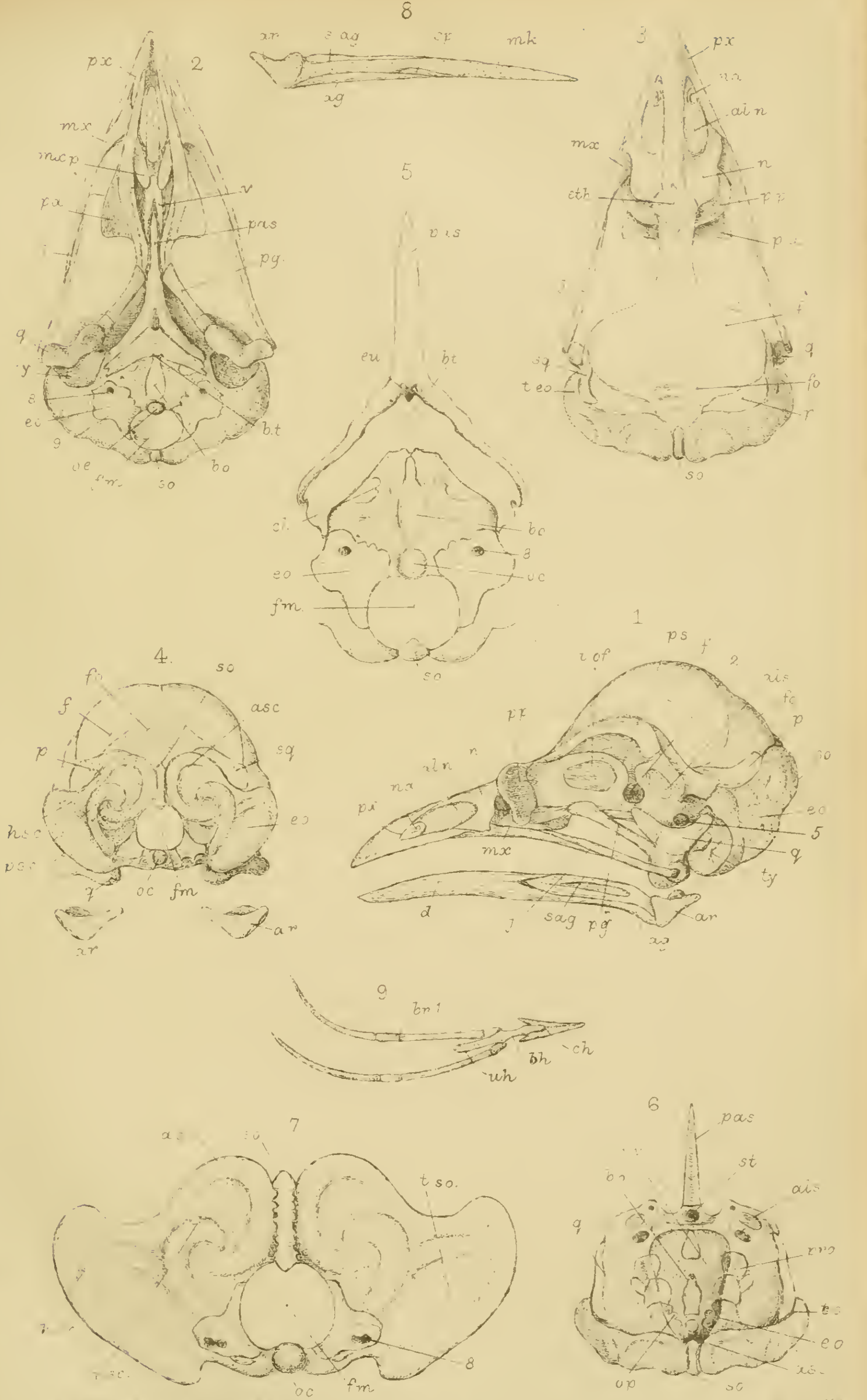
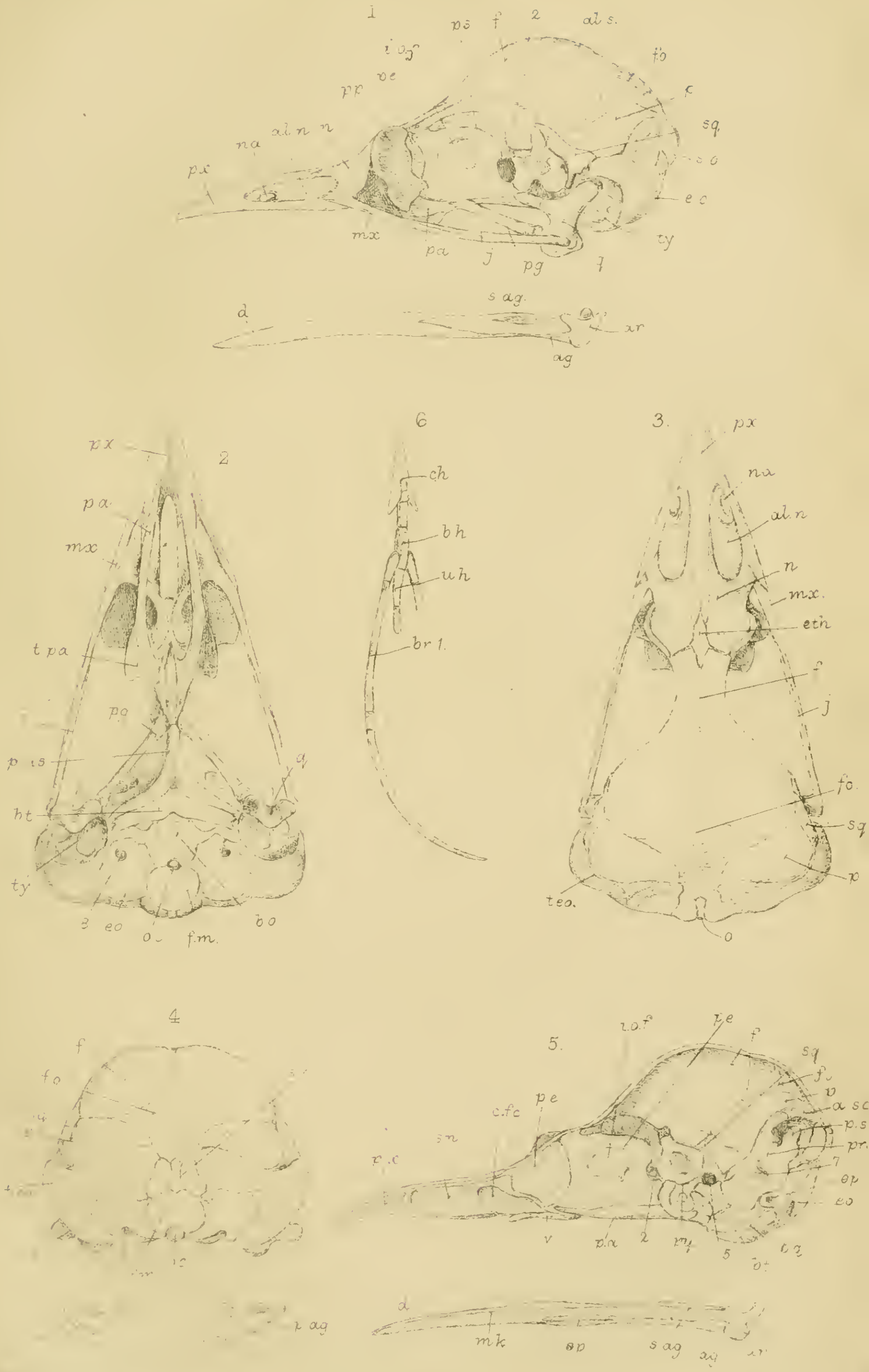
* It is but fair to state that, although this paper was read so long ago before the Royal Microscopical Society, it did not pass into the Editor's hands till February 18, 1873.



Development of Blackbird's Skull



Development of Blackbirds Skull

outstanding point of structure—the development of its occiput—its members agree with the Lizard, and with reptiles generally, and not with the other birds—even the other Passerines.

I have shown the earlier condition of the Passerine skull in my former papers—on the Crow and Titmouse, and similar studies of the skull in embryos of the Song-Thrush (*Turdus musicus*) have been made by me, but not figured. The earliest of these here given is that of the Storm-cock (*Turdus viscivorus*), three or four days before hatching (Plate VIII.). Here the skull is seen to be far developed, although the inworks are mostly composed of cartilage, and the outworks of very thin membrane-bones, easily peeled off the spirit-preparation. Looking at the basis-cranii from below (Figs. 2 and 5), we see that the notochord has become ensheathed by a bony deposit; the basi-occipital (*b. o.*) has the form of a scale of wheat-chaff. This new bone is surrounded by cartilage common to the "investing mass" and the auditory capsule, and in the substance of the latter the rudimentary cochleæ shine through. On each side of the foramen magnum (*f. m.*) we have now a leafy, lobate bone, enfolding the vagus (8) as it passes out between the auditory and occipital cartilaginous regions. This lime-hardened tract of the occipital wall passes upwards on each side the great hole for the spinal chord, but the keystone piece has not appeared (Fig. 3, *s. o.*). Undergirding the skull-base, hiding part of the cochlear cavities, and forming an extraneous floor to the "pituitary space," we see the basi-temporals (*b. t.*) like an inverted saddle; they now form one bone by median union of substance. This bone is notched in front and behind—the remains of the interspace between the two original pieces, and also, laterally, they are fenestrate; the double top in front underlies the converging Eustachian tubes and the pituitary body. These bones, like the spiked and winged bone in front of them—the parasphenoid proper—are ossifications of a thick *felt* of connective fibrous tissue, which, however, was calcified very early, and whilst the cells were scarcely spindle-shaped.

The parasphenoid (*pa. s.*) is a long scoop-shaped bone, ending behind in *wings* that rival the basi-temporals; these wings are extravagant periosteal outgrowths from the bony matter ossifying the apices of the trabecular bars.

The whole bone is an azygous splint applied to the under-face of the soon-coalesced moieties of the first visceral arch—the trabecular arch; and the wild growths behind are economized to form the anterior recess of the complex tympanic cavity—the primary condition of which cavity was the first *post-oral* visceral cleft—a *crack*, as it were, or dehiscence in the facial wall of the embryo. Strange as it may seem, the "parasphenoid" is a *facial*, and not a *cranial* bone, and the harmonious co-working of these facial bars and plates, to form secondary sense-chambers outside the main brain-chamber, is well worthy of notice. The bars of the face not only *conjugate* to form their own basket, they form many and notable connections with the cunning-work of the sense-capsules.

As the cochleæ show through the hyaline cartilage below, so also do the elegant "semicircular canals" appear in the hinder view of the skull (Fig. 3); and their form would seem to have been for *beauty* rather than for *use;* yet Nature obtains both these *ends* by one process. The great fontanelle (*fo.*), or exposed part of the membranous cranium, is still very large; the roof bones, parietals (*p.*), and frontals (*f.*) being scarcely half their proper size. The nasals (*n.*) have already their characteristic form; and the thin sickle-shaped squamosals (*sq.*) are quite like those of a Lizard at this stage. Returning to the basal view (Fig. 4), we see that there is a projection answering to the basi-pterygoid (*b. pg.*), although it is here in these types at its lowest development, and shows scarcely at all in the adult. Farther forwards we see the end of the parasphenoidal rostrum sheathing the base of the perpendicular ethmoid; this base being, indeed, formed by the trabecular commissure, and, somewhat higher, by the crest growing upwards from that arch. Already this part is cleft off from the "septum-nasi": and also between the eyes (Fig. 1, *i. o. f.*) a "fenestra" has appeared, dividing the trabecular crest below from the ethmo-presphenoidal bar above.

The optic nerve (2) passes out of a notch between the presphenoid (*p. s.*) and ali-sphenoid (*al. s.*): this latter part is fenestrate, and has below and behind it a foramen (*f. ovale*, 5) for the trigeminal nerve. The only intrinsic bone in the cranio-facial axis, yet developed, is the basi-occipital; but the parasphenoid (*pa. s.*) is grafting itself upon the cartilage bounding the pituitary space, namely, the fore-part of the "investing mass," behind, and the apices of the trabeculæ around and in front. The nasal septum, like the lateral and median ethmoids, is still altogether cartilaginous; the lateral ethmoids appear on the top of the head. The alæ-nasi (*al. n.*) give off huge turbinals (Fig. 4, *al. t.*), and these are separated by the trabecular bar, which is rounded behind, and alate to a large extent in front: it terminates in the spatulate præ-nasal cartilage, the azygous model on which the præ-maxillaries are formed. This rod is becoming absorbed behind, and will soon disappear.

The ali-nasal cartilage turns inwards behind, and each moiety of the broad-fronted vomer (*v.*) is grafted upon the corresponding cartilaginous flap; this is the true "Ægithognathous" structure. I have not seen any "septo-maxillaries" on the angles of the vomer in the Thrushes. The halves of the great præ-maxillary are indicated by a large notch in front; the various processes, nasal, marginal, and palatine, are all well developed.

The delicate styliform ichthyic maxillaries send backwards the usual jugal style, behind which is the jugal bone (*j.*), with no separate quadrato-jugal, in these, the highest birds. The maxillary also sends inwards and backwards the spatulate "maxillo-palatine process" (Fig. 4, *mx. p.*).

The long palatines, and the short pterygoids (*pa. pg.*), are already well developed, the former sending out its trans-palatine flap of cartilage. The quadrate (*q.*) has acquired an endosteal patch above, but most of it, like the "articulare" below, is cartilaginous; so also is the rest of the mandibular arch; but four investing bones, *viz.* the splenial, dentary, surangular, and angular (*sp.*, *d.*, *s. ag.*, *ag.*), are already far developed.

I do not see a coronoid in this type. The distinct ends of the Meckelian rods are well seen here, underborne by the two distinct dentaries (Fig. 8, *Mk. d.*). The only bones in the hyoid arch are the tiny medio-stapedal rod, and the shaft of the lower piece of the "cornu major" of the "os hyoides."

In about four days later the Thrush's skull shows much to interest the observer (see Plate IX., *Turdus merula*). All the parts described in the last stage can now be seen more clearly in their fuller development, although very few new centres of bone are to be seen. The most important of these hardened territories is now, however, well shown; this is the "super-occipital" (*s. o.*); it is a linear vertical patch, affecting the cartilage which lies between the huge anterior semicircular canals (Fig. 7, *s. o.*, *a. s. c.*). This is a true *reptilian* bone; and I have seen it *single*, as yet, in no other bird; not even in the Redbreast, Sparrow, or Crow.

In an unroofed skull (Fig. 6) we see the large prootic (also shown in the last stage—Plate VIII., Fig. 6, *pro.*); between this main "petrosal" and the ex-occipital is the small "opisthotic" (*op.*). This figure shows the squamosals at the sides, and the "rostrum" of the parasphenoid projecting in front; the more solid part of the bone has worked its way into and behind the deep sella turcica (*s. t.*): between the end of this bone—now a veritable "basi-sphenoid"—and the basi-occipital, we see the "spheno-occipital synchondrosis." Even in this minute preparation the ali-sphenoid (*al. s.*) shows its fenestra, and its two foramina (*f. ovale* and *f. rotundum*). In the basal region (Fig. 5) the other occipital bones—lateral and basal—are much more developed, and the cochleæ (*cl.*) are well seen in the remaining clear cartilage.

Farther forwards, the "basi-temporals" (*b. t.*), and the parasphenoid (*pa. s.*) are seen to have acquired increased solidity and ankylosis on the lower surface. An upper view (Fig. 3) shows well the relation of the various parts of the roof, and the gradual advance of the bony territories. The large size of Meckel's cartilage (Fig. 8, *Mk.*) is shown by removing the dentary.

The "os hyoides" has still the single bone on each side, behind. The extreme beauty of the auditory labyrinth is displayed in the outspread occipital cartilage (Fig. 7), which contains much of the structure of the inner ear. The rest of the figures of this stage will speak for themselves, as mere advances upon the last stage.

In nestlings of *Turdus merula* a week old, the elegance of the sylvine type of skull becomes more manifest (Plate X.); taking figure by figure, we shall observe how the advance is all in the direction from a somewhat generalized to an extremely specialized form of skull. On the side (Fig. 1) we see the skull-roof is much more complete, the beak becoming more ossified and slender, and an osseous centre in the perpendicular ethmoid (*p. e.*), outside of which is the large "pars-plana" (*p. p.*). The ali-sphenoid (*al. s.*) is largely ossified, and also the quadrate (*q.*), and below this the articulare (*ar.*) is now bony. Below (Fig. 2), besides the extension of the bony matter, we have the more typical form of the various parts, everything becoming more and more slender and elegant, as is the wont in these soft-billed singing types. Especially we may note the more out-curved and enlarged extremities of the maxillo-palatine hooks, and the finer shape of the parasphenoidal rostrum.

In the upper and hinder views (Figs. 3 and 4) we find that the azygous super-occipital (*s. o.*), which has now grown on to the arch of each anterior semicircular canal.

The sectional view (Fig. 5) shows the cranio-facial cleft (*c. f. c.*) between the septum-nasi and the fast-ossifying perpendicular ethmoid (*p. e.*): the rest of the interorbital septum is soft. The position of the vomer (*v.*), with regard to the parasphenoid (*pa. s.*), is shown; and the latter bone has grown far up into the sella turcica, and has coalesced with the basi-temporal (*b. t.*). The fenestrate ali-sphenoid (*al. s.*) has on its supero-posterior angle the square inner face of the large squamosal (*sq.*), and behind this and the oblong parietal (*p.*) is the huge "prootic" (*pro.*); behind which is the small opisthotic wedge (*op.*); which in turn is followed by the ex-occipital (*e. o.*). The prootic, basi-occipital, and basi-sphenoid, together form a triradrate suture. I have not been able to find either an "epiotic" or a "pterotic" in these birds. The inner face of the mandible (Fig. 5) shows no "coronoid"; the median part of the "os hyoides" (Fig. 6) has a basi-hyal, and a "uro-hyal" bone; and the upper shaft has been developed on the large cornu (*Cr.* 1).

In the growing and adult birds I have studied *Turdus merula, musicus, viscivorus, pilaris,* and *iliacus;* the skull differs from that of the Crow in *elegance* as well as in size; and the bony tissue is extremely light and delicate. The bony "siphonium" is as large relatively; but the smaller additional bones are fewer and lesser. The septum-nasi does not ossify, nor any of the nasal cartilages in front of the "hinge." The ear-drums, formed by the ex-occipitals, are very large and hollow; the maxillo-palatines are very much scooped for an air-sac, and are like out-bent ladles, with the hollow part below. The ethmo-presphenoidal bar is narrow, and has a large permanent fenestra beneath it, and a pair of post-orbital fontanelles above. The anterior sphenoid is formed by one bone only, with scarcely a trace of orbito-sphenoidal lips. In one specimen of *Turdus merula* (adult) the septo-maxillaries are partly separated from the vomer by a "fenestra" on each side. Altogether, this is a type which well repays the painstaking student.

III.—*Note on Reduced Apertures.*

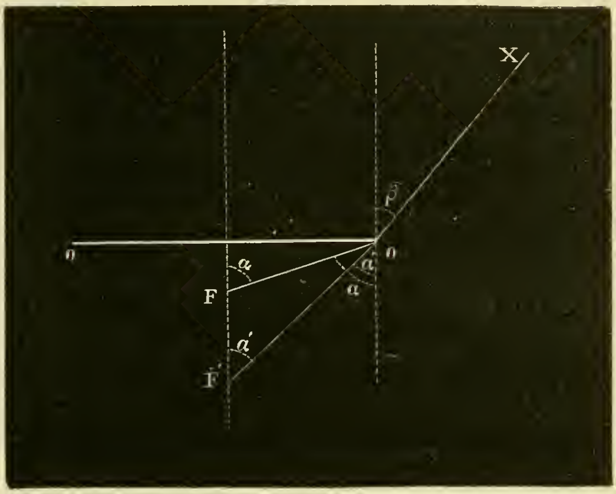
By Rev. S. Leslie Brakey, M.A.

When an object is immersed in any liquid it has been shown, with superabundant proof, that the aperture of the object-glass cannot exceed a certain determinate limit, which limit varies with the nature of the medium. But the limit so fixed is only a *maximum*, which cannot be exceeded; that is, it is not to be understood that it will in general be reached, or even nearly approached. It is, in fact, the angle which corresponds to the theoretical limit of 180° of aperture in air. As in air every glass falls short, more or less, of this angle, which is only theoretically possible, so with preservative media the actual angle will always be less than the maximum referred to. Its absolute magnitude depends upon the absolute magnitude of the angle in air, increasing and diminishing along with it. To determine its amount for any glass, we may of course, if we like, proceed simply by experiment, as exemplified in the case of the glass examined and reported upon in the January number of this Journal, at p. 29. But the kind of experimenting required for such cases is troublesome; and costly as involving a special apparatus; and with some fluids dangerous, since the obliquity of the position required in immersing the whole front will in general necessitate the immersion of the working parts (the screw-collar and screw of the nose-piece). It is very unlikely, therefore, that any microscopist will be found so zealous as to work this experiment himself for his own glasses. Fortunately, this is unnecessary. In a letter inserted in the November number of last year, at p. 247, I observed that the actual angle may always be found by calculation, without experiment. The calculation is easy, and its *rationale* not difficult to follow; and this it is the purpose of the present note to point out.

O O (Fig. 1) is the front of the object-glass, F the focus for air, and F O the extreme ray, refracted to O X. Then a will be the semi-aperture, and of course at the same time the angle of incidence. F′ being the focus for the new medium, the extreme ray F′ O will also be refracted into O X, so that β is the common angle of refraction. Now the sine of a' is to the sine of β as the index of the medium to the index of the glass, inversely; and the sine of β is to the sine of a as the index of the glass to the index of the air, inversely. Compounding these ratios, the sine of a' is to the sine of a as the index of air to the index of the medium; that is (since the index for air is unity), the sine of a' is equal to the sine of a divided by the index of the medium.

Therefore to find the aperture for any preservative medium:—Find by experiment the semi-aperture in air, divide its sine by the index of the medium, and the quotient will be the sine of the new semi-aperture.

FIG. 1.



To exemplify this, let us take the glass experimentally tested in the January number already referred to. In air the aperture was found to be 145°, *i.e.* the semi-aperture = $72\frac{1}{2}$°, and we wish to find, without experiment, the aperture, *e.g.* for water. The sine of $72\frac{1}{2}$° is 0·9537, which divided by $\frac{4}{3}$, the index of water, gives 0·7153, which number is the sine of 45° 40′. This, therefore, is the semi-aperture for water, *i.e.* the aperture = 91°, agreeing with the observed aperture within a fraction of a degree; from which we may remark, in passing, the extreme accuracy with which these experiments must have been conducted.

To find the aperture for balsam, we divide the same number by 1·549, the index for balsam, which gives for the semi-aperture 38°, aperture 76°. The aperture observed was in this case 79°, which, therefore, though within the *maximum* limit, appears to differ slightly from what theory would indicate. This apparent discrepancy, however, is only half what it appears to read; for it is to be remembered that the angles ascertained by theory for comparison are not the apertures, but the semi-apertures, and these in the present case differ not by 3° but by $1\frac{1}{2}$°. In the commentary appended to the record of the experiment, this difference is ascribed to the fact of the balsam used having been very fluid—approaching turpentine. To test this, the index of the balsam being in any case not so low as pure turpentine, let us, at a venture, assume it to be an arithmetic mean between the two. Computing with this index even the slight difference recorded disappears, and the coincidence of the theoretical with the experimental results is shown to be absolutely perfect.

If the glass tested should possess the extreme aperture (170°, or upwards), the fluid balsam used in the experiment would give the reduced aperture within a few minutes of 83°; the reduced angle changing much more slowly than the angle in air when near its limits.

It is to be observed that the investigation given above shows —what before examination might not be suspected—that the results are entirely independent of the kind of glass used for the objective-front. The index of the glass occurring both directly and inversely disappears from the compound ratio; so that the reduced aperture remains exactly the same, whatever may be the nature of the glass, or the value of its refractive index.

IV.—*The Structure of Eupodiscus Argus.* By Samuel Wells.

Plate XI. (Lower portion).

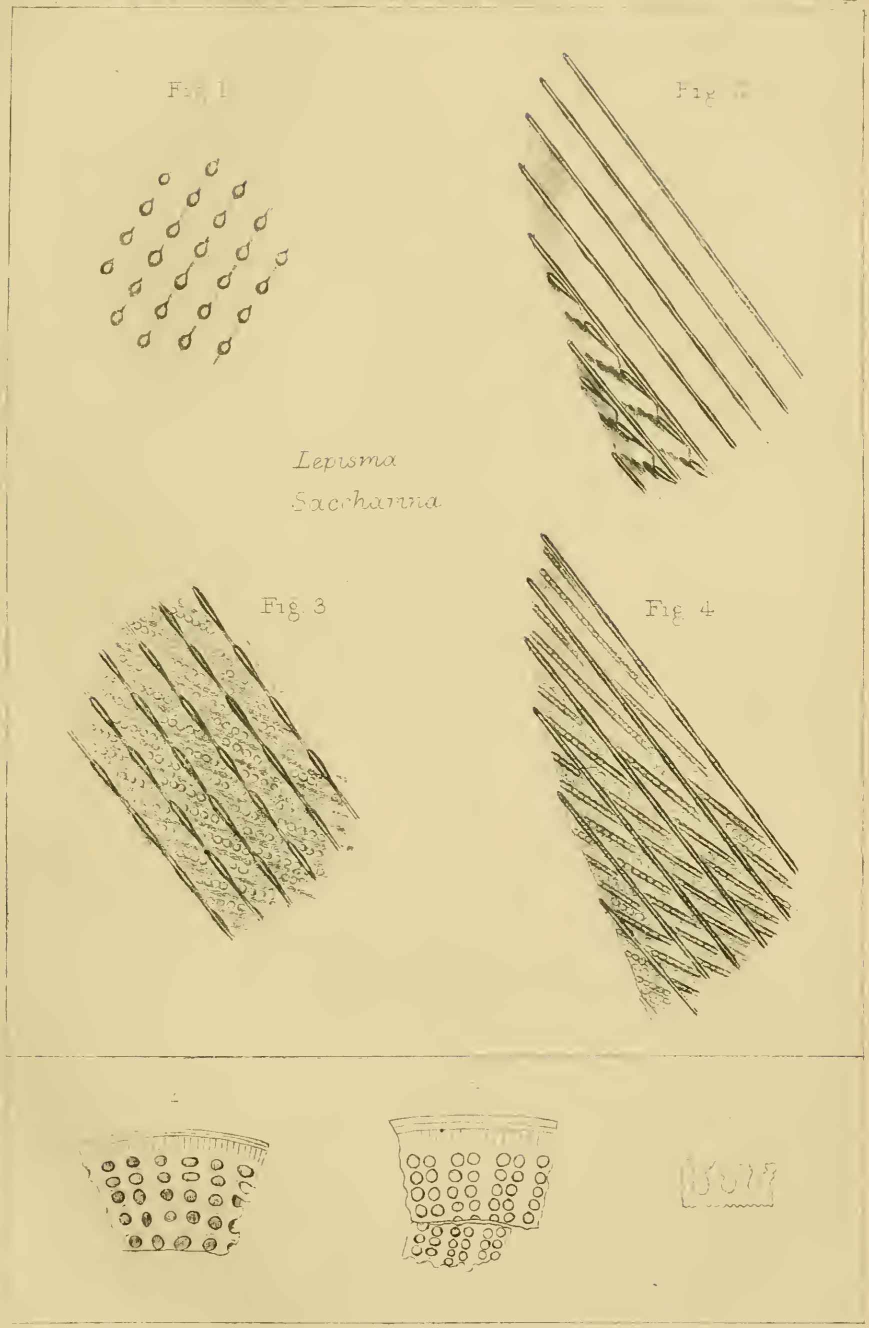
The elucidation of the *Eupodiscus Argus* given by Mr. Slack in your December number did not agree with my previous observations; but as I had a supply of specimens obtained from sea-weed washings in Buzzard's Bay, on the south coast of this State, I made further examinations to see if I could discover the appearance represented by Mr. Slack. I looked at different valves dry and in balsam, covered and uncovered, using Beck's parabolic reflector, as well as Professor Smith's arrangement for opaque objects, the use of which was recommended by Mr. Slack in his paper.

The valve of this diatom is remarkable for its opacity, its thickness being about $\frac{1}{4000}''$; it presents, therefore, a beautiful appearance as an opaque object with a binocular. The structure of the outer or convex surface can be readily made out with a low power.

It is dotted with depressions irregular in size, shape, and arrangement; between these depressions the surface rises in ridges, which glisten and sparkle like fresh snow. No arrangement of light (except transmitted) varies this appearance. The depressions are unmistakable, and, as appears by the use of the binocular, and the examinations of the edges of fragments, are pockets extending nearly, but not quite, through the valve. In Fig. A I have endeavoured to represent the upper surface of a fragment, and in Fig. C a vertical section.

The average diameter of these depressions is about $\frac{1}{8000}''$.

The inner or concave surface is much more difficult of resolution; its structure is quite different to that of the convex surface. It is nearly smooth, has no ridges, and (probably) no



Spurious appearances — Eupodiscus Argus

granulation. It is covered with round dots, radiating irregularly from the centre, and leaving irregular blank spaces between the rows. It is probably this surface that is figured by Mr. Slack, who makes no mention of any difference between the two surfaces, but appears to have made the drawing from a specimen on Möller's typen platte. In my typen platte there are the eighteen-corner Eupodisci, and three others, and all were mounted *concave* side up, which is the easiest mode of making them stay in place.

A difficult question is now met, and that is, what is the nature of these dots? The solution of this question affects Mr. Slack's theory of the depositions of silica in spherules, and also the interpretation of appearances given by Mr. Wenham's "Reflex Illuminator." If we concede these dots to be spherules, then Mr. Slack has gained one side of the valve only, and must further show that the upper side is covered with them; perhaps he can succeed in this; but I think he must fail on the processes, which are as clear and smooth as glass rods.

If we examine these dots by light both from above and below, the transmitted light passes through the large depressions on the convex surface, and thus through four or more of the dots on the inner surface, making them brighter than their neighbours. We have then the appearance figured by Mr. Slack.

I can see them with a $\frac{1}{10}$, but get a very distinct view with a $\frac{1}{20}$, and Professor Smith's apparatus, and to my eye, with that light, they are slight depressions, like dents, with a white spot in the centre as if the bottom of the dent were slightly convex. The average diameter is about $\frac{1}{20000}''$.

I have tried to represent them at B in a fragment more highly magnified than that at A.

The examination of other species of this genus cannot determine the structure of the *E. Argus*, for we cannot affirm the identity of structure of two species until we have satisfactorily determined that of each.

I have no opportunity of using the "Reflex Illuminator," and therefore send with this a slide of fragments mounted dry, which I think will prove instructive to anyone who wishes to investigate the subject, and has access to different modes of illuminators.

Boston, Mass., U.S.A.

[The reader should refer to Mr. Slack's letter on this subject in the present number.—Ed. 'M. M. J.']

V.—*On Spurious Appearances in Microscopic Research.* By G. W. Royston-Pigott, M.A., M.D. Cantab., F.C.P.S., F.R.A.S., M.R.I., M.R.C.P., F.R.M.S., formerly Fellow of St. Peter's College, Cambridge.

Plate XI. (Upper portion).

The "Battle of the Glasses" is gradually being fought out. But until spurious appearances are no longer accepted as the true, it is impossible that definition in the microscope can arrive at that degree of perfection which is required by the spirit of the age.

It has been my unpleasant task to point out the residuary aberration of the best glasses, and until then unsuspected and stoutly denied on all hands. This was followed by very considerable improvement. But the old school of microscopic belief still attracts its disciples; and I wish here to point out another glaring error in definition, and an acceptance by such microscopists of the false for the true. I had the honour of pointing out to the Fellows in 1869,* the structure of the *Lepisma Saccharina;* but as the remark has been often reiterated by writers against my views that beads are merely caused by the intersection of striæ, and as the Lepisma has been figured with such false beading, the accompanying Plate has been drawn from the object in the field of view of the microscope by the talented young artist, Mr. Hollich, to my complete satisfaction.

The first figure on the Plate represents the spurious appearance of exaggerated beads shown at the intersections of the upper and lower ribs of the Lepisma at the part where they cross and intersect at a considerable angle. In this case the objective was improperly corrected for spherical aberration. In each of the examples here described, the direct light of a good half-inch objective used as a condenser is employed. The paraffin lamp flame is also placed in the axis of the condenser.

I now substituted an old one-eighth of 70° aperture, but a fresh scale came into view in the positions of figures 2, 3, and 4. This glass, made by Powell and Lealand about twenty-five years ago, has the middle and back sets of lenses placed in contact; we then saw the form, Fig. 2, Plate XI. Each rib is terminated by an intensely black point, and each rib towards the middle shows beautiful alternate thinning and thickening of the ribs at regular intervals, the intense black lines composing them showing rudimentary *breakings* of their parallel rulings, but at the left side close imitations of the *Podura markings or spines* accompanied by oblique shadows, *the spine* bisecting the angle formed between the oblique shadows and the ribs. On examining the beautiful drawing of Fig. 2 with a pocket lens, these remarkable appearances will at once be evident.

* Page 303, vol. ii., 1869.

In this case, with the direct light of the condenser, no signs of beading appeared; a faint shading exquisitely drawn at the upper part suggested structure of some sort in the intercostal spaces.

Progressing in definition, I now withdrew the antique eighth, and replaced it by the celebrated Powell and Lealand *immersion eighth* of 1870, and re-adjusting the collar, the appearance, Fig. 3, was displayed, with which Mr. Hollich seemed greatly surprised and delighted, judging from his mute signs and expression of countenance. He wrote on paper, he had never seen anything like this before, though he had often drawn the Lepisma. The *double beading* he readily copied and portrayed the false appearances of Podura-like markings with evident glee.

The whole of the blank spaces shown between the ribs appeared crowded with a symmetrical arrangement of double beading straightly radiating from direction of the quill. In the drawing the two sets are distinctly shown as darker (ruby colour) and lighter (pale yellow, and sometimes sapphire), whilst these are broken up by the false Podura-like spine displayed in the figure.

In order to show the true structure of the Lepisma, the *dry one-sixteenth* was then substituted, and the "A" eye-piece.

A minute apertured cap was placed upon the condenser, *between it and the object*. After careful adjustment Mr. Hollich now drew Fig. 4. The spurious spines disappeared. The ribs most brightly, sharply edged with a slightly undulating black border, display the spines shrunken to a slight thickening of the crossings of the darker substratum of beading.

Compare now these delicate developments of structure with the coarse spurious beading drawn in Fig. 1. Common sense at once revolts against accepting this appearance as even a rough approximation to the truth. Again, the spurious spines of Fig. 3, significantly suggest that those of the Podura, once so generally believed in, are also a delusion. And the delusion is the more pernicious, because it compels the makers to construct their glasses so as to show this delusion in the sharpest form. By many persons the spurious beads seen at the intersecting striæ of the Lepisma are considered quite the correct thing. But when once the real structure shown at Fig. 4 is seen by them, their faith in their best glasses begetting these microscopical shams, fades away at once.

My object in writing these papers, is to induce those observers who rest content with glasses imperfectly constructed to search for a higher standard of definition. The day may arise when a verdict involving human life may depend upon microscopic perfection. I may be excused for saying that I consider that I am discharging a duty, as a retired physician, in boldly denouncing spurious appearances when accepted as true.

The beading here described is very distinctly visible, with my aplanatic searcher and a quarter objective.

The spurious spines of the *Lepisma Saccharina* ought to be seen, as Mr. Wenham described the Podura "note of admiration" markings "with that distinctness and sharpness of definition that so delights the eye of the optician."* Both the Lepisma and Podura scales are formed of beaded striæ crossing at a variable angle. The optician ought, therefore, to delight in the note of a 'miration markings developed in (Fig. 3), shown by one of the best objectives now made when incorrectly adjusted.

The study of spurious appearances under the use of a variety of objectives of high and low degrees of excellence, is one of the most instructive subjects of microscopic research. Notwithstanding that the famous Podura mark so delights the eye of the optician, a more profound resolution dissipates this spurious appearance, as well as the false beads of Fig. 1, and the *quasi* Podura "notes" of Figs. 2 and 3.

It is very easy to produce the false appearance of scattered spherules somewhat similar to Fig. 1 in the Podura as described by Mr. Wenham.

VI.—*Professor Smith's Conspectus of the Diatomaceæ.*

By Captain Fred. H. Lang, President of the Reading Microscopical Society.

The study of the Diatomaceæ, instead of becoming easier, is getting more difficult every year, simply because new genera are constantly being added, on most insufficient grounds, to the ever-lengthening list, and because there is no one authority on the subject. Ehrenberg, Rabenhorst, Kutzing, W. Smith, Ralfs, Arnott, Brebisson, Donkin, Kitton, and a host of others, besides Dr. Pfitzer, who has just published a new classification based on the method of reproduction of the frustules, are all more or less authorities; but to whom are we to trust? And now Professor H. L. Smith has published in the American Microscopical Journal, the 'Lens,' his Conspectus of the Diatomaceæ, which, at all events, appears an effort in the right direction to reduce within a reasonable number of genera the vast variety of forms in this most prolific and multifarious order.

Probably any classification of these curiously-beautiful and interesting organisms must for the present be more or less artificial; his is certainly completely so, as he himself allows, based as it is entirely on the *tout ensemble* and general form and appearance of the

* Page 124, vol. ii., 'Microscopical Journal.'

silicious frustules or skeletons, without any reference to the mode of growth and habitat of the living organisms. As an example of his method of treating the subject, it is enough to state that Cocconema and Cymbella, the one a stipitate and the other a free form, are grouped together, Cocconema being, in fact, abolished as a genus and retained only as a synonym. And so Arachnoidiscus is placed in the same tribe as Melosira, the first being invariably a free form and the latter a mere integral portion of a long filament; and then, again, on the same principle, the frondose genera Schizonema, Collotonema, and Encyonema are done away with, their particular habits of gelatinous growths being ignored and the forms of their frustules only taken into account. It is true that Professor Smith follows Mr. Ralfs, and most modern diatomists, in this way of looking at the matter, but it is still questionable whether the plan of almost the oldest English diatomist, the Rev. W. Smith, is not the best, as it certainly is the most natural, *viz.* that of arranging the Diatomaceæ according to their habitat and method of growth, whether free, stipitate, concatinate, frondose, or gelatinous. Our author, however, in vindication of his system, says that he has not considered the conditions of growth sufficient to warrant the formation of new genera, a long study of living forms having convinced him that these characters are fleeting and not to be relied on; and he gives certain examples in proof of this statement.

Professor Smith divides the order into three tribes; the first mostly bacillar, with distinct raphe, cleft, or median line, with nodules; the second, generally bacillar, with pseudo or false raphe, or blank space; and the third generally circular, with a crypto or concealed raphe. His first tribe contains five families, the second three, and the third seven; and these fifteen families are again subdivided into one hundred and ten genera, exactly half of which belong to the third tribe. To one or other of these genera the Professor considers any new form may be ascribed. He has taken great pains in collecting together every known synonym. On looking over his index to the synonym register in the third number of the 'Lens,' it will be seen at a glance what a vast number of genera (189) are abolished, and apparently very properly in most cases. There are two or three familiar genera, however, whose extinction the old diatomists will scarcely relish, though the author may be able to justify it—Campylodiscus, for instance, is now relegated to Surirella, and, possibly, rightly so; there are arguments, however, against the incorporation, as the median line of the opposed valves of the former are at right angles to each other, whilst they are parallel in the latter; otherwise the *tout ensemble* on which Professor Smith chiefly relies is pretty similar. As a general rule certainly Surirella is more or less oval and flat, whilst Campylodiscus is circular and twisted;

but certain specimens of *S. fastuosa* are almost as round as many Campylodisci, and *C. spiralis* departs from a circular outline more than most Surirellæ; whilst in a gathering in my possession from Fish Spring, Salt Lake Desert, Utah, an apparent variety of *S. striatula* is as much twisted as a Campylodiscus. Again, Amphitetras and Triceratium are both combined with Biddulphia. The latter grows in long zigzag chains; I am not aware that the former does so, having never studied it in the living state, but I fancy not; but at all events Professor Smith takes no cognizance of this difference as to method of growth, attaching no importance to it. Biddulphia has two processes, Triceratium three or *more;* that, therefore, appears no valid reason why they should be separated. Biddulphia has usually a few spines, but so has *Triceratium armatum.* As a general rule, Triceratium is seen in its side, and Biddulphia in its front view, when the diatoms, of course, appear very different; but a front view of an entire frustule of Triceratium is remarkably like a Biddulphia, and in a slide of selected diatoms from Singapore which I have, containing entire frustules of *Biddulphia reticulata* and *Triceratium armatum,* these forms would doubtless be considered closely-allied species instead of distinct genera by anyone but an experienced diatomist. Of course, the same arguments would apply to Amphitetras, so perhaps the Professor may be right in abolishing even these old and well-known genera.

In the second portion of the Conspectus, in a sort of preface, the author enunciates his ideas as to the structure of the diatomaceous frustulæ, following out the hints of Mr. Carter in the 'Annals and Magazine of Natural History,' March, 1865, but which are much more fully worked out by Dr. T. D. M'Donald in the January number, 1869, of the same journal, and which it would seem has escaped his attention.

A Table of Species is promised hereafter, without which the present Conspectus would be not only incomplete, but almost useless. Such a Table will involve not only a vast amount of labour and trouble, but will require an intimate knowledge of every form. Though many so-called species may be treated as mere varieties, the Professor will scarcely be able to use his pruning knife as trenchantly as he has done with the genera, and in the case of his present enlarged genera of Biddulphia and Surirella, it is to be feared that their number will be legion.

It is scarcely to be expected that old diatomists, though they may perhaps agree as to the propriety of the proposed changes, will adopt them, as this would involve the renaming most of their specimens, and the learning a new grammar, as it were, for their favourite science; but the Conspectus will probably be a boon to the rising generation of students. I must, however, again express my regret that it has been based on a purely artificial instead of a natural classification; for though many instances may be cited where frustules of the stipitate, concatinate, tubular, frondose, or gelatinous families or genera, may have been found in a free state, I have little doubt that if the entire course of the process of reproduction had been observed they would have been found to revert to their normal condition.

At any rate we must all be thankful to Professor Smith for his attempt to reduce within a reasonable limit, and into something like order, the present chaotic confusion of families, genera, and synonyms.

Reading, *February*, 1873.

NEW BOOKS, WITH SHORT NOTICES.

The Beginnings of Life. By H. Charlton Bastian, M.A., M.D., F.R.S. In two vols. Macmillan and Co. London, 1872.—In these days of bulky volumes every addition to our scientific literature should be well weighed and considered, for every repetition of dubious assertions, every reprint of unproven statements is a cruelty inflicted upon the already overtaxed powers of the student of science. We greatly fear Dr. Bastian has much to answer for in presenting the world with two bulky volumes containing over a thousand pages of letterpress. There is some original research, much of it bearing the strongest evidence of carelessness and too easy credence, mixed with a vast number of pure hypotheses and restatements of the observations of others, which have been considered incredible by the majority of careful observers. The greater part of the work consists, however, of an argument in favour of certain phenomena not hitherto admitted to a place in science, which is conspicuously loose, and in places gives evidence of a negligence of known facts scarcely less remarkable than the tenor of the argument itself.

As an instance of this, the reader is referred to the 221st and following pages of the first volume, in which the author quotes an experiment made by M. Onimus, in which small portions of serum containing no white blood corpuscles were enclosed in little bags of gold-beater's skin, and placed under the skin of a living rabbit. The serum in these, after remaining twelve hours under the rabbit's skin, was found to be loaded with white blood cells, or leucocytes. After minutely describing M. Onimus' experiment, Dr. Bastian says, "Now, by these experiments, Onimus seems to have shown quite conclusively that the corpuscles met with in his experimental fluids had not been derived from the fission of any visible pre-existing cells. It seems almost equally certain that they did not even originate from particles which were recognizable by the microscopic powers employed, since the fluids were at first, to all appearance, perfectly homogeneous. Either, therefore, the minute particles, which were seen at a later stage, must have originated owing to some primitive formative process taking place in a really homogeneous organic solution, or else the fluid, seemingly homogeneous, in reality contained the most minute particles (microscopically invisible), derived in some unknown way from the previously existing protoplasmic elements of the tissues. We, however, incline to the former view; and we believe it to be in the highest degree probable that the fully-developed leucocytes or plastides which were seen in the later examinations, had arisen out of the growth and development of the mere organic specks met with in the earlier stages of the inquiry."—Vol. i. p. 224. After four pages in a similar strain, the author says, "Such a mode of origination of living units, together with their subsequent evolution, *affords, perhaps, the best illustration that can be given of the birth of cells* de novo *in Blastemata*." The italics are our own.

It does not seem to have occurred to Dr. Bastian to ask himself whether gold-beater's skin is, or is not, permeable to leucocytes. If he had placed a piece under his microscope, he would have recognized the structure of an animal membrane. A little inquiry would have enabled him to discover that gold-beater's skin is made from the intestine of the ox, and he would have discovered openings in it with even a low power large enough for millions of blood cells to pass into and out of a little bag made of such a material. The experiments of Cohnheim must be well known to Dr. Bastian; he cannot be ignorant of the fact that white blood corpuscles will pass freely through the walls of a fish's swim-bladder, when it is filled with a saline fluid and buried under the integument of a living animal. This experiment has been verified again and again, and is a favourite one with professors of physiology to show their classes the manner in which leucocytes wander through the living tissues. Does he then seriously ask us to believe in this as evidence of spontaneous generation? or is the *illustration* merely intended to explain what he thinks occurs in other fluids? We can only believe it found a place in his writings without a thought as to the nature of gold-beater's skin—on the authority of M. Onimus. We may take it, however, as a fair specimen of the carelessness which characterizes the book before us, and of the manner in which every possible unsifted statement has been brought to bear on the argument in favour of Dr. Bastian's opinions. It will occur to the reader at once that the minute particles observed at first in the fluid were the result of its degeneration, or were, perhaps, minute particles of precipitated fibrine. It will be difficult, however, to convince most men that they developed into leucocytes.

The experiments on the origin of Bacteria are certainly the most important part of the work, and it must probably be held as proven that such particles originate in fluids in sealed tubes, even after the most careful manipulation and exposure to a temperature usually destructive to all life. Perhaps other low organisms originate like Bacteria in such tubes. The argument that these organisms arise *de novo* reduces itself to a very small compass. Either germs have withstood a temperature of between 212° and 307° Fahr., or such forms have arisen *de novo*. The author assumes, on, we think, very insufficient evidence, the latter alternative.

Without prejudice, we do not see why the other alternative is not equally probable, especially when we remember that the cause of death is chemical change in the organism. A form of living matter capable of resisting such temperatures may, for all we know, exist in the germs of the lowest living forms, and if so, the whole argument falls to the ground.

The second part of the work relates to the development of higher from lower forms of life. The author asserts his belief in the evolution of Tardigrades and Rotifers from highly differentiated plants, as Euglena, Vaucheria, &c. He says at page 540, vol. ii., "I have already in my possession much convincing evidence, derived from personal observation, that some water mites and acari, and also the ciliated embryos of Naïdes may be produced by a direct transformation of masses composed of the protoplasm and chlorophyll of Nitella" —a plant nearly as high in development as a fern.

Dr. Bastian's figures on pages 516 and 527 of the second volume bear a remarkable resemblance to the eggs of animals; and although the vegetable structures which he gives side by side with them bear some resemblance to the eggs in question, the assumption that they are the same is about as justifiable as the assumption that an ovoid white stone, looking exactly like a hen's egg, might become transformed into an egg. It might be impossible to distinguish between them by mere external inspection, if they were only seen under such limited conditions as those which exist in microscopic research.

Much learned matter is contained in Dr. Bastian's thick volumes, but all we can say is, we hope its learned author will reconsider his opinions, and will bring his great stores of knowledge of the works of his predecessors to bear upon this difficult subject in a more careful and logical manner, and that he may ultimately, weighing the laws of evidence with an unbiassed mind, throw great light upon the origin of living things. At present our opinion is, that he has failed to do anything towards advancing our knowledge on the subject.

PROGRESS OF MICROSCOPICAL SCIENCE.

A Hæmatozoon inhabiting Human Blood.—Dr. T. R. Lewis is, as our readers are probably aware, the describer of this curious fact. He has written an important paper on the subject, which we should give much more fully did our space permit. However, the following brief summary will put our readers in possession of the more important facts contained in the author's account:—1. The blood of persons who have lived in a tropical country is occasionally invaded by living microscopic Filariæ, hitherto not identified with any known species, which may continue in the system for months or years without any marked evil consequences being observed; but which may, on the contrary, give rise to serious disease, and ultimately be the cause of death. 2. The phenomena which may be induced by the blood being thus affected are probably due to the mechanical interruption offered (by the accidental aggregation, perhaps, of the Hæmatozoa) to the flow of the nutritive fluids of the body in various channels, giving rise to the obstruction of the current within them, or to rupture of their extremely delicate walls, and thus causing the contents of the lacteals, lymphatics, or capillaries to escape into the most convenient excretory channel. Such escaped fluid, as has been demonstrated in the case of the urinary and lachrymal or Meibomian secretion, may be the means of carrying some of the Filariæ with it out of the circulation. These occurrences are liable to return after long intervals—so long in fact as the Filariæ continue to dwell in the blood. 3. As a rule, a chylous condition of the urine is only one of the *symptoms* of this state of the circulation, although it appears to be the most characteristic symptom which we are at present aware of. 4. And, lastly, it appears probable that some of the hitherto inexplicable phenomena by which certain tropical diseases are characterized may eventually be traced to the same, or to an allied, condition. The importance of a careful microscopical examination of the blood of persons suffering from obscure diseases, in tropical countries especially, is therefore more than ever evident, and opens up a new and most important field of inquiry—referring as it does to a hitherto unknown diseased condition.

NOTES AND MEMORANDA.

Microscopy at the Brighton Aquarium.—We learn that Mr. Lee, F.L.S., who has had so much to do with this most successful institution, has inaugurated a very great and decided improvement in the introduction of microscopes for the use of the public. There are few places where microscopic objects in a very wide and important class can be so well exhibited as in an aquarium. We think that Mr. Lee has done well both for the public and the proprietors in the introduction of the microscope to the general use of the visitors.

Muscles in the Kidneys—Herr Eberth states in a German journal that he has found a network of non-striated muscular fibres in the outer portions of these glands.

Pathological Microscopy.—Dr. Rindfleish's manual of pathological histology has been translated into English by Dr. E. B. Baxter, of King's College. We hope soon to be able to lay a notice of the book before our readers.

CORRESPONDENCE.

Mr. Wenham's Examination of the American $\frac{1}{10}$th.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Very lately I have seen Mr. Wenham's account of measurement of the $\frac{1}{10}''$ sent for test of angle when "immersed" in balsam. It is extremely unsatisfactory and bald of any result. A little must be said, and but little need be said as comment thereon.

Mr. Wenham states that he adjusted the objective "for Podura," no reference to cover of any thickness whatever. This is too indefinite for anybody's use. The meaning, if anything, must be *without cover* (which is not likely), and that would be, *at most open point*, where presumptively the angle of objective *is least.*

At the point at which Mr. Wenham tested the objective, the angle (air) was found to be 145°. Now, without a chance of question or doubt, the maximum angle of the $\frac{1}{10}''$ is 175° at adjustment for maximum angle. Measured at this point (obviously in all respects the proper point), the balsam angle would have been found to be not 79°, three degrees more than Mr. Wenham "predicted," but, using parallel rays of slender sunbeam, 95°, as repeatedly obtained and verified by me.

I have not the slightest objection to an adjustment "for Podura" under a proper thickness of cover to *require adjustment to point of* *maximum angle*,—but Podura in this case is utterly insignificant, and, this related testing for angle is, is it not? in the same category.

As Mr. Wenham, for the second time, refuses to appear again to gainsay what he has distinctly admitted,*—the real issue in my first non-provocative "experiment,"†—as against his broad challenge to "anyone,"‡ it seems proper that I should procure further attested measurement here, and, doing so, it shall be without warp of vision or judgment from any theory; the contrary of which strangely enough Mr. Wenham seems to require! in his expression alluding to Dr. J. Curtis's statement. Yet unhappily for our critic, all that I have set forth is *strictly accordant to accepted theory* in all the matter involved, only, Mr. Wenham (it seems) does not understand how! But perversely it seems to me, and entirely without warrant or need to do so, he pronounces all diagrams wrong unless (as I understand him) drawn for refraction in the front lens according to index of refraction of crown glass, 1·525 (or 1·531), which is a wide error of fact.

Mr. Wenham is solely responsible for the assumption. I gave no indices of refraction. It was not necessary. I did set forth what was possible, but *not* limiting myself to use of *crown glass only*, in the "front." Anyone can conceive of a material in the front having the same refraction *in balsam* as has common crown glass *in air*, and what then becomes of the limit of 82° in balsam? This simple question solves the whole matter. I have not suggested it perhaps distinctly before, but, only to leave it to ordinary discernment to discover.

The quotation, "no collecting power," is not my expression, and Mr. Wenham should not place it to appear so.

Respectfully yours,

ROBT. B. TOLLES.

APERTURE OF IMMERSED OBJECT-GLASSES.

To the Editor of the 'Monthly Microscopical Journal.'

SIR,—By your courtesy I have seen a proof of a letter from Mr. Tolles for publication in this number. The optical question of the discussion having ended, I prefer now to make a brief comment on the aperture trial, which however "bald and unsatisfactory" it may appear to Mr. Tolles, I maintain was an accurate and fair one. When the $\frac{1}{10}$th in question was brought as close as the range of adjustment will allow, apparently it gave a large aperture; but then the aberrations were such that it would not define objects under any thickness of cover. I set it at a position that gave the best definition on a well-known test, and that angle being measured, the very simple question to be solved was how that *same* angle became diminished by immersion in water and balsam? In the transfer of course the adjusting collar was *not* altered.

* See 'M. M. Jour.,' No. xxxvi., p. 292. † 'M. M. Jour.' for July, 1871.
‡ 'M. M. Jour.,' No. xxvii., p. 118.

It is the custom of most of our English makers to stop the closing of the lenses at a point where the definition becomes useless. Whether Mr. Tolles imagines I had predetermined that my "predictions" should be verified or not, is but of little consequence as affecting the facts. I really expected to find some reason for these alleged ultra-theoretical rays, and that I should have to account for such an appearance. Mr. Tolles, in admitting that he closes the lenses within the position of proper definition, gives us the key to his fallacy. I submit that my trial was correct; we are merely dealing with *visual* angles. Whatever these are to begin with, the simple question is next, What is the loss in water and balsam? Any after-alteration in the adjusting collar will falsify the results.

Finally, as this trial has not supported Mr. Tolles' views, he seems to imply that it has arisen from "warped vision and judgment." A very convenient dismissal! If I am honoured by being considered a judge in the matter, I may regret that the evidence of measured aperture had not gone Mr. Tolles' way, and so left me to account for the discrepancy. If the glass in question has not yet been returned, Mr. Tolles may move for a new trial, and get some of his own friends to be present. Though he excludes me from the list, surely he must allow that there are some Englishmen that will do him justice. I have no desire to be present, or to interfere further; but to facilitate the matter, I am willing to provide any apparatus that may be called for.

Mr. Tolles proposes to have further "attested measurements" made in America. I only hope that they will explain their method in detail, as plainly as I have done. Let us have their names by all means, and if there is one amongst them known to be capable of discussing the question on the admitted laws of optical science I will be happy to exchange notions with him.

I noted the facts under the conditions named as I found them, and should have recorded any measurements, however adverse to theory, and sought reasons for the discrepancy subsequently. It was Mr. Tolles' own desire, and not mine, that I should make the trial. I ask whether his request was a fair one if he had predetermined to impugn my competence to conduct the experiment properly if the result did not confirm his own views? I could not accept the measurements at the closest position of the lenses, for then all definition had gone.

Yours very truly,

F. H. Wenham.

The Structure of Eupodiscus Argus.

To the Editor of the 'Monthly Microscopical Journal.'

Ashdown Cottage, Forest Row, Sussex, *Feb.* 7, 1873.

Sir, — I am much obliged by a sight of Mr. Wells' letter. My remarks on *Eupodiscus Argus* were by no means intended as exhaustive. I wished to show that the drawings hitherto published were founded on erroneous views, and that the real structure conformed to the ordinary diatom type, though no doubt with variations.

I find no difficulty in showing the aspects I figured on parts of good specimens, as in Möller's type slide, with a stop of an achromatic condenser, limiting the illumination to a pencil of central rays of 20°. Mr. Wenham's illuminator gives similar and more striking results.

With Möller's slide I can examine the upper or lower surface, as the glass of the slide is thin enough for a large-angled ½ inch of Ross to work through, and that with the D eye-piece shows the *framework* of the diatom to be composed of minute beads, whichever side is uppermost.

The irregularities, elevations, depressions, &c., are well worth more attention than I have paid to them.

Mr. Stewart—one of the best observers—made valuable remarks when my paper was discussed, and he caused me to examine *Coscinodiscus oculus Iridis* in fractured specimens, and I found the depressions real, and the hexagonal framework real. These depressed surfaces and framework seemed, however, composed in the usual way, of minute beads.

I recommend to Mr. Wells' attention Mr. Stephenson's valuable experiments of mounting fractured and whole diatoms in bisulphide of carbon.

I remain, Sir, your obedient servant,

Henry J. Slack,
Sec. R. M. S.

The Estimation of the Magnifying Power of Lenses.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—The world is advancing so fast in these days that people ought to be prepared for hearing anything; especially people who live, like me, in the mountains, and know they are behind the age. Still, I own I was unprepared for what I saw to-day in your Journal. When I was at school—a good time since—we used to do Algebra and Optics; not much, but some. And one of the things I remember we learned was how to find the magnifying power of a lens. Now, I see in this Journal of February, near the end of page 52, that this problem is claimed by Dr. Pigott, of Cambridge University. He says that the person who has "worked it out" is himself. But if everyone knew it long ago, how can it be that it is now discovered by him? It can never be, surely, that the world has taken a turn the other way for a change, and is going backwards instead of forwards? Or, is it that, like the rest of the age, lenses have got advanced ideas, and refuse to magnify according to the former rules, as being too old-fashioned? This, no doubt, would call for a new solution, and might explain why a learned Professor lately came to tell your Society that the old Optics was now "played out." I was encouraged the more to think it must be this, when I looked at the new "working out"; for, if the problem is old, the working out is quite new. He says, to get the magnifying power, you divide 10 inches by the focal distance and take one less. Very well, so I did. I took a 5-inch lens and, doing this, there came out 1. So a 5-inch lens neither magnifies nor diminishes, but acts like a piece of window-glass. Then I tried an

8-inch lens, and by the same rule got $\frac{1}{4}$. So this lens, which used to magnify, now diminishes and shows you things one-fourth their natural size. Lastly, I took a 10-inch, and the power came out 0. So this lens not only diminishes things, but diminishes them quite out of existence, and what you get is—total darkness.

Would some of the people about your Magazine tell me how these things can be, and whether this is the Optics that has been played in? There is no one here I can ask about it except our parson, and he "gives it up."

Yours in perplexity,

RUSTICUS.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY—ANNUAL MEETING.

KING'S COLLEGE, *February* 5, 1873.

Wm. Kitchen Parker, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

The Secretary intimated that in conformity with their usual custom upon the election of a new President, the list of Fellows would be revised, and requested that any alterations or corrections to be made might be notified to Mr. Walter W. Reeves, the Assistant-Secretary, as early as possible.

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

The Secretary called attention to two slides which had been sent to the Society by Mr. Alfred Allen, of Felstead, one of which was exhibited in the room by Mr. Reeves. The slides were of some crystals obtained from a liquid which condensed from the vapour of coke burnt in a stove, and which was found to drop from the joints of the sheet-iron stove-pipe.

The Secretary said that it would be remembered that at the last meeting, in reading the list of gentlemen nominated as officers of the Society, he had mentioned that for the reason then stated the nomination of Mr. Hogg as one of the Hon. Secretaries must be considered as provisional, and that in view of the possibility of that gentleman resigning his position, he had been directed to propose the name of Mr. Chas. Stewart as Secretary, and that of Mr. B. T. Lowne as a member of the Council. After that meeting, however, Mr. Hogg addressed a letter to the President of the Council, in which he resigned the office of Secretary on the ground that having been appointed President of the Medical Microscopical Society, his leisure time would be too much occupied to enable him to fulfil both duties. The Council had therefore, in conformity with the by-laws, appointed Mr. Chas. Stewart to replace Mr. Hogg, and it remained now for the Fellows to elect him if they chose for the ensuing year. It was the circumstance of not having received Mr. Hogg's letter until after the last meeting which led to some little ambiguity at the time, but which the Fellows would now readily understand.

Mr. Beck suggested that it would be better in future if the balloting lists for Officers and Council could be sent by post to the Fellows before the meeting; he believed that this was the usual practice in some other societies, and he believed it would tend to give a greater interest to the proceedings, by more generally intimating what was about to be done, as well as affording a better opportunity of making any alterations in the list which might be desired.

Mr. Chas. Brooke said that this was the practice in the Royal, Royal Medical, Pathological, and other Societies, and it was found to render the ballot more efficient, and certainly created a greater general interest in what was going on in the Society.

The Secretary saw no objection whatever to this being done in future, and accordingly made a memorandum to the effect that the lists should be enclosed in the copies of the Journal which were posted to the Fellows previously to the Annual Meeting, or sent by post.

Mr. Beck and Mr. Suffolk having been appointed scrutineers, the ballot for Officers and Council for the ensuing year was taken; at the close of which it was declared that the whole of the gentlemen whose names had been printed on the house-list were duly elected.

The Secretary then read the Annual Report of the Council, and also the Treasurer's statement of account; after which,

The President delivered the Anniversary Address, which will be found printed *in extenso* at p. 97.

It was moved by Mr. Chas. Brooke, and seconded by Dr. Millar, that the reports now read be received and adopted, and that they be printed and circulated in the usual way. The same gentlemen moved and seconded that the best thanks of the meeting be presented to the President for the admirable Address which he had delivered to them on that occasion, and that he permit it to be printed in the Society's Transactions.

The motion having been put to the meeting by the Secretary, was carried unanimously.

Mr. Beck said that he was desirous of saying a few words with regard to the report, and also of calling attention to one or two matters in connection with the Society, which he thought affected its interests. He felt that they were deeply indebted to those gentlemen who had driven the coach of the Society so well and safely in the past, and he believed that as "outsiders" they knew but little of the difficulties met with by the way. Theirs was not like many other scientific societies, because they had a specialty, and consequently were at a disadvantage as compared with the Zoological, Astronomical, or Linnean Societies, to which many papers of special interest could be taken, and this difficulty was one which was not experienced to the same extent in the earlier days of the Society. He thought that the ordinary meetings of the Society were not so well attended as they used to be, and he regretted that this should be so, though he felt sure that the Council would be very glad to have this obviated if it were possible. One reason for this want of interest in the meetings was, he thought, due to the fact that they had a Journal in which persons thought they could read the papers, and that the Journal contained many other papers which were not brought before the Society. Discoverers were, no doubt, anxious to get their papers brought before the public, and could do so at once by sending them direct to the Journal. He thought it would be much better if the papers could be brought there, even though they were taken as read, and were then passed on to the Journal. He had strong objections to the Transactions of the Society being brought out as they were merely as a trade speculation. He thought that all papers published in their Journal should pass through the hands of the Council, and then be printed in a separate and independent publication; it was needless for him to say how this was done by other societies. The question was not so much one of quantity as of quality. There were cases which he could mention in which papers had appeared in the Journal, when it was quite certain that had they been first brought before the Society they would have been either very much modified or withdrawn altogether. He thought that these were important reasons why the meetings had fallen off. Another matter worthy of attention was the want of the social feeling amongst the Fellows, and no one could doubt that a great deal was done by giving a social character to the meetings. Persons now went off directly after the meetings were over; whereas they used to stay when the Society indulged in the good wholesome practice of a cup of tea after the business of the evening was over. He could well remember the pleasant conversations and introductions which used to take place at those times, and where persons lived out of London it was frequently their only opportunity of becoming acquainted with or of holding conversation with other Fellows of the Society. For his own part he regretted exceedingly that money should be spent upon a Journal rather than upon a genial cup of tea. He regretted also very much the increased tendency towards individuality amongst microscopical workers, and wished that he could have seen something done to draw them more together instead. His regret was increased by seeing the formation of another new Society, which he thought must have the effect of drawing away young men from them who would otherwise be workers with them. Was there any reason why the Quekett Club, or the new Medical Society, could not be brought into closer union with their own? One more subject he should like to mention, and that was that he considered it had been a very great mistake on the part of the executive to retain a law that persons, because they happened to be makers of microscopes, should be excluded from the Council. He did not say this from any feeling as to himself, because he had never desired the honour, and therefore might, he thought, allude to the matter; but there had been instances in which this rule had been prejudicial to the Society, and he need only mention the case of his late brother in proof; they all knew how his active energies were turned from the Society in a great measure, and what he had done for the Quekett Club was very well known. In the Astronomical Society, not only were makers of instruments eligible as members of the Council, but that Society had gone further, and even stated the opinion that there ought to be a maker upon the Council who might be able to give them the benefit of his experience upon practical questions connected with their instruments. If persons supposed that makers could not be admitted to such positions, because they were jealous of one another, he could assure them, that so far as he was aware, no such jealousy had any existence, nor did he see why there should be any; for if any of them thought that any extra amount of trade would result from connection with that Society he believed it was a very great mistake. The persons who came to makers as customers were for the most part young men, students seeking information, and whom makers had frequent opportunities of introducing to societies such as that. He hoped it would be understood that he did not make these remarks from any feelings of hostility to the Council, because he believed that they had done all that they could, but he thought that the discussion of these few things would perhaps be of use as regarded the future, and the annual meeting was, he also thought, the proper time to bring them forward, and he felt much obliged to the meeting for hearing what he had to say.

The Secretary said he should like to be allowed to divide what he had to say upon the subjects mentioned by Mr. Beck into two parts, and to say the first in his capacity as Secretary, and the last in that of a private Fellow. Speaking as Secretary he might say that the Council were quite of opinion that all original papers should be sent through the Society to the Journal, and not to the Editor independently; it was, however, a matter which it was not altogether within their power to regulate as they wished. It was due to Dr. Lawson to say that he was always willing to aid them as far as he could in this respect. With reference to the tea, he thought he might say from his own personal knowledge of them, that almost every member of the Council was as fond of tea as Mr. Beck himself, and when they occupied rooms of their own the question of tea would receive their best attention. With regard to the numbers at the meetings, he certainly thought that they stood very well, and would favourably compare with those of former years, and he thought that if Mr. Beck would refer to the attendance-books he would find this to be the case. They would, no doubt, remember that they were obliged to leave the rooms upstairs in consequence of the increase in the attendance which caused them to be inconveniently crowded. As to union with other societies he could assure them that the Council had always desired it, and indeed one reason why it was decided that the Journal should contain other matter than their own, was that it should publish the reports of other societies also, and thus be a means of bringing them together. And now to speak in his private capacity, he might say that he heartily agreed with Mr. Beck in his remarks as to the exclusion of makers from the Council; he was strongly of opinion that the Society ought to know nothing about what a man's occupation was, or to let his occupation in life in any way prejudice him. If a man distinguished himself in scientific pursuits, and was also—as most scientific men were—a gentleman, it ought not to matter what else he did for his living. He had, perhaps, no right to say this in his official capacity, and therefore hoped it would be considered as his private opinion.

Mr. Charles Brooke, the newly-elected President, then moved, and Mr. F. H. Wenham seconded, the following resolution:—"That the cordial thanks of the Society be returned to their retiring President for the manner in which he had discharged the duties of his office." He also moved, "That the thanks of the Society be presented to Mr. Jabez Hogg on his retirement from an office he had long filled with energy and assiduity."

A further resolution was also moved by Mr. Frank Crisp, and seconded by Mr. Ingpen, "That the thanks of the Society be presented to the Council, the Secretary, and Assistant-Secretary for their valuable services during the past year."

Both resolutions, having been put to the meeting by the President, were unanimously carried, and the meeting was then adjourned to 5th March.

List of Officers and Council of the Royal Microscopical Society, Elected 5th February, 1873.*

President.—*Charles Brooke, M.A., F.R.S.

Vice-Presidents.—William B. Carpenter, M.D., F.R.S., &c.; Sir John Lubbock, Bart., M.P., &c.; *William Kitchen Parker, F.R.S.; *Francis H. Wenham, C.E.

Treasurer.—John Ware Stephenson, F.R.A.S.

Secretaries.—Henry J. Slack, F.G.S.; *Charles Stewart, M.R.C.S., F.L.S.

Council.—*James Bell, F.C.S.; John Berney, Esq.; Robert Braithwaite, M.D., F.L.S.; William J. Gray, M.D.; Henry Lawson, M.D.; *Benjamin T. Lowne, M.R.C.S., F.L.S.; Samuel J. McIntire, Esq.; *John Millar, L.R.C.P., Ed., F.L.S.; Henry Perigal, F.R.A.S.; *Alfred Sanders, M.R.C.S.; *Charles Tyler, F.L.S.; Thomas C. White, M.R.C.S.

Donations to the Library and Cabinet, from Jan. 1st to Feb. 5th, 1873:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
Popular Science Review, No. 46	*Editor.*
Bulletin de la Société Botanique de France, 2 Parts	*Society.*
Certain Wingless Insects. By J. W. Wonfor	*Author.*
The 19th Annual Report of the Brighton Natural History Society for 1872	*Society.*
On a Hæmatozoon Inhabiting Human Blood. By T. R. Lewis, M.B.	*Author.*
A Report of Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera. By T. R. Lewis, M.B., and D. D. Cunningham, M.B.	*Authors.*
The Canadian Journal, No. 77	*Canadian Institute.*

* Those with an asterisk before their names were elected new members.

	From
Journal of the London Institution, No. 18	*Institution.*
Two Slides of *Cysticercus Bovis*, from Soldiers' Rations in India	*Dr. J. Fleming.*
Three Slides of Crystals, a "Distillation from the Vapour of Coke"	*A. Allen, Esq.*

The following gentlemen were elected Fellows of the Society :—

Robert Kemp, Esq.
William Bevan Lewis, L.R.C.P., London.

WALTER W. REEVES,
Assist.-Secretary.

Annual Report of the Royal Microscopical Society.

The subjoined accounts of the Society, property, income, and expenditure show that its financial position is satisfactory, and the number of fresh elections to its Fellowship exceeds that of most former years.

The books and instruments of the Society are in a satisfactory state, with the additions mentioned below.

The following list of papers read before the Society during the past year, and arranged according to the subjects to which they relate, afford a highly satisfactory proof of the value of its labours.

Relating to Comparative Anatomy, Physiology, and Natural History, we find "The Structure and Development of the Skull of the Crow," and that of the Tit and Sparrow-Hawk, illustrated in two papers by the President (W. K. Parker).

Dr. Klein contributed "Remarks on the Finer Nerves of the Cornea," and "Researches on the First Stages of the Development of the Common Trout" (*Salmo fario*).

Dr. Lionel Beale supplied a paper in reply to Dr. Klein, and one on the "Nerves of Capillary Vessels and their probable Action in Health and Disease."

Mr. Lowno contributed "Notes on the Development of the Nervous System of the Annulosa."

Mr. Stewart described "Some of the Characteristics of the Negroes revealed by the Microscope," and researches "On the Structure of the calcareous framework connected with the Locomotive Apparatus of the Echinus."

Dr. Maddox described "Some Methods of Preparing the Tissues of the Tadpole's Tail."

Mr. Cubitt contributed "Remarks on the Homological Position of the Members constituting the Thecated Section of Rotatoria."

Mr. Maplestone supplied Drawings and Descriptions of the Palates of Victorian Mollusca.

Dr. Hudson described "*Euchlanis triquetra* and *E. dilatata*," and contributed further information on Pedalion under the title "Is Pedalion a Rotifer?"

Dr. Anthony supplied investigations on "The Structure of Battledore Scales" of Butterflies.

Botanical subjects have been represented by a series of papers exhibiting the latest researches into the structure and character of "Bog Mosses," by Dr. Braithwaite; also, by Mr. Carruthers, "On the History, Histological Structure, and Affinities of *Nematophycus Logani*."

Mr. Hogg contributed a paper "On Mycetoma: The Fungus Foot Disease of India."

Dr. Murie described "Vegetable Organisms in the Thorax of Living Birds."

Mr. Slack furnished observations "On the Supposed Fungus on Coleus Leaves;" and "Notes on *Podisoma fuscum* and *P. Juniperi*."

Dr. Woodward wrote on the "Resolution of *Amphipleura pellucida* by Objectives of Beck and Wales."

Mr. Slack remarked on "The Structure of *Eupodiscus Argus*."

Dr. Woodward furnished "Remarks on the Resolution of Nobert's Nineteenth Band, with Wales' New Immersion $\frac{1}{18}$th."

New Apparatus and Instruments were treated in the following papers:—By Mr. Stephenson, "Stephenson's Erecting Binocular," a "Silver Prism for the Successive Polarization of Light," and "On Bichromatic Vision."

On an "Improved Reflex Illuminator for the Highest Powers of the Microscope," by Mr. Wenham.

"On a Micro-pantograph," by Mr. Roberts.

"Proposal for a Standard of Comparison of the Magnifying Powers of Microscopes," by Mr. Ingpen.

"A New Form of Micro-spectroscope," by Mr. Gayer.

"An Aërial Stage Micrometer," on an improved form of improved Lens-Micrometer, and on finding Micrometrically the Focal Lengths of Eye-pieces and Objectives, by Dr. Pigott.

Dr. Murie contributed a paper "On the Classification and Arrangement of Microscopic Objects."

Books Purchased during the past Year.

Annals of Natural History. 2 Vols.
Quarterly Journal of Microscopical Science. Vol. XX.
Cooke's Fungi Britannici. 3 Parts.
Allman's Gymnoblastic Hydroids.

Books Presented.

Royal Society's Catalogue of Scientific Papers. Vol. VI.
Transactions of the Linnean Society.
The Natural System of Botany. By B. Clarke, F.L.S.
Several pamphlets and papers, as well as the journals of other societies in exchange for our own, have been periodically announced in the 'Monthly Microscopical Journal.'

Apparatus and Slides.

Safety Stage for Ross's Microscope	*J. W. Stephenson, Esq.*
6 Injected Preparations	*Moritz Pillisher, Esq.*
9 Slides of Insects' Scales	*J. W. Wonfor, Esq.*
4 Slides of Mycetoma	*Jabez Hogg, Esq.*
2 Slides of Battledore Scales	*Dr. Anthony.*
12 Slides of Podura Scales	*S. J. McIntire, Esq.*
24 Slides of Starches	*Waldron Griffiths, Esq.*
986 Slides and Cabinet	*Mrs. Farrants.*

NUMBER OF MEMBERS ELECTED DURING THE YEAR.

Elected 20. Deaths 6.

**Jonathan Bagster, elected 1840, died ——.
*Geoffrey Bevington, elected 1857, died October 31, 1872.
*Richard Hodgson, F.R.A.S., &c., elected 1849, died May 4, 1872.
*John Hollingsworth, M.R.C.S., &c., elected 1860, died May 23, 1872.
*Rev. Douglas Cartwright Timins, M.A., elected 1867, died May 5, 1872.
James How, elected 1864, died December, 1872.

OBITUARY.—The Society have to regret the decease of Richard Hodgson, who died at Hawkwood, Chingford, Essex, the 4th May, after a very long illness, induced in great measure by his untiring exertions in the cause of the debenture holders of the London, Chatham, and Dover Railway: was born in Wimpole Street, in 1804. He was educated at Lewes, passed some time in a banking-house in Lombard Street, and eventually became leading partner in the firm of Hodgson and Graves, the publishers in Pall Mall, from which he withdrew in 1841, and thenceforth gave up his time to scientific pursuits, first taking up daguerreotypy (then in its infancy); he introduced many improvements in the manipulative part of the process; was very successful as an amateur in portraiture, and in obtaining magnified representations of microscopical objects which have rarely been surpassed. He also spent some time in endeavouring to print from the silver daguerreotype plate, by submitting it to chemical treatment, and proceeding as is usual in copperplate printing. Though he had some fair results, the process was too delicate and uncertain for general use, and he abandoned it to devote himself more exclusively to the microscope and telescope. In 1852 he built an observatory at Claybury in Essex, in which a 6-inch refractor was mounted equatorially. This was afterwards removed to Hawkwood, and a transit-room added, which now contains the 4-inch instrument formerly in the possession of Dr. Lee, of Hartwell. In 1854 he designed the diagonal eye-piece which bears his name, by which the whole disk of the sun can be observed without contracting the aperture of the object-glass, a description of which appears in the Royal Astronomical Transactions for that year. For many years he was a constant observer of the sun, and made a series of drawings of many solar spots. Whilst so engaged, at 11.20 A.M., on the 1st September, 1859, he was fortunate in witnessing the remarkable outbreak in a large spot, which was simultaneously observed by Mr. Carrington at his observatory, Reigate. He became a Fellow of the Royal Astronomical Society 14th April, 1848, a Member of Council 12th February, 1858, and an Honorary Secretary in 1863; a Fellow of the Royal Microscopical Society 25th April 1849.

The Rev. Douglas Timins, M.A., &c., who died on the 5th May last, at the early age of thirty-five, was educated for the Church, and obtained his degree of M.A. at Oxford with honours. He was always delicate and suffered from a defect of sight, which threatened to become destructive to vision. He was, therefore, recommended to travel, and being excessively fond of natural history, he took to entomological pursuits, having been forbidden to continue the use of the microscope. He attempted, however, to make the scales of the Lepidoptera a means of their classification, but was unable to do much in this direction, although he contributed several interesting papers to the Transactions of the Entomological Society. He was an able writer, and was better known for theological papers and contributions to religious publications.

Mr. James How, the well-known philosophical instrument maker of Foster Lane, recently complained of a small tumour that had appeared behind one of his ears, which, on the following day, became so painful as to compel him to leave his business. The tumour rapidly developed into a carbuncle, and erysipelas supervening, he died on Sunday, the 8th of December, 1872, at the comparatively early age of fifty-two. Mr. How was for many years manager of the business of the late firm of George Knight and Son, Foster Lane, whom he afterwards succeeded. He was intimately conversant with the practical details of photography in its numerous ramifications, and several years since published his method of producing negatives by the waxed-paper process—a process which he simplified to a very great extent, and by which he produced many fine photographs. He was elected a Fellow of the Royal Microscopical Society on the 10th of February, 1864. Mr. How introduced a cheap students' microscope, which was admitted to be one of the best instruments of its class, and his skill in exhibiting microscopic photographs and similar objects was widely known and appreciated.

The Secretaries have not been able to obtain any biographical notices of the three other gentlemen who have died during the year.

JOHN WARE STEPHENSON IN ACCOUNT WITH THE ROYAL MICROSCOPICAL SOCIETY.

Dr. | *Cr.*

1872.	£	s.	d.	1872.	£	s.	d.
To Balance brought from 31st Dec. 1871	51	9	1	By Cash paid for Journal ..	248	11	6
,, Dividend on 1059*l.* 6*s.* 2*d.* Consols	15	9	10	,, Rent to King's College and Attendance	57	17	8
,, Dividend on 1082*l.* 0*s.* 11*d.* Consols	15	19	3	,, Reporter	9	9	0
,, Compositions	33	12	0	,, Cash paid for 22*l.* 14*s.* 9*d.* Consols	21	0	0
,, Annual Subscriptions, &c.	481	18	0	,, Mr. Reeves' Salary ..	63	0	0
,, Screw-tools sold	0	7	0	,, ,, Commission	12	16	6
				,, Ray Society for 1871 and 1872	2	2	0
				,, Fire Insurance	1	4	0
				,, Stationery and Printing	18	11	6
				,, Petty Cash	28	2	2
				,, Balance remaining 31st Dec. 1872	136	0	10
	£598	15	2		£598	15	2

Feb. 1, 1873. Examined and found correct,

WILLIAM T. LOY, } *Auditors.*
JAMES HILTON, }

Medical Microscopical Society.

At the first ordinary meeting of this Society, held at the Royal Westminster Ophthalmic Hospital, King William Street, Strand, on Friday, January 17, at 8 P.M., Jabez Hogg, Esq., President, in the chair, the minutes of the previous meeting were read and confirmed. The certificates of thirty-two gentlemen proposed for membership were read, amongst whom were Drs. Rutherford; G. C. Wallich; W. Mackenzie, C.B., C.S.I.; T. Tebay; Duplex; Messrs. Power, T. Smith, T. Harvey Hill, &c.; after which the President read an introductory address describing the rise and progress of the science of histology. A vote of thanks to the President was passed for his very interesting address, and the meeting was resolved into a conversazione, at which many most valuable and interesting specimens were exhibited by Mr. Jabez Hogg, Dr. U. Pritchard, Mr. J. Needham, Messrs. White, Ackland, Atkinson, Baber, and Groves. Several of the makers, amongst whom were numbered Messrs. Baker, Horne and Thornthwaite, How, Pillischer, Ross, Swift, &c., very kindly assisted by the loan of microscopes, specimens, and lamps.

President's Address on the Opening of the Medical Microscopical Society of London.

January 17, 1873.

The doctrine of the elementary structures, whether in plants or animals, first took its root in men's minds about the latter part of the seventeenth century, when Malphighi and his contemporaries introduced into their anatomical investigations the use of the simple microscope.

The employment of anything better than a single lens appears to have been almost unknown to the anatomists of the middle ages, for although it has been observed that Aristotle and Galen wrote of *partes similares et dissimilares*, and that Follopia had some idea of "tissues," it is quite certain that neither of those philosophers possessed more than a faint notion of the intimate condition and connection of the various tissues of the human body.

The first steps in histological science were cut out by those who followed long after—Leeuwenhoek, Ruysch, Swammerdam, Adams, Hook, &c.; and even these anatomists were too much absorbed in other pursuits, and in the teaching of anatomy, physiology, and embryology, to find time to assist in the advancement of microscopical physiological investigations. Thus it came about, throughout the greater part of the eighteenth century, histology almost stood still, or, at best, found only a few men of science, Lieberkuhn, Fontana, Hewson, &c., contributing towards the knowledge bequeathed to them by their predecessors. It was not, indeed, until the commencement of the present century that any great effort was made to secure a solid and scientific position for the microscope in the teaching of the medical schools.

The master mind of Newton was not fully alive to the importance of the instrument; for speaking of the extreme tenuity of the ultimate molecules of bodies, he seems to have had but an inadequate idea of their minuteness, and supposes that they might be seen through microscopes magnifying some three or four thousand times (linear), and in speculating upon the possible resolution of the colouring matter of bodies—a speculation, as Herschel observes, "in the highest tone of a refined philosophy, irrespective of its theoretic bearings," he goes on to say:—"In these descriptions I have been the more particular, because it is not impossible but that microscopes may at length be improved to the discovery of the particles of bodies on which their colours depend, if they are not already in some measure arrived at that degree of perfection. For if these instruments are, or can be, so far improved as with sufficient distinctness to represent objects five or six hundred times bigger than at a foot distance they appear to our unaided vision, I should hope that we might be able to discover some of the greatest of these corpuscles, and by one that would magnify three or four thousand times, perhaps they might be all discovered except those which produce blackness. . . . However, it will add much to our satisfaction if those corpuscles can be discovered with microscopes which, if we shall at length attain to, I fear it will be the utmost improvement of this sense. For it seems impossible to see the more secret and noble works of nature within the corpuscles by reason of their transparency."

Much of what must have appeared to be impossible to the earlier workers with the microscope has been slowly and surely accomplished. During the year 1801, histology became indirectly indebted to the genius of a member of the medical profession, who, although not himself a great discoverer, yet he so well understood how to arrange existing materials, and bring them into harmony and close relationship with physiology and medicine, that it soon acquired for itself an independent existence.

The future of histology was secured the moment Bichat gave to the world his admirable treatise, 'Anatomie Générale.'

This work may certainly be said to be the first scientific monograph on histological physiology; for in it the tissues are not only treated of fully and logically from a morphological point of view, but their physiological functions and morbid conditions are discussed somewhat in detail. About the time this book made its appearance, many improvements were effected in the optical part of the microscope—a circumstance which gave an impetus to the growing desire for a more careful and systematic study of natural history — so that more appears to have been accomplished in a few years than had previously been effected in two hundred. Discovery quickly followed discovery, and in 1823 the first attempt to furnish the instrument with an achromatic objective proved successful both in France and in this country simultaneously. In the following year, 1824, Mr. Joseph Lister, the father of Professor Lister, of Edinburgh, fully accomplished what he had long laboured to produce, namely, a perfect combination of achromatic lenses, together with the mode of obtaining better corrections of their spherical and chromatic aberration. This important improvement in objectives furnished what had long been wanting—increased power with better definition and penetration. Mr. Lister also placed in the hands of opticians a projection for an $\frac{1}{8}$th objective, which, until very lately, was the standard for high powers, and the basis of all subsequent improvements.

Many men, eminent in physical science, now engaged in a race after greater perfection, Tully, Goring, Brewster, Brown, Herschel, &c., in this country; and on the Continent, Selligues, Chevalier, Amici, Fraünhofer, &c., eagerly set themselves to work, grinding and constructing lenses for the microscope, and workers were not long wanting who understood how to apply them.

Sir John Herschel, writing about the improvements made in the instrument, says:—" I have viewed an object, *without utter indistinctness*, through a microscope by Amici, magnifying upwards of 3000 times in linear measure, and had no suspicion that the object seen was even approaching to resolution into its primitive molecules." In the year 1828, C. A. S. Schultze made many valuable observations on the " primitive molecules of matter"; but it was not until ten years later (1838), that Schwann gave the world his remarkable generalization of cell development. If, therefore, Bichat laid the foundation of theoretical histology, and supplied it with a backbone, it was Schwann who discovered and propounded the great significance of the cell, in the development of the simple and complex tissues which enter into vegetable and animal bodies. This discovery led the way to great advances in our microscopical knowledge. Indeed, the microscope was now seen in the hands of very many men of science, and at the same time small bodies of the medical profession were in the habit of meeting together at each other's houses for the regular study and discussion of matters connected with the instrument. It was at one of these evening meetings that the happy idea was conceived of establishing a society for the more systematic and methodical prosecution of microscopical work. Accordingly, in the year 1839, the first Society was formed, in this or any other country, " for the promotion of microscopical investigation, and for the introduction and improvement of the microscope as a scientific instrument."

Professor Owen, then a rising young general practitioner, fresh from St. Bartholomew's Hospital, became the first President of the Society. During the first year of its existence 177 members joined the little band; a number fully large enough to justify the anticipations of its founders, that such a society was wanted and would prove a success. May the rise and progress of the Royal Microscopical Society foreshadow the future of the bark we launch on the ocean of time to-night. May the Medical Microscopical Society fulfil in every way the wishes of its founders, and become a pillar of strength in the promotion of " Practical Histology" among students, young and old, in our profession. From the history of the Royal Microscopical Society we learn that members of the medical profession were more eager and zealous in the promotion of its objects than any other class of men; and that the earliest and most frequent contributors to its

Transactions were chiefly Owen, the brothers Edwin and John Quekett, Arthur Farre, Dalrymple, Lindley, Busk, and others. Dr. Lindley strongly advocated the formation of committees to conduct particular branches of inquiry, because, as he said, " The application of the powers and advantages of an associated body of observers to gain an intimacy with nature, is more important in regard to the microscope than to any other instrument of philosophical research, to conceive clearly the aim of our researches and to give a right direction to our exertions." I venture to differ from Dr. Lindley's view of the value of committees for the promotion of microscopical inquiries. In my opinion it is far better that each member of this Society should be free to pursue his investigations in his own way, and unfettered by the restraint imposed by association, or in committee. We know by experience how prone committees are to express an authoritative opinion, without accepting the responsibility which always attaches to individual action. It will conduce more to the promotion of original thought and methodical care in the prosecution of microscopical research, if each one takes his own course, and follows the bent of his inclination, for by his works shall he be known. Nevertheless, we truly feel that we are knit together by the kindliest bonds of brotherhood for the attainment of a common object, each and all striving to obtain a broader and firmer objective basis for histology than it has heretofore enjoyed. To make our labour one of more solid worth, we must join heart and hand in the careful investigation of every phase of the intimate morphological condition of the animal organism; starting from the earliest germ of existence, to development or growth, and proceeding onward to the more permanent forms in all created beings.

The eye only sees what it brings the light to see, in spite of all our well-contrived instruments. It must therefore acquire the faculty of seeing accurately before it can be trusted to draw conclusions. A period of apprenticeship must be passed at the microscope, the earth must be tilled and the sowing done, before the harvest can be reaped. With all our boasted knowledge of histology, what do we really know? As yet we only possess a tolerable idea of the elementary parts of the higher classes of animals. We are not perfectly familiar with the structure of any—man not excepted—much as the human body has been scrutinized by the 50ths and 80ths of modern ingenuity and workmanship. The higher organs, the senses, and some few other portions of the body, have been partially worked out; but there is even more waiting to be accomplished; and as we proceed to investigate, we shall find something new to arrest attention, and waiting to be discovered, in the most familiar organ.

In comparative histology, the greater part of the work has yet to be done, and here we shall find a mass of material requiring years for its perfect elucidation. And whoever will perform something useful in this department of nature, must first acquaint himself with all that has been done by others. He must then prepare to discard authority, abandon old methods of research, and adopt new ones. He must also employ better and hitherto untried reagents; otherwise he will fall into errors of interpretation conspicuous enough in the writings of many of those who have preceded him. Again, in pathological microscopical work, an immense field remains unexplored, waiting the hand of the diligent to become rich in gems of priceless worth.

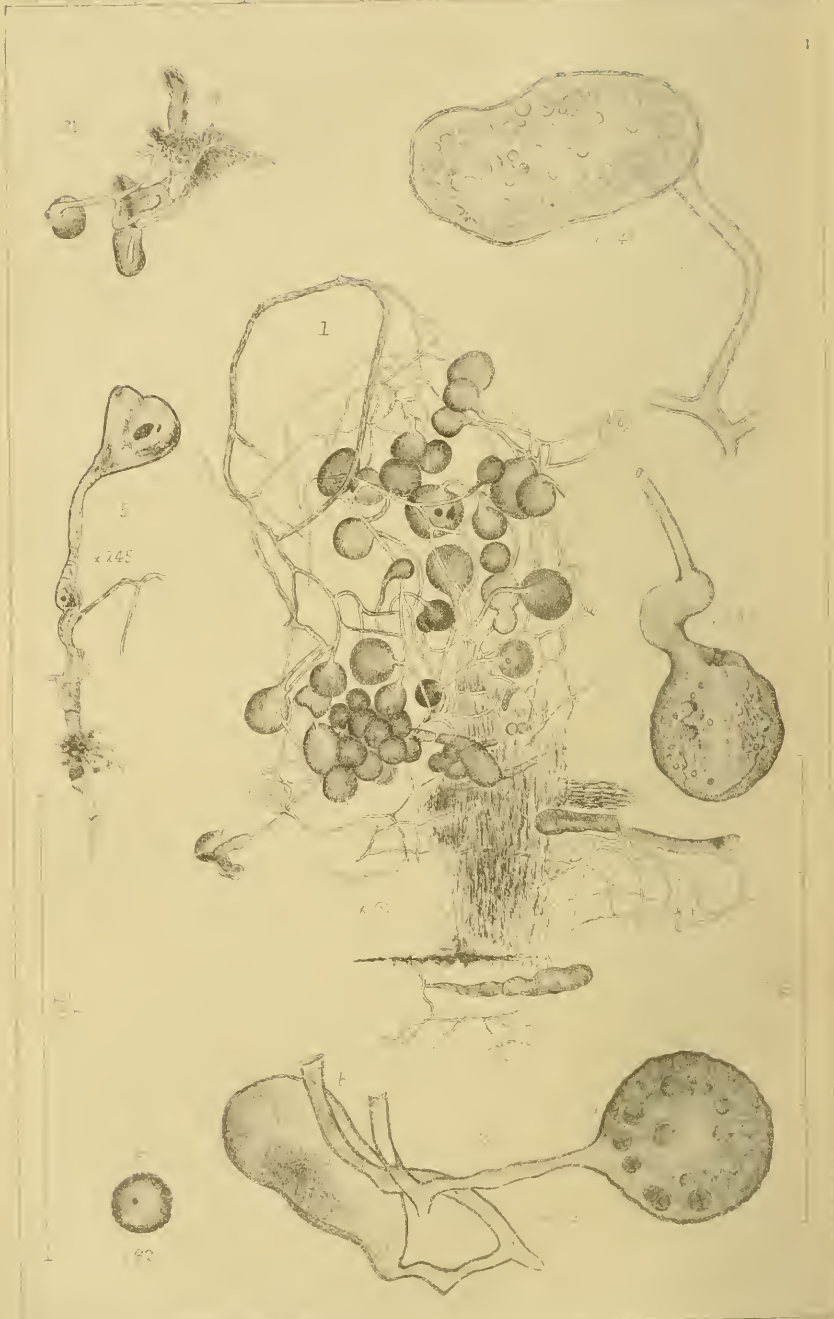
But few students who commence the work of the microscope are able to recognize the fact that, under high powers, the natural appearance of almost every object is in some way influenced or altered by the refractive nature of the fluid medium in which it has been immersed or is examined. The remarkable changes effected by Graham's law of diffusion when colloid substances enter into a preparation, at once illustrates the necessity for caution in the use of preservative fluids. The many changes brought about by glycerine in substances containing alkaline salts is another instance.

There are, indeed, many other sources of error, to which, however, I need not more particularly allude in addressing the members of this Society, most of whom have already acquired skill in histological science. Such work as I have endeavoured to sketch out, is necessarily laborious and requires time and patience for its execution; but he who is prepared to undertake it will ultimately find his reward in having extracted some secret from nature of inestimable worth.

It is encouraging to think, and experience teaches us, that such work can be done with instruments of an inexpensive kind. Nevertheless, I must candidly confess that I am unable to offer a *model microscope*, well suited in every way for the work of the student in practical histology. This has arisen from the circumstance that hitherto persons, in no way fitted for the task, have volunteered to dictate the form and accessories of students' microscopes. A society in no especial manner engaged in the promotion of microscopical pursuits a few years ago, ventured, I think, so far out of its way as to offer a premium for a "*students' microscope*," and not knowing anything about the requirement of the class it was preparing to cater for, the whole thing turned out a miserable failure. It would not have mattered much if the mischief done could have been confined within the four walls of the Society; but this was impossible, and makers of microscopes looked upon the Society of Arts' instrument as a model worthy of imitation; the result has been to drive teachers of practical histology to use and prefer instruments of foreign workmanship. The Medical Microscopical Society will, I feel sure, stimulate English opticians to furnish a better and more efficient stand than either that of Hartnack, Merz, or Nachét. We are favoured to-night with an unusual display of students' microscopes, some of which are decidedly in advance of the instrument usually met with. Mr. Baker contributes a new microscope after Hartnack's model; and Messrs. Beck, Ross, Browning, and Pillischer, well-known forms. But, in all, there are faults of construction and room for improvement; some are wanting in firmness, the fine adjustment moves the object out of the field of view, proving the instruments to be unsuited for the use of high powers, and all lacking in one essential to a working microscope; a perfectly concentric turning stage, and without which it is almost impossible to employ every kind of illumination or obtain high-power definition. It would occupy too much time to go fairly into the question of a good stand, but I am gratified to hear that Mr. Browning is engaged, in conjunction with Mr. John Mayall, jun., in perfecting a new microscope for students which will embody every practical improvement, consistent with simplicity and the use of high-power glasses.

But there is one point about a good working instrument of even more importance than a perfect stand, and that is a first-rate objective. In selecting a magnifying power for scientific work of any kind, it must be our endeavour to secure one giving the very best definition and penetration. These are two of the most essential qualities in every good objective; as on the first depends the truth of the optical image, and on the second the proper appreciation of its histological characters or structure. The defining power of an objective, as I dare say most of you know, chiefly depends on the perfection of the corrections for spherical and chromatic aberration. A fourth of 120° and an eighth of 150° angular aperture may be looked upon as standard objectives. Greater angular aperture in *dry objectives* is not in my experience beneficial for medical microscopy. Increased angle of aperture frequently means impaired definition; the explanation of this is, that the manufacture of glasses with the utmost angle of aperture is attended with increased difficulties, and requires the most skilled workmanship. It is, therefore, a somewhat rare thing to meet with an objective of great angular aperture that *approaches* to *freedom from spherical aberration.* The *absolute* correction of chromatic aberration in an objective is of far less importance than the correction of spherical aberration, and just so is it less important to the histologist to have a colourless image than one with perfectly sharp definition. By this test will the student be inclined to estimate the value of any object-glass. Increased angular aperture enables us to bring to our aid, when needed for the resolution of an object, a more oblique pencil of light than we otherwise could; and we should be prepared to employ every kind of illumination in our work. Indeed, we should not in any case pass judgment upon a structure until an exhaustive series of trials has been made upon it by every method of illumination. The cover-glass, as Amici long ago pointed out, exerts considerable influence on the perfection of the image. An object or preparation without a cover-glass gives a sharper image than one covered. A thick glass cover increases spherical aberration. In the immersion objective, the film of water removes or lessens many of the evils inherent to the *dry objective.* In the *immersion system* the stratum of water becomes, as it were, an adjustable film between the objective and the object, and greatly assists in the correction of spherical and chromatic aberration. As water is a stronger refracting medium than air, the reflexion of the rays of light is much diminished at the upper surface of the cover-glass, and on its incidence on the objective; here, indeed, it is almost entirely neutralized, and hence a greater number of rays do actually contribute to the formation of the image. The thin film of water produces very nearly the same effect as enlarged angle of aperture. It also collects the peripheral rays from the object, and sends them on to assist in the formation of a more perfect image in the eye-piece. In short, the water becomes an integral part of, and a new optical element in, the combination, and doubtless assists in the removal of residuary secondary aberration in the lens. In awarding praise to the immersion system, I by no means intend to disparage the dry objective; I am convinced, however, it is to the immersion objective we must look for increased power and usefulness in histological work. I am glad to acknowledge that hitherto I have been quite content to employ what may now be called our old half-inch and a quarter, made by Andrew Ross more than twenty years ago; neither having a greater angle than 75°. Lately, I have used a $\frac{1}{4}$ by Dallmeyer (Andrew Ross's son-in-law), the angular aperture of which is 120°. The excellent workmanship of this optician is well known and recognized, both on the Continent and in this country, and therefore needs no praise from me. I must, in justice to Mr. Dallmeyer, say that his $\frac{1}{4}$ objective works through almost any thickness of cover-glass, and its aberrations are equally well balanced for uncovered objects. It bears the highest power eye-piece, and gives a magnification of 1000 diameters in every way satisfactory; this perhaps, after all, is one of the best tests of a good objective. It proves beyond a doubt whether the angular aperture of the objective is brought to the maximum of utility, and increases its value in the eyes of the pathological microscopist.

To enter, however, into the history of the discoveries and improvements which each has effected, or to assign the share of honour which each labourer has reaped in this ample field, forms no part of my present discourse. In the language of Herschel,—"of the splendid constellations of great names which adorn the history of the microscope, we admire the living and revere the dead far too warmly and too deeply to suffer us to sit in judgment on their respective claims or merits; to balance the mathematical skill of one against the experimental dexterity of another, or the philosophical acumen of a third, is scarcely possible. So long as one star differs from another in glory, —so long as there shall exist varieties or even incompatibilities of excellence,—so long will the admiration of mankind be found sufficient for all who truly merit it."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THE

MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1873.

I.—*Some Remarks on a Minute Plant found in an Incrustation of Carbonate of Lime.*

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

(*Read before the* Royal Microscopical Society, *March* 5, 1873.)

Plate XII.

The interesting substance popularly called "petrified moss," which forms the subject of the present communication, was brought from a spring at Reedwater, the property of the Duke of Northumberland, situated about fifteen miles from the nearest market-town, Jedborough, by an intelligent lad, Wm. Anstiss, who on his return here presented some of the same to a lady of this neighbourhood, who is greatly interested in microscopic and natural history pursuits, Miss Davies, of Peartree Vicarage, and who, towards the close of November last year, kindly forwarded some specimens to the writer for examination. Some time after the receipt of the specimens, Miss Davies kindly sent the lad to me, affording thus an opportunity of obtaining additional particulars. The lad had been engaged by a party on a shooting excursion over this property, and whilst occupied in his duties, had his attention directed by the gamekeeper to the strange masses seen in the foot-bed of a spring that flowed down a moss-covered bank at about 4 or 5 feet above the level of a stream, into which it ran at the distance of a few yards. This spring was the only one in which he noticed anything peculiar, though there

EXPLANATION OF PLATE XII.

Fig. 1.—Supposed to be an entire plant.
„ 2.—Shows two different conceptacles, the round one, possibly "oogonal," and the oblong one, "antheroidal" in function, though doubtful if belonging to the minute plant.
„ 3.—Represents a large oblong cell containing a few highly refringent cellular and narrow curvilinear bodies.
„ 4.—Is considered as a very early stage of Fig. 1, or to represent a very young plant of *Botrydium minutum*.
„ 5.—Possibly belongs to the early condition of some other plant; its tubular portion is distinctly septate.
„ 6.—One of the round reddish bodies found with the minute plant; its nature is doubtful, whether a minute spore or ovum.
„ 7.—Apparently a rather earlier stage of the condition of the nearly mature cell represented in Fig. 2.

are other spots known on the same property where these moss incrustations exist. This little outflow, with several others, helped to feed a stream or streams that ran for some considerable distance before falling into the river Reedwater,—as Longfellow beautifully says, with poetic licence—

> "Mute springs
> Pour out the rivers' gradual tides,"—
> "And, babbling low amid the tangled woods,
> Slip down through moss-grown stones with endless laughter."

By the help of a pickaxe and spade some of these incrustations were removed from *beneath* the water, and treasured up until his return here. The specimens sent to me had very much the appearance of a keratose sponge, densely incrusted with inorganic matter—carbonate of lime. The masses averaged about 3 inches in height, and measured from 4 to 5 inches round the upper part.

Upon ordinary examination, the bases that had been adherent to the shallow bed at the fall of the spring, were noticed to be very compact, whilst the upper portions presented a more or less open moss or sponge like appearance, and where the small stems or cross junctions had been accidentally broken, a minute dark round aperture could be seen occupying the centre of the ramifications. On the outside, there remained in the hollows, some earthy and sandy matters, but nothing beyond these to indicate the nature of the body that had furnished the nucleus for the incrustation; nor did the microscopic examination by a low power lead to anything more.

Some small pieces about half an inch long were sawn off from the top, middle, and base of one of the pieces. These were set in a small beaker with some diluted acetic acid, and the solvent action of the acid kept up until the pieces were dissolved. On looking across the vessel at the light, several very small flocculent substances were noticed depending from the bubbles of gas on the surface. These were lifted out of the fluid by a fine hooked platinum wire, and examined on a slide under the microscope, when amongst the débris of organic matters—evidently portions of vegetable tissue, chiefly softened decayed coarse and fine stems and stem leaves of (bog ?) moss, one or two fine septate filaments of a confervoid alga with other matters, such as starch grains, butterfly or moth scales, empty ova cases, hairs and claws of insects, ligneous fibres and particles of sand, yet not a single diatom—was seen a minute well-preserved fungus-looking plant, consisting chiefly of microscopic coarse and fine tubular threads and branches, terminating in small globular, oval, or variously-shaped heads or conceptacles, varying in colour, due probably to decay, from a brown to a very pale almost colourless greenish yellow; the whole plant being woven as it were in a most intricate manner over and into the decayed débris of the moss structure. (*Vide* Fig. 1.)

Not knowing of any plant that corresponded with the above, and supposing it to be some form of mildew that had formed on the dead moss, before incrustation, the writer was disposed to regard it as belonging to the Mucoroideæ. Fortunately, however, having left a mounted slide in the hands of the Assistant-Secretary, Mr. Walter Reeves, he kindly promised to try and obtain the opinions of others more versed in these matters. The slide was shown by him to my friend Mr. Slack, who has been much interested in the subject, and thus expresses himself, in his note of January 19th, which I take the liberty of quoting. He says, " The bodies certainly look much like fungi, with asci full of spores, but I have never met with and cannot find in Berkeley or Cooke any that have such mycelium *threads* or such *asci*." " The round bags in your objects are evidently strong and thick; their fractured edges, I think, leave no doubt of this. The tubes also seem much firmer and with a more decidedly cortical layer than any I know of in fungi." " Although I cannot refer them to any other class, I should doubt their being fungi unless we had some undoubted fungi in a fresh state and sufficiently resembling them, to strengthen that view." " Their interest is undoubted," &c.

Mr. Reeves also brought it to the notice of Mr. Renny, and thus kindly writes on February the 10th, " Mr. Renny thinks the little plant is not a fungus, but probably a species of Botrydium, a genus of confervoid Algæ, only one species of which has as yet been found in England, and very little is known about it." He thinks " it would be found growing on the banks of the stream near where your deposit was obtained." To Mr. Slack I am indebted for procuring the opinion of Mr. Currey, who also examined the specimen and was disposed to agree with Mr. Renny. With permission I beg to quote the remarks in his note of February 20th:—" I am decidedly of opinion the object is not mucedinous, nor does it, I think, belong to any class of fungi. It seems to me to be certainly an alga allied to Hydrogastrum (Botrydium) and Vaucheria. The chitinous appearance I think only arises from the brown colour which is common on decaying filamentous Algæ," and adds " no truly microscopic species of Botrydium has hitherto been described, at least to my knowledge."

At the same time Mr. Slack pressed me to make further examinations, and contribute the same to the Royal Microscopical Society, in conformity with which, I have been induced to spend a considerable time in the further elucidation of the subject, and endeavouring to secure good specimens for figuring.

Although the masses had been found *beneath* water, it seemed just possible that this part of the foot-bed of the spring might occasionally be dried up, and that if the fine Botrydioid structure had formed on the incrustations at such a time, that I should be able to find it on the surfaces by soaking the masses and examining the outside crust in water. Most of the sand and dirt was in this way washed out, and lying in some of the crevices could be seen a little blackish flocculent matter, which was gently torn away with fine forceps, removed to a slide, and mounted. This flocculent substance, apparently, contained some of the broken and coarse threads of the plant, but evidently the entire plant did not grow on the surface of the incrustation, for in no case was it discovered in this loose matter, but always on solution of the inorganic substance. The deposit from the washings of the moss was now treated with diluted acetic acid and very carefully examined in small quantities under the microscope, but only a very few disjointed heads and minute portions of the mycelium threads were seen amongst fine, quite decayed vegetable structures, the former of which were possibly from the abraded parts from which the pieces had been sawn, and those exposed on removing the mass from the bed where it was formed; hence why the term apparently, has been used in reference to those portions of the flocculent substances found on the outside.

Several fresh pieces were again sawn from the mass and dissolved in weak acetic acid; the flocculent bits as well as the organic débris were very carefully examined in small portions at a time. The entanglement of the long threads or tubes of the little plant and the sort of network it appeared in most cases to form around or upon the decayed moss structure, just opened up the question whether the plant may not have originally been parasitic on the moss; but tracing the threads in some cases more than a quarter of an inch, some part of the coarser portion of the tubular stem was generally found attached to a little mass of a soft dark-brown substance (? humus) which had resisted the action of the acid, but which broke up directly under the needles, for it was only by considerable patience that the vegetable débris of the moss could be cleared away from these long, much-branched, interlacing threads, or could be cleaned so as to furnish a more or less satisfactory specimen for figuring, and then always with the loss of some of the little heads or sacks at the tips of the branches.

Comparing the plant with the figures at hand of *Botrydium granulatum*, and notably the excellent one by Mr. E. Parfitt, in the January number of 'Grevillea'; the conceptacles or heads of the plants were seen to be very much more minute; *Botrydium granulatum* being described as of the size of a pin's head, whilst these average from 2 to 6 one-thousandths of an inch. Again, many of the heads, though having more or less the shape of Botrydium, are not as sessile, but are borne at the ends of fine long tubes as in mucor; others again being very sessile, almost seated on the tubes; others being intermediate; whilst the dense strong-looking root threads or tubes furnished a resemblance justly in favour of Mr. Renny's and Mr. Currey's opinions, to which the writer gladly conforms. For the present, failing the successful endeavour to procure fresh living specimens, for which, thanks to Miss Davies, steps have been taken, it will perhaps be as well to place the little plant in the genus Botrydium, and from its quite microscopic character name it *Botrydium minutum.*

Another curious point to be noticed is that *Botrydium granulatum* is described as decaying rapidly, whilst here we have a minute plant, though discoloured, yet resisting the decaying influences of time ; who can say the exact period ? though

> "Little avail it now to know
> Of ages passed so long ago."

whilst the coarser vegetable tissues of the moss have almost entirely succumbed to their action ; the best preserved parts of the moss being the small leaves of the stem, in which in several cases the intimate structure was left very perfect, yet readily breaking up on being handled with the dissecting needles.

The plant as represented in Fig. 1 may be briefly described as consisting, so far as known, of long coarse, dense, and somewhat roughish non-septate mycelium tubes, from which arise narrower and finer branches, whilst from these, chiefly, spring shorter or longer fine stalks, which bear on their tips globular or variously shaped receptacles or heads, containing apparently a fine plasma, though in some can be seen fine and coarser granules or spores, and a few still rather thinner threads, most likely rootlets. The receptacles appear to present two chief forms, one of which, more or less oblong or contorted, and somewhat, as in some of the figures of Vaucheria, may perhaps be regarded in the absence of fresh living specimens as antheroidal in their function, and the other, the round and somewhat granular, as oogonal : though whether the fertilization takes place by the passage of spermatozoids along the tubes from the former receptacle to the "oogonium," as suggested by Mr. Parfitt, to be the case in *Botrydium granulatum*, is mere conjecture.

By the careful examinations made, were detected some conceptacles in form oblong and globular (*vide* Fig. 2 *b* and *a*), almost close together, of rather larger size than the ordinary heads ; one of the latter having enclosed several brown, apparently ripe spores ; whilst in a separate oblong receptacle (Fig. 3) may be seen some minute bodies, in the specimen a few highly refringent, which may be antheroidal in function. At the lower end and sides of the plant in Fig. 1 may be noticed some long more or less tubular portions, which differ considerably from the rest, and which have some marked resemblance to some of the early stages of fungi, yet are connected, as is seen, with a collapsed cell and a larger irregularly flattish lobular cell. Whether these differ from or form a primary or secondary phase in the life history of this minute plant must be left doubtful, but sufficient has been said to prove the correctness of Mr. Slack's remark, that the plant "is of undoubted interest."

In Fig. 4, which is regarded as a very young plant, are seen the following parts: a collapsed, dark-brown globular cell, furnishing a short, curved, simply-branched tube, having at the end of one of the branches a globular receptacle, and at the other end a short mycelium thread, to which apparently is connected a somewhat collapsed oblong cell; whilst at the lower part of the figure is seen an obovoid cell with a longer tube, which doubtless joins the mycelium branch of the round and oblong cells. At the bend of the first, or supposed primary tube, may be noticed two fine rootlets passing into the attached mass of vegetable débris.

Fig. 5 is apparently an entire plant, which from its appearance is questionable whether it belongs to this species; the tubular portion is septate.

Fig. 6 represents one of two small reddish cells with dark outlines, found in the examinations; but whether they should be regarded as ripe winter-germ cells of the plant, or ova of some rotifer or infusorian, it is difficult to say. There was apparently a small nuclear-looking body in one.

Fig. 7 is from a contorted receptacle intermediate in size between the cells of the entire plant of Fig. 1, and the larger cells given in Figs. 2 and 3; it was found as drawn, disjointed; the globular part contained small granular bodies, and the outside investment had broken up to a certain extent on part of the surface. Whether the contorted portion under further development would have become a supposed antheroidal cell, or is such undeveloped, or whether it only forms part of the tube of the receptacle widened out, must be left undecided.

The examinations have induced me to place this little plant rather with Botrydium than with Vaucheria, from its closer resemblance to the former, yet possibly it may not be new to other observers, from whom may it be hoped, further information may be obtained than is given in this imperfect sketch,

"To win but such a form, as thou mightst love to look upon."

Since writing the foregoing remarks, through the kindness of Miss Davies, some of the fresh moss was obtained, just after the last heavy fall of snow, from the bank within a few yards of the spring. Upon a very careful examination of the attached soil and of the lower part of the plants after gentle washing, likewise after placing some of the moss and the soil in weak acid, only two small globular heads of the little plant, each (enclosing in the centre an opaque dark spore-looking body) attached to very long irregular unbranched mycelium threads or tubes, were found. Each plant had quite a decayed appearance. The autumn may furnish a better chance of finding recent specimens. The writer is indebted to Dr. Braithwaite for kindly naming the fresh specimen of moss, "*Hypnum commutatum*." Probably the incrustations were formed over the same kind of moss.

II.—*Notes on the Micro-spectroscope and Microscope.*

By E. J. Gayer, Surgeon H.M. Indian Army.

(*Read before the* Royal Microscopical Society, *March* 5, 1873.)

In continuation of my paper on "A New Form of Micro-spectroscope," read before the Royal Microscopical Society on the 4th December, 1872, I would wish to add the following few remarks. On a trial it will be found that the arrangement with the erector placed in its usual position in the draw-tube, as described in the above-mentioned paper, may have a low-powered object-glass substituted for the erector, and that a very useful combination will be thus obtained for viewing the slit, and the object under examination, in the same field. A small ring screw adapter is all that is required to enable the object-glass to be screwed into its place at the end of the draw-tube. This combination, without a slit, can also be used when an erect image is required, as in dissections and arrangement of objects on a slide when placed on the stage of the microscope; and it will be found that, with this arrangement of two objectives, the magnifying power can be diminished or augmented almost at pleasure, without changing either of the object-glasses, by simply altering the relative distances between the object under examination and the first object-glass, and the distance between the first and second objectives, so that almost any degree of magnifying power which is found expedient, can be easily and rapidly obtained without any alteration in the apparatus. The least magnification will be obtained when the two objectives are near each other, and the farther they are separated the more the image will be enlarged. For those persons who have not got an erecting eye-piece, the arrangement herein described will prove a useful and very cheap substitute. It will also be found, if a micrometer of any convenient construction be placed in the position of the slit in this form of micro-spectroscope, and the arrangement just described be used, that a very sharp image of the micrometer and the object on the stage will be obtained; and in some instances this plan will be found more efficient than any of the usual forms which have, from time to time, been adopted. I would also wish to suggest, that when a micrometer with lines ruled on glass is used either in the shape of a stage micrometer or that form which is placed in the eye-piece, that, instead of having the lines ruled in the usual way, they should be made up of concentric circles placed at equal distances from each other; an object would in this way be very readily measured in all its diameters, and facilities for making drawings would also be afforded. Since my paper of the 4th December was read, it has been brought to my notice that the instrument described by me is similar in principle to the original form of the spectroscope applied to the microscope proposed by Dr. W. Huggins in a paper read by him before the Microscopical Society on the 10th May, 1865, and described and figured in the 'Microscopical Journal' for that month, entitled "Note on the Prismatic Examination of Microscopic Objects." I regret that I did not know of this paper by Dr. Huggins before reading my own, but a reference to it will show that the dispersive power employed was that obtained by the use of two prisms. There can be no doubt that the amount of dispersive power which will be the best for one kind of object, will not be always the most suitable for all other objects, and that therefore sometimes one and sometimes two prisms will give the best results. Much will depend upon the amount of light available, and it would therefore be useful to so arrange matters that either one or both prisms could be used at pleasure, but as there is plenty of light available when the slit is placed sufficiently close to the object-glass, and the collimating lens is of adequate angular aperture, two prisms, under ordinary circumstances, can easily be used without rendering the absorption bands at all undefined or indistinct. Dr. W. Huggins doubtless places his slit at a distance of three or four inches from the object-glass, because it gives a larger image of the object relatively to the slit, and because the angle of divergence is greater the nearer the image or slit is to the object-glass. The maximum of light (irrespective of the size of the object) will be when the angle of the collimator is equal to the angle of divergence. Now, when a very accurate inspection of the minute portions of small objects is required, the position of the slit must accurately correspond to the place where the image of the object on the stage is formed behind the object-glass; and as this position of the image can be made either to approach or to recede from the object-glass, at the will of the operator, by merely altering the distance between the object on the stage and the object-glass, by means of the usual focussing arrangements, almost any position of the slit can be easily made to be in the conjugate focus behind the object-glass; and as the nearer the slit and image are to the object-glass the farther the object-glass must be removed from the object, the motion is in a possible direction; and as the image diminishes as it approaches the object-glass, its brightness rapidly increases, and therefore the closer the slit is to the object-glass, the more the light will be increased which is received within the jaws of the slit; but the closer the image is to the objective the larger will be the angle of divergence, and therefore the diameter of the collimating lens must be increased in proportion, and its focal distance lessened.

Suitable proportions appear to be reached when the slit is placed at a distance of $1\frac{1}{2}$ inch from the objective, and the collimating lens is $1\frac{1}{4}$ inch in diameter with a focal length of 4 inches. The prisms should be of the same width as the collimating lens; and, as Dr. Robinson and others have suggested, they might be made up of any of the forms of compound prisms which were considered most suitable to the object for which this spectroscope was chiefly selected. The magnifying power required is best obtained by changing the objective.

III.—*On the Structure and Function of the Rods of the Cochlea in Man and other Mammals.* By Urban Pritchard, M.D., F.R.C.S., Demonstrator of Physiology, King's College, London.

(*Read before the* Medical Microscopical Society, *Feb.* 21, 1873.)

Plate XIII.

Before entering upon the description of the rods themselves, it may be well to refer briefly to the mechanism of the ear, and the method by which we are able to hear and distinguish certain sounds.

The ear itself, as is well known, is one of the most complicated organs of the body, consisting of the external, middle, and internal sections: the two former are merely concerned in collecting and conducting sounds or vibrations; while the duty of the internal portion consists in receiving, localizing, and, moreover, clearly distinguishing them. Now, it is simply with this last and most delicate function of the organ that I purpose to deal, my aim being to describe the true construction and use of the cochlea so far as its task of distinguishing the various sounds is concerned.

I may state that, for convenience sake, I shall in the course of my remarks speak of the cochlea with its apex uppermost. This cochlea, it must be borne in mind, consists of a spiral canal, in form and shape very similar to the inside of a snail-shell. From the axis, or modiolus as it is called, of this spiral, there proceeds horizontally a plate of bone, the lamina spiralis, which almost divides this canal into two. From this plate again there extend two membranes to the walls of the canal, thus separating it into three minor canals.

Of these two membranes the upper one or membrane of Reissner (Fig. II.) arises just behind the teeth of the limbus (as the peripheral end of the bony lamina is called), and passes upwards and outwards to the upper part of the ligament of the cochlea; it is exceedingly delicate, and is composed of a single layer of flattened nucleated cells, closely adhering to each other, and which are situated on a very thin membrane.

EXPLANATION OF PLATE XIII.

Fig. I.—The upper extremities of the rods (dog), showing the mode of articulation and processes. Drawn by camera, × 500 diam.; *i*, inner rod; *o*, outer rod.

,, II.—A *diagramatic* drawing of a vertical section of the central canal of the cochlea, × about 150 diam. 1. Scala vestibuli. 2. Central canal of cochlea. 3. Scala tympani. A, Membrane of Reissner; B, Membrana tectoria; C, Bony lamina spiralis; D, Membrana basilaris; E, Ligament of cochlea; F, Nerve plexus; G, Epithelium; H, Membrana reticularis; L, various cells; *i*, inner rod; *o*, outer rod.

,, III.—Three pairs of rods carefully drawn from three sections of the cochleæ of the same animal (cat), showing the mode of graduation—1, from near the apex of cochlea; 2, from about the middle; 3, from near the base, × about 250 diam.; *i*, inner rod; *o*, outer rod.

Fig. I.

Fig. II.

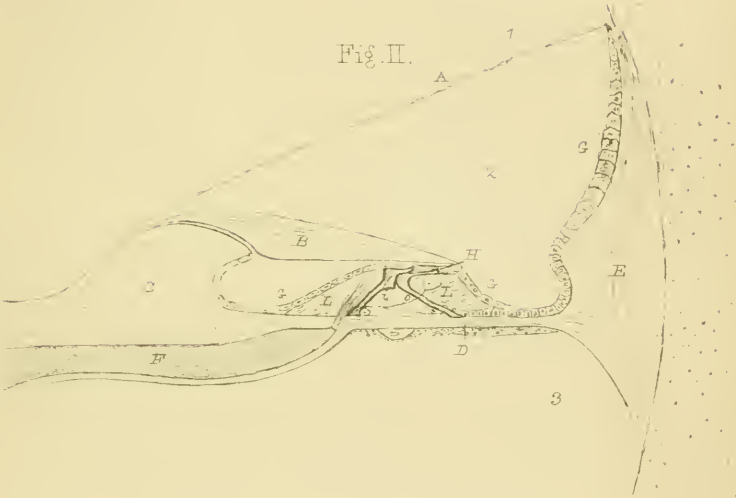
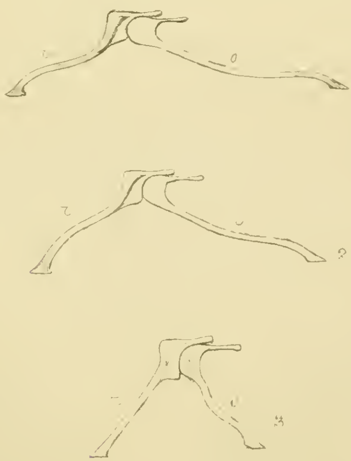


Fig. III.



Dr. Pritchard

W. West & Co. imp.

The other, the lamina spiralis membranacea, proceeds directly outwards from the limbus to the ligament of the cochlea.

This membranous lamina is composed of two horizontal membranes, having between them certain delicate structures. The upper of these (the membrane tectoria) arises from the limbus just external to the membrane of Reissner, and passes directly outwards, covering and overlapping the end of the limbus, but not actually reaching the ligament of the cochlea (Fig. II.). It may be well to state, by the way, that on this point I differ from most of the previous writers, with the exception of Böttcher. The portion which covers the limbus is moderately thin, but the outer portion which overlaps it is exceedingly thick, and the whole is marked with radiating wavy lines.

The lower, or membrana basilaris, arises from the lower lip of the limbus, while its other end is firmly attached to a well-marked ridge on the ligament of the cochlea.

Between the membrana tectoria above, and the membrana basilaris below, are situated the so-called rods of Corti.

The Rods of the Cochlea.

These interesting little bodies were first discovered and described by the Marquis of Corti, whose name they now bear, and although since then many observers have studied and written on the subject, yet scarcely two are agreed as to their exact form, while many of the later authors have gone so far as to state that their shape is of a most varied character. The greatest difference of opinion exists as to the shape of the rods at their articulation. Deiters described them very minutely in his first paper, but in a second communication he completely altered his description of them.

The following is a free translation of an extract from Deiters' first paper in Siebold and Kölliker's 'Zeitzchrift von Wissenschaft Zoologie,' vol. 10.

"The outer rods do not directly join the inner, but are connected by a curious body, which partly belongs to the true rods of Corti and partly to the lamina reticularis, and which we will call the middle union joint. These middle joints vary much in form, owing to their easy compressibility, and have not on that account been as yet properly distinguished." "This (middle joint) will best be likened to a boat which terminates at one end in a pointed keel, and at the other in a straight back or plate."

"In the natural position this back plate is turned upwards and lies parallel to the membrana basilaris; the keel, on the contrary, looks downwards and somewhat forwards. . . . . . . The back plate is nearly rectangular in form, but only the anterior corners are perfect, the posterior being rounded, and perhaps only appear angular on account of the plates lying side by side."

In his later work on the subject he figures these rods very differently, and much more accurately.

Kölliker, Henle, and others appear to agree with Deiters' later view of the form of the rods, and most of our text-books have copied their drawings.

Recent writers, such as Drs. A. Böttcher, Waldeyer, Göttstein, and Nuel, give varying drawings, some of which are nearer the true form of the rods than that of Deiters, while others exhibit the rods in all kinds of extraordinary shapes.

I will proceed at once to detail the results of my own observations.

In a general view of the rods from above (that is to say, looking at the lamina spiralis lying flat) they appear similar to two rows of pianoforte hammers, rather than like the keys of that instrument, to which they have been likened, the heads of the rods lying close together.

In a lateral view, these two rows of rods are seen sloping towards each other like the rafters of a gabled roof, and it is by reason of the difficulty in obtaining these side views (vertical sections) that such very different ideas exist as to the question of shape.

The rods, as before mentioned, lie between the membrana basilaris and membrana tectoria, and pass directly outwards from the lower lip of the limbus; they are both firmly attached by their lower extremities to the membrana basilaris, their upper extremities being covered by a peculiar membrane (membrana reticularis), but they are not in any way connected with the membrana tectoria. On every side they are surrounded and supported by cells of a more or less delicate structure. The rods are best described like a long bone, as consisting of a shaft, and two enlarged extremities, but the two rows differ considerably in form. The inner rods (those nearer to the bony lamina) are attached by their lower extremities to the membrana basilaris at its junction with the lower lip of the limbus and just external to the spot where the nerve filaments emerge (the habenula perforata). They are directed outwards and upwards, with a slight undulation to meet the outer rods.

The lower extremity is enlarged and rounded, gradually tapering to the shaft. The shaft is cylindrical, although Deiters, Claudius, and nearly all other observers state that they are flattened; but by referring to preparations in which the inner rods are cut through their shafts, the cut ends will be seen to be quite circular.

Curiously enough, although these very investigators say the shaft is flattened from above downwards, they give a thick lateral view of the same. The upper extremity is peculiar in form, and as I differ from all observers on this point, it requires special attention.

Its superior surface is rectangular, but longer than it is broad, as is well seen when looking from above, in flat preparations. Externally it is prolonged into a process which overlaps the superior extremity of the outer rod, and terminates somewhat abruptly. The form of this process will be better understood by looking at Fig. I. Below it is continuous with the shaft; the lateral surfaces are somewhat quadrilateral in form, with the anterior and posterior edges concave. These surfaces are divided obliquely by a curved ridge; the upper and inner is smaller, raised and marked by curved lines—the external and lower division is smooth and more transparent. The inner surface is concave from above downwards, and is continuous with the lateral surfaces.

The external surface is deeply concave, and receives the head of the outer rod very much in the same manner as the glenoid cavity receives the head of the humerus. The upper lip of this concavity is continuous with the process mentioned above, the lower one is rounded, and forms a sort of tubercle.

The outer rods are attached to about the middle of the membrana basilaris by a broad base, which is very similar to that of the inner rods, but somewhat larger, and this also gradually tapers towards the shaft. The shaft is cylindrical, and equal in diameter to that of the inner row, as may be proved by carefully measuring the two as seen in most of my preparations. The upper extremity of these outer rods is also peculiar, but very different to that of the inner. The superior surface is quadrilateral, but both broader and longer than that of the corresponding extremity of the first row. Below it is of course continuous with the shaft.

The inner surface is very convex, forming a head which articulates with the corresponding concavity of the inner rod. The outer surface is slightly concave, and from the upper part a long slender process extends outwards. This process lies at first under that of the inner rod, but is prolonged much farther outwards; it is rather more slender in form, and has a handle-like enlargement at the extremity: the whole will be better understood by referring to Fig. I. The lateral surfaces are apparently smooth, but marked by fine radiating lines.

The articulation of the two rows is not movable; there are no ligaments, unless the membrana reticularis, which is finely adherent to the upper surfaces, may be regarded as such, but the articulating surfaces may be seen to be glued together in some peculiar way.

I now come to one of the most important features with regard to these interesting little rods, namely, their relative length. Most authors state that there is very little difference in the length of the two rods; this is quite a mistake, as I am about to prove, for not only do the two sets of rods differ in this respect, but the length of each varies according to its position in the cochlea. Thus, at the base of the cochlea, the outer rods are as nearly as possible equal in length to the inner, but as we proceed upwards, both rods increase in length with great regularity, although not in the same ratio. The outer increases with much greater rapidity, so that near the apex they are twice the length of the inner. This fact is clearly demonstrated by referring to one of my preparations, in which the various measurements were found to be as follows; beginning at the lowest section of the lamina and proceeding upwards in regular succession:—

1st Section	Inner rod measures $\frac{21}{10000}$ of an inch.	
	Outer rod „ as nearly as possible the same.	
2nd Section	Inner rod measures $\frac{23}{10000}$ of an inch.	
	Outer rod „ $\frac{35}{10000}$ „	
3rd Section	Unfortunately this is not sufficiently perfect to admit of measurement.	
4th Section	Inner rod measures $\frac{25}{10000}$ of an inch.	
	Outer rod „ $\frac{45}{10000}$ „	

The 5th and 6th are not sufficiently perfect to allow of measurement, although in the latter the rods may be seen to have increased in about the same ratio. Further confirmation of this statement may be obtained by comparing the rods shown in any vertical sections of the lamina.

Fig. III. represents three pairs of rods carefully drawn from three sections of the cochleæ of the same animal (cat). The uppermost taken from near the apex of the cochlea, the next from about the middle, and the lowest from near the base.

It was generally supposed *a priori* that these rods were graduated so as to distinguish the most minute variation of tone, but no one, until now, has been able to demonstrate this.

The rods, therefore, vary in length from about $\frac{1}{500}$ to $\frac{1}{200}$ of an inch. Their other measurements are as follow:—

	Inch.
Diameter of base of inner rod, about	·0006
„ „ outer rod „	·00075.
„ „ shaft inner „	·00015.
„ „ „ outer „	·00015.
Anterio-posterior measurement of upper extremity of inner rod, about	·0005.
Anterio-posterior measurement of upper extremity of outer rod, about	·0006.
Lateral posterior measurement of upper extremity of inner rod, about	·0002.
Lateral posterior measurement of upper extremity of outer rod, about	·0003.
Length of process of upper extremity of inner rod, about	·0006.
„ „ „ outer „	·00075.

The number of rods in each row is not the same, there being three of inner for every two of the outer, and, according to a rough calculation that I have made, there are about 5200 inner rods, and 3500 outer in the whole cochlea.

Deiters stated that the rods of the outer row were smaller and more numerous than those of the inner, while Claudius positively stated the reverse. Now, Deiters was undoubtedly wrong in both these statements, and Claudius not altogether correct, for although the latter was right about the inner rods being the more numerous, yet he was incorrect in stating that the inner were the smaller, there being, indeed, no difference in the diameter of the shafts, but only in the width of the upper extremities.

The rods of Corti appeared to be composed of a homogeneous substance resembling the matrix of delicate cartilage.

Numerous longitudinal and curved lines are observable, especially at the enlarged extremities, and they may readily be split up into fibres, otherwise they appear quite transparent and contain no nuclei. They evidently possess great elasticity, and are calculated in every way to receive the finest vibrations.

They are to be stained by all the various colouring fluids, as carmine, and aniline blue, magenta, &c., but they are not so deeply coloured as the nuclei of cells, &c.

Most authors, with the exception of Deiters, describe nuclei situated in various parts of these rods, principally in the lower extremities, but although at first sight, and especially when seen from above, this does appear to be the case, yet on closer observation these so-called nuclei of the rods are found to be nothing more than the nuclei of cells surrounding the bases of the rods. In my opinion there is no ground whatever for the belief expressed by some modern authors, that they are composed of fine fibres *continuous* with those of membrana basilaris, and for this reason : the bases of the rods may be easily separated from the membrana basilaris, and in this case are found to be quite rounded, and in no way jagged or uneven.

The Arrangement of the Nerves in connection with the Rods of Corti.

The cochlear nerve fibres from the portio mollis pass up the modiolus and turn off at the lamina spiralis. Just at this junction we find in the bone itself a ganglion ; the cells of this are fusiform, bipolar, with distinct nuclei. From this ganglion fibres proceed outwards, these form a close plexus, and give rise to the broad dark band seen in all transverse sections of the lamina spiralis.

Immediately before the end of the lower lip of the limbus, the nerve filaments pierce its upper surface, by a number of small foramina (habenula perforata), and appear close to the base of the inner row of rods. Concerning the termination of these nerve filaments little is really known. I have traced them on to the inner rods themselves, and to the tiny cells lying on their bases, as also to certain delicate cells between the rods, but I have good reason for believing that some of them terminate in the cells of Corti and

Deiters, on the outer rods themselves, and in the corresponding little cells on their bases.

Filaments also pass directly upwards to the inner side of the first row of rods, and on these filaments little modular enlargements may be seen.

The Function of the Rods.

Corti and most of the subsequent authors considered this system of rods to be the essential portion of the cochlea. They supposed the rods received the vibrations conducted to them, and being set in motion, so affected the nerves as to cause the brain to appreciate the various sounds.

Later German writers have, however, attributed the appreciation of the various vibrations to certain delicate cells (the cells of Corti and Deiters) which are attached to the under-surface of the membrana reticularis. From this circumstance alone, it appears evident that these investigators had not suspected, much less discovered, the fact that the rods are most exquisitely graduated, for otherwise they could surely never have doubted that so beautiful and suitable an apparatus could have any other ostensible purpose than that of appreciating the various sounds. If the rods had been found to be longer in the lower and larger portion of the canal, and shorter in the upper and smaller portion, the matter might naturally enough have been regarded as one of little importance ; but it must be remembered that quite the reverse is the case, for the rods actually increase in length as the canal becomes narrower. This uniform graduation of the rods presents to my mind so plausible and reasonable a key to their use, that there can scarcely be a doubt as to their real function. I consider, indeed, that the cochlea as a whole represents a finely-constructed musical instrument, similar in nature to a harp or musical-box, the strings of the one and the teeth of the other being represented by the rods of Corti. The spiral bony lamina is simply nothing more nor less than a natural sounding board, in connection with the end of which are arranged the rods, attached to a strong membrane (membrana basilaris) by their feet, and supported throughout by delicate cells, the whole being protected above by the thick membrana tectoria, and bathed in a special fluid secreted by the epithelial cell. This fluid, it should be mentioned (endolymph), is cut off from the other fluid (epilymph) in the general canal by the delicate membrane of Reissner. Around the rods are placed the various nerve cells and nerve fibres ; of the former, I believe the cells of Corti and Deiters to be the most important, and these being connected through the medium of the membrana reticularis to the processes (which act as levers) are, of course, suitably placed to perceive the slightest vibration of the rods.

From these cells the impressions are conveyed by the nerve fibres to the brain itself. Thus, I think, we are in a position to trace very completely the course of sounds or vibrations from a musical instrument, or any other source, to the brain, through the medium of the ear. First, the vibrations are caught and collected by the auricle, and transmitted through the external meatus to the drum of the ear, next across the middle ear (the tympanic) cavity, principally by means of the chain of little bones, to the internal ear. Here the sound is appreciated merely as a sound by the vestibular portion of the labyrinth; the direction of the sound is probably discovered by means of the semicircular canals, but to distinguish the note of the sound it must pass on to the cochlea. The vibration then passes through the fluid of the cochlea, and probably strikes the lamina spiralis, which, acting as a sounding board, intensifies and transmits the vibration to the system of rods. There is, doubtless, a rod not only for each tone, or semitone, but even for much more minute subdivisions of the same, so that every sound causes its own particular rod to vibrate. Thus each string sounded on the primary musical instrument induces a vibration in the corresponding rod of the secondary musical instrument (the cochlea). And this rod vibrating so affects the nerve cells in connection with it as to cause them to send a nerve current through the nerve fibres to the brain, which current is no doubt modified or affected by passing through the ganglion cells, situated in the bony lamina near its junction with the modiolus, as before mentioned.

IV.—*A New Formula for a Microscope Object-glass.*

By F. H. Wenham.

A pencil of rays exceeding an angle of 40° from a luminous point cannot be secured with less than three superposed lenses of increasing focus and diameter, by the use of which combination rays beyond this angle are transmitted with successive refractions in their course, towards the posterior conjugate focus: until quite recently, each of these separate lenses has been partly achromatized by its own concave lens of flint glass, the surfaces in contact with the crown glass being of the same radius, united with Canada balsam; the front lens has been made a triple, the middle a double, and the back again a triple achromatic. This combination, therefore, consists of eight lenses, and the rays in their passage are subject to errors arising from sixteen surfaces of glass.

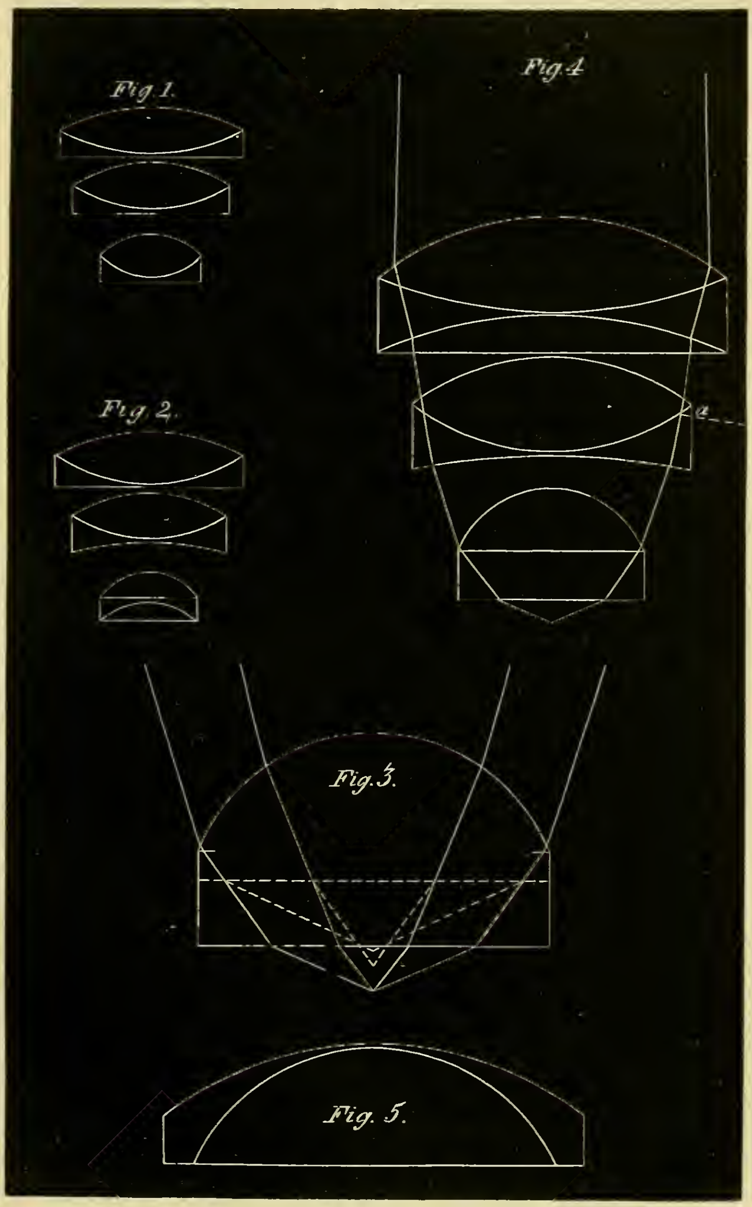
In the new form there are but ten surfaces, and only *one* concave lens of dense *flint* is employed for correcting *four* convex lenses of *crown* glass: as this might at first sight be considered inconsistent with theory, a brief retrospect of the early improvements of the microscope object-glass will help to define the conditions. The knowledge of its construction has been entirely in the hands of working opticians; and the information published on the subject being scanty, this has probably prevented the scientific analyst from giving that aid which might have been expected.

Previous to the year 1829 a few microscopic object-glasses were made, composed of three superposed achromatic lenses; but this combination appears to have been used merely with the intention of gaining an increase of power, in ignorance of any principle, and without even a knowledge of the value of angular aperture.

At this time the late J. J. Lister tried a number of experiments, and discovered the law of the aplanatic focus, and proved that, by separating lenses suitably corrected, there were one or two positions in which the spherical aberration was balanced. This was explained in a paper read before the Royal Society in 1829. In the year 1831 Mr. Ross was employed to construct the first achromatic object-glass in accordance with this principle, which performed with "a degree of success never anticipated."

Mr. Ross then discovered that, after he had adjusted the interval of his lenses for the aplanatic focus, that position would no longer be correct if a plate of thin glass was placed above the object; this focus had then to be sought in a different plane, and the lenses brought closer together, in order to neutralize the negative aberration caused by covering-glass of various thickness. From this period the "adjustment" with which all our best object-glasses are now provided became established. Fig. 1 is the form of object-glass used at this time, consisting of three plano-concave achromatics, whose foci were nearly in the proportion of 1, 2, 3.

No greater angle than 60° could be obtained with this system in a $\frac{1}{8}$-inch objective (the highest power then made), for reasons apparent in the diagram. The excessive depth of curvature of the contact-surfaces of the front pair is unfavourable for the passage of the marginal rays; the softness of the flint glass forming the first plane was also objectionable. In the year 1837 Mr. Lister gave Mr. Ross a diagram for an improved "eighth," having a *triple front lens* in the form shown in Fig. 2. By this the passage of extreme rays was facilitated; and in order to diminish the depth of curvature, a very dense glass was used, having a specific gravity of 4·351. Faraday's glass, having a density of 6·4, had been previously tried, but was abandoned on account of a difficulty in working it. The polished surfaces of both these qualities of dense glass speedily became tarnished by exposure to the air; and thus the dense flint concave could only be employed in a triple combination, that is, when cemented between two lenses of crown glass: this form of front was kept a trade secret, and was not published in any work treating of the optics of the microscope. The front incident surface of the flint of the middle pair was made concave, in



order to reduce the depth of the contact; and for this reason only, as that surface has but little influence in correcting the oblique pencils, or in producing flatness of field, and may be a plane with an equally good or better result. "Eighths" of this form with angles of 80° were made, and remained unaltered till the year 1850, when larger apertures were called for, and Mr. Lister introduced the *triple back lens.*

The necessity for this will be seen by the diagram (Fig. 2), which shows that the contact-surfaces of the back achromatic are too deep, thus giving great thickness to the lens, and limiting its diameter: dense flint would have remedied this to some extent; but its liability to tarnish rendered its use in a *pair* objectionable. The highest density at this time known, quite free from this defect, was 3·686. By means of the triple back, the final corrections were rendered less abrupt, a greater portion of the marginal rays could be collected, and the aperture of an "eighth" was at once brought up to 130° or more.

At this time the author had been making some experiments in the construction of an object-glass in the form of Fig. 2. Mr. Lister having favoured his "eighth" with an examination, was good enough to communicate his late improvement of the triple back. No time was lost in giving this a trial, the result of which proved that excessive negative aberration or over-correction could readily be commanded with lenses of shallow contact-curves. During these trials all chromatic correction was obtained by alterations in the triple *back;* for it was found that the colour-correction could not be controlled by a change in the concave surface of the triple *front,* as the negative power of the flint here appeared to be feeble, requiring a great difference in radius to give a trifling result. For this reason the front concaves were formed of very dense and highly dispersive flint; the cause of this was analyzed by a large diagram, with the passage of the rays projected through the combination, starting from the longest conjugate focus at the back. This proved that the rays from that focus passed through the concave flint of the front nearly as a radius from its centre, or in such a direction that its negative influence was almost neutralized. It is well known that a lens may be achromatic for parallel rays, and under-corrected for divergent ones. The utmost extent of this condition was apparent in the object-glass under consideration.

This led the author to the idea of the *single front* lens of crown glass, which gave a fine result at the first attempt, as the back combinations to which it was applied happened to have a suitable excess of negative or over-correction existing in the triple back alone, the middle being neutral or nearly achromatic. Still there was a defect remaining as positive spherical aberration; and this was afterwards cured by giving *additional thickness* to the *front* lens, which is now recognized as a most essential element of correction. In a "fifteenth," for instance, a difference of thickness of only ·002 of an inch will determine the quality between a good and an indifferent glass. Fig. 3 represents a front lens suitable for bringing the back rays to a focus. The dotted lines indicate the effect of this difference, showing that with a lens of less thickness, the marginal rays fall within the central, producing positive aberration as the result.

The single front introduced by the author is now used by every maker; for several years he could not induce the leading opticians to change their system, though challenged by a series of high powers constructed on this formula, for the purpose of proving its superiority. Fig. 4 represents the curves of the first successful "eighth" on this system, having an aperture of 130°, enlarged ten times. On tracing the passage of the marginal rays through the combination, it will be seen that, though the successive refractions are nearly equalized, the contact-surfaces of the middle pair are somewhat deep, though no over-correction existed or was needed here, for this would have required a shorter radius still (the density of the flint in this was 3·686). If this pair of lenses were not cemented with Canada balsam, total reflexion would take place near the circumference of the contact flint surface, cutting off the marginal rays at *a*, and limiting the aperture. It might be argued that practically this would be no disadvantage, as these surfaces are united with Canada balsam, whose refraction is higher than the crown; so that the rays in this case must proceed with very little deviation. But incidences beyond the angle of total reflexion may be considered detrimental, as they imply excessive depth of curvature; this can be discovered by looking through the front of an object-glass held close to the eye, any air-films in the balsam near the edge of the lens appearing as opaque black spots.

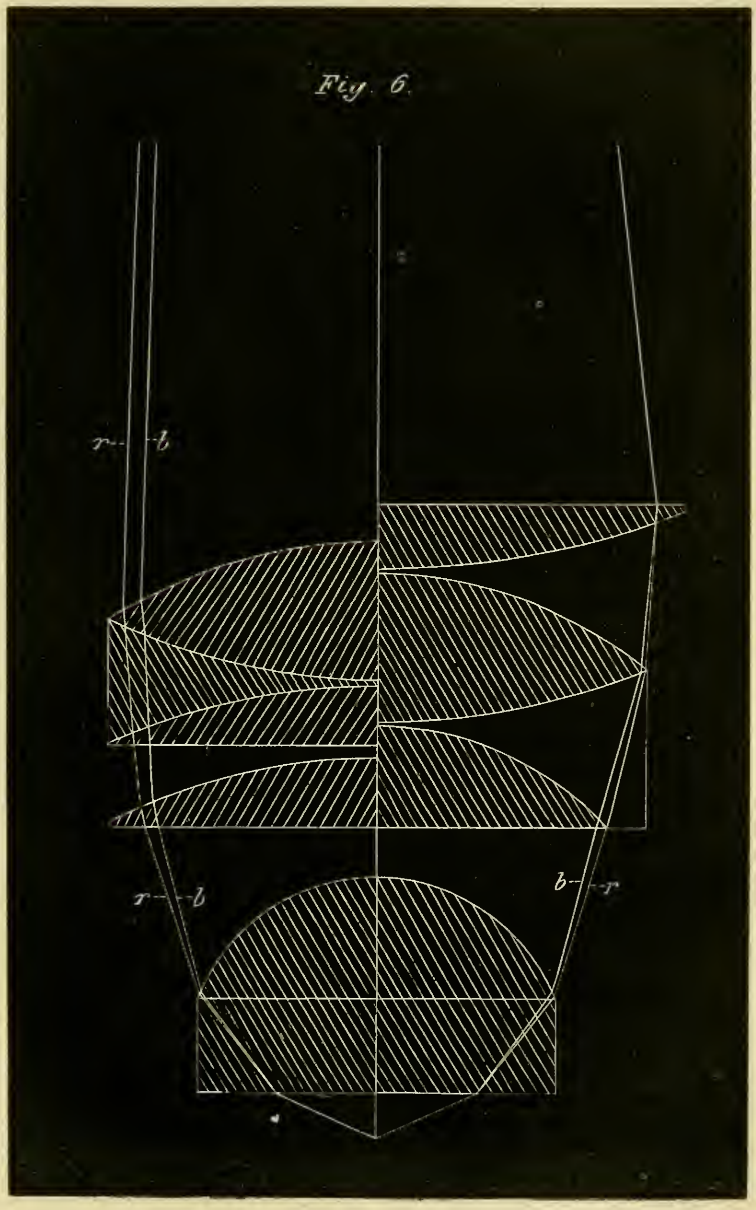
At the commencement of the present year the author caused a few object-glasses to be made, with a middle of the form of Fig. 5, the performance of which was very satisfactory. In this the extreme rays pass at more favourable incidences, and within the angle of total reflexion. The upper lens is of dense flint.

When the experiments on the single front were concluded, and the remarkable corrective power of the triple back in conjunction therewith had been proved, the next attempt was to make the *middle* also a single lens, leaving the entire colour-correction to be performed by the one biconcave flint in the back. After numerous trials it was found that, though something like over-correction or negative aberration could be obtained with the back, in the degree requisite for balancing the under-correction of the single middle and front when set at the prescribed distance of the aplanatic focus, yet by trial on the mercury globule all the results invariably displayed two separated colour-rings: these could not be combined by any alteration in the radius of the lenses. By projecting the blue and red, or visible rays of greatest and least refrangibility through the system, the cause became apparent. The left-hand section of this object-glass is shown in Fig. 6. The rays from the focus are slightly divided by the first front surface. On emerging from the back the separation is increased; the red ray (r) is outwards, and the more refrangible or blue ray (b) inwards. Next, the divergence of these two rays is extended by the middle single lens. The following crown lens extends the angle of divergence so far that the flint lens of the back triple cannot recombine them; and they emerge at two distinct zones, shown by the practical test of the "artificial star" or light-spot reflected from a mercury globule, viewed within and without the focus.

It might be supposed that these rays at their final emergence can be so refracted as to project the blue outwards. A crossing point would then occur at a fixed conjugate focus in the body of the microscope, at which all rays would be combined; and if this focus was adjusted to that of the eye-piece, achromatism and final correction would be the result. But to meet the various conditions occurring in the use of the microscope, the conjugate focus constantly alters in position, this being affected by every change of eye-piece, length of tube, or adjustment for thickness of cover; therefore a correction for a fixed point cannot be maintained. Achromatism in the microscope object-glass, like that of other perfectly corrected optical combinations, must be the reunion of the rays of the spectrum close to the final emergent surface of the system. The remedy suggested by these experiments appeared to be a transposition, that is, in placing the over-corrected triple in the *middle* of the entire object-glass; this would at once cause a convergence of the blue and red rays. A single lens of longer focus at the back would then bring these rays parallel at the point of final emergence.

By projection in a diagram this condition was apparently realized. The dispersive power of the flint (density 3·686) was taken by the refractive index 1·76 of line H in the blue ray of the spectrum, and 1·70 of line B in the red ray. The refraction of the corresponding rays in the crown (density 2·44) was 1·53 H and 1·51 B. With these indices the rays are traced in Fig. 6. The radii in the right-hand half-section are those of an "eighth" of the new form drawn twenty times the size of the original. The *single front* is of the usual form, as this is much alike in all cases. The radius or focus of the *single plano-convex back* is about four and a half times that of the front, and the focus of the *middle* (triple) three times. The passage of the blue and red rays at the extreme of the pencil is shown in contrast with the preceding, the separation from the same front being alike.

The inner and outer, or blue and red rays, after passing the first surface of the triple middle, meet the concaves of the flint,



which refract the blue rays to a greater extent than the red, and cause them to converge (instead of diverging, as in the opposing half-diagram), so that at their exit from the triple they meet and would cross, effecting what is known as "over-correction"; but this is so balanced and readjusted by the single back of crown glass, that the rays are finally united, and emerge in a state of parallelism. This form of object-glass is suitable for the high powers, or such as have a cover adjustment, *viz.* from the "$\frac{1}{2}$-inch" upwards; perfect colour-correction is equally to be obtained in all of them.

It may be asked by some who have devoted their attention to the higher branches of optical mathematics, why the above result should have been worked out entirely by diagrams. But it has been found such a difficult task to calculate the passage of the two rays of greatest and least refrangibility through a combination having sixteen surfaces of glass of three different densities and refractions, that even first-class mathematicians have hitherto shrunk from the attempt.

Diagrams, however, are surprisingly accurate in their capability of indicating causes and results in the microscope and object-glass; for these lenses are minute, with deep curves and abrupt refractions; so that if the projection is worked out some fifty times the size of the original, small errors can be detected. The work should be commenced at the back from a long conjugate focus, which, not being a constant distance, may be taken as very near to parallelism. The high powers all have the means of correction within this distance, and perform better with a long posterior focus than with a very short one. The relative indices for the two or more rays should be marked on a large pair of proportional compasses, the long limbs representing the sine of the angle of incidence, and the short one that of refraction. Both the sines ought to be set off in the diagram *behind*, and neither of them in front of the ray in course of projection; this leaves the way clear, with the least confusion of lines.

At the same time a second or counterpart diagram should be at hand, to which the rays only are transferred as soon as their direction is ascertained; with these precautions a mistake is scarcely possible.

Now it is hoped that some improvements may be effected by this investigation, on account of the simplicity attained in the combination, in which we have *two single lenses* of crown, whose foci bear a definite proportion to each other; while all the corrections are performed by *one concave of dense flint*, the acting condition of which is not altered by the influence of any other concaves acting in the combination, and hitherto taking a share of the duty. This one flint is now to be considered singly as the heart and centre of the system in reference to the correction of the rays entering and leaving.

This memoir is of necessity incomplete, for want of definite information concerning the optical properties of various kinds of glass. Data obtained from working them into small lenses furnish only a rough approximation to the mean dispersive power of the combined flint and crown having the best apparent effect. Of the intermediate rays, little can be known beyond the mere appearance of more or less of a secondary spectrum.

Nothing of importance has been published since Fraünhofer's Table, containing the refractive indices for each of the seven primary colour-lines of the spectrum for ten kinds of glass: great advance has been effected since that date in the manufacture of optical glass, a most complete collection of which of every variety has been made by the Rosses up to the present date. Selected specimens from this will be worked into prisms, and the relative spectra mapped out by the Fraünhofer lines, leading, it is hoped, to the discovery of a combination of crown and flint glass which shall be free from secondary spectrum or absolutely achromatic. The result of this investigation will be subject of a future communication.—*Proceedings of the Royal Society*, No. 141, 1873.

V.—*Professor Smith's Conspectus of the Diatomaceæ.*

By F. Kitton, Norwich.

Captain F. H. Lang will probably excuse the following remarks on his critique upon the above-named Conspectus. The late Dr. Arnott, whose knowledge of the Diatomaceæ was perhaps greater than any other diatomist, always contended that the stipitate, concatenate, or frondose states were not of any generic or specific value.

Professor Smith, in placing *Arachnoidiscus* in the same tribe as *Melosira*, has surely brought together forms more nearly allied than Kützing has in his arrangement. He places the Melosireæ in the same tribe as Eunotieæ, Surirelleæ, and Naviculeæ. Professor Smith does not refer all the Triceratia to Biddulphia. Some are referred to Ditylum, another to Eucampia, others to Eupodiscus, and some to Liradiscus and Stictodiscus.

Captain Lang says he has never seen Amphitetras or Triceratium in zigzag chains, and fancies they do not occur in that state. It is the usual state of Amphitetras, and two species of Triceratium have been found in that condition, *viz. Triceratium arcticum*, described and figured by Mr. Roper in 'Trans. Mic. Soc.,' vol. viii., p. 55, from Vancouver's Island, and *T. striolatum* = *Biddulphia*, Heiberg, 'Dansk Diatomeer,' page 41, pl. 2, fig. 16. I have never seen *Biddulphia reticulata* with spines like *Triceratium armatum*. *B. turgida* bears a greater resemblance to the latter form.

The genus Campylodiscus always appears to me to be the best marked of any of the genera of Diatomaceæ; all the species I have seen (and I possess or have examined nearly all those figured and described, besides many which are possibly new species), I find the circular outline of the valve, its double flexure, and median spaces of the opposite valves of the frustule at right angles to each other, constant characteristics. I have noticed the twisted form of *Surirella striatula* in the Salt Lake gathering, but it differs from the flexures in Campylodiscus; the latter has two bends at right angles to each other, and also in opposite directions. In Surirella the valve is not bent, but sometimes it has a twist in a spiral direction, most conspicuous in *Surirella spiralis*, Kützing = *Campylodiscus spiralis* of the Synopsis.

Guano Diatoms, &c.

Many of the forms found in guanos were at one time considered to be extinct species, like the majority of those in the "fossil earths" from Barbadoes, Virginia, Maryland, &c. The beautiful *Aulacodiscus formosus* was thought to be peculiar to the guano known as Upper Peruvian or Bolivian. *A. margaritaceus* was found rarely in the Chincha guano, but more plentifully in that known as Californian guano. *A. scaber* and *A. Comberi* occurred only in the Chincha guano. I always had an impression that these forms, like many others at one time supposed to be extinct, would one day be found living in the harbour near the localities from which the guanos are obtained, and perhaps other localities. A similar idea occurred to my friend Captain J. A. Perry, of Liverpool, who took the first opportunity of proving the truth of the surmise. In a letter just received, he says, "When I went away my last voyage I made up my mind to find out if there was any similarity between the forms found in the Guanape, Chincha, and Peruvian guanos, the Mexillones deposit, and the recent forms to be found in the various harbours; so I made gatherings in each of the ports we called at, and to the astonishment of all of us here at Liverpool, I have got in great abundance recent forms of those found sparingly in the fossil material, such as *Aulacodiscus formosus*, *A. margaritaceus*, *A. crux*, and *A. Comberi*, *Omphalopelta versicolor*, &c., &c., which you will see much better than I can attempt to explain to you." The recent forms are very fine, particularly *O. versicolor*. This to my knowledge had only been found in two localities, and in both cases in a fossil state, *viz.* "Monterey earth" (not "stone"), and described by Mr. Brightwell under the name of

Actinocyclus spinosus, in the 'Quarterly Journal of Microscopical Science,' vol. viii., p. 93, and the Mexillones guano. *Aulacodiscus crux* of the Virginian deposit I have not seen in Captain Perry's gatherings; but *A. scaber* (which Ehrenberg also called *A. crux*) does occur in these gatherings. Diatomists may perhaps be interested in knowing that a Diatomaceous deposit (? sub-peat) has been discovered in Talbot, Victoria, Australia, and my correspondent (Mr. F. Barnard, of Kew, Victoria, to whom I am indebted for a sample of it) writes me that the deposit is "twelve feet deep, and covers acres." The prevailing form is *Synedra amphirynchus*, Kützing. A few small Navicula and *Cocconeis pediculus* occur in it, but at least 90 per cent. consists of the Synedra.

VI.—*Hair in its Microscopical and Medico-Legal Aspects.*

By Dr. E. Hofmann.

The examination of the hair in its medico-legal relations is a subject hitherto but little noticed, except superficially in the "Year-book of Legal Medicine." Yet many cases might be mentioned in which the microscopic examination of the hair was of great importance.

In the medico-legal examination of hair, two questions are met:

1. Are the hairs from animals or from men?

2. In the latter case from whom do they come? From what portion of the body?

Of course, if the hairs belong to a beast, that may be sufficient to settle the question at issue; but the difference between such and human hair has been too little noticed. A human hair under the microscope shows three distinct layers: the outer, cuticula, or the superficial covering, formed of epithelial cells, with rounded contour, lying over each other like tiles, which clothes the surface of the hair from its exit from the skin to its end. The ends of the scale stand out somewhat from the shaft, and give the outer circumference of the hair a more or less jagged appearance. Seen sideways, the cuticula appears as an undulatory design, more prominent if the hair is treated for a short time with concentrated acid. The scales have their points directed toward the free end of the hair; hence the latter can be easily distinguished from the other broken end.

The cortical substance forms the principal part, and often the whole of the shaft. It consists of a system of closely-packed cells in rows lying nearly parallel to the long axis of the hair, giving the cortical substance an appearance as if striped lengthwise. These cells are so intimately united that without reagents this striped appearance alone shows the cellular structure. Concentrated sulphuric acid breaks up this union, and reveals the spindle-shaped cells, with occasionally a nucleus.

The cortical substance has different colour, according to the colour of the hair; generally the colour is diffused through its whole mass; less frequently the colour depends on granular pigment scattered through its substance in small masses.

Finally, the cortical substance contains a number of cavities filled with air, most evident in the hair from aged persons or in dry hair. These are secondary results of drying, as they are not found in young hair.

The central portion, the medullary substance, forms, when well developed, an axis-cylinder, one-fifth or one-fourth the diameter of the hair, with sharp outlines, generally central, but many times a little eccentric in position. The medullary substance is not constant; it is often wanting in human hair, especially in blond hair. It is wanting less frequently in hair obtained from other parts of the body than in that from the head. In woolly hair it is always wanting; also in the hair of the new-born child. The medullary substance is often interrupted, and sometimes consists only of a few dark points lying in the axis of the hair.

The nature of the medullary substance is still a matter of dispute, some considering it cellular, others denying this. The first is certainly the correct view, as may be seen by following the development of the medullary substance from the papilla, where round and imperfectly-polygonal cells can be seen gradually merging into the medullary substance.

The medullary substance has been thought to contain the pigment; this is not so, the supposed pigment-granules being very minute air-bubbles. The cause of the colour of the hair is found in the diffuse pigmentation of the cortical substance. The cause for the hair becoming grey or white is to be found in the disappearance of the diffuse pigmentation of the cortical substance, the cause of which is not yet known. The medullary substance can be more easily seen in white hair than in coloured.

Turning now to the hair of animals, we find generally the same three layers as in human hair, but differing to such a degree that, as a rule, a hair can be easily recognized as belonging to an animal. The cuticula in most animals has absolutely and relatively larger cells, which give the hair a characteristic appearance, as is seen especially well in the wool from sheep. A toothed or saw-like appearance of the contour of certain animal hairs depends upon the larger development and peculiar relations of the cuticular cells, whose points stand out so far from the hair that the latter has a feathered appearance, as in the field-mouse. Among animals the greater bulk of the hair is formed by the medullary substance, the cortical substance being only a thin layer; often, indeed, is reduced to a hem-like streak. This predominance of the medullary substance is seen best in the shaft of the hair; toward the end the cortical substance predominates, the medullary becoming thinner. Generally, the cortical substance has the same structure as in human hair, and the same variety of pigmentation; in some animals, as the cat, rat, and mouse, the cortical substance is more translucent and of finer structure, resembling, under the microscope, a hyaline envelope of the medullary substance.

The medullary substance in animals is an interesting study, differing greatly from the same layer in human hair. The cellular structure is generally very evident, without the employment of any reagent. The cells vary greatly in size and form.

Though the hair of animals usually is so different from human hair that it can be easily recognized, yet the difference is sometimes less marked; especially may this be the case with single hairs, and at times only a single hair can be had for examination. This resemblance is caused by the absence of the medullary substance. Dogs' hair, especially when brown, is often very similar to human hair, or may be almost exactly the same; fortunately, only separate hairs are thus similar, while generally the remaining hairs which are given for examination have clearly the animal type. Reagents will often help to decide the question.

In medico-legal cases, when it has been decided that the hair examined is human hair, the question arises, from whom it comes and from what portion of the body. In regard to the first question it may be merely said here that the hair examined must be compared with that of the person concerned, both in regard to its gross appearances and microscopically.

In deciding to what part of the body the hairs belong, the length, the size, the form, and the root of the hair, must be noticed.

The hair from the head and beard is less limited in its length than the hair on other portions of the body; though individual and other circumstances may modify the length of the hair from the head and the beard.

The size of the hair differs in different parts of the body, and so may form a diagnostic mark. The beard is the thickest generally, measuring 0·14 to 0·15 mm. Next comes the hair about the female genitals, 0·15 mm.; then the eyebrows, 0·12 mm.; the hair about the male genitals, 0·11 mm.; finally, the hair from the head in either sex, 0·06 to 0·08 mm. The great individual differences which are found may render the value of the size for diagnosis less valuable. Moreover, it must not be forgotten that the same hair may vary in diameter. The shape of the hair modifies its diameter; thus cylindrical hair especially is found only on the head; but when this is curly it is flattened, and the transverse section is then oval instead of round. The beard is generally triangular on transverse section, with one convex side; the hair from the genitals is generally oval, sometimes triangular. Hair which has been exposed to the action of the sweat is sometimes swollen in one part, and so changed in form.

When the hair grows undisturbed it ends always in a fine point. All the hair of a new-born child, hair which grows at the age of puberty, and such as has grown naturally without interference, always has a pointed end, which may be of use in deciding in regard to the age of a person. Later this normal ending is not found. Hair which has been cut has at first a sharply-defined transverse section; later the edges are rounded off, and the end becomes round and diminished in size, or is frayed out. This may lead to an approximate calculation of the time which has elapsed since the hair was last cut. The beard, being less frequently cut, is more often split and frayed out. The hair from the female head, generally not cut, ends regularly in two to three points, often in more, each having the end frayed out.

The shape taken by the ends of the hair depends upon the action of friction and sweat, the former splitting and rubbing off the ends, the latter macerating and acting chemically by dissolving or softening the connective substance. The shaft of the hair is acted upon by the same agents and changed; especially active is the sweat, changing the colour, as is seen in the axilla, on the scrotum, and the labia.

From the form of the hair, especially of its end, we can draw conclusions as to the nature of the influence to which it has been exposed, and by means of this and its other peculiarities we may be able in medico-legal cases, with more or less certainty, to decide from what part of the body it came. But no form of hair is absolutely characteristic of any portion of the body.—Translated by S. G. WEBER in the *New York Medical Journal.*

PROGRESS OF MICROSCOPICAL SCIENCE.

The Distribution of Hæmoglobin in the Animal Kingdom.—One of the, if not the very best papers which have been published on this subject is that by Mr. E. Ray Lankester, B.A., which appears in a late number of the 'Proceedings of the Royal Society.' * It is illustrated by a most carefully drawn plate, which, however, we cannot reproduce. The following, which is a considerable portion of this the most exhaustive essay that has been published on the subject, is most of what this observer has stated:—

"The facts ascertained as to the distribution of Hæmoglobin may now be summarized as follows:—

"1. In special corpuscles.

a. In the blood of all vertebrates, excepting *Leptocephalus* and *Amphioxus* (?).

b. In the perivisceral fluid of some species of the Vermian genera *Glycera*, *Capitella*, *Phoronis*.

c. In the blood of the Lamellibranchiate Mollusk *Solen legumen*.

"2. Diffused in a vascular or ambient liquid.

a. In the peculiar vascular system of the Chætopodous Annelids very generally, but with apparently arbitrary exceptions.

b. In the vascular system (which represents a reduced perivisceral cavity) of certain leeches, but not of all. (*Nephelis*, *Hirudo*.)

c. In the vascular system of certain Turbellarians as an exception (*Polia*).

d. In a special vascular system (distinct from the general blood-system) of a marine parasitic Crustacean (undescribed) observed by Professor Edouard van Beneden.

e. In the general blood-system of the larva of the Dipterous insect *Cheironomus*.

f. In the general blood-system of the pulmonate mollusk *Planorbis*.

g. In the general blood-system of the Crustaceans *Daphnia* and *Cheirocephalus*.

"3. Diffused in the substance of muscular tissue.

a. In the voluntary muscles generally of Mammalia, and probably of birds, and in some muscles of reptiles.

b. In the muscles of the dorsal fin of the fish *Hippocampus*, being generally absent from the voluntary muscular tissue of fish.

c. In the muscular tissue of the heart of Vertebrata generally.

d. In the unstriped muscular tissue of the rectum of man, being absent from the unstriped muscular tissue of the alimentary canal generally.

e. In the muscles of the pharynx and odontophor of Gasteropodous Mollusks (observed in *Lymnæus*, *Paludina*, *Littorina*, *Patella*,

* Vol. xxi., No. 140.

Chiton, *Aplysia*), and of the pharyngeal gizzard of *Aplysia*, being entirely absent from the rest of the muscular and other tissues and the blood of these mollusks. See as to *Planorbis* above (2 *f*).

f. In the muscular tissue of the great pharyngeal tube of *Aphrodite aculeata*, being absent from the muscular tissue and from the blood in this animal, and absent from the muscular tissue generally in all other Annelids as far as yet examined.

"4. Diffused in the substance of nervous tissue.

a. In the chain of nerve-ganglia of *Aphrodite aculeata*.

"The significance of these observations depends to a large extent on the negative results given by very numerous observations not recorded here. I have taken every opportunity, during some years past, of examining coloured animal matters with the spectroscope, and especially where there could be a suspicion of the presence of Hæmoglobin.* Thus, where the absence of Hæmoglobin is generally stated above, it must be understood that examination has been made for it in such cases as have been accessible. I have found that many cases of red coloration of a tissue or liquid, which might be supposed to be due to Hæmoglobin, are certainly not so, such red-coloured matter failing to give the characteristic bands of that body, and, as a rule, giving no detached characteristic bands. Such are the red pigments occurring in the blood corpuscles of *Sipunculus*, in the tissues of many Annelids, in Echinodermata, in compound Tunicata, surrounding the intestine of *Salpa*, in the foot and mantle of many Mollusca, also in their nerve-ganglia and other parts, in the chromatophores of Cephalopoda, in certain Infusoria. On the other hand, among coloured bodies not suggesting Hæmoglobin, I have found an equally large number devoid of characteristic spectra, but some few which exhibit the remarkable phenomenon of detached definite bands of absorption, which enables them to be certainly characterized and recorded. Such are :—a chlorophyl-like body occurring in *Spongilla*, in *Hydra viridis*, and in *Mesostomum viride ;* Chlorocruorin, which takes the place of Hæmoglobin in the vascular fluid of the Chlorémiens and some species of *Sabella ;* Stentorin, giving the intense blue colour to the Infusorian *Stentor cœruleus*, and possessing a very marked and peculiar pair of absorption bands. With one single exception, it appears, from the examination of a great number of cases, both among Vertebrates and Invertebrates, that coloured bodies which may be supposed to be purely pigmentary in their function do not give detached absorption bands. The exception is the red colouring-matter named Turacin by Professor Church, discovered by him in the feathers of birds of the family Musophagidæ, which has other properties quite unusual in pigmentary bodies. In an examination of a large number of birds' feathers, red, yellow, blue, and green, I failed to obtain detached

* I may state that I have not hitherto made any observations on the colouring matters of the biliary secretion in invertebrata and the lower vertebrates, excepting in their fresh condition. The use of the spectroscope, combined with chemical reagents, would no doubt lead to interesting results in that field, since a variety of substances giving characteristic absorption-spectra have been obtained from the manipulation of mammalian bile-pigment.

absorption bands, as also in the scales of fish and in the skin and hair of mammals, and in the pigments of many Crustaceans, Annelids, Insects, Tunicates, and Sponges.*

"From a consideration of the facts stated above with regard to the mode of occurrence and distribution of Hæmoglobin in animal organisms, the following general statements may be made, which are in accordance with the now thorough establishment, by chemical investigation, of its peculiar oxygen-carrying property.

"Hæmoglobin is irregularly distributed throughout the animal kingdom, being absent entirely only in the lowest groups.† It may be present in all the representatives of a large group, with but one or two exceptions, or it may be present in one only out of the numerous members of such a group; or, again, it may be present in one and absent in another species of the same genus. It may occur in corpuscles in the blood, or diffused in the liquor sanguinis, or in the muscular tissue, or in the nerve-tissue. The same apparent capriciousness characterizes its occurrence in tissues as in specific forms. It may be present in one small group of muscles and absent from all the rest of the tissues of the body, or it may occur in one part only of a tissue, histologically identical throughout its distribution in the organism. The apparently arbitrary character of this distribution is to be explained (though only partially) by a reference to the chemical activity of Hæmoglobin. Wherever increased facilities for oxidation are requisite, Hæmoglobin may make its appearance in response; where such facilities can be dispensed with, or are otherwise supplied, Hæmoglobin may cease to be developed.

"The Vertebrata and the Annelida possess a blood containing Hæmoglobin in correlation with their greater activity as contrasted with the Mollusca, which do not possess such blood. The actively burrowing *Solen legumen* alone amongst Lamellibranchiate Mollusks, and amongst Gasteropods only *Planorbis*, respiring the air of stagnant marshes, possess blood containing Hæmoglobin. In the former the activity, in the latter the deficiency of respirable gases are correlated with the exceptional development of Hæmoglobin. But we cannot as yet offer an explanation of the absence of Hæmoglobin from the closely-allied species of *Solen*, and from the *Lymnæi* which accompany *Planorbis*. The Crustaceans *Cheirocephalus* and *Daphnia*, and the larva of *Cheironomus*, possessing, as exceptions in their classes, Hæmoglobin in their blood, inhabit stations where the amount of accessible oxygen must be small (that is to say, stagnant ponds), the last living in putrescent mud; whilst the possession of abundant Hæmoglobin in its vascular fluid may be supposed to be one of the chief properties which enables the oligochæt Annelid *Tubifex* to hold its ground in the foul, and therefore much deoxygenated, water of the Thames at London.

* See 'Journal of Anatomy and Physiology,' 1869–70, p. 119.

† [*Note.* Dec. 24th, 1872.]—It is perhaps of some significance that Hæmoglobin has only been found in that great group of the animal kingdom which in the course of its development gives rise to a middle layer of blastodermic cells or mesoderm, and in examples from nearly every great branch of this stem.

"The known chemical properties of Hæmoglobin furnish a more complete explanation of its peculiar distribution in tissues. That it should occur in a circulating fluid, which is the medium of respiration, is obviously related to those properties. Its occurrence in the voluntary muscles of the most active of Vertebrata, and in the most active muscles of some others (as in the case of the dorsal-fin muscles of *Hippocampus*), is equally so; so also its occurrence in the most powerfully acting part of the intestinal muscles, those of the rectum, and in the only rapidly and constantly acting muscles of the Gasteropods, namely, those used in biting and rasping.

"To connect its occurrence in the nervous chain of *Aphrodite aculeata* with its properties is more difficult, since we have no knowledge that this Annelid is remarkable for nervous energy. The large bulk of the animal in proportion to the size of the nervous system, and the deficient respiration, indicated by the very slightly developed vascular system and the total absence of Hæmoglobin from the fluids of the worm, may be a reason for the endowment of the nervous centre which has to control such a large and complicated organism with a special facility for appropriating what little oxygen may come in its way.

"The complete absence of Hæmoglobin from *Leptocephalus* is an example of the submission of an auxiliary, but not an essential, structural attribute to an all-powerful necessity—that of transparency. The absence of Hæmoglobin from the transparent Annelid *Alciope* may be similarly correlated.

"From what has been stated above as to the Hæmoglobin-bearing corpuscles of *Glycera*, *Solen*, and the Vertebrata, it appears that when Hæmoglobin is present in the blood in *corpuscles*, these corpuscles are of a peculiar character, and are specially related to the presence of the Hæmoglobin. When that is absent, other things remaining the same (as with the blood of *Solen ensis* and the perivisceral fluid of most Annelids), the peculiar corpuscles are absent. Such things as colourless corpuscles, representative of the Hæmoglobin-bearing corpuscles, do, however, appear to exist in the case of the fish *Leptocephalus*. In connection with the relation of the colourless corpuscles of vertebrate blood to the red corpuscles, and of the corpuscles of the vascular fluids of Invertebrata to one another and to those of Vertebrates, these facts seem to be important: the colourless corpuscles in one case are only comparable to the colourless in another; the red corpuscles are something apart, which may or may not be superadded.*

"The corpuscles of the perivisceral fluid of the Gephyrean *Sipunculus nudus*, which is abundant in the Gulf of Naples, present some facts which are interesting in relation to the occurrence of Hæmoglobin; and I may therefore draw attention to them before concluding this paper. The fluid which is contained in the perivisceral cavity of this worm is, as is well known, of a pale madder-red colour. It contains a remarkable abundance and variety of corpuscles, the most

* [*Note.* Dec. 24th, 1872.]—The two kinds of corpuscles may be definitely distinguished from one another as *leucocytes* and *pneumocytes*.

numerous of which are thick circular disks, varying in diameter from $\frac{1}{3500}$ to $\frac{1}{2000}$ of an inch; and in these, and these only, the pink colour resides (Fig. 7). These pink corpuscles consist of a clear homogeneous substance, of high refringent power, in which are scattered three or four bright granules and a small nucleus, which is rendered obvious by the action of acetic acid. Rosaniline stains this nucleus, but does not usually give any other maculæ, such as are to be observed when it is added to Hæmoglobin-containing corpuscles.* Dr. Alexander Brandt, in a recent memoir, very rightly insists on the similarity between these pink corpuscles of *Sipunculus* and the red corpuscles of the blood of Vertebrata: they are something quite distinct from the amœboid corpuscles found in the fluid corresponding to blood in nearly all Invertebrata, and are to be compared to the red corpuscles of *Glycera*, *Solen*, and Vertebrates. The amœboid corpuscles are otherwise represented in the perivisceral fluid of *Sipunculus* by numerous active amœboid cells. Dr. Brandt, naturally enough, regarded the pink colour of these corpuscles as favouring their assimilation to vertebrate red corpuscles. The colour *en masse* is, however, obviously different from that of dilute Hæmoglobin; and I was not therefore surprised to find that it did not give the absorption-spectrum of that body. This pink colouring-matter is soluble in water. When a little fresh water is added to some of the perivisceral fluid in a tube, it takes up all the colour, whilst the corpuscles sink in a colourless condition to the bottom. No detached bands of absorption of any kind were given by the colouring-matter thus obtained; a slight acidulation with acetic acid was sufficient to destroy the colour. Ammonia had the same action, also ether and alcohol.

"Though this pink substance is thus devoid of the spectral properties which characterize Hæmoglobin and Chlorocruorin, it does not seem improbable that it is a body analogous to them in other properties, since the corpuscles in which it resides can only be compared to the respiratory or oxygen-carrying corpuscles occurring in the blood of Vertebrates and the four Invertebrates noticed in this paper. Moreover, this pink colouring-matter occurs in other parts of the organism of *Sipunculus*, namely, diffused in the substance of a remarkable tissue which runs along the wall of the intestine, forming a red streak, which has sometimes been taken for a blood-vessel, and also in the peculiar cellular tissue which surrounds the true nerve-tissue of the nerve-chord.

"The occurrence of colourless corpuscles in *Leptocephalus* identical in form and character with the Hæmoglobin-bearing corpuscles of the blood of other fish, and the apparently capricious distribution of Hæmoglobin among Invertebrata, together with the existence of the green oxygen-carrier Chlorocruorin and the pink colouring-matter of the corpuscles of *Sipunculus nudus*, suggest the hypothesis of the existence of various bodies not necessarily red, possibly colourless, which act the same physiological part in relation to oxygen as does Hæmoglobin."

* On one occasion out of many I obtained an appearance of the kind; and hence further observation on this point is necessary.

The Development of Cancer.—In the 'Medical Record' (Feb. 12) Dr. C. Creighton gives a valuable account of Dr. Carmalt's recent researches on this point, which are published in Virchow's 'Archiv.' (vol. 58.) The writer records the results of the examination of three carcinomatous tumours, removed from the skin of the nose, the cheek, and the eyelid. Thiersch, in his work on cancer, has pointed out that the epithelial cells of the sebaceous and sweat glands, and especially the cells of the *rete Malpighii*, are often the point of departure for cancer of the skin, and he casually includes the epithelium and the hair-follicles in the same category. In the hair-follicles Dr. Carmalt found not only an increase of the outer layer of epithelium, but also offshoots from the follicles, diverticula lined with epithelium, penetrating the connective tissue to various depths and in various directions. A section made either obliquely or parallel to the axis of the follicle, and passing through the diverticula, gave exactly the appearance of the ordinary cancer-alveoli, filled with epithelial cells. In certain preparations, it was possible to see the alveolar groupings of the cells pass into long processes lined with epithelium, which, again, opened into the hair-follicle; so that the appearance was that of a group of acinous glands with their excretory duct. Other sections presented a still more complete picture, *viz.* the enlarged follicles and their offshoots, the alveolar groups of epithelial cells, evidently in connection with the follicular offshoots, and lastly, isolated epithelial alveoli, situated more deeply in the tissues, and showing the ordinary characters of cancer-alveoli. Carmalt thinks it is hardly to be doubted that these isolated cancer-alveoli were also originally in continuity with the hair-follicles and their diverticula. The sebaceous glands were found unchanged, or hyperplastic, or quite undistinguishable, according to the degree of invasion of the cancerous growth. The connective tissue (stroma) surrounding the follicles and their abnormal offshoots was at some points so infiltrated with small round and spindle-shaped cells, that the cancer-alveoli could not be distinguished; at other points, the stroma consisted of a delicate network. In showing that the epithelium of the hair-follicles form a point of departure for the cancerous growth, Carmalt thinks that some light has been thrown on the cause of cancer of the skin. Referring to the statement of Führer, that frequent and rough shaving is apt to produce cancer of the skin of the face, he points out that, out of fifty or sixty cases of cancer of the lip and cheek that have occurred within a recent period in the Breslau Pathological Institute, only two were in women, and not a single case occurred in men with unshaved beards. With reference to the general question of the histological origin of cancer, his conclusions are so far in support of the opinion of Waldeyer and others, that every cancerous growth originates in the epithelial elements of the part, and are in opposition to the opinion of Virchow, that the cancer-cells are the equivalents of connective tissue corpuscles. In cancer of the œsophagus, Carmalt found an analogous condition. Sections showed processes of the deeper epithelial layer (corresponding to the *rete Malpighii* of the skin), penetrating the subjacent tissue. These processes were in connection with the cancer-alveoli, the cells of which had everywhere an epithelial character, and could be readily distinguished from the growth of connective tissue corpuscles near them. In one case the excretory ducts of the mucous glands were implicated, being dilated three or four times beyond the normal width, and covered with six or eight layers of pavement-epithelium. They presented also sacculated enlargements, of the same appearance as the neighbouring cancer-alveoli. In another investigation, Carmalt determined that cancer-cells possessed the property of spontaneous or amœboid movement. Minute particles were removed with a warmed knife from a newly excised tumour, and mounted in serum derived from the blood of the incised wound. The specimen was examined on a Stricker's warm stage, at a temperature of 104–107° Fahr. In the case of cells from two cases of cancer of the breast, and one case of sarcoma of the axilla, the amœboid movements were observed side by side with, and distinguishable from, the movements of white blood corpuscles. The tumour elements comported themselves like amœboid cells, assuming various forms and shooting out short processes. The amœboid movements of the tumour cells were found to be more sluggish than those of the colourless blood corpuscles.

Ehrenberg's Foraminifera.—Messrs. Parker, F.R.S., and Rupert Jones, F.R.S., give, in a recent number of the 'Annals of Natural History,' an interesting account of Herr Ehrenberg's researches on the Foraminifera. They say, we feel certain that the better Ehrenberg's work is understood, his beautiful and lasting illustrations, and his painstaking synoptical registers, will largely advance the progress of biology in its relation to both the present and the past. In removing some obscurity from the highly valuable groups of Foraminifera of which he has treated, we feel the pleasure of being of use to naturalists and geologists, enabling them to put several extensive faunæ and local groups into close critical relation with each other, and with such as have been observed by others. Further, we are sure that Ehrenberg himself, thinking over the improved biological systems of later naturalists, and open to conviction on good arguments, would freshly recognize the force of his own words respecting the importance of rhizopodal studies and their slowly progressive nature; and be pleased to find, also, his own researches not only serving as a broad basis for the study in general and as steps to higher knowledge, but still more freely trodden in the upward ascent, when made somewhat clearer and firmer for the student.

Häckel's work on Calcareous Sponges is admirably reviewed by P. S. in 'Nature' (Feb. 13). After giving some of the principal names of those whose works the author has less or more made use of, the writer of the notice goes on to say, that the first chapter gives an appreciative account of the admirable labours of Professor Grant, and of the subsequent contributions to the subject by Johnston, Bowerbank, Lieberkühn, Carter, Oscar Schmidt, and Kölliker. The defects of Mr. Bowerbank's "Monograph of British Sponges" are clearly pointed out, but its great merits receive equally cordial recognition, while the criticism passed on Dr. Gray's "Classification" is as just as it is severe. After a description of the methods of examination, the author proceeds to give a detailed account of the anatomy and natural history of the calcareous sponges, and this occupies the greater part of the first volume. The second is devoted to a detailed description of the whole group in systematic order, with diagnosis of species and ample synonomy. The plates in the third volume, drawn by Professor Häckel with the camera lucida, are admirably exact, though artistic effect is sometimes sacrificed to a somewhat diagrammatic clearness. They remind one of the excellent illustrations of Bronn's "Thierreich." The class of sponges is divided into *Fibrospongiæ*, including most of Grant's and Bowerbank's silicious and ceratose genera, *Myxospongiæ*, represented by *Halisarca* and *Calcispongiæ vel Grantiæ*. This third class contains three families, Ascones (Leucosolenia Bowerbank), Leucones (Leuconia Bowerbank), and Sycones (Grantia Bowerbank), represented by *Ascetta*, *Leucetta* and *Sycetta* respectively. The genera are chiefly characterized by their spicula. The author agrees with Oscar Schmidt in deducing all known sponges from a single primitive form (Archispongia, Protospongia), which he supposed to have resembled *Halisarca* more than any other existing genus. He regards the class as very distinct from the Protozoa, and most nearly related to the Cœlenterata, a view with which English readers are familiar from Mr. E. R. Lankester's interesting paper on Zoological Affinities of Sponges in the 'Annals and Magazine of Natural History' (vol. vi., 1870). Indeed it was the position taken by Leuckart himself in 1854, seven years after the sub-kingdom of Cœlenterata had been established by himself and Frey. If we admit that each sponge-pyramid is not a colony of Protozoa, but a multicellular organism, its likeness to a polyp is very striking: the chief differences are the absence of tentacles and of thread-cells. The latter structures, however, have, we believe, been detected in some Mediterranean sponges since the publication of Professor Häckel's work. Comparing the "Stammbaum" given at the end of the first volume with that in the third edition of the 'Schöpfungsgeschichte' (1872), published five months earlier, we find that the author now makes all sponges descend through "Archispongia," and "Protascus" from an equally hypothetical "Gastræa," while the Cœlenterata diverge from Protascus as Archydra. This makes the affinity less close between Myxospongiæ on the one hand, and between Calcispongiæ and Coralligena on the other. The modification brings the Stammbaum nearer to the classifications actually used by other zoologists, and is so far an advantage. With regard to nomenclature, Professor Häckel defends the proposal which he made in 1869 to revive the old name of Zoophyta (used by our countryman, Wotton, in 1552) in order to include sponges (or Porifera) and Cœlenterata (or, as he prefers to call them, Acalephæ). Admitting the justice of the classification, there seems no sufficient justification for the change of names. 1. Priority belongs to the name given by those who first establish true affinities, and not to vague and fanciful names given two hundred years before Linnæus. 2. To say "Zoophyta" is no worse a name to revive than "Vermes" is sufficiently to condemn it. 3. Whether the cavity in a sea-anemone is all stomach or partly perivisceral may admit of dispute, but "Cœlenterata" only affirms that the animal is hollow; and if the term suggests either interpretation, it rather lends itself to Professor Häckel's. 4. If another word must be invented to apply to Anthozoa (or "Coralla") and Hydrozoa (or "Hydromedusæ") in common, Huxley's "Nematophora," suggested in 1851, is just as good as "Acalephæ," which was used in a more restricted sense by Cuvier. But it is not impossible that before long neither term will be properly exclusive of sponges.

The Reproduction of the Saprolegniæ.—A very able paper on this subject appears in 'Hofmeister's Handbook' (vol ii., cap. v.), from the pen of Dr. Anton de Bary. This is translated very fully by Mr. M. C. Cooke, M.A., in the February number of his 'Grevillea.' It says that the existence of a sexual generation in a certain number of Fungi has latterly been demonstrated. "The Mucorini offer an example of a copulation which, in my idea, and that of M. Hofmeister, is a particular form of this mode of generation; and, since Micheli and Bulliard a multitude of Fungi are, at any rate, supposed to possess sexes, flowers, anthers, &c. We will first quote the Saprolegniæ, the sexual organs and the fecundation of which were first discovered by M. Pringsheim, and described by him. In the types which may be imagined to be monæcious, such as the *Saprolegnia monoica*, the *Pythium* and our *Aphanomyces*, the female organs consist of oogonia, that is to say, of cells which are at first globose, and rich in plastic matters, which most generally terminate short branches of the mycelium, and which are but rarely seen in an interstitial position. The constitutive membrane of the adult oogonium in *Saprolegnia monoica* is re-absorbed in a great number of points, and is there pierced with rounded holes. At the same time the plasma is divided into a larger or smaller number of distinct portions which are rounded into little spheres, and separate from the walls of the conceptacle, in order to group themselves in its centre, where they float in an aqueous liquid. These gonospheres are then smooth and bare; on their surface there exists no membrane of the nature of cellulose. In the genera *Pythium* and *Aphanomyces*, and in some of the *Saprolegniæ* all the plasma of the oogonia is condensed into one solitary central sphere, surrounded by liquid. During the formation of the oogonium, there arise from its pedicel, or from neighbouring filaments, slight, cylindrical, curved branches, sometimes twisted around the support of the oogonium, and which all tend towards this organ. Their superior extremity is intimately applied to its wall, then ceases to be elongated, becomes slightly inflated, and is limited below by a septum; it is then an oblong cell, slightly curved, filled with protoplasm, and intimately applied to the oogonium; in one word, an *antheridium*, or the organ of the male sex. Each oogonium possesses one or several antheridia. Towards the time when the gonospheres are formed, it may be remarked that each antheridium sends to the interior of the oogonium one or several tubular processes which have crossed its side wall, and which open at their extremity in order to discharge their contents. These, while they are flowing out, exhibit some very agile corpuscles, the diameter of which is barely equal to ·002 mm., and which, considering their resemblance to what are termed 'spermatozoids' in the *Vaucheriæ*, ought to be regarded as the fecundating corpuscles. After the evacuation of the antheridia, the gonospheres are found to be covered with cellulose; they then constitute so many oospores, with solid walls, if I may use an expression specially applied to the algæ by M. Pringsheim. Phenomena which are analogous in several respects and have been studied in the *Vaucheriæ* and other confervæ, as also direct observations which are due to M. Pringsheim, do not permit of any doubt but that the cellulosic membrane, which appears on the surface of the gonospheres, is only the consequence of sexual fecundation, and that this ought not to be attributed to the corpuscles which issue from the antheridea, which would penetrate into the gonospheres, and unite with their substance." It then goes on to deal fully with the subject for two or more pages, but we have not space to proceed farther.

Pathological Appearance of the Jaw after Resection of the Maxillary Bone.—Dr. Goodwillie, of New York, recently read a paper partly on the above subject before the Medical Society of his county. He says that on making a section of the tumour through the longitudinal direction of the teeth, there was to be seen the following: at the apex of the second molar tooth there was a small, soft cyst, containing some pus, and for a short distance surrounding this the bone appeared quite cancellated, but the rest of the tumour was quite dense in structure. The pulps of the canine and first bicuspid had still some vitality, but that of the second bicuspid was dead. The pulp-chambers were decreased in size by a deposit of osteo-dentine to their walls, slight hypertrophy of the cementum on the fangs. A large nerve entered the tumour on its buccal side. The microscopical examination of this tumour, as made by Dr. J. W. S. Arnold, shows that it is "composed of cancellated tissue almost entirely. The outer edge of a thin layer of more compact bony tissue. In the spongy part a small amount of soft marrow, containing the usual constituents of fœtal marrow, *i. e.* medulla-cells, and myelo-plaxes with oil-globules."

The Union of Divided Tendons.—A paper with the title of "The Minute Processes in the Union by the first intention of Divided Tendons," by Herr Paul Gieterbock, of Berlin, is well abstracted in a late number of the 'Lancet.' It appears that the observer's work was most of it done at the well-known Brown-Institution of London. The experiments were made on the Achilles tendons of nearly full-grown white rats. The tendons were never *entirely*, but only *partially* divided, the object being to avoid much separation of the cut surfaces, and any considerable effusion of blood, which necessarily occur when a stretched tendon is completely divided. The operation was done with a sharp-pointed tenotome, the tendons being removed under chloroform, together with the lower portion of the muscles of the calf. The rats, the author states, recover from this procedure quickly, and without any marked lameness resulting, provided the posterior tibial artery be not wounded. The tendons, after being pinned out on pieces of cork, to prevent shrinking, and having been treated with chloride of gold solution ($\frac{1}{2}$ per cent.) and spirit, gave the following results:—Half an hour after the operation blood is seen to be extravasated between the cut surfaces, and for a short distance under the sheath of the tendon. The extravasation does not usually extend deeply into the funnel-shaped wound, but the latter is filled up more or less completely by a process of the sheath, and of the connective tissue outside the sheath. Twenty-four hours after the operation no change is visible in the tendon-cells, particularly none that in any way resembles inflammation. *As a rule, the wound is almost entirely filled with the tissue of the sheath*, so that any "plastic exudation" that may occur must be very small in quantity. It is only exceptionally that the latter is present in considerable quantity, in which case there is only a small process of the sheath in the wound. In preparations treated with chloride of gold, the exudation appears as a yellow, finely-granular mass, in which a few young cells are scattered. The sheath itself is only slightly inflamed over the place of the incision. If the tendon be examined later on, little change is seen to have occurred in the elementary parts, and none in the tendon-cells. The margins of the wound can always be recognized, and a distinct connection between the contents of the wound and the sheath can be demonstrated. It can also be shown that the tissue occupying the wound is prolonged at the edges of the wound between the separate bundles of fibrillæ. By means of this prolongation of the tissue of the wound into the interstices of the tendon-cells, the edges of the wound become, in forty-eight hours, so strong that considerable force is required to make them give way; and after seventy-two hours or more their separation becomes as difficult as the rupture of the tendon itself. The tissue filling the wound then gradually comes to occupy less room, its cells becoming comparatively more approximated; and in a week from the operation the previously finely granular intercellular substance begins to assume a fibrillated structure. In three or four weeks' time, when the funnel-shaped wound has become linear, nothing definite can be made of the intercellular substance even with high powers. The author cannot ascertain, from direct observation, what becomes of the majority of the elements of the tissue occupying the wound, but is of opinion that, in deciding this point, regard must be had to the amount of original inflammatory swelling of the sheath. In conclusion, he says, "whilst on the one hand a true union by the first intention, in its histological sense, of tendon does not occur, on the other hand I have found suppuration very rare after incomplete Achillitomy in rats, and where it does happen, the tendon-tissue takes scarcely any part in it."

Insect Muscles: their Structure.—The following are stated to be the conclusions formed by Herr Grunmach: 1. That the structural element of the transversely striated muscular fibre of insects is the "columna muscularis" of Kölliker (muskel-säulchen). 2. That the columna muscularis is composed of a clear lustrous matrix, in which, at definite distances from each other, lie dull prismatic bodies, the so-called sarcous elements, which are either all of equal breadth or are alternately broad and narrow. 3. That the columnæ are separated from each other by interfibrillar or intercolumnar substance, in which fat-molecules and other granular particles are suspended. 4. That a certain number of these columnæ form the primitive fasciculus (fibre) of muscle which is invested by sarcolemma. 5. The so-called yellow muscles of insects are to be included with the other transversely striated muscles.

NOTES AND MEMORANDA.

The Gull and Johnson Controversy.—Apropos of the late discussion relative to the structure of the kidney, as to whether the term "*arterio-capillary fibrosis*" is correct or not, the 'Lancet' has the following leading article, in the expression of which we entirely concur. It says:—We shall look with much anxiety to the composition of the committee, which the Society very properly decided to appoint, for the microscopic investigation of the true nature of those appearances in vessels to which Sir W. Gull and Dr. Sutton have given the distinctive name of "arterio-capillary fibrosis." Assuredly it ought, as far as possible, to consist of competent microscopic observers who have not in any way adopted a definite view on the subject. We think it is easy to see that, as regards mere histological fact, there is certainly something to be said for both sides. Both among the very beautiful preparation exhibited by Dr. Johnson and among those shown by his opponents there are many which appear to exhibit undoubted evidence of such a hypertrophy of the muscular coats of arteries as Dr. Johnson contends for; but, on the other hand, we should be greatly surprised if it were ultimately proved that there are no further changes of the arterial walls in granular renal disease. Certainly it is not thus that one feels inclined to explain several of the preparations which have been exhibited; nor can the hypothesis of glycerine-modification be received as a sufficient explanation of the discrepancy. Moreover, it is very widely felt, we believe, among the best clinicians of London, that the clinical history of degenerative disease, as observed both in hospitals and in private practice, gives a very large number of instances in which everything seems to point to various other portions of the systemic circulation, and *not* to the renal vessels, as the commencement of the series of degenerative diseases of which granular renal disease may form a part.

Microscopical Experiments on Insects' Compound-Eyes.—Dr. F. W. Griffin, of the Bristol School of Chemistry, sends us the following note which he has had inserted in the 'World of Science.' Any tolerable "mount" of a beetle's eye will show more or less of the results hereafter described, but to obtain the more striking effects it should be prepared with great care. From a shape more or less semi-globular, it has to be flattened out to a perfect plane, that all the images in the field should be in focus together, yet this must be accomplished without materially altering the form of the individual "lenses," which would impair the distinctness of the images produced by them. Judging from numerous specimens of his mounting which the writer has had occasion to examine at different times, he can recommend Mr. Barnett as an excellent preparer of these "eyes" (as well as of insects in general, grouped diatoms, wood-sections, &c.), and whose mounts may be relied on. To obtain all the effects which can be got from this beautiful object requires some patience, care, and skill. No instructions can supply the place of these qualities, but we will endeavour to make our hints as plain and practical as possible. We will first consider the mode of operating by daylight, which is the easiest to work with, and admits of the greatest variety of effects. That we may see the images at all, we must know where to look for them, which is on the very summit of the quasi-lenses, or possibly a little above. One general rule will suffice for finding them with any "power" or arrangement. First, focus down to the hexagonal framework, which is the lowest part; when this is distinct the raised corneules will be utterly lost. Then rack slowly back, the projecting rounded corneules will come into view as their reticulated "setting" fades away. Continue racking-up till they in turn disappear in a general haze, which will be near the point required. Now pass a steel pen or similar object between the mirror and stage with its broad side presented to both. With slight focussing, a perfect image of it, moving backward and forward, will be seen in each of the so-called "facets" of the eye. With the writer's own slide, viewed with a 1-inch objective and a large-field ocular (a "Kelner" gives the finest effect with all such "show-objects"), nearly 2000 separate images are seen at once well focussed. With $\frac{1}{2}$-inch or $\frac{1}{4}$-inch objectives the size and distinctness of the images are greatly increased, but their number is, of course, diminished in proportion. Still even the $\frac{1}{4}$-inch gives 150 ocelli in the field. Either the plane or concave mirror may be employed, but the latter is probably preferable. A capital and appropriate object with the higher powers is a dead house-fly, stuck on a pin passing through a piece of cork, which may be held by a stiff wire attached to any stand or support placed on the right-hand side of the mirror. The nearer the object is brought to the stage, the larger its image will appear, while approximation to the mirror diminishes the apparent size. Further, by suitably directing the mirror, we can make window-bars, sash-fastenings, blind-tassels, &c., appear in each ocellus, though small and somewhat indistinct. The size and perfection of definition of such images may be greatly increased (put in by composition) by employing with a $\frac{1}{4}$-inch a 1-inch objective as an achromatic condenser. By racking this up or down, the size or definition of the image may be regulated to a nicety. A blind-tassel may thus be magnified so as nearly to fill the whole area of the corneules, or be more than half an inch in diameter, while each "strand" will be distinctly seen, and even the mottling of a fly's wing will be plainly visible. If the tassel be set swinging, the effect will be amusing, and if it be drawn aside and an outstretched hand with the fingers in rapid movement be put in its place, the appearance of 150 frantic extremities in simultaneous motion will be found ludicrous in the extreme. A friend of ours substituted a jointed toy suspended from the sash-fastener by an india-rubber string, and he describes the effect when it was set going as comical exceedingly! Nay, more, perfect pictures may be obtained in each ocellus when the 1-inch condenser is used with a $\frac{1}{4}$-inch objective. A group of trees at some little distance from the window may be thus sharply depicted, their leaves and branches waving in the wind; and the writer has had a panoramic view of blue sky with white clouds, minute but clearly defined, flying across, all multiplied 150 times! Many persons, however, object that these figures are not really formed by the lenses of the eye itself, but that any image presented to it is merely repeated in it by refraction. When an objective is used beneath as an achromatic condenser, this doubt not only seems plausible, but it is impossible to disprove it directly. Still it is easy to show the optical completeness of the ocelli, and that their lens-like action is an absolute fact. To do this, set the microscope horizontally, and then raise the fore part of the stand so that the axis of the body is in a line with the upper part of a window five or six yards distant. The mirror being removed, there will then be absolutely nothing behind the "eye" but the slip of glass on which it is mounted, yet with a $\frac{1}{2}$-inch or $\frac{1}{4}$-inch, the steel pen will show clearly, and with the $\frac{1}{4}$-inch a picture of the window, with curtains, tassels, &c., and even objects outside, will be seen in each corneule, though small and rather indistinct. If a large profile face or figure is cut out of brown paper and stuck on a window-pane, it will show very well; or if a person stands on a chair against the light and raises the hands to the head, no one observing the effect could doubt for a moment that the "eye" continues to perform the same work which it formerly did during life. By *lamplight* our range is far more limited. The plane mirror gives only a small speck of light in each of the corneules, which with 1-inch appear like minute nodules of a dull red colour, much resembling some of the thick discoid diatoms (*Coscinodisceæ*, *Aulacodisceæ*, &c.) when viewed *dry*. The concave mirror produces a troublesome image of the flame of the lamp. On parallelizing the light by a bull's-eye, this image is got rid of, and the steel pen shows very well with $\frac{1}{2}$-inch or $\frac{1}{4}$-inch, but to obtain really good effects the 1-inch must be used as an achromatic condenser, with bull's-eye and concave mirror. Then the fly or similar objects (a small transparent photo-portrait has been suggested) will come out admirably, but a patient adjustment of all the appliances should be tried—of the lamp, bull's-eye, mirror, and condensing-objective, as well as exact focussing of the "power" employed. We read in the 'Monthly Microscopical Journal,' No. 42, that at a soirée of the Croydon Microscopical Club in November, 1871, Mr. Butler exhibited an eye of beetle magnified 400 times (probably by a $\frac{1}{4}$-inch and a highish ocular), showing in every facet the moving seconds'-hand of a watch reflected into it. We do not know the precise means by which this curious result can be satisfactorily accomplished. If direct sunlight is allowed to fall on the mirror when a low power (1-inch or $\frac{1}{2}$-inch) is employed on the "eye," beautiful and vividly-coloured geometrical patterns are seen in it, changing like a kaleidoscope with every alteration in focussing. The appearances are eminently beautiful, but of course entirely illusory. The foregoing observations were made with a monocular microscope, the writer not happening to have a binocular at the present time. He does not apprehend, however, that it would occasion any notable difference. In conclusion, some of your younger readers may desire to know the method of calculating the number of ocelli in any compound eye which they may happen to possess, or the number contained in the field with the higher powers. The arithmetical rule for finding the number of square inches in a circular plane is to multiply half the diameter in inches by half the circumference, the latter being three times the diameter, or, more precisely, as 22 to 7. Thus a disk 8 inches in diameter will contain 48 square inches, for $8 \times 3 = 24$, the half of which is 12. This multiplied by 4 (the semi-diameter) gives 48 square inches as the superficial area. Now, by counting the number of ocelli in a straight line across the field in the vertical and horizontal directions, and substituting the numbers so found for the inches in the above example, we shall gain the desired result. Thus, in the field given by the $\frac{1}{4}$-inch and ocular which the writer used, there were $14\frac{1}{3}$ of the ocelli in each direction, which, calculated out, gives 150 in all. With the whole eye, some allowance must be made if, instead of being truly circular, it is oval, or irregular in outline. Ours has an average of 51 "facets" each way in the plane part, besides others on the rounded edges, which gives nearly 2000 optically available.

CORRESPONDENCE.

"Spurious Appearances in Microscopical Research."

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—The test Podura, with some conditions of illumination, displays a variety of indications of false structure, arising from the peculiar form of the longitudinal ribbings. Ribs in such a direction, *viz.* from quill to point, are characteristic of all the lepidopterous scales, and the Podura forms no exception; in this they are undulating, and at the end of each "note" marking the rib drops, and again rises with an increasing expanse. With a good object-glass the continuity is unbroken, and readily seen and traced. I am not inclined to reopen the discussion at present, but take my stand for this positive assertion from having examined torn specimens with ribs partly isolated or displaced, so as to give a clear idea of their form; I have collected a number of these with the view, if requisite, of proving the structure by photographs of fragments.*

* Besides examples of longitudinal and cross fractures, I have a fine slide of test Podura mounted by Richard Beck, kindly presented to me by Dr. Gray. The cover has evidently shifted or spun round while pressing on a large scale, tearing the membrane in the middle of the specimen, and taking with it the fractured ribs at the spot. Some of the club markings are twisted transversely, one so far that the thick end lays the reverse way, as clearly and definitely as a half turn given to the handle of a copying-press.

Dr. Pigott seems not to have seen or recognized such fragments, and, perhaps, never can do so, as they must overturn his long-cherished bead theory, a fallacy in the Podura caused by broken refraction obtained by an illumination that obscures the ribs and substitutes a host of false beaded appearances in their stead; and it is thus attempted to be shown that the so-termed spines are spurious. In the *Seira Buskii* only three or four of these, with sharp cut outlines, occupy the whole length of the scale, more like the "Irishman's shillelah", that Dr. Pigott once tried to floor me with. Against his numerous articles on this one subject I have recently said nothing, as, whatever leisure he may find to pen them, I could not well spare the time to reply; for with noteworthy perseverance he has for years past written the same thing again and again on this scale, with every variety of paraphrase, to keep alive his darling idea. When all the permutations are at length exhausted, I may perhaps come forward with the evidence. At present I am indifferent, because out of all my numerous microscopical friends, I cannot call to mind one that will uphold his views in this particular. But after I called his optics in question, and controverted his statement concerning the admission through immersion lenses of larger apertures than those due to recognized theory, an advocate from over the Atlantic has taken up the argument in such a style, that Dr. Pigott might well exclaim, "Save me from my friends!"

The *Lepisma saccharina* was investigated by my esteemed friend, the late Richard Beck, many years ago, who showed that in this object *spurious markings* (something similar in appearance to those on Podura) were formed by the crossing of oblique striæ, but he never believed, in consequence, that the Podura spines arose from a similar cause. This oblique refractive phenomenon has been very palpably shown by Mr. Hennah's experiments with glass rods. Dr. Pigott seems to claim these investigations as his own. My name having been called in again, I enter an appearance, lest silence should be construed into acquiescence. I bide my time, till then, whether Dr. Pigott stands forward either prophet-like to warn us betimes, assuming a keenness of vision and discrimination far beyond his purblind fellow-mortals, or, as a more congenial character, in "discharging a duty as a retired physician in boldly denouncing" that which may involve a verdict of human life! I shall still trust to my own eyesight for my guidance.

Yours very truly,

F. H. Wenham.

Mr. Stodder's Remarks on Eupodiscus Argus.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—I am glad to find that my paper on *Eupodiscus Argus* has excited the attention of American observers, and beg to make a few remarks on Mr. Stodder's paper in the 'Lens.'

I have already explained that my remarks had no pretensions to be exhaustive of the structure of *E. Argus*, but I still think them correct, as far as they go. Mr. Stodder figures an under-layer (No. 1 in the 'Lens') the existence of which I do not deny, though I think his figure is not quite correct. I hope Mr. Stephenson will soon publish his researches into the structure of *Coscinodiscus Oculus Iridis*, as his mode of showing an inner layer is very instructive. Two of Mr. Stodder's figures (3 and 4)—one, representing very irregular hexagons, with indistinct markings; and the other, irregular black dots—appear to me to be affected by distortion, as well as incompleteness. His figure 3, more regular hexagons with central bright spots, is, I think, true under certain conditions of illumination, with imperfect resolution. The central marks in these hexagons I conclude result from the action of the *lower* layer. Fig. 5 in Mr. Stodder's paper is not reconcilable with anything I have seen. I believe the true formation to be much more symmetrical, and also more complex.

I must leave microscopists who study chemical, as well as optical, probabilities, to consider how far I am justified in thinking diatom silica to be uniformly deposited in spherules. Many individual diatoms show no spherules with means at present in use, but I know no *group* in which they are not apparent, and as objectives and modes of illumination improve, more and more spherules are seen. They can now be traced in many species close to the limits of (present), optical visibility; I see no reason why they should be supposed not to exist beyond it.

I quite agree with Mr. Stodder in noticing that in many diatoms (Coscinodisci, &c.) lines of fracture pass through the apparent depressions, showing them to be the weakest parts. In most of such cases the hexagonal borders appear to me composed of beads, and in many cases the floors of the hexagons are beaded too. The lines of fracture can often be traced to pass between the rows of these minute beads, just as Mr. Wenham showed in the case of *Pleurosigma*, &c.

Mr. Stodder thinks me wrong in objecting to the terms "areolæ" and "cellules" being applied to diatom markings. I do so because I do not believe the diatom marks coincide in character with the objects in other plants known by these names.

I remain, &c.,

15*th March*, 1873. HENRY J. SLACK.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *March* 5, 1873.

Charles Brooke, Esq., President, in the chair.

The President said he felt some little diffidence in occupying the chair for the second time as their President; he should not have thought of doing so himself, and he must ask them to consider him as a stop-gap (No! No! from a number of Fellows), because it unfortunately happened that two gentlemen, whom it was thought desirable to have as Presidents, had been prevented from accepting the office. He trusted that with this explanation they would receive his humble services during the coming year.

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.

The Secretary announced that the special vote of thanks passed at the last meeting had been sent to Mr. Hogg, and duly acknowledged by him.

The Secretary read the following letter which he had received that day, in explanation of an error which had been printed in a former number of the Journal, and which had been noticed in the number for March.

2, LANSDOWNE CRESCENT, W., *March* 5, 1873.

MY DEAR MR. SLACK,—I shall feel obliged if you will communicate to the meeting to-night that I admit I have made an error at page 52 by inadvertence and haste. The magnifying power should be *one more* than the distance of distinct vision divided by the focal length, *instead of one less;* so that the examples will read $8\frac{1}{7}$ times instead of $6\frac{1}{7}$; 81 instead of 79. I regret the error; but console myself with the sentiment that we are none of us infallible.

I am, yours very truly,

G. W. ROYSTON-PIGOTT.

The Secretary exhibited to the meeting a pattern chimney for microscope lamps which had been placed in his hands by Mr. Wenham. It was a cylindrical brass tube with a space cut out of one side of it, this being closed by an ordinary plain glass slide held in its place by means of a spring clip. The chimney itself was indestructible, and if the slip of glass got broken by accident, it could, of course, be very easily replaced. He also wished to mention that at the last meeting some slides were sent to the Society from Mr. Allen, of Felstead; they contained some crystals obtained from a liquid distilled from coke, as described in the last number of the Journal, at page 125. There was not time then to say much about them, but having had his attention directed to it, and having in his greenhouse a slow-combustion coke stove, he had obtained and examined some of a similar liquid. He found that when the coke was wet or impure a great deal of matter came over, it appeared to be a mixture of tar with a corrosive fluid. On a damp day it formed freely, and it was so corrosive that it perfectly riddled a piece of ordinary tinned iron piping placed to receive it. He had sent some which was unusually free from tar to Mr. Bell, who had kindly examined it.

Mr. Bell said that he found the crystals deposited on evaporation to be proto-sulphate of iron. The liquid contained sulphuric acid; there was also with them a little hydrochloric acid. Probably in the first instance the sulphur was given off from the coke as sulphurous acid, and this, by contact with the air and moisture, would become free sulphuric acid, the action of which upon the iron would of course be very rapid.

Mr. Richards reminded the meeting that he had some time ago introduced a metallic chimney for microscope lamps, similar to the one brought there that evening, but he used to put a glass tube inside, which he thought was more simple.

Mr. Wenham explained that his idea in making that chimney was not so much for simplicity as for the purpose of providing one in which a blank slide could be made available in case of breakage, that being a thing which everyone was sure to possess. He had tried the plan of putting an exterior tube to increase the draught, but he found that it did not succeed.

The Secretary thought that Mr. Wenham's idea was a good one, and would relieve microscopists from a great deal of trouble if they happened to be away in the country. He found that he never could get such a thing as a chimney for a microscope lamp at a country shop, and the consequence was, that if he met with an accident to his chimney he had to wait until he visited London before he could get another.

Mr. Beck observed that this chimney was cylindrical, and many paraffin lamps had flat wicks, and would not burn well with a straight chimney; they required one bulged out at the bottom and tapering towards the top. His own idea was that a flat wick lamp was far superior to any other for microscope use, because so much better light could be obtained by using it with the flame turned edgeways.

Mr. Wenham said that the chimney which he had brought was used on a flat wick lamp; it slightly elongated the flame, but was found to burn very well.

The President said that many years ago when he had occasion to go carefully into the subject of lamps, he found that the best flame was obtained by a flat wick bent into a circular arc; this would always burn in a straight chimney.

Mr. Richards had used a small circular wick in his lamps.

A paper was read by the Secretary, entitled "Some Additional Notes on the Microscope and Micro-spectroscope," by Mr. E. J. Gayer, Surgeon H.M. Indian Army, being a continuation of his paper upon the same subject read at the December meeting. (The paper will be found at page 147.)

A vote of thanks to Mr. Gayer for his paper was unanimously carried.

A paper was also read by the Secretary, "On a Minute Plant found in an Incrustation of Carbonate of Lime," by Dr. Maddox; the paper was illustrated by coloured drawings and specimens exhibited under the microscope. (The paper will be found at page 141.)

The thanks of the Society were unanimously voted to Dr. Maddox for his communication.

The Secretary hoped if any gentlemen present had paid special attention to this subject, that they would look at the object, and give the meeting the benefit of their opinions. It was curious that this plant was so much better preserved than the other vegetable matter with which it was associated; this would seem to show that it was of more recent growth. He also observed that Dr. Maddox stated that it did not seem to be parasitic on the moss.

The President thought that it must be a plant growing in the interstices of the calcareous concretion.

Mr. T. C. White inquired whether other plants in the neighbourhood were also incrusted. He thought this would be the case if the water were highly charged with lime.

The Secretary said that such would no doubt be the case ; indeed, any substance placed in a highly-charged spring would in time become semi-fossilized in the same manner.

At the next meeting, on the 2nd of April, Mr. W. K. Parker, V.P.R.M.S., will read a paper on "The Development of the Sturgeon's Facial Arches," and Mr. Henry Davis will read one on "A new *Callidina :* with the result of experiments on the Desiccation of Rotifers."

Donations to the Library, from Feb. 5th to March 5th, 1873 :—

	From
Land and Water	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal	*Society.*
Quarterly Journal of the Geological Society, No. 113	*Ditto.*
Transactions of the Northumberland and Durham Natural History Society, Vol. 10, Part II.	*Ditto.*
Bulletin de la Société Botanique de France, 2 parts	*Ditto.*

Edward Cresswell Baber, L.R.C.P. Lond., &c., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

MEDICAL MICROSCOPICAL SOCIETY.

At the second Ordinary Meeting of this Society, held at the Royal Westminster Ophthalmic Hospital, Friday, Feb. 21st, Jabez Hogg, Esq., President, in the chair, the minutes of the previous meeting were read and confirmed. The Secretary announced that six microscope lamps, as well as a cabinet for the use of the Exchange and Cabinet Committee, had been purchased since the last meeting, and the President notified that the Committee had decided to provide tea and coffee at the meetings in future.

Thirty-three gentlemen, proposed at the last meeting, were duly elected, and twenty-eight others proposed for election at the next meeting.

The President then called upon Dr. Pritchard to read his paper "On the Cochlea." See p. 150.

In the discussion following the reading of the paper, the President asked whether Dr. Pritchard had tried staining the nerves with chloride of gold, and also whether he had succeeded in setting up inflammatory action in the cochlea previous to the death of the animal experimented upon.

Mr. Crétin asked whether the animals used by Dr. Pritchard were similar to those employed by previous investigators.

Mr. Schäfer considered that the form of the rods was a question which would never be settled. He asked why Dr. Pritchard did not mention the striation of the rods, as this was to be seen by teazing with bichromate of potash, and stated that he had traced the fibrillation along the outer rods and into the membrana basilaris in osmic acid preparations. The fact that the rods increased in size towards the apex of the cochlea, he believed had been previously mentioned by some German author. He considered that teazed preparations were better for examination than sections, and doubted with Helmholtz whether the rods vibrated as they were stated to do, since they were firmly fixed the one to the other. He asked if cells existed between the rods, and said he considered it easy to demonstrate cilia on the rods, and accounted for Dr. Pritchard's not having seen them by the fact that he used chromic acid in the preparation of his specimens. The nerve cells described by Dr. Pritchard he believed to be simply epithelium.

Dr. Bruce asked how Dr. Pritchard prepared his specimens.

Dr. Pritchard, in reply, stated that he had used chloride of gold for staining the nerves, but with no very good result, and he had not succeeded in setting up inflammation. The animals he had made use of were cats, dogs, rabbits, guinea-pigs, man, and a kangaroo, but he had found very little variation in the form of the rods in any of them. He believed he had stated that the rods could be split up into fibres. He had discovered the difference in the length of the rods in 1871, and believed that the rods in a living animal might vibrate, although fixed, and thought it hardly fair to compare them to a mechanical instrument. The cilia mentioned by Mr. Schäfer he considered to be the fibrillæ of the rods torn off from the membrana tectoria, and the cells which Mr. Schäfer regarded as epithelial he still considered to be nerve cells, and Dr. Beale had also expressed his opinion in favour of their being nerve cells. With regard to methods of preparation, Dr. Pritchard referred those interested to the 'Quarterly Journal of Microscopical Science' for October, 1872.

A cordial vote of thanks was accorded to Dr. Pritchard for his valuable and interesting paper, which was illustrated by many excellent models, diagrams, and specimens.

The following presents were announced:—An Italian Medical Journal from Signor A. Tigri. Nine Slides from Mr. J. W. Groves.

The meeting then resolved itself into a conversazione, at which several interesting specimens were exhibited.

Manchester (Lower Mosley Street) Microscopical Society.*

Report of Annual Meeting.

The third annual soirée of the Microscopical section of the Natural History Society, in connection with the Lower Mosley Street schools, was held on Tuesday evening, February 4, 1873. There were about 200 present, amongst whom were Professor Williamson, Mr. John Barrow, Mr. Thomas Peace, Mr. Councillor Nield, Mr. Thomas

* Contributed by Henry Hyde, Hon. Sec.

Armstrong, F.R.M.S., Mr. Tozer, Superintendent of the Fire Brigade, Mr. Thomas Brittain, Secretary of the Manchester Aquarium, and Mr. Plant, of the Salford Museum. In a lower room were ranged, on tables, a number of microscopes, under which were shown numerous interesting objects, including specimens of the grains of various flowers; the calcareous covering of marine objects; the anatomy of insects; the cuticle of plants; portions of the human lung, and other objects. Each table was presided over by a member of the Society, who gave such information as was necessary to the spectators. After the company had had an opportunity of examining the interesting collection, they adjourned to an upper room, where arrangements had been made for a lecture, upon Pond Life, to be given by Mr. R. Horne, of Oldham. The chair was taken by Mr. Thomas Armstrong. In opening the proceedings, he said that, as President of the Society, he would venture to offer a few remarks upon the subject which had brought them together. It was about two hundred and fifty years since the microscope was invented, and to the valuable discoveries made thereby they stood indebted for a great amount of knowledge in various branches of science. At first difficulties and discouragements surrounded its introduction, but by degrees its use extended until it had attained to what they then saw it. Among the earlier workers as microscopists were Dr. Boyle, Mr. Hooke, Dr. Lieberkühn, Culpepper, and Henry Baker, F.R.S., who, so far back as 1743, wrote an admirable work upon the subject. A great impulse had been given, during the present century, to that branch of knowledge by societies; amongst which were the Royal Microscopical Society, the Old Change Society, and others, till they got down to their own little one there. There were people at that time, however, who looked upon the microscope as but a thing to excite wonder, and as a plaything, but he had no doubt that these opinions would soon be dissipated. Speaking upon the use of the microscope, he said, its results must materially lead a thinking mind to a consideration of organisms of all kinds, from the most minute to the most immense, until it was lost in the variety and magnificence of them. There remained a boundless field for inquiries in that department of science, and every step they took enlarged their ideas, and gave them greater capacity to understand the wonders of nature. Histology, or the science of the minute structure of the organs of plants and animals, might be said to be the creation of that century; some glimpses of organic structure having been, however, obtained by the earlier observers, but without system, and from which it would have been impossible to get a proper idea of the laws of formation and development. It was only within the last forty or fifty years that the microscope had been made capable of yielding such a magnifying power, combined with such clearness of definition, as was necessary for the investigation of that most interesting and important field of research. In organized beings nature worked out her most secret processes by structures far too minute to be observed by the naked eye, hence the microscope was of great importance to the physiologist. The medical profession were greatly indebted to it. Referring to animals and plants, he said the difference between the two seemed very great, but upon investigation it would be found that they gradually approached each other, and it took a skilful microscopist to determine sometimes to which of the two kingdoms an individual belonged. Formerly the power of motion was considered the characteristic of an animal, but then it was known that some plants possessed that power. Histological inquiry had rendered the matter complex by the discovery of a common character, namely, the primary cell as a starting point for all organic beings. The microscope had taught them that the simplest plants were composed of cells, and also all others of the higher order were made up of such cells, of course arranged according to the functions they had to perform. In the earliest condition of animals the cells were nearly the same as those in plants. In the latter the cells continued present throughout their growth, but in animals, except in those tissues called cellular, they soon disappeared. The minute structure of the skeleton of plants, and the lower order of animals, was a most interesting study, and would amply repay them for the investigation, and he (the Chairman) knew of none more calculated to make them forget time and place. There was something so entrancing in the way Nature gave up her wondrous secrets, that the mind seemed to be entirely taken out of the world—the hours flew past as in a dream, and the day became too short for the pleasant labour. An interesting lecture on Pond Life was then delivered by Mr. R. Horne, of Oldham, who illustrated his remarks by means of a large picture thrown on a screen by means of an oxyhydrogen lantern. The originals of the objects of animal and vegetable life, depicted on the drawing, were taken from a pond in Essex, but it was shown that every pond contained more or less the same objects. Mr. Horne explained those phenomena in a scientific, popular, and even humorous manner.

Oldham Microscopical Society.

Recently the members of the above Society held their sixth conversazione in the club-room of the Oldham Lyceum. After spending an agreeable half-hour in conversation, and in the examination, under the various microscopes lent by members, of objects illustrative of the subject of the evening, the chair was taken by the President, Dr. A. Thom Thomson, and a paper read upon "Common Moulds" by Mr. Pullinger. At its close some interesting discussion took place upon the question, "How can we account for the presence of mould in the inside of nuts, in the core of apples, and other unlikely places?" which gave the advocates and opposers of the theory of spontaneous generation an opportunity of airing their peculiar notions thereupon. After an inspection of a further supply of objects, the meeting was brought to a close by the usual vote of thanks. The following is an abstract of the paper :—

The term "mould" has been applied generally to a whole host of minute plants, belonging mostly to the natural orders Mucedines and Mucorini, which include some of the great scourges of the day, attacking and destroying our grape crops, our potato crops, our silk-worms, and many forms of useful vegetable life. These moulds, however, belong to special species, and are not commonly met with, and it is my purpose to confine myself to those met with continually in every-day life, and which infest our bread, cheese, preserves, pickles, ink, beer, fruits, and decaying vegetables; also our boots, our linen, our cotton goods *en route* for India or China, and even our very teeth and the mucous membrane of our throats. These belong, for the most part, to the genera *Aspergillus*, *Penicillium*, and *Mucor*, the two former being hyphomycetous, and the latter physomycetous.

The mould which has most frequently come under my notice is *Aspergillus glaucus*, the presence of which in its favourite nidus, cheese, is considered by some of my friends (and I must plead guilty myself to the soft impeachment) to greatly improve its flavour. I have found it on Manilla cigars, on preserves, on *Radix althæ*, or the marsh-mallow roots of the shops, on horn, old oak, mistletoe, old shoes, and, in fact, everywhere. The name *aspergillus* has been given in consequence of some resemblance to the *aspergillus* or mop-like brush used in Roman Catholic countries to sprinkle the holy water with. In its young state it presents nothing to our view but a rapidly-spreading white articulated *mycelium*, which, however, soon, under favourable circumstances, throws up erect fertile threads, bearing on their apices globular heads, from which chains of spores radiate, and thus give a mop-like appearance to the ripe fruit. In course of time these chains of spores fall off, and leave the globose head, which may then be observed covered with short spiny processes, probably the points of attachment of the chains of spores. These spores are globular in form, and seem to me to be irregular in size—3, 2½, or even 2, sometimes filling the micrometer space for 1–1000 in. They present a most beautiful appearance under the binocular with a ⅛-inch power. *Aspergillus* has been found in the lungs and air-sacs of birds, also in the external conduit of the ear.

The next form of common mould is *Penicillium*, which also belongs to the hyphomycetous family, and natural order Mucedines. The most common is *Penicillium glaucum*, which is found in great abundance, in the form of bluish and greenish mould, on decaying vegetable substances generally, but especially on semi-fluid or liquid matters, forming a dense pasty crust, slimy on the lower surface, and bearing spores on the upper, Its general appearance is similar to that of *Aspergillus glaucus*, and it is only by the aid of the microscope that we can distinguish them. Its *mycelium* consists of interwoven articulated filaments, extensively ramified, and bearing fertile threads, also articulated, upon the apices of which are developed septæ or branchlets, consisting of an elongated cell, or cells, sometimes simple, sometimes forked, but each bearing a chain of spores, frequently arranged in a peniciliate or brush-like form; hence its name. The spores are of various colours, according to age and circumstances, but green of some shade generally prevails. They are elliptic in form, and thus easily distinguishable from those of *Aspergillus*. They are also smaller, and more even in size; at least, such is my experience of them, about six placed side by side filling the micrometer space for 1–1000 in. The specimens on the table are mostly from fruit—oranges and apples—and also from bread.

Years ago a considerable interest was created by the introduction of a new article of domestic economy in the form of a slimy mass of gelatinous matter, very much like inferior boiled tripe, and called the vinegar plant. It was said to have been introduced from India or South America. It was usually placed in a jar containing a solution of treacle or sugar, and, on being allowed to remain in a warm situation for a month or six weeks, the liquid was found converted into vinegar by the action of this strange plant, which also propagated itself by subdivision, for on looking underneath laminæ were observable, which could be separated, and, when placed in the proper media, would develop into new plants. This curious plant has been undoubtedly resolved into a *Penicillium*, the gelatinous mass being only an abnormal condition of the *mycelium*, due probably to its submerged position, for when allowed to dry up, the fruit of *Penicillium glaucum* is invariably produced. The general mass of the vinegar plant is structureless, but near the middle are chains of cells of all sizes, many of which are undistinguishable from those of the yeast plant, which fact suggests the idea of a family likeness, an idea now fully established ; and as the yeast plant is a known cause of vinous, so also the vinegar plant seems to be a cause of acetous fermentation, and, as both are but different forms of *Penicillium glaucum*, so it comes about that the common mould of our bread paste, &c., becomes the presiding genius over the great regenerating work of fermentation, giving us not only our yeast wherewith to make our bread, but also vinegar for our pickles, and, what is better still, "wine, which maketh glad the heart of man," and last—but not least—our "far-famed bitter beer."

The yeast plant, as you well know, consists of round or oval cells, which live, expand, and give rise to new cells or plants by budding until the fermenting principle is exhausted. The cells are round at first, and as the fermenting principle is nearer exhaustion they become oval, then linear and filamentous, advancing to the primary stage of *mycelium*, until finally they develop themselves into the normal threads and fruits of the common *Penicillium glaucum*. Berkley says that he and Mr. Hoffman followed up the development of individual yeast globules in fluid surrounded in a closed cell with a ring of air until the proper fruit of *Penicillium glaucum* was developed. Some years ago the bread of Paris was much infested with *Penicillium*, the spores of which were found capable of sustaining a heat equal to that of boiling water without destroying their germinating power. The disease known as apthæ or frog, and which is one of our earliest troubles, is now generally believed to be a species of *Penicillium*, as is also the filamentous growth constant in the tartar of the teeth.

The last of these common moulds is known as Mucor. It belongs to the family *Physomycetes*, and order *Mucorini*, the genus being *Mucor*, which, as in the case of *Aspergillus* and *Penicillium*, a number of species exist. The mycelium consists of delicate branching filaments, forming a beautiful network, which is distinguished from the mycelia of *Penicillium* and *Aspergillus* by its consisting of simple tubes, without articulations, which is also the case with the fruit stalks, which bear on their apices, not naked spores, but bladder-like sporangia enclosing sporidia or spores. It is common on decaying fruits, paste, and vegetable matters, and *Mucor mucedo* is very often met with, though, strange to say, I have not been able to meet with a single specimen. I have, however, a beautiful specimen of a more uncommon one—*Mucor tenerrimus*—which is developed in large quantities in a Wardian case I have set up, and in which I put the trimmings of the ferns chopped small to lighten the soil, and shortly after the whole case was one mass of mycelium. Its fruit is scarcely visible to the naked eye, but when viewed with the half-inch it is an object of rare beauty and elegance.

The result of my examination of its sporangia and sporidia is, that the sporangia are about equal in size to the spores of *Aspergillus*, whilst the sporidia—which are liberated on the bursting of the sporangium, which takes place on the application of a drop of water—are very minute indeed, and elliptic in form. They displayed great molecular activity, and in consequence I was unable to measure them. *Mucor stolonifer*, and its life history, is a subject dwelt upon by Professor Wyville Thomson in his address before the Botanical Society of Edinburgh, and he gives the results of the most recent investigations by De Bary, Pasteur, and others. He states that from the mycelium, at certain points, long, rather wide tubes start from the surface, on which the fungus is growing, obliquely into the air, and after running along for a time, again dip down and give origin to other tufts of myceline tube roots. At the point where these roots come off, as at the bud of a strawberry runner, a little tuft of tubular stems rise up vertically, and end in round vesicles or sporangia, which are at first entirely filled with transparent protoplasm, which ultimately breaks up into a mass of black polygonal spores. These spores are thus produced by no process of true reproduction, but are simply separated particles of the protoplasm of the parent plant, and may be regarded as buds, since they are capable of producing new plants like themselves. True reproductive spores exist in the secondary form of fruit of the *Mucor*, and this is the case also with *Aspergillus*. Thus we see these plants are reproduced in two ways—by buds and by true spores born in asci.

Eastbourne Natural History Society.

A meeting of the members of the Eastbourne Natural History Society was held at the Society's Rooms, Lismore Road, on Friday, December 20, when about 30 members were present. Mr. Roper occupied the chair, and the minutes having been read and confirmed, the Hon. Secretary read a paper "On Geoglossum Difforme or Earth-Tongue," by C. J. Muller, Esq.

The plant belongs to the order Elvellacei, which includes within its limits the rare and delicious Morel (*Morchella esculenta*), the no less favourite curled Helvella (*Helvella crispa*), the lovely Peziza (*Peziza coccinea*), the curious and elegant *Ascobolus ciliatis*, and many other genera attractive to the Fungologist. The character of the order is, that the fruit consists of sporidia contained in asci, that the hymenium or fruit-bearing surface is more or less exposed, and that the substance of the plant is soft. The character of the genus Geoglossum is that the receptacle or fruit-bearing part is club-shaped, and that the hymenium surrounds the club. Seven distinct species are found in England.

If a longitudinal section of the plant be made, it will be found on examination with the microscope that the entire substance consists of nothing more than delicate filaments, like the threads of a common mould, interwoven and more or less compacted, and that these threads, as they approach the external surface of the plant, become differentiated into what are called asci and paraphyses. The asci are little elongated bags of transparent texture, which contain, within each of them, eight dark brown spores closely packed together. It is these dark brown spores which partly give to the plant its black and dingy appearance. They are for the most part 7 septate, but some may be found with only 3 septa, and others divided into as many as 14 distinct cells. These spores, or sporidia, are the fruit of the plant, and by germinating under certain conditions are believed to reproduce the parent form.

In looking at the fruit, one cannot but be struck by the very ample provision made for the propagation of the plant. The spores are all but innumerable, and are carefully packed away in parcels of eight in delicate little bags ready for future use. In what condition they remain during the spring and summer, no one has yet discovered. The plant does not appear until late in autumn, so that they may be supposed to lie dormant in the ground during the greater portion of the year.

I have mentioned paraphyses at part of the fructifying surface. These delicate filaments are believed by many mycologists to be simply abortive asci, but I have noticed in the case of Geoglossum that they occasionally thicken and give origin to distinct uniseptate spores.

In conclusion, I have only to remark that this plant, like many other species of fungi, consists of nothing more than septate threads like the threads of a common mould; and that it differs from a mould only in the nature of its fructification, and the way in which these threads are compacted into an object of definite shape, and considerable consistence. The same remark applies to mushrooms and many other species of fungi, and indicates the vast resources of nature in multiplying forms from one simple element, a delicate tubular filament.

F. C. S. Roper, Esq., F.L.S., then read a "Note on the Wall Pellitory." The *Parietaria officinalis*, or Wall Pellitory, is a plant so common on old walls and buildings that it is probably well known to most of our members. It has, however, some peculiarities of structure, not generally noticed in botanical works, but at the same time of much interest to the microscopical observer. As the minute examination of the structure of both animal and vegetable organisms is of great interest to the really scientific investigator of the wonders of creation that are spread around us in such boundless profusion, I propose to direct attention to some points of interest that may have escaped notice, even of those well acquainted with the general habit and appearance of a plant that is met with in so many localities.

The *Parietaria officinalis* belongs to the Urticaceæ or nettle family, which, although abundant in tropical regions, is represented in England by very few species; the stinging-nettles, the hop, and the elm, being the only other members of the order. I do not, however, propose to enter into any detail of the characteristic or common peculiarities of these genera, but merely to point out some points of interest in the leaves of the common pellitory.

The only description generally given of these leaves in botanical works is, that they are slightly rough or hairy, and Loudon in his Encyclopædia notices that they are marked with pellucid dots. If a leaf is placed in water under the microscope, or, better still, if a small section is made and examined in the same way, the hairs are very plainly seen, and are of two kinds. The most abundant consists of long slightly curved transparent spine-like hairs, with rather blunt points, apparently hollow at the other extremity, and attached to the centre of some cells arranged somewhat in a stellate manner, and larger than those forming the general substance of the leaf. Interspersed with these, but not so abundant, are found small recurved hairs, about one-fifth the length of the others, which in shape and peculiar curve exactly resemble small fish-hooks; these are scattered apparently at intervals, especially on the younger leaves, but are less abundant on the older leaves towards the base of the stem. But the structure of most peculiar interest in these leaves consists in the so-called "pellucid dots" of Loudon, which may be readily seen by holding a leaf up to the light. If the leaf is placed in water, and the upper surface examined with a half or quarter inch objective, these dots are seen to consist of seven or eight rather large cells, radiating from the sides of a centre cell, which appears slightly raised above the surface of the leaf, so that the surrounding cells appear to slope from it to the surface of the leaf; below these, and in the parenchyma, or substance of the leaf itself, is a large single cell, within which is suspended a sub-globular or slightly pear-shaped mass with a papillated surface, but with no clearly defined crystalline structure. These bodies are known as Sphæraphides, and have also been called "Crystoliths" by Continental writers; they are sufficiently large and hard to be easily separated from the parenchyma of the leaf when thin sections are made, or small portions torn up under the microscope. When treated with muriatic acid they dissolve rapidly with considerable ebullition, and when burnt are reduced to a white powder; there can be no doubt that they are, therefore, chiefly composed of lime, and probably in the form of carbonate. They differ from the true Raphides, so abundant in many plants, by being almost amorphous, though occasionally a slight semi-crystalline appearance may be detected in small fragments if examined with a quarter objective. Although not so often noticed as true Raphides, they are characteristic of many tribes of British plants—as the Caryophyllaceæ, Geraniaceæ, Bythraceæ, Chenopodiaceæ, and especially the Urticaceæ, and it is thought by some botanists that they afford a good diagnostic character for species. In some exotic plants these Sphæraphides occur of considerable size, forming a weighty grit, and are especially large and fine in the prickly pear and others of the Cactus tribe.

If we look to the use of this curious and elaborate structure in the leaves of plants, and ask what is their object in the economy of nature? It is a question easier to ask than to answer. Some suppose that Raphides are perhaps rather a disease than formations of natural growth in plants; but they are of too common occurrence and too universally distributed over the whole tissue of certain species for this to be the case. In some instances they are doubtless useful as a medicine, and the genuineness of sarsaparilla, guaiacum and squills may be tested by the presence or absence of Raphides. Dioscorides says that the juice of the wall pellitory tempered with ceruse is good for the shingles, and Pliny affirms it is also a remedy for gout. But it is more probable, as Dr. Gulliver suggests, that the large proportion of these crystalline bodies being compounded of phosphate or oxalate of lime, or some other compound of this earth, and remember the value of these substances in the growth and nutrition of plants, that nature has established in some plants a storehouse or laboratory of such calcareous salts, and that we may thus get a glimpse of the utility of these crystals.

A vote of thanks was passed to the authors.

Both papers were illustrated by sections and specimens showing the points of interest, which were exhibited under the microscope, at the close of the meeting.

Sheffield Naturalists' Club.

Last month the first meeting of the Sheffield Naturalists' Club was held in the Cutlers' Hall. Mr. Henry C. Sorby presided.

The President, in delivering the inaugural address, said he proposed to give a few of his views with reference to the formation of the Society. He had been asked what was the use of such an institution, and he would tell them. If they were to look upon the study of natural history as the discovery of rare plants in the district which did not exist in other parts, such a society as this would be of little use. The knowledge of natural history was not to be limited to the mere knowing the names of animals and plants, and the chronicling of them. That would be about equal to knowing the name of a man and thinking they knew his character, or knowing the name of a country and thinking they knew its history. Such a society as this had two characters. First of all, the subjective influence it had on the members who composed it. The study of natural history was most desirable in many ways. Man had a certain amount of energy; it must be expended in some way or other, and the examination into natural history furnished them with a study which was advantageous to both body and mind. The explorations into the country would be exceedingly beneficial in point of health, and they might learn many interesting facts during those excursions which would have a beneficial influence on the intellect. By being joined together in a society they might greatly help one another. With regard to the objective value of such a society as this, he thought they ought not to limit their efforts to the mere making out of accurate lists of flor nd fauna which occurred in the district. The efforts of naturalists also ought to be devoted to the discovery of general philosophical principles, as applied to both animals and plants. He thought they could learn a great deal more by the careful study of the commonest things than by looking for rarities. They could not hesitate in saying that a great deal remained to be done in the study of natural hist n every district. They might come to such a question as this: "What is life, and how have the various species of animals and plants originated?" Such a problem was one of the greatest that could be presented to the human intellect. Then, again, a very difficult subject was, why particular plants grew in particular localities. That was a question easily asked, but most difficult to answer. Sooner or later, science ought to be able to say why certain plants grew in certain localities and not in others, and the determination of that question would have a most important bearing on geological theses. Another problem for study was, what was the effect of dry or wet seasons on certain plants? If that question were settled, they might know the effect that must have been produced in bygone ages, by the alteration of climate, on certain plants and animals. Another most interesting subject for investigation was the influence of plants on plants, animals on animals, and one on the other; the fertilization of plants by insects, and the attractability of different colours for different insects. The speaker recommended for study the following subjects:—The manner in which the habits of animals have been acquired; the manner in which varieties or species have been formed; the limit of the successive generation of insects through none but females; the diseases of plants due to parasitic fungi and insects. He concluded by remarking that he might say much more on this subject, but he had shown sufficient to prove that much might be learnt by studying the commonest things seen almost everywhere.

Mr. Edward Birks read an interesting paper on the botany of the district, after which, the following gentlemen were added to the members of the Society:—Messrs. M. du Gillon, G. W. Hawksley, W. H. Booth, F. Trickett, R. Lokley, F. Lawton, D. K. Doncaster, J. Hobson, W. K. Peace, W. Smith, S. Osborn, E. Allen, J. Bedford, J. H. Wood, H. Seebohm, A. Ellin, J. Webster, H. I. Dixon, and the Rev. J. T. F. Aldred.

A vote of thanks to the President concluded the proceedings.

W. West & Co. sc.

CALLIDINA VAGA.

THE

MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1873.

I.—*A New Callidina: with the Result of Experiments on the Desiccation of Rotifers.* By Henry Davis, F.R.M.S.

(*Read before the* Royal Microscopical Society, *April* 2, 1873.)

Plate XIV.

It is just six years since the Rev. Lord S. G. Osborne forwarded by post to several microscopists in London, small parcels of a few grains of a dry dusty powder, labelled "Philodina roseola"; and one of these being kindly consigned to me, no time was lost in watering the dust in a stage-tank as directed. Almost immediately the contained pink ovoid bodies, seen in profusion under a low power, began to swell, lengthen, and, finally, either put forth a pair of rotatory organs for purveying and swimming, or everted a proboscis and crawled out of the field.

As a first experience this revival was sufficiently interesting, but it was no novelty, for the same thing was witnessed by Leuwenhoek more than 150 years ago; but a careful scrutiny of the lively inhabitants of my tank brought to light the fact that it contained two distinct kinds—the *P. roseola*, as expected, and a very few crawling strangers, having blunt rounded heads, in contour like those of the Tardigrades; and although undoubted "wheel animal-

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EXPLANATION OF PLATE XIV.

Fig. 1.—*Callidina vaga:* ventral view. × 200.
,, 2.—Ditto: side view.
,, 3.—Ditto contracted: dorsal view.
,, 4.—Ditto ,, In the "dry" state.
,, 5.—Ditto: side view of a single detached ramus: one of a pair of hard processes beneath the teeth-plates of the mastax. × 500 about.
,, 6 *a*.—Ditto. Hard parts of the mastax: the rami seen through the teeth-plates.
,, 6 *b*.—Ditto. Ideal section of 6 *a*, made through the part corresponding to the dotted line.
,, 7 *a*.—Ditto. Hard parts of mastax: with teeth-plates opened and rami turned on their long axis, showing short branches.
,, 7 *b*.—Ditto. Ideal section of 7 *a*, through dotted line.
,, 8.—Ditto. Ideal section of mastax, when the teeth-plates are brought into contact crushing food. In these ideal sections, the *probable* positions only, of muscles are indicated.

cules," yet bearing no wheels at all. On communicating this fact to Lord Osborne, with my opinion that the rotifer was new to science, he looked through his large stock of tanks, and found scores—hundreds of them. He could not, however, be induced to undertake the task of introducing the stranger to the scientific public.

Little less reluctant at that time, I merely exhibited the new rotifer under the name of *Callidina vaga*, at a soirée of the Old Change Microscopical Society, in February, 1869, and again let it retire into private life.

Some time after this, Dr. Hudson met a rotifer whose head was shaped like the "top joint of a thumb," * and he speaks regretfully of their short acquaintance in the first excellent paper on his *Pedalion mira*. Seeing that he had happily given the most striking peculiarity of *C. vaga*, I at once sent a few dry specimens, which he moistened and immediately identified; thus reviving with pleasure an old friendship, and giving me the great advantage of forming a new one with the best active observer and describer of rotifers we now possess.

It is, perhaps, barely necessary to state that all due care has been taken to prevent the introduction of a previously-described rotifer under another name. I can find nothing like *C. vaga* described anywhere; the *C. constricta* of Dujardin bears, indeed, in the head a very remote resemblance to it, but description there is next to none; while the *C. bidens* of Mr. Gosse is very plainly a different species, as shown by that gentleman's notes and figures in an invaluable MS. volume, most courteously lent to me by the author.

For ranging with the most generally accepted classification of Rotifera, the specific character of this new member of the family Philodinæa, genus Callidina, may be thus condensed:—

C. vaga (mihi).—Figure depressed-fusiform; crystalline, and nearly colourless; flat, frontal lobes continuous with ventral surface, and uniformly covered with short cilia, not disposed as peripheral wreaths; non-retractile proboscis, with broad anterior hook; two coarse and numerous fine teeth in each jaw. Progression by crawling. Length 1-50″ to 1-36″.

As regards habitat little information can be given: the rotifers are found in certain open stone vases in the grounds of Lord Osborne's house near Blandford; and these at times get partly filled by the rain, while the wind drifts in dead leaves, and other matter, which by their decomposition seem to yield suitable food. So far as my experience teaches, similar circumstances in and near London do not produce the same results. Philodinæ of different species have been gathered sparingly and in sooty condition from a house-

* 'Monthly Micro. Journal,' Sept. 1871.

top in Islington, while moss on a garden wall at Battersea has furnished *C. bidens*, but no *C. vaga*.

With the exception of the strange head, and the gliding crawl necessitated by the arrangements of its parts, there is no great peculiarity to distinguish this from most other philodines; it has, like them, two lobes, but these—although separated by a clear unciliated space—are not split up into two independent rotatory organs inclined laterally right and left when in action, and slightly over to the dorsum, but are generally kept down nearly in the same plane as the ventral surface on which the creature swiftly crawls, while the cilia brush over the path traversed and sweep all loose matter, including food, towards the buccal cavity;—not directly into it, however, but sufficiently near, on each side, to permit the sucking action of its ciliary current to have full play on the passing atoms.

Running at the back, or unciliated portion of the confluent lobes, we have the representative of the ordinary proboscis: in this case soldered to, and having no action apart from, these lobes: it is furnished with a permanent frontal hook, only seen as such in a side view (Fig. 2). Unintentionally I may be straining analogy too far in considering this a proboscis; except in bearing the unusually well-developed hook, it is not at all like the ordinary proboscis: but it is somewhat remarkable that occasionally a philodine may be seen crawling with half retracted rotatory organs and proboscis arched over them, affording most perfect resemblance to *C. vaga*; for the ciliary frills in this rare case are drawn together into a thick brush, sweeping the way, while at the back of these, in close contact, comes the extended proboscis, with its soft anterior finger or hook. Is it too fanciful to think that the head of *C. vaga* may be a permanent form of an exceptional and transient figure assumed by that of other philodines?

Each clump of cilia, or lobe, is bounded at the neck by a strong process—perhaps the homologue of the trochal pedicle in Rotifer, &c.,—rigid, except at its base, and deeply serrated at the free curved ends, which appear to come level with the tips of the cilia (Fig. 1). Between these comes the capacious oral aperture with its strong ciliatory current, inwards or outwards, apparently as the creature wills.

The œsophageal tube as usual goes direct to the mastax, and this delivers the masticated prey to a slightly convoluted central duct of a large cylindrical stomach. Beyond, and, I think, divided by a valve, comes a small spherical intestine, with a short, commonly closed, passage round the contractile vesicle to the anus. In fully grown specimens the period of the contractile vesicle averages two minutes, but it evidently grows slower with age; thus, when the animals are very small and young, the sac fills and suddenly empties in twenty seconds, or less; while the oldest inhabitant of my oldest tank had a pulse calmly beating one in five minutes.

With difficulty, but certainty, five pairs of "vibratile tags" may be traced in a selected victim, by crushing (or otherwise persuading it to be quiet), and seeking the flicker before the creature dies. A tag may be seen on each side beneath the base of the serrated processes, or a little lower, and so on downwards, four more at about equal distances approaching the contractile vesicle; but I fail to detect any tube connecting the tags with each other, or with the vesicle. On either side of the abdomen is seen a group of rudimentary ova, or a single large granular egg.

C. vaga bears on its back an antenna a little broader laterally, and shorter than common (shown in Figs. 2 and 3), and at the extremity of its foot-tale three soft toes; mounted on the next joint above are the usual two spurs or horns. These, I find, act as supplementary toes in this species, and in *P. roseola*:—whenever an extra firm hold is required, the penultimate false joint is carried down, and the spurs, as well as the small toes, are brought into adhesive contact at the anchorage; assisted, no doubt, by the viscid fluid which all rotifers appear to secrete more or less abundantly (*vide* foot-tail, Fig. 3).

As to the mastax of any philodine, it would be discreet in me only to refer to Mr. Gosse's memoir in the 'Philosophical Transactions';* but the worst part of valour urges me to make a few remarks on the most simple part of a difficult subject.

If *C. vaga* be killed by boiling, poisoning, or unduly roasting; the body, left in a tank and not compressed, will slowly rot away, and leave all the hard parts of the mastax clearly displayed, generally adhering undistorted and in natural positions. As an example, take one of these (Fig. 6 *a*): two transparent plates curved like watch-glasses, convex side outward, each having two thickened lines or teeth to lock into each other, and from thirty to forty fine teeth, all in the one-thousandth part of an inch; on focussing through the faces of these striated plates (just on the right and left of the straight edges where they are in contact), we see a pair of rami, the strongest and thickest hard part of the entire rotifer. The rami have two notches on the parts corresponding to the coarse teeth of the plates described, and on the side opposite to the notches each ramus has a short branch (Fig. 5). Take another example: In this the straight edges of the curved plates are forced apart, only being kept together by the hinge at the top; while the inner rami will be seen to have turned on their long axes, bringing their short branches into sight (Fig. 7 *a*). Many self-

* "On the Manducatory Organs in the class Rotifera," by P. H. Gosse, Esq., 'Phil. Trans.,' Feb. and March, 1855.

prepared specimens like these, with observations in the life, go to show that the rami, by a rolling motion (actuated by muscles), cause one of the many movements of the apparatus, that of opening and closing—scissors-fashion—the teeth-plates; while the surface-contact and pressure-together of the latter are effected by the direct action of the muscular cushions on which they rest.

This branch of the subject may be concluded by a short history of the life, manners, and vicissitudes of the little collection of *C. vaga* I now exhibit [a 3 × 1 glass slip with a central cell holding about ten drops of water.] It is a colony of modest virgins; eager eyes have been on (and off) them for nearly six years anxious to detect instances of feminine frailty, but in vain. They literally scrape up a living thus: keeping the toes attached to the glass, one crawls swiftly straight forward, partly by greater and greater extension of the body, and partly by the action of the ciliated discs, and all the time feeding as it goes; having attained its greatest extension, the animal suddenly retracts, reducing its length more than half, and without releasing its foothold, turns slightly in another direction, again extends, and again retracts. At last it may have grazed over a circular plane of a diameter double the greatest length of its body; it then starts for pastures new, generally by crawling somewhat in the manner of *R. vulgaris*. But it travels most quickly when it releases its foothold, contracts some of its posterior joints, and glides on by its cilia only (Fig. 2). Its attempts at swimming are always failures; one may cast itself from the cover of a tank and wriggle helplessly, as an earth-worm might, until it reaches the bottom; or it may attach itself to a passing free-swimming philodine, and ride safely to shore.

Originally this tank contained two species, but one, after keeping apart in groups, finally died out. In a larger tank the reverse was the result, the Callidinæ dying off and leaving the Philodinæ flourishing. In this fact a careless observer might readily imagine a transmutation of form—one species changing into the other. Much greater changes than these Dr. Bastian supposes he witnessed, such as spores of Algæ developing into highly-organized rotifers,* but to say little of my own rather lengthened experience, which points directly against Dr. Bastian's belief, I could, were permission given, state the adverse opinions of gentlemen whose studies of Rotifera extend over as many years as Dr. Bastian's over weeks; but in truth his conclusions are founded on such absurdly insufficient evidence that serious refutation is not needed.

To return to the colony of rotifers. Since its establishment in 1867 it has received no new immigrants, but as it increased and multiplied, some of its members, in a dry state, have been removed to stock new tanks for my friends. It is generally kept in a

* 'The Beginnings of Life,' vol. ii., by Dr. Bastian.

cabinet, with other objects, and watered for examination when required, or, as a rule, once a month: so small a quantity of water dries up rapidly in summer; in a day sometimes. The longest time it has kept continuously dry is ten months; in winter, after watering, it has been frozen into a mass of ice; it has been heated on a brass mounting-table, with a spirit-lamp very often, in order to melt the marine-glue when a new cover has been required; it has been exposed dry to the sun in a photographer's glass room, all through a broiling summer; taken a sea voyage to the south of Spain, revived there, and brought home again; taken to Ceylon; to India; revived on ship-board, to the astonishment of the passengers; brought home, and a few of the dry inhabitants immediately posted off again to a friend in Ceylon, who revived and has them still. As a final indignity and injury this much-enduring family has been put into the receiver of an air-pump for twelve hours and thoroughly exhausted. This was almost too much for it, but still there is a little life in the tank.

Recently I determined to make a methodical series of experiments, to see if any new light could be cast on what I thought were disputed points regarding the perfect drying and revival of certain rotifers, but reference to books, old and new, soon proved that now at least there was no disputed point at all. The battle had been fought and won, and controversy ended. As a matter of history it had a certain interest to know how Spallanzani and Doyère experimented and Ehrenberg doubted, but it is far more interesting to learn the ultimate result as recorded unanimously by our modern text-books. Thus, Pritchard *:—"They have the power of preserving their vitality when *thoroughly desiccated.* Leuwenhoek and Spallanzani experimented on them, and announced the fact of their revivification on the addition of moisture months and even years after their complete desiccation. Schrank, St. Vincent, and Ehrenberg questioned the truth of the statement. . . . Schultze and Doyère have repeated and confirmed the experiments of the old observers, and the latter authority concludes that Rotifera may be *completely dried* in a vacuum without losing the capability of being revived by moisture. Many, indeed, are sacrificed in the process, but enough recover to demonstrate the possibility of the fact."

And what does Dr. Carpenter say? "Certain † Rotifera are remarkable for their tenacity of life, even when reduced to a *most complete state of dryness;* for they can be kept in this condition for *any length of time*, and will yet revive upon being moistened. Experiments have been carried still further with the allied tribe of

* 'History of Infusoria,' 4th edition.

† 'The Microscope and its Revelations,' 4th edition.

Tardigrades; individuals of which have been kept in a vacuum, with sulphuric acid and chloride of calcium (thus suffering the most complete desiccation that the chemist can effect), and yet have not lost their capability of revivification . . . . . their return to a state of active life, after a desiccation of *unlimited duration*, may take place whenever they meet with moisture, warmth, and food."

All the modern scientific works tell the same tale; a dry rotifer must be the driest thing in existence; but privately and out of book several good observers have expressed their doubts. They cannot believe that the rotifers are completely dried, and yet they cannot explain away the undoubted drying effects of an oven at 200° (Fahr.), or of the vacuum of an air-pump. The only modern writer I can find bold enough to put his doubts into print is to be read in the first volume of the 'Cornhill Magazine.' He cleverly attacks the "dry" subject, shows that when separated from sand or dirt the rotifers die in drying; concludes that sand and dirt somehow keep moisture in them under trying circumstances, and thinks there have been "mistakes somewhere" about the air-pumps and ovens.

My experiments at first were merely repetitions of those of the old observers, and the results were nearly the same. The rotifers could be heated gradually up to 200° (Fahr.) in an oven, and some revived with water afterwards: with thermometer standing at 300° two hours' baking completely cooked and killed them all; so did boiling for three hours; but most of the experiments were made with a modern air-pump of the best construction. Two large vessels of strong sulphuric acid were put into the receiver, and a dry tank containing *P. roseola* in great numbers. The receiver was exhausted to the utmost, and so kept for a week. Then the tank was removed and moistened; about a dozen rotifers crawled feebly out, and two or three put forth their rotatory disks: they all died in a few days.

Now we are taught that this experiment is conclusive, the dozen rotifers having suffered "the most complete desiccation the chemist can effect," must needs have been dry. By no means. According to my belief, the few rotifers revived because they were *not* desiccated, in spite of the effective chemist, while the scores of non-revivers *had* been dried, partially or entirely, and killed!

Then, how are rotifers protected against desiccation? The philodines constantly give off a slimy secretion; in drying they contract head and foot-tail (Fig. 3), and as they gradually assume an ovoid form (Fig. 4), the gelatinous fluid dries over them into a hard thin shell, and effectually secures them from even "the most complete desiccation the chemist can effect."

And now for proofs: as evidence that they really *have* such a secretion, I merely quote one witness of many, a valued correspondent, who made his statement quite unexpectedly and without a question being put. "They are very slimy, and when first getting out are much hampered by the stuff sticking to them." But admitting the gelatinous varnish over a naturally dried rotifer, would such a coating be sufficient protection against the searching absorption of sulphuric acid in vacuo? Yes: I produce some grapes which are coated with gelatine; they have been with sulphuric acid in the exhausted receiver for seven days and nights. On breaking the protecting envelope they are found fresh and juicy; far more so than some from the same bunch which, put by in a cupboard, are now shrivelled and mouldy.

We can now see how it is that isolated rotifers put singly, or in small numbers, on a glass slip, and with thin cover, and allowed to dry, very seldom recover when moistened; for in crawling excitedly over the slide, as they generally do, trying to find more water or protection in their usual refuge—sand and dirt, they part with much of their adhesive covering, the evaporation of the small quantity of water is so rapid that they have no time to settle down quietly as usual while more coating is secreted; they roam about almost to the last minute, when they are overtaken by the drought, shrink hastily into a ball to dry and to perish.

Having shown that the creatures need not of necessity be really dry, however we may endeavour to dry them by moderate continuous heat, or by the so-called complete desiccation of the air-pump, it only remains to demonstrate the fact that they are *not* dry. A dry trough rich in philodines was kept in the vacuum for three days with acid, then removed, and at once transferred to a long dry test-tube, and the closed end immersed in boiling water for two hours; then with a pencil and the aid of a hand-magnifier, the pink contracted bodies were picked out, put on a ledged glass slip, and covered with a thin circle. They were then examined under a quarter-inch, and seen to be highly refractive, and like rubies; with increasing pressure the tough yielding balls at last burst, and after a few minutes, and repeated squeezings, emitted two distinct fluids, one watery, which diffused through the broken mass, and another oily—a yellow-pink fluid in minute smears—changing to drops when water was run under the cover. In another case oil was applied to the edge of the cover, run in, and the yellow fluid was at once dissolved.

From these experiments, and from other considerations, it seems only reasonable to conclude that these rotifers become torpid in drying externally; that their vitality, though intact, is maintained only by the slow consumption of their bodies. It is true they are in air-tight cases; but experiment shows that they can, even when most active, bear the absence of air with impunity for a considerable time. I have had them in water in the exhausted receiver of the pump (of course without acid) for two days without their dying or showing much signs of distress.

As regards the "unlimited duration" of time they may be kept alive in their dry cases, authorities differ: my experience, founded on unsuccessful attempts to revive three samples of dust after five years' drought, two after three years, and one after one year, leads me to rate their longevity within moderate limits; but there is evidence of their revival after four years' torpor.

In conclusion, I will quote the great Ehrenberg, who without these experiments, in spite of apparently overwhelming evidence to the contrary—evidence considered up to the present time as perfectly conclusive—and led only by analogy and reason, arrived at the same results that I have now the honour to place, with proofs, before you:—"Whenever these creatures are completely desiccated, life can never again be restored. In this respect the Rotifera exactly correspond with larger animals; like them, they may continue in a lethargic and motionless condition; but there will be going on within them a wasting away of the body, equivalent to so much nourishment from without as would be needed for the sustentation of life."

II.—*On Agchisteus plumosus* (*Parfitt*). By E. Parfitt.

(*Read before the* Royal Microscopical Society, *April* 2, 1873.)

Plate XV. (Upper portion).

The accompanying sketch is one of a very remarkable animal I met with in the mucous matter surrounding *Hæmatococcus binalis* (Hassell). I do not remember to have seen anything before like this. At first sight it has something of the appearance of a large rotifer, being transparent, white, and showing very clearly the pale yellowish stomach or intestine; as this vessel seems to combine both, the contraction would appear to divide the stomach from the intestine proper, although I did not observe any valve separating or dividing the two; but I presume the siphon-like bend and contraction would perhaps answer the purpose to an animal like this.

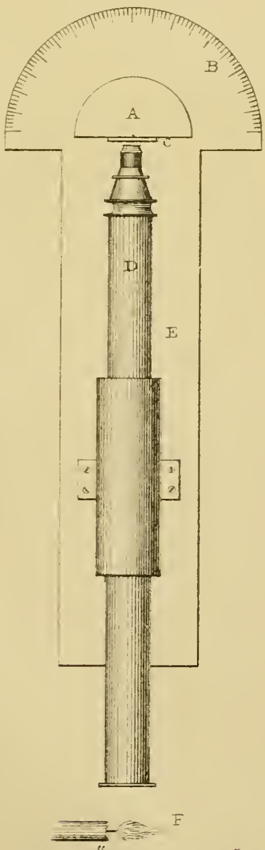
This creature is provided with a double coating, an ectoderm and an endoderm; the outer is wrinkled into transverse folds, with many small protuberances arising therefrom; out of each of these springs a beautiful plumose bristle or spine; by the side of each of these springs a short spine, bearing at its apex a number of fine bristles, reminding one of a little aspergilliform brush. On the anterior part of the body and placed somewhat laterally are six bundles of simple slightly-curved spines or spinetts; and below these, on the posterior half of the body, are five rather long slightly-curved furcate spines directed backwards.

The oral aperture is lateral and inferior. This creature progresses by contracting its body, and with the assistance of the spines very similar to the progress of an Annelid. I could not discover any rings or annulations, the nearest approach to this is the folds of the dermal coat.

Viewing this animal as a whole, it is from the form and disposition of the spines very closely related to the Annelids, but at what position to approach this group to connect them I do not see my way clear except through *Chætogaster*. On the other hand, the simplicity of its interior organization seems to prevent a very close alliance. Although the general appearance is that of a large rotifer, and which by some are considered to approach very closely, if not quite, to connect the Rotifera with the Annelids, this creature does not quite fill the requirements of that position to connect them at this point; the position of its mouth and the simplicity of its internal organization precludes it. At the same time I think there can be little doubt but that its nearest allies are the Annelids. I have named it provisionally *Agchisteus plumosus*, presuming it to be next of kin to the Annelids, and *plumosus* from the beautiful plumous spines.

I can say nothing as to its habits, except the finding of it in the

Âgchisteus plumosus.



Mr. Tolles's "Balsam angle" apparatus.

W. West & Co. lith.

mucous surrounding the Hæmatococcus, and that its intestine contained same greenish granulose matter, from which I presumed that it fed on the plasma of the Algæ.

It is four or five years ago since I found this, and made the sketch of it, and I was then in hopes of finding more specimens, but have been disappointed, and I am now sorry I did not keep the specimen, as I might have dried off the creature on a slip of glass, and so preserved the spines *in situ*.

The following letter was addressed by the author to Mr. Stewart in answer to one suggesting the possibility of some mistake as to the mouth of the animal:—

March 22, 1873.

Dear Mr. Stewart,—I thank you for your suggestion, as regards the mouth of the animal. I had thought so myself, that the mouth ought to be inferior, or beneath; but on studying the movements of the creature, I could not persuade myself that it was so. At the same time I would not insist upon it that it is not inferior; for a creature that is more or less cylindrical, or, perhaps, clavate, would be more in accordance with its form. One could scarcely insist upon the one or the other. At the same time I have stated what I believe to be the fact.

As regards the spines, there are two lateral rows of six fascicles; although I could not show them all, as it would make confusion with so many; but I have stated this, and shown three fascicles, to show their arrangement and position.

You are at full liberty to read the paper at the next meeting of your Society; it might probably elicit some remarks worth recording.

I am, my dear Sir, yours truly,

Edward Parfitt.

C. Stewart, Esq.

III.—*An Apparatus for Obtaining the "Balsam" Angle of any Objective.* By Robert B. Tolles, U. S. America.

Plate XV. (Lower portion).

[The following two communications, though intended by the author to appear in different numbers of this Journal, have reached the Editor simultaneously. They are therefore published in the same number of the Journal.—Ed. 'M. M. J.']

In reference to Mr. Wenham's comments on my last communication there is just a word to add.

I am not averse to the ordeal of a test of balsam angle in London. I am not afflicted with Anglophobia; never have before, I am sure, been suspected of that.

I impugn, not the verdict, nor the testing, so far as that trial went. At air angle of 145° I get the same results, doubtless.

My method was Mr. Wenham's tank-plan, as stated. I have since used quite extensively a different method, putting the light down through the microscope tube, the cone of light being measured as emergent from the front of the objective, the object also in position, in balsam, under covering glass, as in practice in use of the compound microscope, and under identical circumstances, and in view at option through the eye-piece. Therefore and thus the extreme rays measured for angle can be identified as traversing and giving view of the object. This seems conclusive, and when I describe the apparatus I have no doubt the action of it will be tested in England, and I would gladly entrust such trial to the committee already according me their attention, and by all means including Mr. Wenham. As a sort of explanation I am bound to add that I am almost certain no English objective will be found to go above 83° or 85° of balsam angle, which by the method I allude to (which I doubt not will be accepted), those of mine of *compound fronts*, will reach 90°, at all events in most cases, *i. e.* when the air angle is 175° or upwards.

More than this, the sort fairly denominated in your heading of my article, "Peculiar Objectives," shall, in balsam angle, exceed 100°, and range above that according to the intention in their construction.

As yet I have done nothing about any further attestation here, and when I do so it will be without knowledge of the opinions of the gentlemen I shall call upon, or care either about their partisanship of whatever theory.

Respectfully yours,

Robert B. Tolles.

P.S.—Since writing the above I have tested the angle of the objective measured in London, and find the air angle at *open point*, *i. e.* for *uncovered* objects, 145°. By the new method hinted of above, the balsam angle is 77°, less by 2° than Mr. Wenham's trial made it.

At closest, to the extent of one-half of its whole adjustment, I now get 90° in balsam instead of 93° as before. These discrepancies are, I am confident, chargeable to the tank method which I before used, and which I used *because* it was *not* my method.

I now use a semi-cylinder of crown glass, reading the angle on the cylindrical surface, where the rays emerge. Of course the cylindrical surface has a non-polished portion to facilitate the true reading. I will send a description of the apparatus, but not in time to appear with this note.

Boston, *March* 19, 1873.

I conclude to send a sketch of the apparatus I have used to get the real balsam angle actually available in use of whatever objective.

The candle-flame F (Plate XV., lower portion) shows the position in which the apparatus is seen in the figure.

B and E, the base of the apparatus. B is a sector of 180°. A is a semi-cylinder of crown glass closely to the refractive index of balsam, and having an elevation above the base B, beyond the axis of the tube and objective D. A film of covering glass is represented at C.

As the light from the candle-flame F proceeds down the tube and emerges at the front of the objective, and traverses the balsam-mounted object at C, it will have divergence and reach the cylindrical surface of the glass half-cylinder, emerging without sensible refraction.

By means of a proper shutter swinging over the surface-cylindrical of A, the light can be admitted from the outside of it, and by means of shutter the exact limits of angle be found, while the observer has the object in view through the eye-piece and objective. That this can be utilized as a condenser, with peculiar advantages, is evident enough.

The rude drawing will answer present purposes, but I hope to communicate explicit results hereafter. But I desire to state here that the results given in my table of measurements of balsam angle are substantially verified by this method of measure.

40, Hanover Street, Room 30, Boston,
March 21, 1873.

P.S.—The author adds, "the drawing is now to scale of 0·65 to 1 in.," giving at the same time permission for its reduction. This permission has been accepted. The sketch is now but $\frac{2}{3}$rds of its original size as sent by Mr. Tolles.

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

By R. Braithwaite, M.D., F.L.S.

Group A, addenda.

6. Sphagnum papillosum Lindberg.

Contrib. ad Fl. Crypt. Asiæ Bor–Or. p. 280 (1872).

Plates XVI. and XVII.

Syn.—*Sph. Cymbifolium* p. p. omnium auctorum.

Dioicous; more or less ochraceous or brown, never tinged with purple; base of stem-leaves and bracts, as also the points of branch leaves, darker brown. Stems more slender than those of *S. cymbifolium,* reddish-brown; *bark composed of* 4 *strata of cells,* the outer perforated by several large foramina, but without fibres, the others with very fine spiral fibres. *Branches* 3–4 *in a fascicle,* 2 *divergent, short, acute;* 1–2 *pendent, appressed to stem, attenuated; cortical cells, with fine spiral fibres and large pores.* Cauline leaves rather rigid, spathulate, minutely fringed at apex, their cells large, empty below, with a few fibres and pores above.

Ramuline leaves closely imbricated, strongly cymbiform-concave, *cucullate at apex, very broadly ovate, obtuse,* cells large, the hyaline filled with spiral fibres and perforated by large pores, *their walls combined with those of the chlorophyll cells, and there covered internally with minute short papillæ; chlorophyll cells central, elliptical.* Male plants more slender, amentula more or less ochraceous, short, ovate, pointed, the bracts broadly ovate, concave, obtuse, the margin minutely fringed toward the apex, the areolation like that of the branch leaves. Fruit not greatly exserted; *peduncular bracts large,* oblong, convolute, plicate, *the cells very large and of two forms; in the lower half of the bract, the central part consists of narrow pleurenchymatous empty cells, with thick walls, the margins and the upper half of the bract of normal porose and*

EXPLANATION OF PLATE XVI.

Sphagnum papillosum.

a.—Female plant. *a* ♂.—Part of male plant.
1.—Part of stem with a branch fascicle.
2.—Catkin of male flowers. 2 *b.*—Bract from same.
3.—Fruit with its peduncle.
4.—Peduncular leaf. 4 *b a.*—Areolation of basal wing of same.
5.—Stem leaf.
6.—Branch leaf. 6 *p.*—Point of same. 6 *x.*—Transverse section of same. 6 *a a.*—Areolation of half of apex of same, expanded under pressure. 6 *c.*—Single cell × 200. 6 *γ.*—Branch leaf of the var. Stenophyllum.
7.—Intermediate leaves from base of a divergent branch.
9 *x.*—Part of transverse section of stem. 9 *c.*—Outer cortical cells of stem.

N.B.—The dotted line in Fig. 4 indicates the parts occupied by the two kinds of cells.

R. Braithwaite del. ad nat.

W. West & Co. sc. & imp.

Sph. papillosum

fibrose hyaline cells like those of the branch leaves. Spores ferruginous.

Var. *β confertum.*

Plants very short, in dense tufts, usually dichotomous; outer cortical cells of stem with 3–1 very large foramina, and usually without fibres. Ramuline leaves, round, deeply cochleariform-concave, obtuse, the apical cells less prominent at the back. Peduncular bracts shorter.

Var. *γ stenophyllum.*

Plants somewhat lurid, short, dense, robust, irregularly branched. Outer cortical cells of stem rectangular, with 3–1 large foramina, and some very fine distant fibres. Branch leaves ovate-oblong, less concave and cucullate, and almost entire above.

Hab., peat bogs in subalpine districts.

Scandinavia, frequent (Lindberg), Germany, Ben Lawers (R. B.), near Penzance (Curnow), Sutton Park, Birmingham (Bagnall), Staveley, Westmoreland (Barnes), *β*. near Penzance (Curnow), *γ*. near Penzance (Curnow), Staveley (Barnes).

This fine species is no doubt quite as common as *Sph. cymbifolium,* from which it may be at once distinguished by its more rigid texture, brown colour, and less elongated branches, with shorter closely imbricated leaves.

In *Sph. cymbifolium* the branches are much more attenuated, the leaves less closely imbricated, more divergent, and more attenuated at the points. The chlorophyll cells also are nearer to the concave surface than to the back of the leaf, and at their point of union with the hyaline the internal surface is quite smooth; the texture of the plant is also remarkably soft. It will be seen that the two varieties of *Sph. papillosum* are quite analogous to those of *Sph. cymbifolium,* and indeed a careful study of the four species referable to this group would lead us to the conclusion that they have all descended from some common type or parent.

7. Sphagnum Austini Sullivant.

In Austin's Musci Appalachiani.

Plate XVII.

Sullivant, Ic. Musc. Supp. 1 ined. No. 2, Lindberg Contrib. ad Fl. Crypt. Asiæ Bor-Or. p. 280 (1872).

Dioicous; much resembling *Sph. papillosum* and the American *Sph. Portoricense,* more or less ochraceous. Stems frequently dichotomous, dark brown, *the bark composed of* 4 *strata of cells,* the outer quadrato-hexagonal without fibres, the inner with very fine fibres and large pores.

Branches closely placed, 3 *in a fascicle,* 2 *divergent, attenuated at points,* 1 *pendent, short, slender, appressed to stem; cortical* *cells with fine spiral fibres.* Cauline leaves lingulate obtuse, minutely fringed at apex, the areolation as in *Sph. cymbifolium. Ramuline leaves closely imbricated, ovate-oblong, concave,* more deeply coloured at apex, which is also less cucullate, but with cells strongly projecting on the back; cells large, the hyaline filled with fibres and having several large foramina. *The chlorophyllose obtusely trigonous, projecting between the hyaline on the concave surface of the leaf. The internal wall of the hyaline cells, where united to the chlorophyllose, densely crested with prominent papillæ.*

Fruit but little exserted; peduncular bracts oblong, convolute, minutely fimbriate at the rounded apex, cells of the lower third, empty, narrow, parenchymatous, above normal, more or less fibrose, with large pores. The adjacent walls transversely striate by the large papillæ. Spores ferruginous.

Hab., swamps. Farrago, Ocean County, New Jersey, United States (Austin). In Europe only found in Sweden, Hunneberg Mountain, Westrogothia, 1859 (Lindberg). Viby, Nerike, 1860 (Zetterstedt), both sterile.

I am indebted to Prof. Lindberg for specimens of this fine sphagnum, which we may reasonably hope will some day be found in Scotland.

EXPLANATION OF PLATE XVII.

Sphagnum Austini.

a.—Female plant, from Austin's collection in the Kew Herbarium.
b.—Barren ditto from Hunneberg.
1.—Part of stem and branch fascicle.
3.—Perichætium and fruit.
4.—Bract from ditto. 4 *c.*—Single cell from middle of same × 400.
5.—Stem leaves.
6.—Leaves of divergent branch. 6 *p.*—Point of ditto. 6 *a a.*—The same, expanded under pressure. 6 *c.*—Cell from middle × 200. 6 *x.*—Section of same.
7.—Basal intermediate leaves.
8.—Leaf of pendent branch.
9 *x.*—Part of section of stem. 9 *c.*—Outer cortical cells. 9 *c′.*—Inner ditto, × 100.
10.—Part of a branch denuded of leaves.

V.—*Binoculars for the Highest Powers.*

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

I have recently made some experiments for this particular application; the result, however, claims no novelty beyond an adaptation of plans already known and tried.

There have been three separate methods by independent inventors, in which the whole aperture with a full field can be

R. Braithwaite del. ad nat.

West & Co. imp

Sph. Austini.

secured in each eye with the highest powers. These have not come into use, because each image is similar, and no stereoscopic appearance obtained, even by plans of illumination all effects of which are identical in each. Beyond the relief afforded in the use of both eyes together nothing is gained; and I have been told by some who have tried this system for difficult test observation, that for perceiving delicate structure they preferred the single tube, rather than endure the slight loss of definition caused by the partially reflected image.

We are thus driven back to the original principle of dividing the pencil of the object-glass, and bringing the separated images into each eye.

I have before demonstrated the reason why the present binoculars do not give a full field with powers from the $\frac{1}{5}$th upwards,* attributable to the distance of the prism from the back lens of the object-glass.

Immediately after this, the late Richard Beck ingeniously adapted my form of prism to some $\frac{1}{8}$ths in the following way. The prism was set just to clear the back lens, on the end of a thin elastic strip or spring fixed by its upper end to the inner tube of the setting. When the spring lay close to the side, the prism was drawn without the pencil from the object-glass. By turning a screw resting on the spring, the prism was pushed forward, so as to bisect the rays. This plan was successful, and gave a clear, full field in each eye-piece. It did not come into extensive use, probably on account of the difficulty of making prisms on such a minute scale—the necessity of having one fitted to each object-glass, and the inaccessible position in which they were situated.

In all forms of binocular prisms, it is important that the bisection should be fine and sharp without waste; as any edge or space at the junction will be like a thread across the centre of an object-glass obscuring its most valuable portion, and in a high power sadly impairing performance—knife edges are therefore preferable. Nachet's equilateral prism is perfect in this respect, and might easily be made as small as requisite; but from the wide angle at which the rays emerge, it could not be placed close to the back lens of any present form of objective.

Finding difficulties in setting any compact form of *reflecting* prisms in the end of a tube sufficiently small to drop down in the setting close behind the lens of a high-power object-glass, I have applied the achromatized refracting prism described by me in a paper read before the Microscopical Society, June 13th, 1860, by which the images from the right and left half of the object-glass are brought into the opposite eye with the true orthoscopic effect. About this time near a dozen microscopes were fitted with these

* 'Quart. Journ. of Micro. Science,' April, 1861 (new series).

prisms, which gave a definition that has never been surpassed by any succeeding arrangements. This was abandoned in favour of the present well-known form, as it required a separate pair of bodies with which direct single vision could not be obtained. But as for some special observations separate bodies are still used, I have re-arranged the prism to suit the angle required. If the vision is taken off at right angles to the axis of object-glass by prisms, in the manner first suggested by me in a paper read before the Microscopical Society in May, 1853, and figured in the 'Transactions,' the vision will be erect in one plane. Such was the case in the instrument then described, but the effect was *pseudoscopic*.

The following diagram, Fig. 1, shows an enlarged sectional view of the achromatic separating prism now used. The lower

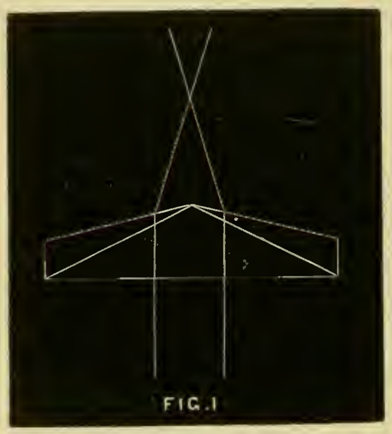


FIG. 1

FIG. 2

prism is of crown glass. Fig. 2 (full size) represents its application to a right-angled or diagonal erecting microscope with horizontal stage.

The reflecting prisms at *a* are the same as formerly described, and are set so that the faces are perpendicular to the emergent ray. Some play should be left between them, to allow for lateral adjustment, in case a point does not fall in the centre of each eye-piece. If the bodies are set at any other angle, the prisms are tilted to correspond. The achromatic separating prism, *b*, is only ·08 in thickness from apex to base, and is turned to a circle ·26 in diameter; it is set in the end of a thin sliding tube, that can be slid down, more or less, within the body of an object-glass as circumstances allow. The small range of motion thus given is not found appreciably to disturb the central position of an object in the eye-pieces. For optical effect, it is of no consequence at what distance the right-angled or erecting prisms are placed; they may be half-way up the bodies and the bend made there, or even employed in diagonal eye-pieces; these being swerved round, would enable two observers to see the same object with *any* form of binocular.

VI.—*Professor Smith's Conspectus of the Diatomaceæ.*

By Professor H. L. Smith.

I notice in the March number of the 'M. M. J.,' which I have just received, the criticism of my "Conspectus," by my friend, Captain Lang. He had already notified me of his intention. Although he deprecates it as an artificial, instead of a natural classification, my own opinion is, that the Diatomaceæ are grouped in a much more natural manner by it than by W. Smith's method, in the 'Synopsis of the British Diatomaceæ,' and which the Captain seems to think more natural! It is quite true I do not call my "Conspectus" by any higher name than an "Artificial Key," for I am very far from presuming, in the present state of our knowledge, to say that I know enough about our little friends, to construct anything better; I fancy, however, that W. Smith's classification will be found quite as artificial, and even less natural.

In having regard, for this purpose, to the "general build," or structure, I am quite sure it is what a student in any other department of natural history would do, sooner than depend upon circumstances or conditions, which the slightest experience would show to be variable; and so Captain Lang will find out one of these days, "when knowledge shall be increased," and he tries the system, that stipitate, tubular, frondose, persistent and non-persistent membrane, are characters absolutely of no value in a natural classification. He knows very well that Staunner's acuta, *e.g.*, grows in filaments; shall we therefore put this species, following W. Smith's more natural classification, along with Melosira in Sub-Tribe V., keeping the others of the genus in Sub-Tribe I.? It seems to me that this is worse than associating Arachnoidiscus with Melosira. But let us see where this so-called natural classification would put the latter. Why, we actually find Arachnoidiscus along with Navicula! in W. Smith's classification, the only point of resemblance being that both are free: for same reason we may associate an ant and an antelope. In my own classification, which is not so wholly artificial but that it is based upon the position and character of the "Raphe," or medium line, into three tribes, Arachnoidiscus, not so very unlike Melosira in S. V., is associated with that genus, and also as to its being free I may mention that chains of three or four individuals are not uncommon. Again, some doubt, both on the part of Mr. Kitton and Captain Lang, appears to exist about the propriety of my union of Triceratium and Amphitetras with Biddulphia (which, by the way, is only in part, as all those forms, without processes, go with the Coscinodisceæ). As Captain Lang makes a point here, that Biddulphia coheres, forming a *zigzag* filament, and the others, a *straight* one, if any (preferring the natural (?) classification of W. Smith, who puts Biddulphia with Diatoma! and Triceratium and Amphitetras with Navicula!!!) I commend to him Mr. Roper's little woodcut, 'Trans. Mic. Soc.,' vol. viii. p. 57, where he will find Triceratium *zigzag*, and Tuffen West's representations of Biddulphia Arinta, Pl. XLV., S.B.D., where he will find this diatom forming a *straight* filament. Endless anomalies and confusions result from considering these evanescent conditions as of value. Mr. Kitton's remarks about Campylodiscus* are reiterated by Captain Lang, though he is not so decided. That Campylodiscus has, in some cases, crossed valves, is true; I think it will be hard to show that it has in all. I shall not shrink from extending a genus, because, forsooth, the number of species will become inconveniently large; to found a new genus on such grounds would be far from natural. Captain Lang is right that the Table of Species will involve a vast amount of labour and trouble. In the next number of the 'Lens' I give the species of the genus Amphora. Unless I am aided by generous sympathy and kind advice, I know I must go hesitatingly, and often erroneously forward. Most thankful shall I be for any original specimens, memoirs, or other assistance, in the arduous task which involves, not only the consideration of every hitherto (so far as I know) named species, but a classification, with descriptions, figures, and index.

I was not before aware of Dr. McDonald's paper in the January number of the 'Annals and Mag.,' 1869, nor have I yet seen it. If I have done him injustice by any seeming neglect, I beg to apologize. As for Dr. Pfitzer's classification on the method of reproduction, I only know it through the abstracts published in the journals; that method of classification I long ago tried and abandoned, as he will be obliged to. With all the enthusiasm of a novice I fancied here was the key to unlock the mysteries of the

* 'Grevillea,' vol. i., p. 63.

diatom world. Alas! the anomalies were so many, and the difficulties so great, that I abandoned it in disgust.

As regards, then, my own Conspectus, I candidly acknowledge it is faulty. Not, however, because I have expunged too many genera, but too few. I have trusted too much to the statements and figures of, especially, the Continental authors, and I am becoming more and more distrustful—not a very pleasant state of things. I am very much obliged to Mr. Kitton and Captain Lang for the kind manner in which they have received my Conspectus, and feel encouraged to go on.

GENEVA, N.Y., *March* 19, 1873.

VII.—*A New Method of Preserving Tumours and certain Urinary Deposits during Transportation.* By JOSEPH G. RICHARDSON, M.D., Microscopist to the Pennsylvania Hospital.

IN the early days of medical microscopy, partly because all revelations of the science were looked upon by most practitioners with suspicion or positive distrust, partly, I presume, on account of real unskilfulness among its students, microscopic examinations were rarely called for, and there was little need of devising plans for securing the portability of specimens. At present, however, when the value of the microscope, not merely as an aid, but even as the most reliable guide for diagnosis, prognosis, and treatment, in many forms of disease, is becoming almost universally recognized, some means of transporting urinary and other deposits, tumours, &c., over long distances, in the unaltered condition, has become a great desideratum. As a contribution towards this important object, I offer to the profession the subjoined method, originally contrived to meet the exigencies of a recent case in my own practice.

The clinical history in this particular instance, being accurately noted by the patient himself, a highly intelligent physician, gives such an exquisite picture of one form of the special renal malady in question, that I am confident most of my auditors will feel some interest in its relation, which is briefly as follows:—

About the 20th of August last, I received a letter from Dr. ——, residing in one of the trans-Mississippi States, informing me that he had forwarded to my address two specimens of deposit let fall from samples of his own urine, which he wished me to examine. In speaking of his condition, he remarked:—

"I am forty years of age, and for the last four years my health and strength have been steadily failing. From my normal weight of 165 pounds, I have declined gradually to 132, at the rate of about eight pounds per annum. My condition at first was attributed to malarial fever, but this cause has not involved the case for more than two years. My symptoms have, during this time, been as follows: great general debility, more or less dyspeptic symptoms, aggravation of rheumatic stiffness and soreness (not pain), from which I have suffered for years. A constant tendency to lose the erect position, and droop in the neck and shoulders. A gradual impairment of virility, amounting for the last six months almost to extinction. In fact, to sum up all in a word, general debility without apparent cause covers the case. I have run the gamut, under the ablest advice, of tonics, stimulants, and nutritious diet, and have taken a sea-voyage of three months' duration, with no appreciable benefit. A few months ago I accidentally discovered—what had never before been suspected—the presence in my urine of albumen in large amount. Its presence is persistent, and its quantity on the increase. The deposit after being precipitated, and allowed twelve hours to settle, just half fills the tube. I have never had any sign of dropsical effusion, but for the last year or more have suffered from periodical attacks of almost uncontrollable diarrhœa, developing themselves with much regularity about every two months, and lasting from three days to as many weeks. The amount of urine secreted is about thirty-two ounces for twenty-four hours, variable in colour, but never turbid when fresh. I micturate a little oftener than when in health, always having to rise at least once during the night; urine much inclined to foam, sp. gr. normal (1021). Prof. ——, of Philadelphia, has pronounced my liver, spleen, and heart healthy, while my vital capacity as indicated by the spirometer is fifteen per cent. above par. . . . May I trespass upon your professional courtesy so far as to ask you to solve the problem, in which I am so much interested, by a microscopic examination? The presence of casts will of course demonstrate renal degeneration; but suppose no casts are found, what then? How in that case would my malady be termed? Certain it is I am suffering from something which, unarrested, must hurry me to a goal not dim or far distant. Please give me your views in reference to diagnosis, prognosis, and treatment. . . . Soliciting an early reply as the greatest favour that could be rendered to one in my present condition of suspense,—it is the *suspense alone* that tries me,—I subscribe myself, &c."

This letter—which, I should mention, Dr. —— with brave unselfishness, and in the spirit of a true philanthropist, has most generously given me permission to make free use of in preparing any paper upon the subject—was accompanied by a small box, enclosing the two samples of urinary deposit. Each specimen was contained in an ordinary two-drachm vial, stopped with a cork that previous to its insertion into the mouth of the bottle had been wrapped in a piece of thin india-rubber, and then, being pressed in to the level of the lip, had been firmly tied down with another small circle of sheet caoutchouc. The ingenious precautions thus employed to prevent leakage were entirely successful, but the long journey of some twelve hundred miles, occupying more than a week of the hot weather with which we were visited during the past summer, had given time enough for complete decomposition to occur, and, although one of the specimens was prepared with a small portion of carbolic acid solution, entire putrefaction had taken place in both before their arrival. The vial which had been merely sealed up gave forth when uncorked a strongly ammoniacal odour, and its deposit was composed only of amorphous granular matter. The other specimen, to which carbolic acid had been added, contained an abundant white coagulum, without any tube-casts, epithelial cells, or leucocytes. Numerous mycelial threads of fungous vegetation presented themselves, and were probably capable of developing in the solution to which carbolic acid had been added, because that acid was deprived of its parasiticidal properties when it combined with the albumen of the urine.

On mentally reviewing the preservative agents at our disposal, and rejecting, of course, alcoholic and arsenical fluids, on account of their power of coagulating albuminous substances, it occurred to me that solution of acetate of potash, whose admirable properties as a preservative menstruum for microscopic objects formed the subject of one of my communications to this Section last year, would best serve our purpose; and I therefore wrote to my correspondent, informing him of the ill-success of his first venture, and requesting him to prepare another specimen by filling a similar small vial with dry acetate of potash, and then pouring in a fluid drachm of the sediment let fall from his morning urine after standing twelve hours in a cool place.

On the 12th of September I again received two samples, one of which had been mixed with the washings of a bottle that had formerly contained acetate of potash, and which comprised the Doctor's entire stock of the salt; the other prepared with a small portion — about twenty drops — of alcohol. Both of these were worthless for microscopic examination; and I therefore procured a two-drachm vial of solid acetate of potash and forwarded it to my patient by return mail, requesting him as before to add to its contents a fluid drachm of his urinary deposit.

This last experiment in the preservation of a urinary sediment for transportation, the fifth of the series it completed, was entirely successful, the preparation reaching me about the 1st of October, not only in such a condition as to show well-defined hyaline, granular, and fatty epithelial casts of the uriniferous tubules in great abundance, but likewise embalming, so to speak, those pathognomonic signs of Bright's disease so perfectly that a drop of the fluid, which I have placed beneath one of the academy's microscopes this evening, exhibits numerous tube-casts with admirable distinctness, even although more than six weeks have now elapsed since this identical sample which I here hold in my hand was prepared for examination, upwards of twelve hundred miles away.

During the past few years I have repeatedly felt the need of some method for preserving specimens of tumours and other pathological formations for microscopic investigation, which might prevent the alterations in the cellular elements which are so apt to occur with the media now in use, and also avoid the difficulty of sending fluids by mail, or the delay and expense attendant upon carriage by express. Since employing the plan described above, for ensuring the portability of specimens of tube-casts, in spite of their exposure to either very high or very low climatic temperatures, I have made a few observations upon the effects of the acetate of potash solution upon morbid growths, and, as a result of my researches, recommend the following method:—

Place a small fragment of any tumour or pathological structure, say a quarter to half an inch square and one-tenth of an inch thick, in a couple of drachms of saturated solution of acetate of potash, and allow it to fully imbibe the fluid by soaking therein for forty-eight hours. The solution referred to is best made by simply pouring half an ounce of rain-water upon one ounce of dry granular acetate of potash, in a clean bottle. When the tissue is thus fully saturated with this saline liquid, remove it by means of a pair of forceps, without much pressure, and insert it in a short piece of india-rubber tubing, or wrap it up carefully in a number of folds of thin sheet rubber or of oiled silk, tying the whole firmly at the ends with strong thread. When thus prepared, specimens can be enclosed with a letter in an ordinary envelope, and sent long distances, doubtless thousands of miles, by mail, without danger, on the one hand, of decomposition, because of the preservative power of the potassic acetate, or, on the other, of desiccation, on account of its exceedingly deliquescent nature.

One very important advantage which this plan has over those in which alcohol or glycerine is employed as a preservative agent, is that the menstruum has little or no effect upon the oil-globules contained in cells. Hence by its aid we are enabled to recognize fatty degeneration in the cellular elements of a tumour, and easily to detect the same metamorphosis in the kidneys from minute oil-drops in the epithelium attached to tube-casts of Bright's disease, under circumstances where specimens preserved in glycerine or alcohol would afford a doubtful or wholly negative result.

Urinary deposits composed of oxalate of lime or of triple phosphate are not, according to my experience, readily preserved in solution of acetate of potash, possibly on account of chemical decompositions which occur. When these crystalline bodies are met with, as is usually the case, in non-albuminous urine, they could probably be best retained in an unaltered state by adding from twenty to thirty per cent. of solution of carbolic acid to the renal secretion in which they are found.—*From the Philadelphia 'Medical Times.'*

NEW BOOKS, WITH SHORT NOTICES.

A Report of Microscopical and Physiological Researches into the Nature of the Agent or Agents producing Cholera. By T. R. Lewis, M.B., and D. D. Cunningham, M.B., Calcutta, Office of the Superintendent of Government Printing, 1872.—Although this work is not especially microscopical, it deals less or more with the histological character of the blood, and more especially with those cases in which Bacteria are present. It seems to us somewhat of a mistake on the authors' parts to have published the volume at all; for though it narrates much excellent experimentation, there is very little in the shape of sound conclusion to be drawn from the researches it narrates. We think, therefore, that the authors would have been wiser to have waited longer before publishing their inquiries. But for all that the book is of considerable interest. In the first place we must state that the entire volume does not exceed a hundred pages of clear readable type, and of this by far the larger portion has no relation to microscopic work. Yet is the histological part of value, especially in regard to those peculiar protoplasmic masses or corpuscles in the blood of cholera-patients, which are admirably figured in the folding plate which precedes the volume. The following account of them we give in the authors' own words, for they are clear, minute, and to the point. We may, however, mention that the specimen of blood was taken from the finger of the patient, and was placed in a wax-cell of a similar character to that described by Berkeley as specially adapted for the observation of fungi. The powers employed were the $\frac{1}{25}$th of Powell and Lealand and $\frac{1}{8}$th and $\frac{1}{12}$th immersion of Ross.

"It now remains," say the authors, "to make a few remarks on the principal points of interest in connection with these observations. The conveniences afforded by a tropical climate for any such series of observations as these are very great, as the temperature as a rule is sufficiently high to secure that the activity of the bioplasts contained in the blood is not too rapidly checked. During a period of frequent observation in the course of the past season, the thermometer ranged from a maximum of 98·2° F. to a minimum of 76·3° F. It is not devoid of interest to remark that the use of immersion objectives involves a disadvantageous depression of temperature, due to evaporation of the film of water which is placed between the lens and the covering glass. The prolonged use of such a lens has frequently appeared in this way to check the activity of the bioplasts in the blood. One of the most important points determined by these observations is the fact, that the blood in cholera is, as an almost invariable rule, free from bacteria, either actual or potential. This is the case as well shortly after death as during life, and holds in regard to every stage of the disease. In one or two cases, a slight development of distinct bacteria has occurred during the course of observation, but this is no more than may occur in the most healthy specimens of blood, and the idea that bacteria are normally present in the blood in cholera may be finally dismissed. It is not improbable that certain of the appearances observed in series of observations, such as those described above, may afford a clue to the origin of such an idea. At an early stage, when the bioplasts are of great fluidity and tenuity, monad-like granules, contained in and moving with them, may be supposed to be free and endowed with independent motion, but this will be found, on prolonged observation, not to be the case, and as the density of the bioplasts increases, the true relations of the granules will appear. At a much later stage, namely, at that of escape of the contents of the cells, patches of molecular matter and scattered granules may result; and finally, when general disintegration of the bioplasts occurs, large sheets and masses of evenly molecular matter may occupy much of the preparation; but these granules, micro-coccoid patches, and molecular flakes, are no new developments, but are clearly traceable to mere disintegrative changes in bodies previously present. The molecular matter so produced, be it scattered or aggregated, undergoes no further development, and shows no motion or any other indication of vitality. The term bacteria is often very vaguely and loosely employed, but it is under no pretext applicable to mere dead particles due to simple disintegration.

"As regards bacteria, so it is in regard to the presence of fungal elements as a normal and constant characteristic of the blood in cholera. There is absolutely nothing in favour of any such view; there is absolutely no evidence of the existence of fungal elements in the blood whilst in the body, and only very rare and clearly accidental development of such bodies after its removal from it. Possibly the most important result to be derived from observations on the blood in cholera, conducted in the manner described above, is the explanation which they are capable of affording of the nature of the bioplastic bodies and cells so abundant in, and so characteristic of, evacuations passed during the course of the disease. We have previously pointed out that such evacuations frequently contain evidences of the escape of blood into the intestines, either by the presence of red corpuscles in greater or less abundance, and occasionally included within the characteristic cells of the discharges, or by that of a more or less pronounced pinkish and sanguineous tinge of the fluids, with the subsequent appearance of blood crystals in them. Now if, as observation has proved, the bioplasts contained in the blood are capable of such activity and multiplication when removed from the body, and with quite abnormal surroundings, it is surely fair to allow them an equal, if not superior capacity, when exuded on the interior surface of the intestines.

"Such bioplasts, in passing through the various changes described above, will come to present every modification of appearance and characters presented by those found in the discharges. In their earlier stages they will correspond with the freely motile amœbæ of the evacuations; when rather older they lose their freedom of motion and show mere feeble changes of form, ultimately becoming motionless and pus-like or rather exudation-like cells, such as are observed in the flakes of lymph in peritonitic and similar effusions, and such cells we know to form the great bulk of those present in perfectly recent choleraic dejections. Whilst in this condition it has been already mentioned that they frequently show one or more distinct nuclear vacuoles in their interior, and they are then identical in aspect with the large mother-cells containing bioplast-masses, previously described in connection with the subject of the evacuations.

"There is one class of bodies in the evacuations, the nature of which has hitherto been peculiarly puzzling and obscure—namely, that of flattened, whitish or pale-yellowish hyaline cells showing no evident structure or contents, but the observations on the changes occurring in the bioplasts of the blood explain the nature of these also, for the empty capsules persisting after the escape of the molecular contents of the pus-like cells, are exactly similar to the hyaline bodies of the evacuations, and unless the actual steps in their formation had been followed, their nature would have been as obscure as that of the latter cells has till now remained. Hyaline vesicles, somewhat resembling these, are more or less generally found in all intestinal discharges, and are probably the result of endosmotic processes acting on the epithelial cells, as was long ago pointed out by Heidenhain and Brücke in connection with appearances observed in healthy epithelium; they may occasionally be seen closely attached to the cells in those very exceptional cases in which epithelium can be detected in choleraic discharges, as well as very frequently in connection with the loose epithelium found in the intestines after death, as figured and described in the last report."

Besides these important observations the authors give some others, in the course of which they discovered evident Vibriones and Bacteria in considerable abundance. There is much in the book to excite the interest of the medical microscopist, and those who are likely to be in the Indian colony should certainly study the researches of Messrs. Lewis and Cunningham.

On a Hæmatozoon inhabiting Human Blood: in relation to Chyluria and other Diseases. By T. R. Lewis, M.B., Calcutta, 1872.—We have in an earlier number given a summary of the contents of this book. So we now merely commend it to the notice of our readers as an excellent little volume.

PROGRESS OF MICROSCOPICAL SCIENCE.

Asterophyllites not the branch of a Calamite.—Professor Williamson, F.R.S., stated, at one of the February meetings of the Manchester Philosophical Society, that the second fossil plant described by Mr. Binney at the meeting of the Society, on January 21st, and of which a notice appeared in the Society's 'Proceedings,' does not belong to some new genus, as Mr. Binney supposed, but is one that he has already described on two or three occasions as being the stem or branch of the well-known genus *Asterophyllites*. In his description of the *Volkmannia Binneyi*, published in the Society's 'Transactions' in 1871, respecting which Professor Williamson showed that it possessed a vascular axis exhibiting a triquetrous transverse section, the author gave his reasons for believing that the strobilus was the fruit of *Asterophyllites.* In a letter addressed to Dr. Sharpey on Nov. 16, 1871, and published in No. 131 of that Society's 'Proceedings,' Professor Williamson gave a brief description of a stem having a similar triangular vascular axis, with lenticularly thickened nodes, and which he again referred to the same verticellate-leaved genus. In a second letter to Dr. Sharpey, dated May 3, 1872, the author confirmed the above conclusions by stating that he had "got an additional number of exquisite examples showing not only the nodes but verticels of the linear leaves so characteristic of the plant. These specimens place the correctness of my previous inference beyond all possibility of doubt, and finally settle the point that asterophyllites is not the branch and foliage of a calamite, but an altogether distinct type of vegetation having an organization peculiarly its own." The author said that he had obtained the plant in almost every stage of its growth, from the youngest twig to the more matured stem, and that the genus would be the subject of his next, or fifth, of the series of memoirs now in course of publication by the Royal Society.

Experiments on the question of Biogenesis.—Dr. William Roberts exhibited, at one of the meetings of the Manchester Philosophical Society (February 4th), some preparations and experiments bearing on the question of biogenesis. He stated that in the last two and a half years he had performed over 300 experiments. His results supported the conclusion that the fungi, monads, and bacteria which make their appearance in boiled organic mixtures, are not due to spontaneous evolution, but arise exclusively under the influence of pre-existing germs or ferments introduced from without. His method of experimenting consisted chiefly in exposing organic solutions and mixtures to a boiling heat in glass flasks whose necks had been previously tightly plugged with cotton wool. Two modifications of the experiment were adopted.

I. In the first modification a 4-ounce flask was employed, and the heat applied directly by means of a gas flame.

II. In the second modification—after the introduction of the materials to be operated on—the elongated neck of the flask was sealed hermetically by the blowpipe above the plug of cotton wool; the flask was then weighted with a collar of lead and immersed in a large can of water; the can was then put on the fire and the water boiled for 20 or 30 minutes. During the process of boiling, the flask was maintained in an upright or semi-upright position, in order to prevent any wetting of the cotton-wool plug by the contents of the flask. When the can was cold the flask was removed, and its neck filed off above the cotton wool, so as to permit free ingress and egress of air.

Flasks thus prepared were maintained at a warmth varying from 50° to 90° Fahr. for long periods—many weeks and months—some in the dark and some exposed to the light with the following results:—

I. Simple filtered infusions of animal or vegetable tissues—a very considerable variety were tried—boiled over the flame for five or ten minutes, in flasks previously plugged with cotton wool, remained permanently barren. This result was absolutely invariable.

II. More complex mixtures—milk, neutralized or alkalized infusions of vegetable and animal tissues, similar albuminous and gelatinous solutions, mixtures containing fragments of animal or vegetable substances or cheese—yielded variable results. In none of them did fungoid growths make their appearance—but monads and bacteria frequently appeared in abundance.

This seemingly contradictory result was inferred to be due to the ineffective application of the heat in the process of direct boiling over a flame. It was found that many of these more complex mixtures frothed excessively when boiled—*brisk* ebullition could not therefore be maintained—particles were spurted about on the sides of the flask, and, in this way, apparently escaped effective exposure to the heat. Even when the boiling was prolonged for 20 or 30 minutes the results were still uncertain—sometimes the flasks remained barren—sometimes they became turbid and swarmed with bacteria.

III. By the second modification of the experiment much more constant results were obtained—the flasks remained almost always permanently barren—and the few exceptions were found to be due to some imperfection in the conduct of the experiment. No exceptions occurred with milk, nor with substances, however complex, which were in actual solution, but when considerable pieces of vegetable or animal substances were introduced into the flasks, bacteria and monads with putrefactive changes occasionally made their appearance in abundance. In these exceptional cases, when the experiments were repeated with the pieces finely comminuted, or introduced in some other way more favourable to the diffusion of the heat, the flasks remained permanently barren.

Dr. Roberts called attention to the crucial significance of experiments on this subject made in flasks whose necks are plugged with cotton wool. A plug of cotton wool acts as an absolutely impervious filter to the solid particles of the atmosphere, while it permits a free passage to the gaseous constituents.

When one of these experiments is effectively performed, the fluid or mixture in the flask may be exposed to the full influence of light, of warmth, and of air, and yet it remains permanently barren. As slow evaporation takes place the liquid passes through all grades of concentration, possibly chemical changes of various kinds take place within it, and still no organic growth makes its appearance for months and even years; but if the plug of cotton wool be withdrawn for a few minutes, or a single drop of any natural water, however pure and well filtered, be introduced, then all is changed—in a few days the clear solution becomes turbid from bacteria and monads, or a mass of mildew covers its surface and soon half fills the flask.

In the face of these experiments it was impossible to doubt that the biogenic power of the atmosphere resides in its dust, and not in its gaseous ingredients; but as to the exact nature of that biogenic power—whether it be a specific germ or a ferment—no sufficient evidence has yet been adduced. Dr. Roberts did not find that diminished pressure of the atmosphere, obtained by sealing flasks hermetically in ebullition, after the mode suggested by Dr. Bastian, materially affected the results. At the conclusion of the paper Dr. R. Angus Smith, F.R.S., said that he was glad to see such uniformity of results. His own experiments, which were very numerous on a similar point, were made differently, but were without exception proving the same. As to the name of the substances in the air, he preferred *germ:* it involved no theory. A germ may be considered that which germinates. *Dust* is an equivocal expression, which may cause a popular error. *Polarity* introduces a theory which is so entirely without basis that in our present state of knowledge we may call the inference it presupposes decidedly false.

On the Origin of Bacteria, and on their relation to the Process of Putrefaction.—Dr. Charlton Bastian lately read the following paper before the Royal Society: *—He says that in his now celebrated memoir of 1862, M. Pasteur asserted and claimed to have proved (1) that the putrefaction occurring in certain previously boiled fluids after exposure to the air was due to the contamination of the fluids by *Bacteria*, or their germs, which had before existed in the atmosphere, and (2) that all the organisms found in such fluids have been derived more or less immediately from the reproduction of germs which formerly existed in the atmosphere. "The results of a long series of experiments have convinced me that both these views are untenable. In the first place, it can be easily shown that living *Bacteria*, or their germs, exist very sparingly in the atmosphere, and that solutions capable of putrefying are not commonly infected from this source. It has now been very definitely ascertained that certain fluids exist which, after they have been boiled, are incapable of giving birth to *Bacteria*, although they continue to be quite suitable for the support and active multiplication of any such organisms as may have been purposely added to them. Amongst such fluids I may name that now commonly known as 'Pasteur's solution,' and also one which I have myself more commonly used, consisting of a simple aqueous solution of neutral ammonic tartrate and neutral sodic sulphate.† When portions of either of these fluids are boiled and poured into superheated flasks, they will continue quite clear for many days, or even for weeks—that is to say, although the short and rather narrow neck of the flask remains open, the fluids will not become turbid, and no *Bacteria* are to be discovered when they are submitted to microscopical examination.

"But in order to show that such fluids are still thoroughly favourable media for the multiplication of *Bacteria*, all that is necessary is to bring either of them into contact with a glass rod previously dipped into a fluid containing such organisms. In about thirty-six hours after this has been done (the temperature being about 80° F.), the fluid, which had hitherto remained clear, becomes quite turbid,

* 'Proceedings,' 141, 1873.

† In the proportion of 10 grains of the former, and 3 of the latter, to 1 ounce of distilled water.

and is found, on examination with the microscope, to be swarming with *Bacteria.**

"Facts of the same kind have also been shown by Dr. Burdon Sanderson † to hold good for portions of boiled 'Pasteur's solution.' Air was even drawn through such a fluid daily for a time, and yet it continued free from *Bacteria.*

"Evidence of this kind has already been widely accepted as justifying the conclusion that living *Bacteria* or their germs are either wholly absent from, or, at most, only very sparingly distributed through, the atmosphere. The danger of infection from the atmosphere having thus been got rid of, and shown to be delusive, I am now able to bring forward other evidence tending to show that the first *Bacteria* which appear in many boiled infusions (when they subsequently undergo putrefactive changes), are evolved *de novo* in the fluids themselves. These experiments are, moreover, so simple, and may be so easily repeated, that the evidence which they are capable of supplying lies within the reach of all.

"That boiling the experimental fluid destroys the life of any *Bacteria* or *Bacteria*-germs pre-existing therein is now almost universally admitted; it may, moreover, be easily demonstrated. If a portion of 'Pasteur's solution' be purposely infected with living *Bacteria*, and subsequently boiled for two or three minutes, it will continue (if left in the same flask) clear for an indefinite period; whilst a similarly infected portion of the same fluid, not subsequently boiled, will rapidly become turbid. Precisely similar phenomena occur when we operate with the neutral fluid which I have previously mentioned; and yet M. Pasteur has ventured to assert that the germs of *Bacteria* are not destroyed in neutral or slightly alkaline fluids which have been merely raised to the boiling-point.‡

"Even M. Pasteur, however, admits that the germs of *Bacteria* and other allied organisms are killed in slightly acid fluids which have been boiled for a few minutes; so that there is a perfect unanimity of opinion (amongst those best qualified to judge) as to the destructive effects of a heat of 212° F. upon any *Bacteria* or *Bacteria*-germs which such fluids may contain.

"Taking such a fluid, therefore, in the form of a strong filtered infusion of turnip, we may place it after ebullition in a superheated flask with the assurance that it contains no living organisms. Having ascertained also by our previous experiments with the boiled saline fluids that there is no danger of infection by *Bacteria* from the atmosphere, we may leave the rather narrow mouth of the flask open, as we did in these experiments. But when this is done, the previously clear turnip-infusion invariably becomes turbid in one or two days (the temperature being about 70° F.), owing to the presence of myriads of *Bacteria.*

"Thus if we take two similar flasks, one of which contains a boiled

* The 'Modes of Origin of Lowest Organisms,' 1871, pp. 30, 51.

† Thirteenth Report of the Medical Officer of the Privy Council (1871), p. 59.

‡ How unwarrantable such a conclusion appears to be, I have elsewhere endeavoured to show. See 'Beginnings of Life,' 1872, vol. i. pp. 326–333, 372–399.

'Pasteur's solution,' and the other a boiled turnip-infusion, and if we place them beneath the same bell-jar, it will be found that the first fluid remains clear and free from *Bacteria* for an indefinite period, whilst the second invariably becomes turbid in one or two days.

"What is the explanation of these discordant results? We have a right to infer that all pre-existing life has been destroyed in each of the fluids;* we have proved also that such fluids are not usually infected by *Bacteria* derived from the air; in this very case, in fact, the putrescible saline fluid remains pure, although the organic infusion standing by its side rapidly putrefies. We can only infer, therefore, that whilst the boiled saline solution is quite incapable of engendering *Bacteria*,† such organisms are able to arise *de novo* in the boiled organic infusion.

"Although this inference may be legitimately drawn from such experiments as I have here referred to, fortunately it is confirmed and strengthened by the labours of many investigators who have worked under the influence of much more stringent conditions, and in which closed vessels of various kinds have been employed.‡

"Whilst we may therefore infer (1), that the putrefaction which occurs in many previously boiled fluids when exposed to the air is not due to a contamination by germs derived from the atmosphere, we have also the same right to conclude (2), that in many cases the first organisms which appear in such fluids have arisen *de novo*, rather than by any process of reproduction from pre-existing forms of life.

"Admitting, therefore, that *Bacteria* are ferments capable of initiating putrefactive changes, I am a firm believer also in the existence of not-living ferments under the influence of which putrefactive changes may be initiated in certain fluids—changes which are almost invariably accompanied by a new birth of living particles capable of rapidly developing into *Bacteria*."

Balanoglossus and Tornaria.—The history of these two interesting creatures is fully given in a recently-published memoir of Professor Agassiz's (published from the Memoirs of the Academy of Sciences, on January 14, 1873). In this memoir, says Professor Verrill, who gives an analysis of it in 'Silliman's American Journal' for March, Professor Agassiz gives us a nearly complete history of the development of the larva long known as *Tornaria*, and until recently universally regarded as the larva of an *Echinoderm*, into the very remarkable worm *Balanoglossus*. That *Tornaria* is the larva of this or some allied genus, had been rendered very probable by the observations of Metschnikoff,

* [*Note*, Jan. 31, 1873.]—In 'The Beginnings of Life,' vol. i. p. 332, note 1, I have cited facts strongly tending to show that *Bacteria* are killed in infusions of turnip or of hay, when these have been heated to a temperature of 140° F. They also seem to die at the same temperature in solutions of ammonic tartrate with sodic phosphate.

† See 'Beginnings of Life,' vol. ii. p. 35, and vol. i. p. 463.

‡ See a recent communication by Prof. Burdon Sanderson, in 'Nature,' January 9th, the greater portion of which appeared in 'M. M. J.' for February.

published in 1870, as stated by M. Agassiz, but the evidence was not conclusive, for the complete development and metamorphosis had not been observed. It is, therefore, very gratifying to have this important point settled so soon and so satisfactorily. M. Agassiz gives an excellent account of the structure, both of the larva in its various stages and of the adult *Balanoglossus*, and discusses their relations to other worms. He concludes that when, at last, the true structure of this remarkable larva and its history have been ascertained, its true relations to the larvæ of Annelids are sufficiently clear, while the relations to Echinoderm larvæ are not so close as had been supposed, for the parts are not homologous with those of the latter, although the external resemblance is quite remarkable. Nor does he admit that this worm, either by its structure or mode of development, can be regarded as connecting the Annelids and Echinoderms. "It is undoubtedly the strongest case which could be taken to prove their identity; but when we come carefully to analyze the anatomy of true Echinoderm larvæ, and compare it with that of Tornaria, we find that we leave as wide a gulf as ever between the structure of the Echinoderms and that of the Annuloids." The plates are excellent, and illustrate well both the external appearance and anatomy of the Tornaria stage, the young *Balanoglossus*, and the adult. This worm is of large size when mature, and lives in the sand at low-water mark. It occurs on the sandy shores of southern New England and southward; M. Agassiz has found it at Beverly, Mass., as well as on the shores of Vineyard Sound and at Newport. The writer also has specimens from Naushon Island. M. Agassiz does not mention, and therefore has doubtless overlooked the fact, that Mr. Chas. Girard, just twenty years ago, described a species of *Balanoglossus* from South Carolina, under the name of *Stimpsonia aurantiaca*. It is true that Girard's description was quite imperfect, like all the early descriptions of this singular genus, but no one can doubt that his species was a *Balanoglossus*, and judging from the description, it is most likely identical with the *B. Kowalevskii*, so well described and illustrated by M. Agassiz in the memoir before us.

Histology of the Tympanum.—Mr. John C. Galton, M.A., says in a late number of the 'Medical Record,' that Dr. Rüdinger, Prosector in the Institute of Anatomy, at Munich, has just published some contributions towards the Minute Anatomy of the Tympanic Cavity (*Beiträge zur Histologie des Mittleren Ohres*), illustrated by twelve lithographic plates, each containing, on an average, two large figures. Six chapters deal respectively with the following subjects:—1. Osseous substance and medullary cavity of the auditory ossicles. 2. The annulus cartilagineus and membrana propria, with their relation to the malleus. 3. The processus brevis of the malleus, and its relation to the tympanic membrane. 4. The folds (*Taschenbänder*) of the tympanic membrane, with relation to the malleus. 5. The lodgment of the tympanic membrane in the sulcus tympanicus. 6. Additional observations on the articulation between the auditory ossicles. The chapters are rather short, but in these due reference is made to the published works of previous observers, among which the beautifully illustrated monograph of Professor Gruber, of Vienna, on the tympanic membrane and auditory ossicles (*Anatomisch-physiologische Studien über das Trommelfell und die Gehörknöchelchen*, Wien, 1867), may be commended to all who are studying the histology of the organ of hearing. The figures, though somewhat coarsely executed, are, nevertheless, rendered effective by the employment of brown and grey colours.

Vulpian on the Septic Virus.—Dr. J. B. Sanderson, F.R.S., states in a recent number of the 'Medical Record' that M. Vulpian has communicated to the Society of Biology an account of six experiments on rabbits on the production of septicæmia (*Contribution à l'Etude de la Septicémie*) by the development of bacteria in the blood. He regards the results as confirmatory of those of M. Davaine. In the first experiment, two drops of blood taken after death from a patient affected with gangrene of the lung, produced death in twenty hours. The blood of this animal, taken during life, contained innumerable "granulations" and "rods." In the second experiment, exudation liquid, from the pleura of a guinea-pig infected by inoculation with the blood of the same patient, was used; death occurred in twenty hours. In the third experiment, a couple of drops of a mixture of blood (of the subject of the second experiment) with fifty times its volume of water, were inserted; death followed in twenty-four hours. In the fourth experiment a similar quantity of water containing $\frac{1}{1000}$ of the same blood was used. Death occurred in twenty-three hours. In the fifth experiment, water containing $\frac{1}{1,000,000}$ of the same blood was used. The symptoms were much less marked; death occurred after fifty hours. In the sixth experiment the dilution was increased to $\frac{1}{1,000,000,000}$. The animal was affected with leucocytes, but recovered. It is well worthy of record that, even in liquids diluted a million times, granulations and bacteria could be found by the microscope. They could even be detected in the liquid used in the sixth experiment. All the infected animals displayed what M. Vulpian calls "bacteriæmia." In using the term, he means to imply that the bacteria are the efficient cause of the infection. In most of them there was peritonitis, the exudation liquid containing innumerable bacteria. All the experiments were made on rabbits. Guinea-pigs were tried, but abandoned, because the results were less marked.

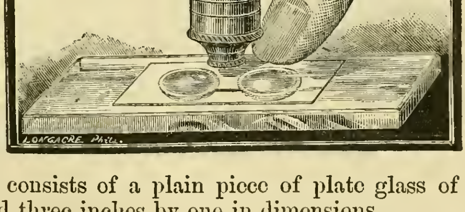
Canella alba and Pomegranate Barks.—These are minutely described as to their microscopic anatomy in a paper in the 'Pharmaceutical Journal' (March 8), by Mr. H. Pocklington. Of *Canella* he says that beginning with the outside of the bark we have first "stellate" cells, analogous to those found in cassia and cinnamon, but somewhat different in size and shape, and are wholly situate on the outer surface of the bark, where they form a tolerably continuous layer of varying thickness, ranging from two to six or eight cells thick. They are porous, the pores being few and large. The successive deposits of thickening matter are not very evident without the use of powerful reagents, and they stain intensely with magenta, prolonged boiling in alcohol not removing the colour entirely. Indigo and logwood solutions do not permanently stain them, but the latter stains the original cell wall of these thickened cells and also the pores, rendering these latter very perceptible. This latter reaction is probably not chemical but mechanical, the minute pores retaining the dye longer than the exposed cell surfaces. The whole of the other tissues of the bark, excepting the resin receptacula, it may be noted, permanently stain with the logwood fluid. The general shape of the thickened cells is ovate, but more or less globose ones are frequent. Within the layers of stellate cells are many layers of thin-walled parenchymatous cells containing various minute granules of starch and other matters, some of which, apparently allied to chlorophyll, stain intensely with magenta. Amongst these cells are distributed very irregularly large receptacula, containing a light yellow coloured oleo-resinoid substance. On removing this it is found that the walls of the containing-cell are thin, imperforate, that they stain intensely with magenta, and do not permanently stain with logwood. That their walls are very thin is shown by their slight action upon a selenite plate by polarized light; for when freed from their contents they scarcely raise or depress the colour of even the *teint sensible* film, the very delicate red-violet. With these parenchyma cells are a few liber bundles, not many. The liber proper forms the lighter internal surface of the B.P. description. It is chiefly remarkable for the great number of sphæraphides arranged linearly among the liber cells, as seen in cross section, and apparently composed of oxalate of lime. These are almost wholly confined to the layer bounding the inner surface of the bark. The liber cells are hollow, little thickened, and frequently contain minute granules of starch and other granular substances. The medullary rays and oblong or sub-cylindrical cells associated with the liber are not very interesting. The cells of the medullary rays are nearly square, with incomplete parietal adhesion, and contain considerable quantities of minute starch granules, spherical or ovate, doubly refractive, and giving a black cross. A large number of cells associated with these square cells, and forming part of the medullary rays, contain, each cell singly, sphæraphides imbedded in a semi-granular substance (probably semi-fluid when the bark is fresh) of apparently a saccharine nature, and unless this be removed by maceration in water, and subsequently alcohol, the polariscope and chemical reactions of the crystals will be but feeble. In conclusion it may be remarked that the structure of *Canella alba* bark is somewhat complex, but not difficult of study to a microscopist of moderate experience, although there are features, such as the nature of the contents of some of the cells, notably of the liber cells and medullary rays, that are almost or quite beyond the reach of present micro-chemical histology. *Granati Radicis Cortex.*—Pomegranate Root Bark.—The cells of this bark are altogether very different from either of the preceding, both as regards size and shape. The external cells are small, sub-globose, and some of them porous. The cells of the layers beneath, excluding certain ligneous cells, are sub-cylindrical, thin-walled, and small in cross outline. Their contents are starch granules, very minute and sphæraphides, of which each cell contains one, imbedded in a substance of which I am unable to determine the nature. These raphides are minute, with feeble optical qualities, and their prismatic constituent crystals not clearly discernible as is usual in this class of crystalline cell contents. Their great number is the first thing one notices in examining the section, and further study shows that the inner layers of cells contain by far the greater number of them, very few cells in this position being without one. The more external cells contain fewer, the outermost cells none. The starch granules are very minute and most numerous in the middle layers. Their polariscope reactions are obscure. The ligneous cells are distributed in twos and threes, are large, porous, much thickened, and the successive layers of thickening deposits very evident without the aid of reagents. In conclusion it may be remarked that the only difficulties in the examination of this bark arise from the minuteness of the cells, and their being filled with various matters that are difficult to remove without altering the general structure. Maceration in very dilute sulphuric acid for a few hours appears to be most effectual in preparing sections for examination, so far as a general view of the size and shape of the cells is concerned.

Pollen of Petasites fragrans.—A writer to 'Science Gossip,' who signs himself Q. F., says that though this is not a British plant, as it was originally introduced to England from Italy in 1806, it is doubtful whether any of our indigenous species abound so much and so early in pollen as *P. fragrans*, or Sweet-scented Butterbur. It grows rampant at Canterbury in deserted gardens, where this Butterbur has been profusely in bloom from Christmas to January. And the pollen-grains are so remarkably beautiful as to afford very delightful microscopic objects even at this season. Each pollen-grain is oval, having a length of $\frac{1}{500}$ of an inch, and a breadth of $\frac{1}{700}$; muricated on the surface like those of so many other compositæ; becoming globular or sub-triangular, with three scars appearing for the passage of the future pollen-tubes, when treated with diluted sulphuric acid. The pollen-grains are so large that they may be very easily examined under an object-glass of half an inch focus.

The Septic Virus.—The French paper, 'Union Medicale,' for January and February of this year, contains an interesting account of the researches of Davaine and others on this subject. A brief sketch of these researches has been sent by Dr. B. Sanderson to the 'Medical Record.' From this we learn that M. Davaine's research is a continuation of his former one on the induction of septicæmia in rabbits by inoculation. As it deals with the fundamental experiments on which he founds his claim to have discovered a new virus, it may be taken, not only as the most recent, but the most complete exposition of his present position in relation to the subject. M. Davaine uses the rabbit as a test for the detection of the septic virus in blood. He finds (as others have done before him) that that animal is specially liable to be affected by the introduction under its skin of blood which has undergone a certain degree of putrefactive change; and he has endeavoured to measure the virulence of such blood by comparative experiments, in which the activity of the liquid in question was judged of by the degree of dilution to which it could be subjected without losing its power of destroying life. The results are stated by him as follows :—Ordinary blood kept at a temperature of about 100° Fahr. becomes so virulent in forty-eight hours, that a trillionth of a drop, injected subcutaneously, is enough to kill a rabbit. 2. The blood of a person affected with certain febrile diseases, particularly typhoid fever, possesses a similar activity. Of the numerous experiments related, in which rabbits were killed with *quasi* infinitesimal doses of typhoid fever blood, it may be useful to reproduce one—the last of the series—as an example of the rest. In a case of typhoid fever, blood was taken at various periods during the progress of the disease; the plan followed being to inject, at each period, blood of two dilutions subcutaneously into two test-animals, of which one received $\frac{1}{1000}$ drop, the other $\frac{1}{1000000}$ drop. When this was done at the eighth day of the disease, the first rabbit survived seven days; the second, twenty-seven days: at the eleventh day, the first rabbit survived seven days; the second, three and a half days: at the sixteenth day, the first survived eleven days; the second, twenty-five days. After complete convalescence the experiment was repeated. Neither of the test rabbits was in the slightest degree affected.

NOTES AND MEMORANDA.

A New Slide for the Microscope.—At a recent meeting of the Optical section of the Franklin Institute, U.S.A., there was described and exhibited in operation a new adjunct to the microscope, designed by Mr. D. S. Holman, a member of the Section, whose life-slide recently attracted so much attention and comment. The new device may be called a current cell, or moist chamber, and is designed to afford the microscopist the opportunity of observing and studying the constitution of the blood and other organic fluids with much greater ease and precision than it has heretofore been found possible to attain. The accompanying illustration will serve to make the description of its construction and operation manifest:—



The slide consists of a plain piece of plate glass of considerable thickness, and three inches by one in dimensions.

This is furnished at equal distances from its centre with two well-polished shallow cavities of circular form, which are connected with each other by one or more capillary channels. These channels are likewise polished, and to permit of a greater field in focussing for their contents, the groove of the tube is made triangular in section, with one side forming a right angle with the surface of the slide, and the other forming with it a very large angle.

The arrangement of the cell, or moist chamber, is as follows :—In order that the current shall be most sensitive, the slide should first be brought nearly to the temperature of the body, by holding it for a few minutes in the hand. A small quantity of the liquid to be examined (blood, for example) is then to be placed in each cell, and a thin cover-glass placed upon them. If held down for a moment with the hands, the air within the cavities will become slightly rarefied, and the cover-glass so firmly held in place by atmospheric pressure as to require no artificial attachment. Upon removal of the fingers, it will be found that the centre of the cavities is occupied with a bubble of air, while a thin annulus about the circumference as well as the connecting capillary tubes are occupied by the fluid. The slide is now ready for inspection. If placed beneath the microscope, and the instrument is focussed upon the connecting channel, a number of corpuscles, red and white, will be observed, but quite quiescent. Let the finger be now approached to the neighbourhood of either cell, when at once a current, more or less rapid, according to its proximity, commences to flow beneath the object-glass; remove the finger, and the direction of the current is reversed. The current is caused by the expansion of the air-bubble in the cell, in consequence of the heat radiated from the finger; and its rapidity may be controlled to a nicety by regulating the proximity of the finger. So sensitive is the apparatus, that even with the highest powers, a corpuscle, granule, or cell, in the field of view, may be leisurely turned over and over in any desirable position, thus affording an unequalled means of observation and study to the microscopist; and while the eye is examining at leisure the behaviour of the objects beneath it, the mind is charmed with the simplicity of the means by which these motions are controlled. In the cell here described, no foreign liquid is added to the material under examination. Moreover, if each cell be entirely filled with liquids of different densities, the cell holding the denser liquid being placed slightly uppermost upon the rotating stage of the microscope, the action of gravity will cause two currents to flow in opposite directions through the communicating channels, and in this way the phenomena of transfusion, crystallization, &c., may be observed for a considerable length of time, which otherwise are brought to sight only with difficulty. We have to thank the Editor of the 'Journal of the Franklin Institute' for the block.

CORRESPONDENCE.

A "Society's" Microscope Stand.

To the Editor of the 'Monthly Microscopical Journal.'

87, Bold Street, Liverpool, *March* 27, 1873.

Sir,—The Royal Microscopical Society has on two occasions performed a useful service to the whole body of microscopists by the appointment of committees to investigate questions of general interest. The last (I think) resulted in the adoption of the universal screw for the attachment of object-glasses.

I beg to suggest another investigation, which, I think, would be well worthy of the Society, and which no other body could so well perform. It is, that the Society should appoint a committee to ascertain and recommend the *best form of stands* for microscopes.

Two forms are in general use, one in which the compound body slides in a groove in a fixed bar, the other in which the compound body is held at the lower end by a transverse bar, which is again attached at right angles to another bar, the whole of these being moved in order to focus the object, except when the fine adjustment suffices. The fitting and position of the fine adjustment necessarily differs in these two forms.

Many instruments in which the optical parts are the best which have been constructed, exhibit, when used with high powers, a tremor which greatly impairs definition, and which, I believe, to be due—not to the imperfection of the workmanship, nor to unavoidable defects—but to a faulty model.

The subject has become increasingly important since the more general introduction of high powers, and their use in rooms where numbers of persons are assembled.

I am, Sir, your obedient servant,

John Abraham,
President of the Liverpool Microscopical Society.

The Battle of the Glasses.

To the Editor of the 'Monthly Microscopical Journal.'

Cincinnati, Ohio, *April* 1, 1873.

Sir,—Two weeks of convalescence have given me time and opportunity to become interested in the "Battle of the Glasses"; and the March number of the 'M. M. J.' coming opportunely to hand, I concluded to try what I could do towards reproducing the appearances of *Lepisma saccharina*, figured in that number, Plate XI., by Mr. Hollich, for Dr. Pigott. Beginning with a Tolles' $\frac{1}{4}$th, 1868, ang. ap., 150° dry, first with the B eye-piece, and then with Tolles' solid eyepiece, the appearance of Fig. 1 was easily produced; and then rotating the stage, so as to put the scale in the position shown in Figs. 2, 3, and 4, Plate XI., there was little more difficulty in producing appearances which I could not distinguish from Figs. 2, 3, and 4, except the note of admiration shown in Fig. 2. *That* I was not able even to glimpse through the whole series of observations, with all the apparatus employed. Then substituting first a Powell and Lealand $\frac{1}{8}$th dry, 1870, the appearances were all a little more distinct by reason of the increased amplification, and so when the Powell and Lealand $\frac{1}{4}$th dry, 1870, was substituted for the $\frac{1}{8}$th. The illumination was by the St. Germain students' lamp, with coal oil, and a $\frac{1}{2}$-inch condenser in all the observations. The different appearances were produced by slightly altering the focus, or the cone adjustment, and other appearances, *bearing an equal appearance of truth*, were produced in the same way. There can be no doubt that all the appearances shown in the figures in Plate XI. may be produced by proper (or improper?) manipulation. But, like Dr. Pigott, I only saw them near the margin, when the oblique and longitudinal ribs cross each other. In the central portion of the scale I did not find them. But the question is not what may be seen? but what is? Which of the various appearances, if any, represents correctly the structure of the scale.

Whilst using the $\frac{1}{16}$th I removed the dry front and substituted the "immersion" front, when—"presto—change"—all my pretty beads were in a moment gone. I had forgotten that the cone glass of the slide was cracked. The water flowed in, and *in place of the beads* were only the oblique ribs as plain, palpable, distinct, and as real as the longitudinal ones. No coaxing could bring back the beads, with any apparatus. The minute transverse, irregular wrinkles or corrugations of the membrane were there, and the water formed little *lacunæ* between them and the ribs. At separated points, when the wrinkles crossed the ribs, they formed little elevations, which, when not properly focussed, formed little isolated bead-like points, as different from *the* beads before seen, as they were from the large ones in Fig. 1, which surely no one would ever mistake for those of Fig. 4.

It would be difficult to persuade the late President of the R. M. S. that a pig's head is formed upon a type altogether different from that of other *Vertebrata.* And it would be none the less so to persuade an entomologist, who is familiar with the appearances and structure of the scales of *Diptera*, *Coleoptera*, and *Lepidoptera*, that those scales are of a typical structure, different from that of the order *Thysanura;* and that in place of the ribs' wrinkles and corrugation of the membrane in the three first-named orders, there is substituted in the last-named order an internal framework of beads.

However, Mr. Editor, I do not profess to be knowing as to these matters, nor capable of throwing light upon them. But like, no doubt, hundreds of others, I want to know what the truth is when I see it through the microscope, and whether there is any infallible rule by which false appearances may be distinguished from the true ones; and I shall have attained my object if some one who knows more and sees better is able to shed some light upon this subject in the mind of a benighted

Tyro.

Desiccation of Rotifers.

To the Editor of the 'Monthly Microscopical Journal.'

April 7, 1873.

Sir,—I have not been able to find the latest papers on the desiccation of rotifers, to which I referred on 2nd April, when Mr. Davis brought the subject before the Royal Microscopical Society, and which, I thought, had appeared within three or four years in 'Comptes Rendus.'

The following extracts and references will, however, show that the question was investigated and settled at an earlier date.

F. A. Pouchet speaks thus in his 'Universe' (English translation, p. 52): "This pretended revival is only the same phenomenon as is exhibited by the snail which, when placed in a dry spot, buries itself in its shell till a little moisture is imparted to it." Mr. Wenham mentioned an interesting instance of this revival on the 2nd, which will be found in your report. Pouchet continues, "It has been maintained that the contracted rotifer is absolutely dry, and consequently dead, but this is not the case. When it is thoroughly dried it never recovers. The prestige of these resurrections was doomed to vanish in the laboratory of the Museum of Natural History at Rouen. Many of my pupils joined with me in bringing back science to rational views. Professor Pennetier, by his memorable labours, proved that the anguillulæ do not revive. M. Tinel did the same with the tardigrades; I myself as far as regards the rotifers."

In a foot-note M. Pouchet says, "Dr. Pennetier, in a series of valuable observations, has proved the complete absurdity of resurrections in general. In his special experiments upon anguillulæ, he noticed that, so far from supporting complete desiccation, they succumbed at a heat of 70° C. (158° F.). See 'Mémoires sur les Rotifères,' *Ann. des Sciences*, 1859; 'Mémoires sur les Tardigrades,' *Ann. des Sciences*, 1859; 'Mémoires sur la Révivification des Rotifères,' *Soc. de Biologie*, 1859; 'Mémoires sur les Anguillules des Toits,' *Soc. de Biologie*, 1859; 'Recherches sur les Anguillules,' *Ann. des Sciences*, 1860; 'De la Révivescence des Animaux dits Resuscitants,' *Actes du Muséum d'Histoire Naturelle de Rouen*, 1862. He gives references to M. Tinel's experiments, *Soc. de Biologie* and *Union Médicale* 1869, and to his own experiments, showing that desiccation carried to 90° C. infallibly kills rotifers.—*Comptes Rendus*, 1859, &c.

In 'L'Origine de la Vie,' par le Docteur George Pennetier, Paris, 1868, in the chapter entitled "Les Prétendus Incombustibles," the author investigates various questions of alleged resurrections of organized beings after exposure to heat sufficient, or more than enough to dry them, and gives various details of opinions, and experiments with rotifers. He quotes Pouchet to the effect that "rotifers and tardigrades may be subjected to a cold of 17° C., then exposed suddenly to 95° C. without losing their property of revival." At p. 157 he figures the apparatus he used to ensure the thorough drying of rotifers—a process which killed them all. He exposed them to a current of dried air at temperatures gradually brought up to 100° C. (212° F.). The rotifers used were first dried by fifteen days' exposure to a burning sun; they were then kept in the shade at a temperature of 18° C. for another fortnight, and subsequently from July to October under the receiver of an air-pump, with quick-lime, showing a pressure of only 3 mm. In October he found half the rotifers thus treated capable of revival, which he accounted for by the quantity of earthy matter mixed up with them, and the average low temperature of the season during their sojourn under the pump. All the tardigrades associated with the rotifers died. The final experiment at complete desiccation was made with two decigrammes of the rotifers and sand that had been in the air-pump.

He cites Claude Bernard, saying, in 1864, "that infusoria ordinarily (*convenablement*) dried lose their vital property, *at least in appearance*, and may remain so for whole years; but when supplied with a little water, they become as lively as before, *provided that a certain degree of desiccation has not been exceeded*."

I have also a reference to a paper by Gavarret, *Ann. des Sci. Nat.* (*Zool.*), vol. xi., 1859, relating to "*dessiccations à froid*" et "*chauffés*," in which he speaks of "the coagulation of hydrated albumen as speedily fatal to most organized beings."

The whole question, in fact, turns upon the amount of desiccation; and no chemist, accustomed to analysis, would consider an organic substance really dry until it had been sufficiently exposed to air that had passed through some desiccating material, and heated to 212° F. or more. No chemist would have expected to dry the rotifers by the process which did not succeed. Professor Miller mentioned 212° to 250° as the temperatures *generally* required to dry organic matter, and when the greatest possible dryness is required, the heat should, as long ago pointed out by Faraday in his 'Chemical Manipulation," only stop short of charring the material.

I remain, Sir, yours obediently,

HENRY J. SLACK.

PROCEEDINGS OF SOCIETIES.

ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *April* 2, 1873.

Charles Brooke, Esq., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

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

Mr. Henry Davis read a paper "On a New *Callidina*: with the Result of Experiments on the Desiccation of Rotifers." The paper was illustrated by a carefully-executed drawing of the new species (*C. vaga*), and by living specimens exhibited under the microscope. (The paper will be found printed at pp. 201–209).

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

Mr. Slack thought that Mr. Davis's paper had been an exceedingly interesting one; but with regard to the observations upon the desiccation of Rotifers, he believed that Mr. Davis was not the first to make them.* If he hunted over the 'Comptes Rendus,' a few years ago, he would have found observations coming to the same conclusions. Mr. Davis was doubtless not aware that such was the case; and in the present day, when so very much is written on every subject, it was almost an impossibility to say what was new. He did not think that the fact of some one else having previously made similar observations detracted in any way from the independent conclusions of Mr. Davis. It was curious that, whilst these Callidinas seemed at first to have been regarded as rarities, Mr. Davis should have found them in such abundance. *Callidina elegans*, discovered by Ehrenberg, was for some time the only species known. He believed that all the known specimens had been females, and it would be very interesting to discover the males, of which at present they knew nothing. The number of teeth seemed to vary; Ehrenberg said that *Callidina elegans* had two jaws, and a number of very fine teeth, whilst another species was described as having only two teeth in each jaw.

Mr. Wenham said that revivification was not confined to Rotifers, although common amongst them. He remembered when in the lower part of Egypt, near the Pyramids, finding some snails upon the ground which seemed quite dried up, so much so that they cut with a knife like a piece of dry cheese-paring. They were found in a place where the heat of the sun was very great, the stones they were on being almost too hot to bear the hand on. He took some of them down with him to the boat, and put them in a plate with some moisture and some slices of cucumber, when he found that in a short time one of them filled out with the water, put forth his horns, and began to feed on the cucumber. He mentioned the circumstance of finding them in such a place where there was very little moisture or vegetation, and rain scarcely ever fell, and Dr. Carpenter thought they must have been brought there by the wind from some more fertile part. In reply to a question from the President, Mr. Wenham said they were a species of Planorbis.

Mr. Davis said that Mr. Slack's remarks as to the species of Callidina were quite correct, but he took exception to the statement that some one had previously shown what he had himself just demonstrated. No doubt, it had been said that Rotifers would not revive after being actually dried up, but it had never been shown that those which did revive had not been actually dry.

Mr. Slack said the subject had been investigated before, and the same conclusions had been arrived at; he would endeavour to find the article to which he had referred.

Mr. Davis said he had proved that, under certain circumstances, the air-pump was not such a perfect desiccator as was generally believed, and he had also shown how it was that these Callidinæ did

* See Mr. Slack's letter, p. 241.

not become entirely dried up when placed under it. He should be glad to see where this had previously been shown.

Mr. Charles Stewart observed that the late Mr. Woodward had mentioned the case of a snail which resembled in some respects what had been related by Mr. Wenham. In this instance the snail had been fixed to a slab in the British Museum for many years, when one day it began to show signs of returning to life, and removed the lid which had closed the opening of the shell, and crawled round as far as its attached end would permit.

The Secretary read a communication from Mr. Parfitt, of Exeter, descriptive of a presumed new animal, to which the name of *Agchisteus plumosus* (*Parfitt*), had been given; the communication will be found printed at pp. 210, 211.

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

Mr. Stewart exhibited a preparation of a rabbit's kidney, showing the epithelial nature of the lining of the Malpighian capsule, a similar squamous epithelium was seen passing down the narrow neck by which the capsule became continuous with the convoluted uriniferous tube.

Mr. B. T. Lowne thought that Mr. Stewart's specimen was quite a demonstration of a fact which was now becoming generally received; the specimen exhibited was a remarkably good one.

A vote of thanks was passed to Mr. Stewart for his observations.

The President said that they were in possession of a paper by their late President, Mr. W. K. Parker, upon the Facial Arches of the Sturgeon, which was announced as one of the papers to be read that evening; unfortunately, however, Mr. Parker had been unavoidably prevented from reaching the meeting in time, and under these circumstances it was thought best to postpone the paper until the next meeting, when he hoped the author would be able to read it to them himself, in which case they would have the advantage of hearing from him any *vivâ voce* observations which he might wish to make in addition to what he had written.

Donations to the Library, from March 5th to April 2nd, 1873:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Popular Science Review	*Editor.*
Bulletin de la Société Botanique de France	*Society.*

Thomas Palmer, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
Assist.-Secretary.

R. L. M. ad nat. del. cam. lucid. W. West & Co. imp.

From an Entozoon encysted in the muscles of a Sheep.

THE

MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1873.

I.—*On an Entozoon with Ova, found encysted in the Muscles of a Sheep.* By R. L. Maddox, M.D., H.F.R.M.S.

(*Read before the* Royal Microscopical Society, *May* 7, 1873.)

Plate XVIII. and Upper Half of XIX.

The following particulars, though scanty and imperfect, may be of interest to Helminthologists as pointing to an unrecognized or at least a very rare occurrence connected with the development of a Cestoid parasite, found with ova, yet encysted, in the muscles of the lower part of the neck of a sheep, and which was removed by myself a few weeks since (April) from the exposed chop part of the joint,

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EXPLANATION OF PLATE XVIII. AND UPPER HALF OF XIX.

Plate XVIII.

Fig. 1.—One half of the small nodulated body found embedded in the muscles of a sheep, × 2.
,, 2.—Thin slice of the integument of the parasite, × 160.
,, 3.—Section of the ovarium, or may be vitelligene organ, compressed with immature ova and so-called calcareous corpuscles, × 160.
,, 4.—Small chitinous plate, × 160.
,, 5.—Hooks from proboscis of head, × 160.
,, 6.—Eight small spines, × 160.
,, 7.—Small narrow or denticulated pieces, × 160.
,, 8.—Larger smooth chitinous pieces, × 160.
,, 9.—Intromittent organ (?), × 160.
,, 10.—So-called calcareous particles, × 362. It is doubtful if the largest may not be an ovum in one stage of development.
,, 11.—A delicate cell filled with pale cells, × 362.
,, 12.—A little granular body or cell, covered with minute spines; ? if one phase of development of ovum, or spermatic corpuscles, × 362.
,, 13.—Two granular cells, supposed to be the earlier stages of the ovum.
,, 14.—The well-formed ovum of the parasite marked as in tænia, × 410; the one below is figured at rather less magnification, and the lower ungranulated cell-like body appears to be similar, but arrested.
,, 15.—Shows the mammillated condition of the integument, × 160.
,, 16.—Immature and mature ova of ordinary *Tænia solium* for comparison with Fig. 14, × 410.
,, 17.—Ideal figure of the entozoon.
,, 18.—Entozoon, nat. size, about.

Plate XIX. (Upper portion).

,, 1.—Head of entozoon, much compressed, mounted in Canada balsam, × 85.

which, in household phraseology, passes by the name of "the best end of the neck of mutton."

Projecting from the cut surface of the joint, but well covered by the muscular structure, was a small nodulated, yellowish-looking mass, hard to the touch, and situated just in the centre of the muscles of the longissimus dorsi, forming the chief fleshy part of the chop. After its removal, much of the surrounding muscular structure was cut away and a section made through the centre of the encysted mass, exposing a small pisiform fibrous-looking body, bounded externally by altered tissue containing small cretaceous nodules, which gave rise to the little nodulations, whilst centrally, was seen an irregularly sinuous linear-looking cavity, itself bounded by a narrow band of hardened tissue, paler than the rest; the space between this, the endocyst (?) and the outer border or ectocyst (?) was occupied by a more or less compact, well-marked fibrous and connective tissue. These boundaries were well distinguished by the differences in their colour, due apparently to the cretification which had happened to the outer one, and to the amount of *minute* so-called calcareous particles, found afterwards, in the inner one. The general appearance of the cut surface of one half is given in Fig. 1, twice the natural size. With a low-power hand-lens, the central cavity was seen to be filled with a soft grumous-looking body, retaining the figure of the boundary, apparently without any definite structure, but which on one of the halves had been slightly dragged above the cut surface on dividing the cyst. This little nodulated mass was enclosed in a distinct capsule, formed of the altered surrounding muscular structure and fibrous tissue, set up, as usual, in self-defence against the extraneous body. The pisiform nodulated mass appeared to be free in this capsule. The neighbouring flesh, as indeed all others of the exposed parts of the joint, looked perfectly healthy and sound, and had been taken from a young sheep.

A thin section was made across one of the halves of the encysted mass and placed with a little distilled water on a slide for examination under the microscope. Besides a quantity of grumous matter and minute calcareous particles and corpuscles, the section furnished a slice of free integument, bordered on one side by what I took to be rugæ. This is seen in Fig. 2, compressed;—a section of the ovarium, or may be vitelligene organ, with immature ova and very many calcareous corpuscles, Fig. 3;—portions of the endocyst, which, in this case, was of some thickness, and, to some slight extent, appeared as if imperfectly laminated; but the cut edge did not curl itself up, as is usual in the hydatid cyst of *Tænia echinococcus*, though this might have been due to the quantity of fine calcareous particles with which the whole boundary seemed loaded, nor could any fine distinct cellular membrane, the ordinary granular layer, be detected, as forming a distinct membrane to the endocyst (?), whilst the outer border or boundary was noticed to be formed chiefly of altered fibrous tissue very much cretified, yet otherwise structureless, and between these was seen very distinct fibrous and connective tissue free of cretaceous particles. Some free ova were also seen. Seeing these particulars, naturally awakened considerable interest; but being otherwise much occupied at the time, the encysted body was flushed with water, and put aside in a mixture of weak glycerine and saturated solution of the acetate of potash, for future further examination.

It remained in this solution for some days, when I had the opportunity of showing it to my friend Dr. Aitken, Professor of Pathology of the Royal Victoria Hospital, Netley, who at once, from his extended knowledge of these subjects, recognized the novelty of the example of an encysted parasite yet furnished with ova. Later it became a question as to the best method of seeking for the entozoon which had furnished these particulars.

Taking the encysted body between the fingers to make fresh sections it slipped as if non-attached from the fibroid capsule, and was found too soft to furnish successful sections; hence I commenced, under a hand-lens, trying to remove the parasite from the central cavity, but finding it break away in small portions, these were taken up on a flattened curved needle, and placed on several slides with a little glycerine, temporarily covered from dust, until the whole was removed. The cavity was now seen to be narrow yet broad, or sole-shaped, and the section had been carried longitudinally through the narrow axis. Small portions from the slides were removed by needles to another slide with glycerine, and covered with a thin cover. Thus the whole was examined very carefully at the magnification of 160 diameters. The first part found of chitinous structure, was the small peculiarly-constructed plate, represented in Fig. 4, and of which only one was seen. To what part of the parasite this may belong is to me quite unknown, though it is suspected to have been from one of the suckers. Continuing the examination very carefully in this manner three hooks were discovered, Fig. 5, and the shaft of a fourth; also eight small spines, one barbed or bifid, Fig. 6, and numerous small denticulated fragments of thin chitinous narrow plates, Fig. 7; likewise several larger structureless horny plates, sharp at one edge and rather thicker at the other, apparently a broken surface, Fig. 8. Having found the hooks, I was anxious to learn more, if possible, of the structure of the parasite. Examining some of the larger portions, they appeared to form parts of the ovarium, or may be the vitelligene organ; they were loaded with highly refractive so-called calcareous corpuscles and immature ova; a small portion of one of these masses is represented in Fig. 3. The outline of these parts on one side corresponded closely before compression to the rugose or mammillated border of the integument, Fig. 2; but uncompressed, they were too dense or opaque to give a fair view of the structure, so that by compression the outline of the figure is altered. The addition of acetic acid rendered the opacity rather less, without any kind of effervescence. Besides the parts of the ovarium, or may be vitelligene organ, a considerable mass was seen much denser than the rest, and apparently lobulated, though the compression that had been used might have caused this. Near to it, and connected to a very thin tissue or membrane (if such it may be termed, for it appeared to be composed of almost transparent irregular non-nucleated little bodies held together by some common adherence), was noticed a small well-defined truncated tube, with two sharp spicules exserted for a short distance beyond the open end, one of these spicules being continued back for the entire length of the little tube; this was supposed to be the intromittent organ, Fig. 9. After long and patient search continued through several days I was rewarded by finding the head of the parasite; it was however so opaque that it seemed very doubtful if its particular features could be recognized, especially as the previous compression had evidently displaced several of the parts of which it consisted. The addition of acetic acid availed little, hence I decided on trying to mount it in Canada balsam, by first dehydrating it with chloroform followed by alcohol, then soaking in absolute alcohol, draining the slide, covering the specimen with oil of cloves, and finally with cold Canada balsam. This enabled me to obtain the view given in Fig. 1, Plate XIX., × 85 diameters. This with what was noticed previously to mounting, showed a double row of hooks and hooklets. Three of the small ones seemed somewhat different in shape and density, and besides these, four suckers were visible, though their exact relationship and structure had been disturbed by the compression used before finally mounting the specimen. A count of the hooks and hooklets gave the number, including the four separate hooks on the slides, as 12 large and 16 small, but as these in the double-crowned tænia are generally, when perfect, alternate, it would perhaps be more correct to fix the number of large ones as equal to the small ones, giving thus 32 in all, though even here it is very possible some of the small ones also may be missing.

Besides the various points enumerated amongst the so-called calcareous corpuscles which had been set free, some of which are figured with their dense covering at 360 diameters in Fig. 10, is one with the granular contents escaping under pressure; also a peculiar delicate cell-like body, enclosing very pale, scarcely perceptible cells in the interior, which apparently was attached to one of these ruptured corpuscles, which even appeared operculated or perhaps broken at one point, and the delicate mass to have advanced a stage towards some developmental condition, Fig. 11. The corpuscles varied considerably in shape and size. Likewise were seen several pale, non-nucleated, globular little masses or cells, without any distinct cell-wall being evident, beset all over with minute bodies; only one is figured, though the others were alike, Fig. 12; but whether these represented spermatic cells or stages in development of the ova I could not determine; others, smaller and very distinctly granular, appeared to be only earlier stages of these bodies or ova, Fig. 13. Likewise well-developed mature ova, rather smaller than the ova of ordinary tænia, were seen, Fig. 14. I had the satisfaction of showing many of the slides to my friend Dr. John Macdonald, F.R.S., of Netley Hospital, who has paid considerable attention to these subjects, and to him I am indebted for pointing out the mammillated condition of the integument in a specimen which had been mounted about four days in glycerine with acetic acid, which I had not previously noticed, Fig. 15. He likewise confirmed the opinion of immature and well-developed ova being present, and he kindly procured for me a specimen of *Tænia solium*, which was handed to me after the drawings had been made, and from which two ova have been figured for comparison, Fig. 16; thus, I think, solving any doubt as to the nature and condition of the little entozoon.

These sundry particulars formed the chief features obtained by the microscopic examination, on which to try and build up anatomically this peculiar parasite, which evidently falls into Dr. Cobbold's fifth order, Cestoda; Sub-class, Anenterelmintha; Class, Helmintha. And I do not think we need hesitate to place it with the Tæniæ, but to which particular species, I feel great diffidence, for it is exceedingly difficult, even with this amount of detail, to arrange the several parts in their exact respective positions; for example, where are the eight short spines to be placed? how was the vitelligene organ or the ovarium situated? where the intromittent organ, centrally or at the side? and where the dense lobulated mass? Again, to what parts are the small denticulated plates and the larger flat ones to be referred? Any attempts to construct an ideal whole would be likely to be very erroneously given; yet it may be as well, looking to the position which the sundry larger portions removed *seemed* to occupy or to bear to each other, to make the attempt, though it must be perfectly understood *as only ideal and subject to correction.* Such is given in outline in Fig. 17. No transverse line was seen marking a distinction between the head and caudal parts; no separation into proglottides; in fact, the creature appears to me a paradox, and had not this name been already employed, I should have pressed it into use for the nomenclature, as certainly this little puzzling entozoon, from the high and unusual point to which its development had been carried in its encysted state or without the necessity of another host, has special claims on our attention, more especially from the presence of ova;

hence it may conveniently, if not systematically correct, be designated *Cysticercus ovipariens*, habitat sheep.

Doubtless sufficient will have been said to awaken the curiosity and stimulate to further research, on the part of those who may be more fortunate than myself, to procure an entire or undamaged specimen from one of our ordinary herbivora.

For the benefit of those who may be unacquainted with the curious phases of these migrating parasites, I venture to briefly run over some of the important points, and in so doing shall borrow largely from Dr. Cobbold's very valuable and learned treatise on Entozoa, at the same time noticing his opinions, which must have great weight from his large experience, yet which are contrary to the facts above mentioned,—the exception, so to say, proving the rule.

The ordinary tapeworm condition, Van Beneden termed the strobila, the joints of which are called proglottides; these sexually mature joints may retain an independent life, and the ova developed in them furnish each the proscolices of Van Beneden—a 6-hooked embryo. According to his view these become the nurses, or scolices, and are represented by the well-known hydatid, or cysticercus. These Cestodes are stated to be bisexual, have alternate generations, and migrate to various hosts for the purpose of preservation of their kind. Dr. Cobbold says the tapeworms are characterized by the possession of a small distinct head, furnished with four simple oval suckers and commonly with a more or less strongly pronounced proboscis placed at the summit of the median line; this is retractile, frequently furnished with double or a single row of chitinous or horny hooks and hooklets; sometimes the rows are greatly augmented, and aid to determine the species. The joints are bisexual. In one of the six species of tapeworms of the dog, which he selects for example, the *Tænia serrata*, the double row of hooks amounts to 48, 24 in each row. In experiments made by administering its separate joints or proglottides to a rabbit, Leuckart found after 24 hours in parts of the venous system, the minute 6-hooked embryos, which had been developed from the ova in the joints, and on the fourth day small cysts in the liver, each containing a minute embryo of $\frac{1}{800}$ of an inch in length. These grow rapidly, and two days later reach the length of $\frac{1}{25}$ of an inch, and after eight or nine days the spots are visible on the liver. Later on, these increase, becoming longer and narrower at one end than the other. The organ attacked sets up a sort of self-defence, as a protection against the presence of the parasite, and produces a closed cavity of connective tissue. The embryos themselves, by differentiation, further obtain structural characters, as epidermis, muscular fibre, &c., and make use of their boring propensities. The anterior end of the embryo becomes turbid in appearance, forming eventually the head of the so-called *Cysticercus pisiformis.* At the anterior end the epidermis is folded inwards, and at this point small calcareous particles appear, which become more abundant; later on, further changes of structure become visible in the little parasite, as vessels circulating a clear fluid by means of cilia beneath the subdermal muscular layers. Those destined not to run to the advanced stage die down, the cyst becomes softened, granular little masses of a mixed chalky and cheesy character are formed, and the remains of the embryo are generally lost by this retrogradation. A little later, some of the embryos escape into the peritoneal cavity, being free of the cyst, and are termed "wandering larvæ," as yet imperfect in their development, which requires to be perfected in another encysted stage. At the end of about the eighth week the receptacle in which the head part is formed, and where the crown of hooks and sucker-pits are situated, is capable of retraction within the receptaculum, forming with the caudal sac the complete development of this stage, and here they degenerate if untransferred to their requisite host. It appears that these imperfect or wandering larvæ may be administered without producing in the intestine the final phase of the *Tænia serrata,* though the administration of the secondly encysted form will produce the Cestoid, the caudal vesicle being quickly destroyed by the digestive function in the dog; the young entozoon rapidly acquires length and segmentation, so that at the end of a fortnight it has become more than 4 inches in length, and Dr. Cobbold puts the period for the sexes to be matured in the segments at the twentieth day or earlier. Dr. Cobbold points out that in the *Cysticercus fasciolaris* found in the liver of rats and mice, the resting larvæ, or scolex, whilst in the mouse, frequently assumes the tænioid condition; but in all such cases the incomplete development of the sexual organs shows that the parasite is still a larva, and has not yet gained access to its proper ultimate host. Dr. Aitken also informs me that this corresponds with his experience. In most cysticerci there is usually no trace whatever of the future reproductive apparatus of the tapeworm.

In reference to Botriocephalus, an entozoon found in man, but the normal geographical distribution of which appears somewhat limited, Dr. Cobbold, after stating Von Siebold's views, "that it is not until the worm reaches the intestine of the ultimate host that its segments acquire sexual completeness," says, and "this is a law, as I have before had occasion to remark, which pervades all classes of parasites." He also notices that the scolex form of entozoon in the sun-fish may even take on the tænioid condition to the extent of 2 feet or more without acquiring sexual organs. Dr. Cobbold is sceptical of the self-impregnation in the proglottides of *Tænia echinococcus,* though Leuckart supports such with his authority.

These tænia are caused in us "by our swallowing the ova containing embryos of *Tænia echinococcus* after they have made their escape from the alimentary canal of the dog," and in their larval condition "this species is probably more injuricus to the human race than all the species of entozoa put together, or to say the least, it is more frequently the immediate cause of death than any other internal parasite." Referring to measled pork, he points out the care the butcher should take in using the same knife which, after cutting measled pork, is "often indiscriminately used to cut up any other meat at hand, and not unfrequently the vesicles are transferred from meat to meat, and from meat to mouth."

Dr. Thudicum, in his able Report on the principal parasitic diseases of the quadrupeds used for food, speaking of the echinococcus and its frequency in sheep, in one of his investigations, several years before his report to the Medical Officer of the Privy Council, published 1864, says, "Amongst the worst classes of sheep sold in Camden Town, I then sometimes could not find a single animal that was entirely free from echinococci." Staff-Surgeon Dr. P. Davidson, late of Victoria Hospital, Netley, in his 'Remarks on the Cestoid, or Tape-like Worms,' speaking of the geographical distribution of *Tænia echinococcus*, says, "In Iceland the statistics of hydatid disease are appalling. According to Schleisner, Eschricht, and Guérault, it is endemic to such an extent that a sixth part of the whole population are afflicted by it." Again, Dr. Thudicum, speaking of the hookless tapeworm: "from the 5-cupped bladder-worm which lives in the calf and heifer, and not in the pig, is the exclusive tænia of Austria Proper (Archduchy)." "Every case of tapeworm of this kind was therefore preceded by the existence in a piece of veal or beef of the hookless 5-cupped bladder-worm, which had escaped cooking, and, being swallowed, was set free in the intestine to grow and effect propagation."

These passages, quoted from such authorities, may well invite the attention of those who may be purposing a visit to the International Exhibition in Vienna this year, by pointing to the risks of partaking of these meats in an imperfectly cooked condition. To those interested in these matters—and it is really a question that widens with man's dispersion—let me recommend the perusal of Dr. Cobbold's work on Entozoa, and Dr. Thudicum's Report, found in the 'Seventh Report of the Medical Officer of the Privy Council, 1864,' published by Eyre and Spottiswoode, London, 1865.

The disposal of the sewage of large or small communities, the patent earth-closet system, the food of swine and dogs, the contamination of streams and water-supplies by parasitic ova, with many other points, all relating to the mitigation of a life-long suffering and premature agonizing death, hence to the well-being of man, to say nothing of the state of the animals from which he derives so much of his daily food, are bound up in the consideration of much of entozoic life. The fearful ravages amongst the inhabitants of Iceland, the occasional outbreaks in other countries, remain as warnings for the adoption of necessary precautions, and from what has been elucidated from the before-named specimen, may, I think, well make the timid shudder at the "underdone chop." Thus "the more we learn of parasitic disease in man, as Dr. Gamgee says, the better we can understand how even the underdone roast beef of old England may prove our poison as well as food."

II.—*On the Development of the Face in the Sturgeon* (*Accipenser sturio*). By W. K. Parker, F.R.S.

(*Read before the* Royal Microscopical Society, *May* 7, 1873.)

Plate XX.

Very remarkable protrusible mouths are to be seen amongst ordinary Osseous Fishes, such as the Dory (*Zeus faber*), and in the species known as *Epibulus insidiator*.* But the Sturgeon belongs to another "Order"—the "Ganoids"—and is, indeed, one of the group farthest from the Osseous Fishes: forms of Ganoids that come much nearer to our ordinary Fishes are to be found in the North American lakes, namely, the Bony Garpike (*Lepidosteus*); and in the *Polypterus* of the Nile.

Yet the mechanism of the mouth and tongue of the Sturgeon comes much nearer to that seen in Osseous Fishes than to the curious *Embryonic* mouth of the Skate and Shark ("Elasmobranchii"). But in the latter group, in the Ganoids and also in the Osseous Fishes ("Teleostei"), the arch of the mandible is loosened from the skull, is confluent with the pterygo-palatine arch, and is swung on the front of the lower end of a huge pier, which forms part of the broken-up arch of the tongue. In some Fishes, as the Pipe-fish (*Fistularia*), and the *Hippocampus* and its allies, this *pier* is of enormous length, and the double arch carried at its extremity is very short; so that while a mouth of this kind is capable of great extension forwards on its hyoid hinge, it is itself very small indeed. Those who have looked at the Sturgeon's head will remember that it has a transverse, *inferior*, thick-lipped mouth, which can be drawn downwards as a short, highly-arched tube: the Fish is a *ground-feeder*; and its mouth is very effective for the purposes of its possessor.

The proper *skull* of the Sturgeon is a huge mass of solid, hyaline cartilage, covered, externally, by large ganoid, bony plates; behind it has the fore-end of the large, persistent notochord entering it, and has several of its unossified vertebræ coalesced with it behind.

Nothing could have thrown any certain light upon the morphological meaning of the parts of the Sturgeon's face, except the study of development; as this has not been possible in the early

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DESCRIPTION OF PLATE XX.

Fig. 1.—Side view of facial arches of a young sturgeon—one foot long.
,, 2.—Lower view of the mouth-roof of ditto.
,, 3.—Inner view of mandible of ditto.
,, 4.—Section of lower part of stylo-hyal with cerato-hyal attached, of ditto.
All these four figures are magnified.
,, 5.—Side view of palate and mandible of an adult sturgeon.

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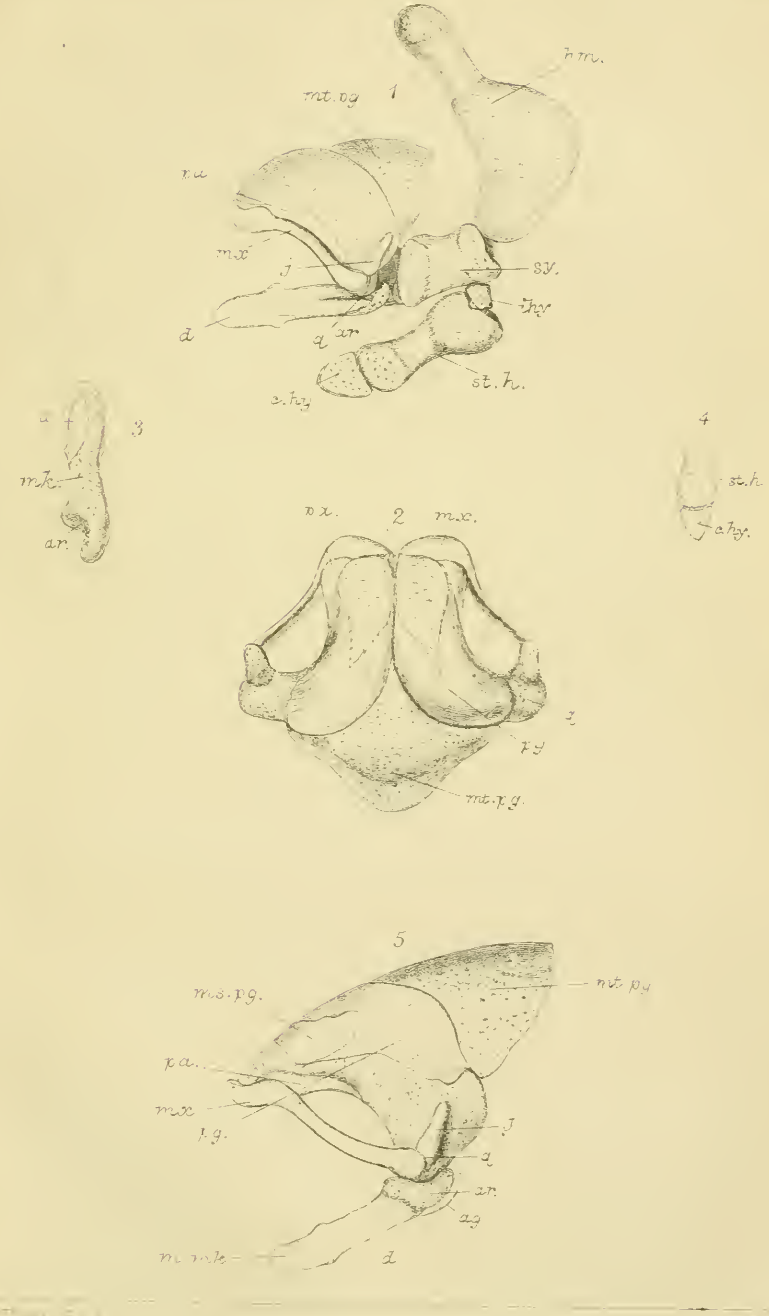
* See Owen, 'Lect. Comp. Anat.,' vol. ii., p. 108, Fig. 37.



Facial arches of Sturgeon.

stages of this Fish I have used the collateral light of the study of these parts in Salmon.

What seems to be a continuation of the axis of the skull in front is, really, the "trabeculæ cranii," or first facial arch; and there is such a copious growth of hyaline cartilage that all the primordial parts are fused into an immense beaked box. In front, above, scattered ganoid plates represent, in a general way, the nasals and præ-maxillaries, and under the conical snout one, two, or three fibrous bones do duty for the specialized "vomer" of the bony Fishes. Farther back, on the under surface, a huge bony splint—the "parasphenoid"—forms a strong and elastic balk to the main part of the skull and the contiguous vertebræ.

Laterally, thin fibrous plates are applied to the dense mass of cartilage, and these represent the "præ-frontals," "orbito-sphenoids," and "ali-sphenoids"; I find these in very young specimens; in the adult they only lie like splints on the cartilage. These plates are sub-cutaneous, or rather sub-mucous. They are formed under the skin of the mouth and palate, and answer to the outer plates, minus the ganoid layer. They are parts of the skeleton of the skin that have begun to yield to the organic attraction of the endo-skeleton, and to be used up in its metamorphic changes.

In a young sturgeon, one foot long, for which I am indebted to Frank Buckland, Esq., who sent it me at the instance of Dr. Murie, I found very much of what is seen in the adult, but not all; yet the metamorphosis of the original parts was almost complete; I *ought* to have specimens from the *roe*. In this specimen, the second præ-oral arch "pterygo-palatine," had formed a very curious connection with the first post-oral ("mandibular"). These two arches being arrested in growth, as compared with the second post-oral ("hyoid"), are so attached to that arch and swung upon it, that they are capable of being protruded far from their original (embryonic) position. Each pterygo-palatine cartilage is a crescentic plate attached to its fellow of the opposite side, anteriorly, by a dense fibrous band. But the posterior part of this plate (Fig. 1, *pa. pg. q.*) belongs to the pier of the mandibular arch—not *all* the "pier," but its lower half. To understand this, let the reader imagine the first condition of the mandibular arch, to be a sigmoid rod; this rod becomes segmented off into two shorter pieces above, and one longer piece below. The upper or "metapterygoid" segment flattens out into a three-cornered piece, and coalesces with its fellow-piece at the mid-line, above the mouth-tube, and thus the lozenge-shape piece is formed, as seen from below, Fig. 2 (*mt. pg.*). The next piece is squarish, and performs a very common morphological feat; namely, its fore-edge becomes completely united to the hinder edge of the "pterygo-palatine" plate (Figs. 1 and 2, *pa. pg. q.*); we thus get the "palato-quadrate," or great "sub-ocular" bar, so familiar to ichthyotomists. And now we find most familiar bones applied to the compound, metamorphosed cartilage. Antero-inferiorly, its edge is further selvaged by a sickle-shaped "palatine" (*pa.*), and *beneath* a large "pterygoid" bony plate appears (Fig. 2, *pg.*), which just peeps from under the edge behind to the outer side (Fig. 1, *pg.*). Farther outwards, a strong, arched bar of bone runs from the fore-part of the palatine territory to the outer face of the quadrate hinge; this is the "maxillary" (Figs. 1 and 2, *mx.*); it is quite normal in shape, and bows outwards so as to leave space for muscular bands. Contrary to rule, it binds strongly on to the quadrate, its posterior end doing duty for quadrato-jugal—a bone I have never seen in Fishes. But the "jugal" or malar is not uncommon in Osseous Fishes, and here it is in the Sturgeon, sitting bolt-upright on the end of the maxillary. The rounded condyle formed by the quadrate (Figs. 1 and 2, *q.*) is received into a scooped joint on the rest of the mandibular arch—the "articulo-Meckelian" bar, or mandible proper.

This massive, somewhat compressed rod has a large angular process (Figs. 1 and 3, *ag.*); a "dentary" bone (*d*) has been formed on its outside, and it has turned over the top of the bar to clamp the inner face to some degree (Fig. 3, *d. mk.*).

In the adult (Fig. 5), which may be well studied in the fine new preparation which Professor Flower has put into the museum of the College of Surgeons,* the "palatines," "maxillaries," and "malars" have not altered much; the "metapterygoid" lozenge keeps free from bony matter, but the huge "pterygoid" plates (*pg.*) have grown over half the upper surface from the inner edge. A sub-oval bone in front of that tract has appeared, which is at once seen to be the familiar "meso-pterygoid" (*ms. pg.*) A plate of bone behind the great "dentary" splints the angle, and is the "os angulare" (Fig. 5, *ag.*). Antero-internally there is a "splenial" plate, and on the left side a rare bony nodule, the "mento-Meckelian," (*m. mk.*), is seen. This is described in my paper on the Frog's Skull,† and is also shown to be an element in the lower jaw of man by Mr. Callender.‡ It is very remarkable that in the tailless Batrachia, and down here amongst the lower ganoids, a bone should turn up which goes to form our especially human chin, which part, if it had not projected, would have left us with very foolish-looking faces. Hitherto, these three types are the only ones that I know of as possessing a special *chin-bone.*

The hyoid arch, or arch of the tongue, is immense, and is chopped up into five pieces on each side. Moreover, these elements are in a very different position from what they occupied at first; and if I had not watched the shuffling of these pieces in the Salmon I

* The solid cartilage in that invaluable preparation has been ingeniously imitated by *wood;* the cartilage itself, in drying, shrinks so as to spoil the form of the preparation.

† 'Phil. Trans.,' 1869, Plates 8 and 9, 'M. Mk.,' pp. 171 and 183.

‡ 'Phil. Trans.,' 1869, Plate 13, Figs. 6 and 7, p. 170.

could not have guessed in the least the original state of these parts in the Sturgeon. Let the reader imagine a second *f*-shaped bar behind the mouth like the first and of the same size; this bar, round at first, flattens out, and then divides into two similar rods, the hinder the slenderer of the two. These get a distance from each other, and apply themselves to the ear-capsule, as if they were primary rods. The foremost has a small nodule segmented off from its lower end, and a larger piece above this becomes separate. Then the hinder piece gradually lets itself down near the lower end of the bar in front, gets the lowest nodule attached to its extremity, fastens itself to the middle segment near its top, and has a nodule of cartilage formed in the suspensory ligament.

Then the upper segment of the anterior bar, the "hyomandibular" or "incus," flattens out, and projects backwards to form a *shoulder*, on which the great "opercular" bony plate is situated; and above this part a sheathing shaft-bone is formed, below the rounded articular head. This piece is attached by a fibrous band to the segment below, which is like a phalangeal bone, with its shaft; this is the "symplectic," which in Osseous Fishes is only separated from the "hyomandibular" by an unossified tract of cartilage. The oblique inferior end of this free phalangiform "symplectic" is bound by ligament to the quadrate region and to the angle of the jaw. The little *secondary* block of cartilage which is formed in the ligament which binds the two hinder to the two front cartilages of the hyoid arch, occurs as a larger rod in Osseous Fishes, and is constant, I believe, in Mammals. In *Man* it is known to anatomists as a little rod running with the tendon of the stapedius muscle towards the "stylo-hyal," and is attached to the neck of the "stapes." It is the "inter-hyal." Below this binding-joint is the true "stylo-hyal"; it is phalangiform, and has enlarged ends like the rickety phalanges of a weak, young, captive Mammal; its shaft-bone surrounds it. The lowest segment is the counterpart of the "lesser horn," "cornu minor" of the human tongue-bone. This "cerato-hyal" is attached to the "stylo-hyal" by ligament (Fig. 4, *st. h.*, *c. h.*) and by ligamentous fibres, without a *joint-cavity* are nearly all the parts attached in the Sturgeon's face, the exceptions being the "glenoid" articulation, or that of the mandible with the "quadrate," and the hingeing of the hyomandibular on the ear-capsule. It may sound strange in the ears of some that the elements that go to make up the mouth of such a creature as the Sturgeon should be boldly named by the very terms used in the description of the human palate, mouth, and throat; but it must be remembered that a knowledge of the true representative elements, in forms so wide apart, has not come of itself to anatomists. There has been long and anxious work, by many skilled workers, to bring this about.

III.—*On Cutting Sections of Animal Tissues for Microscopical Examination.* By Joseph Needham, F.R.M.S., &c., Demonstrator of Histology at the London Hospital Medical College.

(*Read before the* Medical Microscopical Society, *April* 18*th*, 1873.)

Knowing the primary object of this Society to be the diffusion of practical knowledge amongst its members, I shall endeavour to further that object, this evening, by taking into consideration the various methods of cutting sections for microscopical examination.

It is not my intention to enter into the history of the subject, for time will not permit; we will, therefore, proceed at once to the more interesting part, which will be practically demonstrated.

We have three classes of tissues to deal with, each differing in consistence. Bone may be taken as the type of the first or hard; cartilage of the second or intermediate, and kidney of the third or soft.

I. Sections of *hard structures*, as bone, teeth, &c., are to be made by a gradual wearing away of the tissues on two opposite sides, corresponding in position, the planes of these sides being kept parallel to each other till the required thinness is attained; this may be accomplished in two ways, as follows:—

1*st Method.*—Deprive a bone of the ligaments, muscles, and tendons attached to it—in a way that will be presently described—and dry it; then firmly fix it in a vice, and divide it into thin plates by means of a very fine bow-saw, the blade of which should be made of watch-spring and held by screws: place a portion of bone so obtained on a flat surface, and remove the first excess by means of a file; several files may be used for this, commencing with a coarser and finishing with a finer. The section is to be now placed on a good flat hone, and rubbed down on both sides to the required thinness, being kept in contact with the stone by the pressure of the finger or thumb, or fixed on a piece of cork. Although the section is now thin enough, and sufficiently smooth for mounting in Canada balsam, yet when viewed as a dry object, will be found to exhibit

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EXPLANATION OF FIGURES.

Fig. 1.—Section of plano-concave razor.
„ 2.—Section of bi-concave „
„ 3.—Section of flat „
„ 4.—Upper surface and vertical section of Refrigerating Microtome. *a*, brass plate, with hole in centre; *b*, tube fixed to ditto; *c*, plug; *d*, graduated screw; *e*, indicator; *f*, oblong box to contain the freezing mixture; *g*, tap to carry off water.

Drawn to a scale of one-half.

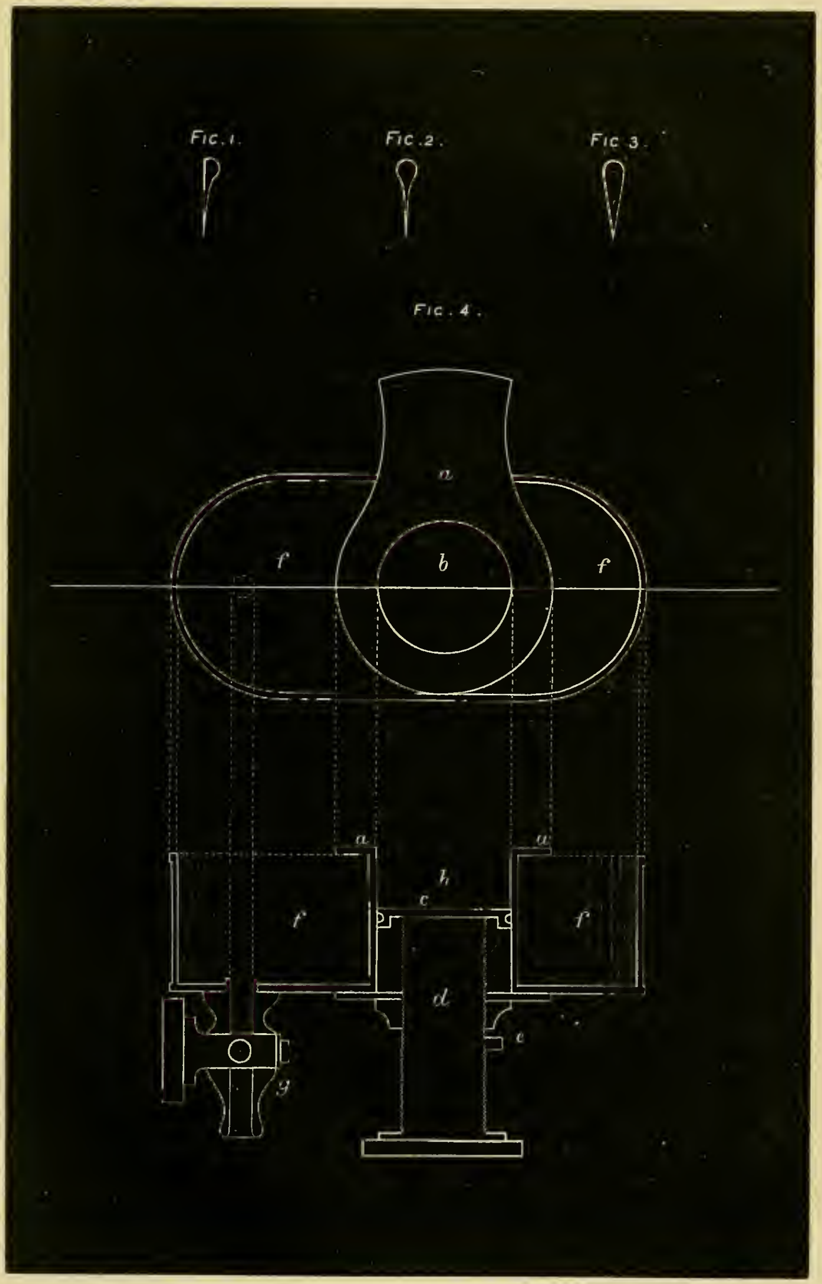


Fig. 1.
Fig. 2.
Fig. 3.
Fig. 4.
a
b
f
f
a
a
h
c
f
f
d
e
g

numberless scratches, giving it a confused appearance; but this may be easily removed by polishing the section on a glass plate, with a little Tripoli or fine emery powder; a piece of leather firmly spread on a flat piece of wood, with a little powder sifted over it, will also answer the same purpose.

2*nd Method.*—The bone is sawn into lamellæ, as in the first method, or by a thin circular rotating saw, then ground down to a moderate thinness on a small grindstone, made to rotate in an ordinary lathe, the stone being kept moistened with water. It should then be further ground on both sides on a fine flat whetstone, also rotating, and finally polished as in the previous instance. The sections obtained by both processes should be cleaned in water, either with a camel's-hair brush, or—which is far preferable—a soft toothbrush, and dried; they are then ready to be put up in Canada balsam or dry. The last method was adopted and used by the late Mr. Carter for many years. A sufficient guarantee of its success is the splendid collection of sections of bones and teeth, made by him, in the possession of the Royal Microscopical Society.

Bones may be prepared either by removing the surrounding tissues with a scalpel, and drying, or, after cleaning in this manner, steeping them for some months in a large quantity of water, which should be changed occasionally to prevent putrefaction. During the maceration they should be scrubbed from time to time with a hard brush; and when perfectly clean, they should receive a final scrubbing in clean water, and dried by exposure to the atmosphere. By simply drying, a bone, saturated with fat, is generally the result, from which a dry, white specimen cannot readily be obtained; by the latter process, however, a perfectly white bone——especially if it be from a dropsical subject—will be the reward for time and patience expended on it.

II. *Tissues of Intermediate Density.*—Under this head may be classed decalcified bone, cartilage, tendon, and many tissues hardened by chromic acid, and other agents.

Sections of bone prepared by the methods already described, although very instructive and beautiful, do not show the soft organic structure, but only the bony framework; if we desire to exhibit the relations existing between the periosteum, blood-vessels, and nerves, it will be necessary to soften the bone; to effect this, after being cleaned from surrounding tissue, it is to be placed in a large quantity of one of the following solutions:—Chromic acid, 3 or 4 per cent.; or a mixture recommended by Professor Rutherford,* consisting of nitric acid, 2 per cent.; chromic acid, 1 per cent. When the softening is carried on in this solution, the tissues assume a bright green colour, due to the decomposition or reduction of the chromic acid into sesquioxide of chromium, Cr_2O_3. Nitric

* Rutherford, No. 45, 'Quarterly Journal of Microscopical Science.'

and hydrochloric acids in a state of extreme dilution have been recommended by Dr. Frey * and Dr. Beale.†

Cartilage, whether hyaline or fibrous, needs no preparation. Glandular, nervous, and muscular tissues, and other soft structures, require hardening in methylated alcohol, solution of chromic acid and its salts, either singly or combined, or in union with sulphate of soda, varying in strength from 2 to $\frac{1}{8}$ per cent., or even less. Saturated aqueous solution of picric or carbazotic acid, strongly recommended by Ranvier;‡ solutions of osmic acid, $\frac{1}{4}$ per cent. (Schultze), bichloride of platinum (Merkel), bichloride of mercury, and chloride of palladium (Schultze). Of all these, chromic acid, its salts, and alcohol are preferable. It is not my province to enter into the subject of softening and hardening, suffice it to say that tissues may be made to assume—by subjecting them to the action of these solutions—a sufficient degree of softness in the first case, or of solidity in the last, to permit of sections being made with an ordinary razor or scalpel. For this purpose, a piece of cartilage, or of any tissue which has been softened or hardened to a density resembling it, may be held in the hand, or placed on a small, flat, plate of wax, being fixed in a convenient position by the middle finger and thumb of the left hand, whilst the thickness of the section is regulated by the nail of the forefinger of the same hand. The razor, which should be wetted with water, spirit, or glycerine, in this case must be held horizontally, with the cutting edge directed downwards; the section is made by drawing it from before, backwards. This method has been in use for some time at the London Hospital, and is universally liked.

For the purpose of hardening, the following agents may also be employed:—Aqueous solution of oxalic acid; drying, or boiling in a mixture composed of creosote, vinegar and water, and then drying; but they cannot be recommended; all have been deservedly superseded by those previously enumerated, and are now entirely relinquished.

III. We will now direct our attention to the preparation of sections from those tissues classed in the third division, which includes nearly all fresh material, from which good preparations cannot be obtained, without the assistance of some special arrangement, *e. g.* Valentine's knife, embedding, or freezing.

(A.) The double-bladed knife invented by Professor Valentine is especially recommended by Dr. Beale.§ It has been made to assume an endless variety of forms, every maker of the instrument modifying it in some way. Four forms only are worthy of notice. (1.) The original consists of two blades, differing in length; the longer is firmly secured in an ivory handle, the shorter is fixed to the longer

* Frey, 'Das Mikroskop und die Mikroskopische Tecknik,' American translation by Dr. Cutler.

† Beale, 'How to Work with the Microscope.'

‡ Frey, *loc. cit.*

§ Beale, *loc. cit.*

by means of a screw, the distance between the blades being regulated by a second screw situated nearer the cutting portion of the knife; the blades are sharp at the point and wide at the base, so that the cutting-edge slants downwards from the point. (2.) The second form is that made by Mr. Matthews: the whole of this knife is constructed of metal, the blades being continuous with the handle; they are short, and of equal breadth throughout; the cutting-edges are convex from point to base. An eye is attached to one blade, and on the other—in a corresponding position—is a longitudinal slot. In bringing the two portions of the knife together, the eye passes through the slot, and they are secured in position by sliding through the eye a spring-side thumb catch. The distance between the blades is regulated by one or two screws. (3.) The next form is that invented by Dr. Maddox, and manufactured by Mr. Baker, of High Holborn. It is a triple-bladed section knife. The circumstances that led to this invention are briefly described by him * as follows:—" I felt the want of some method by which a double section might be cut, so as to present, when removed, the opposite, but contiguous, surfaces of the part through which the section had passed, and which with the ordinary double-bladed knife is quite impossible." This knife overcomes the disadvantage referred to; at the same time by removal of one of the outer blades it is convertible into an ordinary double-bladed knife.

In using these knives a difficulty will be experienced in regulating the blades so that the interval between them may be equidistant throughout; to render this more easily accomplished, Mr. Hawksley, of Blenheim Street, has constructed an improvement on Matthews' form. (4.) A spring is fixed between the two blades, the distance being regulated by one screw, which has a graduated milled head. By this arrangement parallelism is obtained with facility; it also has the additional advantage of indicating and regulating the thickness of the section.

The tissue to be operated on may be placed on cork, on the wax tablet before referred to, or on leather, and held steadily between the fingers and thumb of the left hand, or may be simply retained between the fingers and thumb. The method devised by Dr. Fenwick is remarkably good for membranous structures, *e. g.* a portion of stomach is drawn around the thumb and held in position by the fingers, the section being taken from that part covering the thumb nail. The knife must be drawn by one continuous stroke completely through the tissue, then the cutting-edge slightly turned, so that the section may be severed from the surrounding material. The section is now made, and is to be liberated by opening the blades and gently agitating the knife in water, when

* Maddox, No. 1, 'Monthly Microscopical Journal.'

it will float off; or it may be displaced from the blade with a camel's-hair brush. In removing thus, great care is necessary, otherwise the section will be lacerated. As regards wetting the blades, the same precaution obtains here as in the method described in Section II.

(B.) Finer sections can always be made when the tissue is firmly supported. For this purpose embedding is necessary. Various mixtures have been proposed by different authorities, of which the following is at best an incomplete list:—

Stricker * employs equal parts of white wax and olive oil. Drs. Urban Pritchard and Ferrier † recommend a mixture composed of solid paraffin, five parts; spermaceti, two parts; lard, one part. His ‡ covers the object in pure paraffin, and a mixture of white wax and cocoa-butter is used by Mr. Moseley. §

Of the above mixtures, perhaps the best and cheapest is the wax and oil mass. In its preparation the finest white wax and purest olive oil must be used; the proportion of wax to oil will greatly depend upon the firmness of the tissue to be embedded. The greater the density, the larger will be the amount of wax required, and *vice versâ;* but the mass generally used is made as follows:—Equal parts of the ingredients are placed in a porcelain dish and heated till all the wax has melted, being continually stirred with a glass rod, that the mixture may be well incorporated; the mass is now ready for use. If the material from which sections are to be made has been hardened in aqueous solutions, it must be removed and steeped in ordinary methylated or absolute alcohol, so that the water may be replaced by spirit; this will occupy a longer or shorter time, according to the strength of the alcohol and size of the tissue. When perfectly saturated with spirit, an oblong piece is to be removed from it with a scalpel. A paper box must now be made according to the size of the piece, and about half as long again, the breadth and depth being in proportion; say, for example, the piece to be embedded is 1 in. long, and $\frac{1}{4}$ in. in breadth and depth. We must take a piece of stiff, well-glazed paper, $2\frac{1}{2}$ in. long and $1\frac{1}{2}$ in. broad, which is to be folded on itself for about $\frac{1}{2}$ in. on both sides in the long diameter, then the ends for a similar distance, so that it now appears to be only $\frac{1}{2}$ in. broad and $1\frac{1}{2}$ in. long. Now unfold the sides to half the distance, so that four walls are formed; the triangular pieces projecting from the corners are to be folded on the ends, so that they overlap each other, and, kept in position with a little gum or a pin, our box is now complete.

* Stricker, in Introduction of 'Manual of Human and Comparative Histology,' translated for New Sydenham Society, by H. Power, M.B., &c.

† Pritchard and Rutherford, p. 16, No. 45, 'Quarterly Journal of Microscopical Science,' and p. 382, No. 48, ibid.

‡ Frey, *loc. cit.*

§ Moseley, p. 337, No. 40, 'Quarterly Journal of Microscopical Science.'

It should be placed on a flat piece of cork and filled with the melted mass, sufficiently to cover the piece of tissue; as soon as the wax mixture begins to solidify around the edges of the box, the tissue is to be introduced as follows: *—"A needle is stuck slightly into the end opposite to that from which sections are to be cut, and the bit is plunged into the mass with its long diameter horizontal, and in such a position that the end furthest from the needle is near, but not in contact with, the side of the box, and consequently the other end is at a considerable distance from the side. In this way, although the whole is surrounded with the wax mass, there is a greater thickness around the end into which the needle is stuck, so that the whole can be securely and conveniently held." By passing the needle directly through the tissue and into the cork upon which the box rests, "the operator is saved the trouble of holding the needle till the wax mixture solidifies. In finally withdrawing the needle, the greatest care must be taken to give it a twisting motion, as otherwise, especially if the object is thin, it is apt to be displaced." "If a thin membrane is to be embedded, of such tenuity that a needle could not be introduced without danger of destroying it, the following method may be used:—A box is half filled with the mass, and then, as soon as it begins to solidify, the membrane is applied to the half solid surface; the box is then filled with a thoroughly fused mass, care being taken that it is not too hot." In embedding any of the fatty masses, great care should be taken that the surfaces of the piece are dry previous to immersion in the mass, otherwise the medium will not adhere to it.

Besides the media already mentioned, others, as gum,† and a mixture of gelatine and glycerine, ‡ have been used for some tissues with great success. The first is prepared by making a clear concentrated solution of the pulverized gum acacia. The gelatine mixture is prepared as follows:—Two parts of concentrated solution of isinglass and one part of pure glycerine. It is not necessary to place the tissues in alcohol previous to embedding in these. A paper box or cone having been prepared, it is filled with the mixture—the gum cold, the gelatine hot; the piece of tissue is then thrust into it. The gelatine mass when cold becomes solid. Both are then placed into common alcohol until a sufficient degree of hardness is attained; on releasing them from the paper they are ready for further treatment.

When cavernous structures, as lung-tissue, cochlea, &c., are embedded by the foregoing methods, good sections cannot always be obtained, in consequence of the tissue not being supported within, as well as without; but if it be placed in any one of the melted

* Klein, in 'Handbook for the Physiological Laboratory.'

† Stricker, *loc. cit.*

‡ Klebs in Frey, *loc. cit.*

embedding mixtures, or in the gum mass cold, under the receiver of an air-pump, and exhausted till bubbles cease to come from the tissue, the mass will penetrate into the spaces, previously occupied by air; after solidification has taken place they may be dealt with as an ordinary embedding, and in this way it will be possible to obtain fine preparations.

Professor Quekett used to inject hot tallow through the bronchi into the air cells of an injected lung, which after cooling and drying yielded a splendid mass. All transparent lung injections put up by Mr. Topping are prepared in this way; although this plan answers the purpose very well, it is doubtful if it be even equal to the last.

For making sections of the tissues thus embedded, razors or section knives are used. I have employed for a considerable time the flexible-edged, concave-sided razors made by John Heiffor, and stamped "made for the army"; they are extremely thin for some distance from the edge, and the hollowed-out surface holds plenty of alcohol; this is absolutely necessary to prevent the section sticking to the blade. Mr. Moseley * also strongly recommends them for histological work. Dr. Klein's section knife has one side flat, and the other concave; the blade is eight inches in length. The razors previously referred to are equal and considerably cheaper, being obtainable from any cutler for the small sum of one shilling each. Previous to cutting the tissue embedded in any of the fatty masses, the instrument must be wetted with ordinary spirit, or, still better, absolute alcohol; in which liquid the sections must be placed as soon as made, to clean them from the surrounding material, after which they are ready for staining. If embedded in gum or gelatine, water or glycerine must be used to moisten the knife, and the sections cleaned in water. For lung injected with tallow, the razor is kept wet with turpentine, in which fluid the sections must be immersed; the tallow will readily dissolve, and the sections may be put up at once in Canada balsam, or solution of damma or balsam in benzole.†

(C.) It is often desirable to obtain sections of perfectly fresh tissue, for the purpose of immediate examination, or perhaps for impregnation with some metallic salt. It is obvious that no assistance can be looked for from hardening fluids or embedding, and very little indeed by means of the double-bladed knife. We must have recourse to one of the refrigerating methods. (1.) The process generally adopted is that described by Klein in the 'Handbook for the Physiological Laboratory,' from which the following is quoted:—"A freezing mixture is prepared by introducing alternately small quantities of broken ice, or snow (not so advantageous), and of

* Moseley, *loc. cit.*

† Bastian, p. 96, No. 2, 'Monthly Microscopical Journal.'

finely-powdered salt, into a large vessel, mixing the two ingredients thoroughly after each addition. The object, which must be small, should be cut to an oblong form, and placed on a flat cork, much wider than itself. It must be pinned to this cork at the end opposite that from which the sections are to be cut. In the case of a membrane the object must be folded, and fixed in the same way. The whole is then placed in a platinum crucible, which has been previously plunged into the freezing mixture. The crucible must be at once covered, and a little of the freezing mixture placed on the top of it. The section knife, which must be sharp, is cooled by laying it on ice. As soon as it is ascertained by exploration with a needle that the preparation is firm enough, the knife is handed to an assistant, who wipes it, and holds it in readiness. The cork is then taken out with the forceps, and seized by the fingers of the left hand in such a way that they do not come into contact with the preparation. A succession of sections having been rapidly made, the number varying with the skill of the operator, the cork is replaced in the crucible." This method, perhaps giving very satisfactory results in dexterous hands, seems to be excessively tedious, awkward, and rather primitive, in comparison with that to be next described.

(2.) The best way to obtain sections of fresh, hardened, or softened tissues for immediate examination or further treatment, is, undoubtedly, by freezing in the refrigerating microtome. I refer to Mr. McCarthy's modification of Professor Rutherford's microtome, made by Khrone and Sesseman, of Whitechapel Road. It consists essentially of a brass plate having a hole in the centre; to the under surface a tube is fixed whose bore corresponds to, and is continuous with, the hole; a thin plug is accurately packed in the tube, capable of being moved up and down by means of a graduated screw: external to the tube is an oblong box, through the bottom of which the screw passes into the tube. A small tap communicates with the interior of the box to carry off water, if necessary, as produced by the ice. The plate rests on, and occupies about the middle fourth of two sides of the outer box. The whole is capable of being securely fastened to any table by a second screw. The machine is made of brass, and the sides are padded externally with leather.

The machine is first fastened to a table, a large square piece of flannel being interposed between them, the tap turned on, and the plug forced to the bottom of the tube, after displacing the graduated screw. The pieces of tissues—I say "pieces," because two or more different portions may be cut at the same time, if it be possible to distinguish the sections by the shape or colour from each other; *e.g.* lung and intestine may be readily distinguished by their shape, and easily separated after being cut into sections of extreme tenuity—are placed in the desired position in the tube, which is then filled up with water, the aperture and contents being protected by placing a small piece of oil-skin over the plate. A thin layer of ice, divided into small pieces, is now placed in the box and on the oil-skin, over this is sprinkled a quantity of pulverized salt, another layer of ice is then added, and again more salt, and so on till the refrigerating mixture is piled over the plate; finally, the whole mass, machine included, is covered up with the flannel. After an interval of about twenty minutes from the completion of the operation, the tissues will be sufficiently frozen for cutting. All that is now necessary is to remove the ice mixture *above* the plate and sides of the machine, when the tissue embedded in *ice* will be exposed ready for cutting. By turning the graduated screw—the thickness of the section depending upon the number of turns or part of a turn given to this screw—the frozen mass will be elevated, a razor is placed at a slight angle on the brass plate and drawn obliquely through the mass projecting from the tube; the sections as they are cut can be easily floated off the razor into water. The razors used for this machine must be *flat and kept sharp* by frequent stropping.

Tissues hardened or preserved in spirit must be placed in water, or a very dilute solution of bichromate of potass to withdraw the spirit, otherwise the freezing of it will be retarded, if not entirely prevented.

By this method I have cut large and beautiful sections of *uniform* thickness of softened teeth and bone, of cartilage and tendon, and of hardened and fresh tissues of every description. Further, I may safely say that, if the given directions be rigidly followed, failure will be impossible; moreover, should you give this method a fair trial (a certain amount of experience in this, as in all things, being necessary), I am confident you will readily concur in Stricker's* assertion: "*The simplest and most elegant mode is that of refrigeration.*"

* Stricker, *loc. cit.*

IV.—*Remarks on the Aperture of Object-glasses.* By Assistant-Surgeon J. J. Woodward, U. S. Army. With a Note by F. H. Wenham, V.P.R.M.S.

Plate XIX. (Lower portion).

February 17, 1873, I received a note from Mr. A. B. Tolles of Boston, asking me if I would measure the balsam angle of a $\frac{1}{10}$th objective for him. Having agreed to do so, the objective came to hand before the close of the month. My intention was to measure the angle by the modification of Lister's method proposed by Mr. Wenham,* and afterwards used before a committee of scientific gentlemen in measuring the $\frac{1}{10}$th rashly sent by Mr. Tolles to London for that purpose.† Mr. Tolles, therefore, at my request, supplied a sector and tanks.

Having had some previous experience with the ordinary method of measuring angles of aperture with the sector, I was well aware of the erroneous result likely to be obtained by its use in the case of high angles, but supposed that for the reduced angles to be measured, when the nose of the objective was immersed in water or balsam, it would prove at least as nearly accurate as for similar angles measured in air. I soon found, however, that this was not the case, if the screw collar was fully closed.

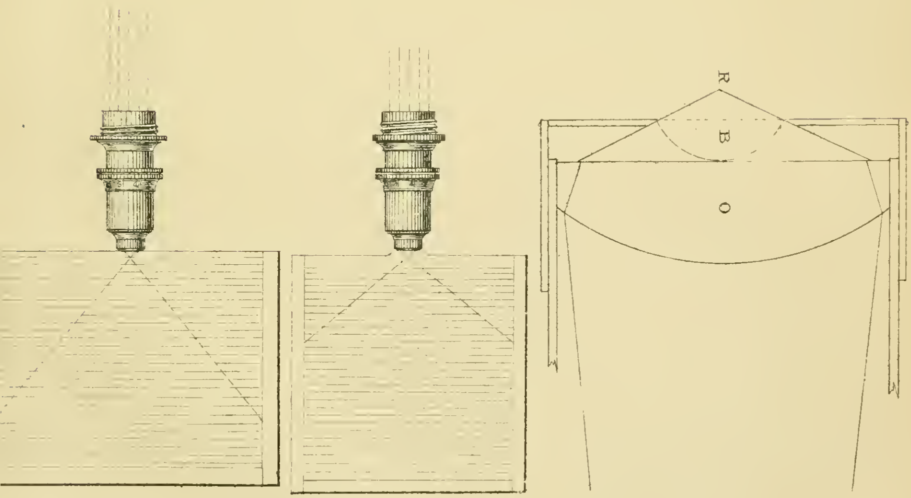
I first measured the $\frac{1}{10}$th sent by Mr. Tolles with the screw collar adjusted to the open point, that is, for uncovered objects. The sector, used precisely as described by Mr. Wenham, gave the angle in air at 160°. When the nose of the objective was immersed in a tank of water, the angle was reduced to 93°, and in fluid balsam to 76°, as nearly as could be read by the sector. When, however, the screw collar was adjusted for the thickest cover through which it could work, that is, when the combination was closed as far as possible, I failed to get definite results either in air, water, or balsam. At no angle was the field of view bisected fairly, bright on one side and dark on the other; but the light gradually faded away in such a manner that no sharp limit could be fixed.

I did not feel at liberty to escape this difficulty as Mr. Wenham did, in measuring the Tolles's $\frac{1}{10}$th sent to London, by setting the screw collar at some more open point ("the best adjustment of a Podura scale," for instance), for I had found by trial that when the lens sent to me was closed as far as its screw collar would go, it would still define very well, provided it was used on an object covered by a correspondingly thick covering glass. Worked at this adjustment the lens, in fact, would show the beads of *Pleurosigma angu-*

* 'Monthly Microscopical Journal,' August, 1872, p. 84.

† Ibid., January, 1873, p. 29.

From an Entozoon encysted in the muscles of a Sheep



Aperture of Object-glasses

R. L. M. ad nat. del. cam. lucid.

W. West & Co imp.

latum or the striæ of *Grammatophora subtilissima* beneath a covering glass one seventy-fifth of an inch thick (by actual measurement). It is fair to say too that this power of working through a thick covering glass with good definition is possessed in a high degree by both the $\frac{1}{18}$th and $\frac{1}{5}$th immersion objectives of Mr. Tolles, described by me in former papers in this Journal. I note, for instance, that both these glasses will work with good definition through covers of the thickness just mentioned, which none of the $\frac{1}{16}$ths, $\frac{1}{10}$ths, or $\frac{1}{8}$ths, and no other high-angled $\frac{1}{5}$th in the Museum collection, will do.*

Having determined, then, that I ought to measure the angle when the combination was closed, and having satisfied myself that the sector was not to be trusted under the circumstances, I devised the following plan, which may be commended for its simplicity and for the definite character of the results.

I had long used an easy mode of measuring the angles of objectives in air, which is, in fact, a modification of the plan of Dr. Robinson, so justly commended by Mr. Wenham.† I screw the objective into a tube which pierces the shutter of my dark room, the back of the objective being towards the light, and I throw through it, by means of a solar mirror, a parallel pencil of sunlight, which, of course, is brought to a focus in front of the lens and crosses, forming a cone of light. By adjusting a white cardboard protractor horizontally in the middle of the cone with its centre at the visible focus, I measure at once, and without the necessity of any calculation, such as was proposed by Dr. Robinson, the angle of the pencil which crosses at the principal focus; and this angle, as Dr. Robinson has correctly shown, is not materially greater than the angle which would be formed if the light radiated from the conjugate focus used to obtain distinct vision with the eye-piece at the extremity of the microscope body.

To measure the angle in balsam on the same principle, I simply made a thin tank rather more than three inches square, by filling with hot balsam the space between two sheets of plate-glass held about the sixth of an inch apart by narrow strips of glass on three slides. When the balsam had cooled I had, of course, a layer of solid balsam of the size of the tank, with one side open. The tank was carefully levelled horizontally in the cone of light, as the cardboard protractor had been, and a drop of fluid balsam on the side where the solid balsam was exposed served to make contact

* I may remark here that the thickness of cover through which an objective will work is not limited by its aperture, though this limits the working distance on uncovered objects, but by the extent to which the motion of its posterior combinations neutralize the increasing aberration produced by increasing thickness of cover. The character given to the posterior combinations by the maker determine the available limit in each case.

† 'Monthly Microscopical Journal,' November, 1872, p. 233. See also 'Proceedings of the Royal Irish Academy,' vol. vi., p. 38, 1854.

with the face of the lens. When now the solar light was thrown through the lens as before, a superb amber-coloured triangle of light started into view, the sharp, well-defined edges of which permitted the angle at the focus to be measured with ease by a cardbòard protractor held beneath the flat tank, or by any similar device, taking care, of course, that the eye should be perpendicular to the edge of the light-triangles at each reading, to avoid displacement by the refraction of the upper glass of the tank, which would have made a small error. The cut, Plate XIX. lower portion, Fig. 1, which is a diagram of the objective and tank as seen from above, will, I hope, make the arrangement clear. The plan has the advantage that no part of the objective is exposed to the balsam except its face (which is easily cleaned by a little coal oil), besides which the measurements are much more quickly effected than with the sector, and are not liable to the errors which affect its use when the lenses are closed.

By this method, then, I measured the balsam angle of the $\frac{1}{10}$th Mr. Tolles had sent me, with the following results: uncovered 75°, or nearly what the sector gave; completely closed nearly 80°. I subsequently extended the measurements to the immersion $\frac{1}{15}$th and $\frac{1}{18}$th by Mr. Tolles, belonging to the Museum, and found that the maximum balsam angle of each was less than 80°. These results, it will be seen, fell within the limits laid down as possible by Mr. Wenham.

To measure the water angle of Mr. Tolles's $\frac{1}{10}$th, I now constructed a thin water tank by cementing strips of glass between the edges of two sheets of plate glass about three inches square, so that they should be held about the sixth of an inch apart. All four sides were closed, but one side had in the centre an opening half an inch long, and the edges of the stripes adjoining this were bevelled as in the cut, Fig. 2. When this tank was filled with water, I had of course a thin sheet of water, which would not run out when the tank was held horizontally, and by levelling this, as had been done with the balsam tank, in front of the objective, the angle was measured in the same way. The luminous pencil was by no means so brilliant as in the case of balsam, but its limits were sharp and clear, and it could readily be measured. With the $\frac{1}{10}$th the results were about 90° at the uncovered point, nearly 100° when the objective was corrected for the thickest cover through which it would work. Neither the $\frac{1}{5}$th nor the $\frac{1}{18}$th exceeded 96° when closed as far as possible.

I promptly communicated these results to Mr. Tolles, and was immediately requested by him to examine yet another objective, a $\frac{1}{5}$th, which reached me March 22nd.

On measuring this objective in balsam, precisely as I had done the others, I got somewhat over 90° at the uncovered point, somewhat over 100° when the combination was fully closed. Measured with the water tank, the angle at the uncovered point was about 130°. Now, in the first place, I must remark that the objective was certainly an exceptional one, and apparently put together with a view to this controversy. Instead of three combinations, I found it to be constructed with four; the posterior two resembled those of other fifths of Mr. Tolles, and were together moved by the screw collar, the anterior two remaining stationary; of the anterior combinations the front was very small, and about a ninth of an inch in solar focus. (It magnified 108 diameters at twelve inches' distance from micrometer to screen.) Immediately back of this was a very much larger combination, concave anteriorly and convex posteriorly. I inferred from the manner in which the brasswork was put together (having no information from the maker on the subject) that these two combinations had been substituted for the front of a previously constructed objective.

In the next place I must remark that, notwithstanding its exceptional construction, this objective, when used as an immersion glass, had certainly very considerable defining power for a $\frac{1}{5}$th. It worked, it is true, even when fully closed, only through the thinnest covers, but it resolved the *Amphipleura pellucida* and *Frustulia Saxonica*, both mounted in balsam (Möller's type-plate), and on my Nobert's nineteen-band plate clearly separated the lines of the fifteenth band. Used dry it would not work through any cover, but when fully open it resolved the twelfth band of a Nobert's nineteen-band plate, remounted with the lines uppermost and not covered. In this performance the front of the objective appeared to be in actual contact with the object. I may add that the combination when in use magnified at twelve inches' distance sixty diameters at the uncovered point, and seventy-five diameters when fully corrected for cover.

As the results of the measurements of the angle of the objective last described are quite in disaccord with the sweeping opinion expressed by my esteemed friend Mr. Wenham, in his recent controversy with Mr. Tolles, I have thought it right to imitate his prudent example,* and secure the testimony of competent witnesses as to the accuracy of my results. I therefore repeated the measurement of the balsam angle of this objective before Professor Simon Newcomb, of the United States' Naval Observatory, and Mr. Renel Keith, of Georgetown, formerly also a professor in the same institution. Both these gentlemen are professional mathematicians, and both are well acquainted with optics as a science. They have not only verified my measurement of the balsam angle of this particular objective, but they agree with me that in the heat of the discussion Mr. Wenham has gone rather too far in concluding that

* 'Monthly Microscopical Journal,' January, 1873, p. 29.

it is theoretically impossible to construct an objective which shall transmit from balsam a pencil greater than 80°. Each of these gentlemen has written a memorandum on the subject, which, with their permission, I append to this paper.

The position taken by Mr. Wenham is certainly true for objectives as ordinarily constructed; that it is not necessarily true for all possible constructions will be seen by a moment's reference to his figure in the November number of this Journal (page 232). The deductions drawn from that figure are in strict accordance with optical theory only so long as we suppose the lines *d*, *a*, and *b*, *e*, which represent the course of the extreme rays in the crown-glass front of the supposed objective to remain constant. It is not possible for the extreme rays to have greater obliquity if the light passes from air into the glass; but if the radiant is in water and nearer than the point *f*, or in balsam and nearer than the point *g*, it does not follow that the rays cannot enter the glass front, but simply that they will take a course more oblique than the lines *d*, *a*, and *b*, *e*. In the case of balsam of the same index as the glass front there will of course be no refraction at the line of junction between the balsam and the glass, and rays of any degree of obliquity can enter. To what degree of obliquity it will still remain possible for such rays to emerge into air from the posterior hemispherical surface of the front lens, will depend upon the precise form given to it, and how far it is possible to collect these rays so as to form an image at the eye-piece will depend upon the construction of the posterior combinations.

In the same way in the excellent paper of the Rev. S. Leslie Brakey, in the March number of this Journal (p. 108), the conclusions drawn by the author are only true so long as we suppose the direction of the ray O, X (which precisely corresponds to the line *b*, *e*, in Mr. Wenham's figure) to remain unaltered; the same reasoning applies in both cases. Mr. Brakey remarks that it follows from his demonstration, "that the results are entirely independent of the kind of glass used for the objective front," which is quite true as far as "the results" go, but both he and Mr. Wenham seem to have overlooked the fact that their demonstrations do not touch the question of the angle possible to be transmitted through an objective from a radiant in water or balsam, but only, to use Mr. Brakey's own accurate expression, the "*reduced angle*" in water or balsam corresponding to a fixed air-angle. Suppose, however, an objective to have such a construction that, when a parallel pencil of solar light is transmitted from behind, the extreme rays shall finally reach the flat surface of the front lens at an angle greater than that formed by the line O, X, in Mr. Brakey's figure, of course if there is air in front of the lens every such ray will suffer total reflexion, while if water or balsam be substituted it will be transmitted.

I am in hopes that the foregoing brief explanation will be sufficiently explicit, and that Mr. Wenham himself will frankly admit that he has overlooked the possible case of an objective made to perform only in water or balsam, without reference to its performance in air. Whether the increased angle which theory demonstrates can be gained at this price, will have any practical value, or be any addition to our optical resources, is another question altogether, and one into which I do not propose to enter at the present time.

Memorandum on the foregoing. By Professor SIMON NEWCOMB, U. S. Navy, Foreign Associate of the Royal Astronomical Society.

I assisted in the measures above described by Dr. Woodward. The angle in balsam, when the lenses were fully closed, measured more than 100°.

The reason why the angle exceeded the limit laid down by Mr. Wenham was quite obvious to me during the experiments. Whether the objective was open or closed, the light was dispersed in air at all angles up to 180°, showing that the light which struck near the circumference of the anterior surface of the objective must have suffered total reflexion, and so made an angle with the normal to the surface exceeding the limit assumed by Mr. Wenham.

SIMON NEWCOMB.

Memorandum. By Mr. RENEL KEITH, of Georgetown.

I witnessed the measurement, by Dr. Woodward, of the balsam angle of the $\frac{1}{5}$th of Mr. Tolles, the method used being that described in the foregoing communication. The angle was over 90° when the lenses were fully open, over 100° when they were fully closed. This result does not seem to me inconsistent with theory. Mr. Wenham's experiments, alluded to in his article in the 'Monthly' for January, indicate an explanation, and it seems singular that they did not suggest to him long ago a method of obtaining what Mr. Tolles has obtained—an objective with large angle for objects covered in balsam. Let O, Plate XIX., lower portion, Fig. 3, be the lenses of an ordinary objective in adjustment for an object uncovered. Let R be the radiant at such a distance that a cone of large angle is brought to a focus at the eyepiece. In order that this state of things shall not be disturbed, when the object at R is covered in balsam, mount in front of O the lens B, so that when in water-contact with the cover it shall be part of a sphere with its centre at R. It will exactly neutralize the negative surface of the cover, and the light will radiate from R without refraction until it meets the objective at O.

It follows that when a lens of ordinary glass makes balsam-contact with the cover of a balsam-mounted object, the exposed surface of the lens is to be regarded as the *first refracting surface*, and the angle with which a pencil of light may emerge depends upon the curvature of that surface, and has nothing to do with the plane surface of the submerged cover. How much of the pencil may be brought to a focus depends upon the succeeding lenses in the combination. This is strictly true for glass and balsam, having the same refractive index, and is nearly true in all practical cases, even if water be substituted for balsam between the lens and the cover.

Renel Keith.

Remarks. By Mr. Wenham, V.P.R.M.S.

After my final reply to Mr. Tolles was sent for publication, I received a letter from my respected friend, Col. Woodward, courteously inviting me to read the above before appearing in print, in order that I might append my remarks. At the same time, I am pleased that the settlement of the question should rest with Col. Woodward, whose phraseology and demonstrations I am sure to comprehend. As the proof, however, came to hand late, scarcely leaving me time to consider all the points at issue, and without any diagrams,* I prefer making my remarks in the next Journal. I can only say at present that, after reading the article, I do not see that there is much left to controvert, as Col. Woodward substantially corroborates my position, any apparent differences, probably arise from some statements that have been lost sight of during this long and tedious correspondence.

F. H. Wenham.

V.—*Remarks on Mr. Henry Davis' Paper "On the Desiccation of Rotifers."* By C. T. Hudson, LL.D.

The alleged revival of Rotifers after a thorough drying has always been a perplexing chapter in their natural history. Those who have written on the subject have come to very different conclusions. Some relate apparently trustworthy experiments in which Rotifers have been subjected to the drying effect of a vacuum produced by an air-pump and sulphuric acid, and to that of an oven heated to 200° Fahr., without losing their vitality. Other experimenters equally trustworthy declare that, if the heating be carried some degrees higher, the Rotifers do not recover; while, of the authorities who sum up the evidence, some say that Rotifers may be "com-

* They had been sent to the artist.—Ed. 'M. M. J.'

pletely desiccated" and yet recover, and others that they do not recover when "a certain degree of desiccation has been exceeded."

The latter statement is, of course, a very safe one; but, cautious as it is, it does not in the least meet the real difficulty of the case, which is briefly this, *viz.* that every Rotifer, when isolated and laid with a drop of clean water on a slip of clean glass, dies if the water be allowed to evaporate. Now, if a Rotifer will bear drying in an air-pump with sulphuric acid, or in an oven, why will it not bear simple evaporation on a slip of glass? No one will maintain that one's sitting-room is drier than the inside of an oven at 200° Fahr. or of a receiver; and if *dryness* kills the animals on the glass, why does not the greater dryness of the air-pump, or oven, kill them? Doubtless Rotifers *can* be killed by heated air; that might have been taken for granted; but, if they survive such heat and drying as have been detailed above, why cannot they recover from the effects of evaporation in the comparatively moist air of a sitting-room?

It has been suggested that particles of sand or dirt saved the Rotifers in the air-pump; and that the animals died when isolated on a glass slip from not being able to bury themselves under protecting rubbish. But this explanation will not meet the case of the Rotifers that survived a heat of 212° Fahr. It is of course easy to conceive that an animal which lives in water may lie dormant out of it, provided its own internal fluids are not dried up; but how can sand as hot as boiling water help to protect from evaporation the internal fluids of a soft-bodied Philodine?

Once more I state what appears to me to be the real point at issue. Why will not Rotifers when freed from extraneous particles bear drying in the open air, and yet survive when surrounded with such particles after drying in a vacuum or an oven? This is the riddle; and of this riddle no one, I believe, but Mr. Davis has attempted the solution. Mr. Davis states that it is by secreting a gelatinous coat that the Rotifer in the air-pump resists a desiccation which would be otherwise fatal to it. The question at once arises, "Why does not the Rotifer do the same when freed from the sand?" Mr. Davis' answer on this point is equally complete; the evaporation on the clean glass slip is too rapid to permit of the necessary secretion, and the animal also is too restless under the unpleasant circumstances in which it finds itself to attempt to form the protecting coat, even if it had the time for doing so. I have dried hundreds of Philodines on glass, and have watched their actions while the water evaporated, and I can fully corroborate Mr. Davis' assertion. I can add also that not one of those many hundreds ever came to life again.

It might be imagined from such discussions as the above that Rotifers are as a class very tenacious of life; but the fact is that the great majority die only too easily, and decay rapidly. Very few Rotifers will bear even a momentary withdrawal of the water in which they swim, and all the species that I am acquainted with can be easily smothered by being kept in a small air-tight cell. Unfortunately the student can obtain but little advantage from so killing them, as they usually begin to disintegrate the instant they die; the trochal disk, for instance, disappearing as it ceases to move. The case is no better if they die naturally. I have seen, for instance, a *Triarthra* suddenly roll down dead to the bottom of a live cell, and decay so quickly that the outline of its muscles had become indistinct before I could change a low power for a high one.

The notion that any sort of treatment and any dirty water will do for Rotifers is a very erroneous one. It is true that Rotifers are to be found in very dirty ponds; but every species has its own habitat, and it would be as useless to look for *Brachionus angularis* in a clear pool, as for a *Euchlanis* in a farmer's duck-pond.

A slight alteration in the conditions of life is fatal to most of them; and, so far as my experience goes, it is impossible to preserve the finer species in tanks at home for more than a few days. A student of Rotifers must therefore be one who knows all the ponds for miles round his own house, and who is constantly out looking for fresh specimens; and he will find that neither air-pumps, nor sulphuric acid, nor hot ovens are wanted to kill off his live stock, but that they will die of their own accord with the most provoking regularity.

I have been tempted by my subject into a rather long digression; but I cannot conclude my paper without reverting to Mr. Davis' happy suggestion, that the *Philodines* protect themselves against the effects of drought by secreting a viscid envelope during the slow evaporation of water entangled among particles of sand. Most of the Rotifers possess, in some degree, the power of secreting such a fluid. Some (as *Hydatina*, *Synchæta*, *Rhinops*, and *Pedalion*) use it to enable them to adhere to external bodies, others (as *Melicerta*, *Limnias*, *Œcistes*) to form an inner tube, round which their outer cases are built. Mr. Davis now tells us that some of the *Philodines* put this secretion to a hitherto unsuspected use, and that they coat themselves all over with it so as to resist a drying that would otherwise be fatal to them. This solution of a much-vexed question is as ingenious as it is probable and new; and although it may possibly require confirmation from future observers and experimenters, I have little doubt that such confirmation it will receive.

PROGRESS OF MICROSCOPICAL SCIENCE.

A Parasite on Peziza.—'Grevillea' for March contains a valuable note by its editor, Mr. M. C. Cooke, M.A., on this subject. The writer says that he has lately received from C. J. Muller, Esq., of Eastbourne, a very interesting specimen of a Sarcoscyphous *Peziza*, which appears to be *P. hemispherica*, Wigg. The surface of the hymenium is rough, with the projecting upper portions of semi-immersed, pale brownish perithecia, each of which is furnished at the mouth with a tuft of delicate, erect hairs. The perithecia are themselves membranaceous and translucent, sometimes wholly immersed in the hymenium, as if proceeding from the inferior stratum, and composed of hexagonal cells, with a brownish tint, so as to render them conspicuous amongst the surrounding hymenium. Many of the asci, and septate paraphyses of the *Peziza* are normally developed. These parasitic perithecia contain free lemon-shaped spores, reminding one of the sporidia of certain *sphæriæ* which occur on dung, as *S. stercoraria*, &c. The spores are dark brown, and near ·001 inch in length, but in no instance could we detect asci, or sterigmata, nor obtain any direct evidence of the mode in which the spores are produced in the perithecia. No perithecia were found with the spores in their early stage, and before acquiring colour, but in all instances they seemed to be matured and free in the perithecia. From these circumstances we have been led to regard the parasite as coniomycetous, although not agreeing with the characters of any genus of which we have any knowledge. It has been suggested that these perithecia are not truly parasitic, but that they are another form of fruit of the *Peziza*. Such is not impossible, but, from present experience, we are disposed to consider it as rather improbable, although the fact that the perithecia seem to originate from the lower cellular stratum would favour the conjecture. Under any circumstances, the specimens in question are of a very interesting character, and we have at once placed on record all the facts which have come to our knowledge, in the hope that by turning attention to the subject, other specimens may be found, and a more complete history elaborated for this rather anomalous production. The whole of the features of this parasite seem to favour the supposition that it may be a species of *Melanospora*, but not asci having been found, it would be too great an assumption to place it in that genus until an examination of specimens in an earlier condition settle the question whether the spores are produced on peduncles, or whether they are at first enclosed in asci. No species of *Melanospora* has hitherto been recorded as occurring in Britain.

Distribution of Blood-vessels in the Membrana Tympani.—This subject is very well dealt with in a recent paper by Dr. Burnett, of America. He describes the arrangement of the blood-vessels in the tympanic membrane of the dog, cat, goat, and rabbit. These are arranged in a double series of loops, one of which is composed of vessels which run from the periphery directly toward the handle of the malleus, and at a point from one-half to a third of the distance between the periphery of the membrane and the handle of the malleus return abruptly upon themselves, thus forming a series of vascular loops round the edge of the membrane. The second series of loops run from the handle of the malleus toward the periphery of the membrane. In consequence of this arrangement a portion of the membrane between the annulus tympanicus and the handle of the malleus remains free from capillaries in its normal condition. In the guinea-pig these vascular loops do not exist, but the vessels are arranged in the form of a net with coarse meshes of a quadrangular or pentagonal form. In this animal, moreover, the radiate are strongly developed in comparison with the circular fibres of the membrana tympani. The arrangement of the nerve in these animals is described as "fork-shaped," the prongs embracing the loops, while the handle unites with a similar projection from the opposite series of loops. In the human tympanic membrane the arrangement of the blood-vessels resembles that of the guinea-pig in the absence of loops. The vessels themselves, however, are coarser, and the meshes finer than in that animal. The radiate and circular fibres are, moreover, equal in amount. The conclusions from these observations are the following:—1. There is a distribution of vessels in the membrana tympani of man peculiar to him. 2. There is a distribution of vessels in the tympanic membrane of the dog, cat, goat, and rabbit, constant in as well as peculiar to them. 3. The arrangement of these vessels in the guinea-pig is peculiar to it.—*The American Quarterly Journal of Medical Sciences*, Jan., 1873.

On Mobile Filaments in the Blood.—In the 'Irish Hospital Gazette' (April 1st), Dr. Ponfick, of Berlin, writes concerning the recent important researches of Herren Obermeier and Nedsvetzki on the above subject. He says that the former, one of the physicians of the Charité, has, within the last few days, directed attention to the presence of a foreign body in the blood; and Professors Virchow, Frerichs, and Langenbeck have acknowledged and confirmed the same. Dr. Obermeier has very kindly submitted several specimens for the writer's examination in the Pathological Institute. The blood recently taken from patients suffering from relapsing fever was immediately brought under the microscope without any addition. On the persistent contemplation of a fixed portion of the microscopic field, peculiar filiform bodies—which are about the same size as the finest filaments of fibrine, with a length of three red corpuscles, and with a very delicate contour —are seen to emerge in the plasma, amongst the blood corpuscles. As long as the blood remains fresh, distinct movements are observed, which manifest themselves not only as undulatory movements in the filaments themselves, but also as a power of locomotion, which enables them to travel across the field of vision. It is seen, especially, that the bodies exhibit spiral contractions, then again extend themselves, sometimes appearing, and as quickly disappearing from the view. Dr. Obermeier has always failed to find these bodies in the blood of healthy persons, and also of patients suffering from other zymotic diseases. It is worthy of observation, that they are visible in the febrile stage, but are not seen in the stage of remission, and shortly before or during the crisis. On the occasion of a short communication which Dr. Obermeier made in the Medical Society of Berlin describing his discovery, Langenbeck pointed out the great importance of this fact. Dr. Nedsvetzki also discusses the elementary mobile corpuscles observed in normal blood by Zimmermann, Hensen, Schultze, Kühne, and lately by Vulpian, and known by Sanderson's designation of microzymes. These corpuscles have been classed by Bettelheim in three grades: corpuscles visible under an enlargement of 650 diameters, those visible only under an enlargement of 1500 diameters, and bacillar corpuscles having half the size of the red blood corpuscles. Nedsvetzki describes in normal blood a considerable quantity of small corpuscles of the size of the nuclei of the white corpuscles. They appear clear or opaque according to the light. They present movements in the direction of their axis, or lateral oscillations. He proposes to designate them nuclei of the blood, or hæmococci. He describes also filaments, probably of a fibrinous character, which are developed in the preparations. He dwells on the transformation of these white globules examined in the wet chamber, and on the movements which the granulations of the white globules present.

The Morphology of Carex.—It is remarkable how much we have to learn yet on this point. At a meeting of the Linnean Society, March 6th, Mr. Bentham read some remarks on the homology of the perigynium of the female flowers of *Carex*, and the subject was again discussed at the meeting on April 14th. He suggested the theory that the perigynium and seta represent the stamens of the male flowers. It appears, however, to be certain that the seta is an axial and not a foliar structure, and that when developed it usually bears rudimentary flowers, as in *C. pulicaris*. The perigynium under these circumstances can hardly be looked upon as perianthial. On the whole Kunth's view, according to which it consists of a single bract with anteriorly connate edges and bearing the ovary in its axil, is probably correct. Some botanists, taking into consideration the manifest bidentate condition of the perigynium, will still probably prefer to compare it with the two lateral bracts in *Calyptrocarya*.

Dr. Engelmann's View as to the Structure of Muscle.—One of much importance, especially now that Mr. Schäfer * has rather revolutionized our ideas on the subject. He (Dr. Engelmann) contributes an article on the subject of the structure of muscle to Pflüger's 'Archiv,' Band vii. Heft i. He laments the difficulty of obtaining the crystalline arthropods of the sea, and observes that the tolerably transparent Cyclops, Gammarus, Asellus, Hydrachinda, and Insect larva as *Corethra plumicoreus* of fresh water are only to be obtained in small and insufficient numbers. For the examination of muscular fibres no reagents or saline solutions should be used; they should simply be placed in a moist chamber, and be examined as rapidly as possible after removal from the living body. Insect muscles can undergo great changes in structure before they lose their excitability. He has used

* We are compelled to "cut out" Mr. Schäfer's paper through the pressure caused by the Index.

a magnifying power of from 200 to 500 diameters, or that obtained by a Hartnack's objective 8 and eye-piece E or F. The structure of normal uncontracted transversely striated muscular fibre is: (1) A light very slightly refracting band divided into two halves by (2) a dark highly refractile stria; (3) a moderately dark, tolerably strongly refracting band, in the middle of which is (4) a brighter, less refracting stria. In every fibre with very broad transverse striæ the simple dark band can be resolved with high powers into three: a middle darker one, and two lateral clearer or brighter ones. Hence we must admit that such division still exists even where our present means of research do not permit it to be seen. Throughout his paper Engelmann makes use of the following terms: the stria in the middle of the isotropal substance he calls the intermediate disk (*zwischenscheibe*) and the adjoining striæ secondary or accessory disks (*nebenscheiben*). Both of these together, when they cannot be distinguished as separate, constitute the fundamental membrane (*Grundmembran* of Krause); the middle layer of the doubly refracting substances forms the median disk of Hensen, and the two lateral he terms transverse disks. In the closely striated muscles of vertebrata *z* and *n* appear united together to form a single and simple foundation membrane in which no subdivision can be seen. The distinctly striated fibres of insects, on the other hand, show the division well, and the whole series of disks in one compartment are here sometimes as much as four times thicker than in vertebrata. The height of each set varies even in different muscles of the same animal. The greatest height or length of one compartment Engelmann found to occur in the abdominal muscles of insects where it amounted to 0·011 mm. The isotropal and anisotropal substances are about equal in height, the proportion of the former to the latter being as 6 : 7. The degree of transparency of the several parts varies considerably, so that now one, now another, may be the darker. Where both are of equal transparency the existence of transverse striæ may at first sight be almost overlooked. The distinction is always well brought out by the polariscope. The remainder of the paper is occupied with a special description of each disk in succession. From his examination of muscle under polarized light and by other means he has arrived at the conclusion that muscular tissue is composed of an infinite number of rods arranged parallel to the longitudinal axis of the fibres which are naturally in immediate contact with each other, but which after death, or after treatment with reagents, shrink and exude or excrete the isotropal substance. The size and form of the rods he supposes to differ in each of the disks that make their appearance in the above scheme. See also 'The Academy,' May 1st, which contains an illustration explaining the complexity of the structure.

A Protozoon in Urine!—V. C. E. Nelson says, in the 'New York Medical Journal' (March), that a gentleman recently brought him a phial of his urine to examine under the microscope: nothing whatever was seen, with the exception of a single protozoon, which forms the subject of this communication. Two phials were brought on that day; in the urine of one only was this protozoon seen; the urine was not left in any open dish, but in a well-corked phial, so that the protozoon did not enter from the surrounding atmosphere, or owe its existence to decomposition. The urine of several phials, subsequently brought to the office, has been carefully examined, but no other specimens have been seen. The microscope used in this instance magnifies 300 diameters; the protozoon figured is drawn of the actual magnified size, as observed, and at the moment of observation. The protozoon would now be motionless; now, the neck would swing upward and downward; now, the worm would bend in sinuosities throughout its entire length, the movements following in rapid succession; again, the protozoon would change its position in the field of the microscope from a horizontal to a vertical one; at other times it would very suddenly contract, and assume a different appearance, the caudal end being truncated or club-shaped; in a few seconds it would, with lightning-like rapidity, shoot out to its full length. The author says he will not hazard any conjectures as to what kind of protozoon this is, but it seems to be allied to what is portrayed in Cobbold's work as the *Dactylius aculeatus*.

NOTES AND MEMORANDA.

The Reproduction of Bacteria.—Herr Grimm, in the 'Archiv fur Mikrosc. Anatomie,' describes the reproduction of Bacteria and Vibriones from his own investigations. He has observed their conjugation and fissiparous multiplication, and also has seen leucocytes breaking up into granular matter, which ultimately assumed the form of Bacteria.

Spontaneous Alteration of Eggs.—A contemporary states that M. U. Gayon comes to the conclusion that the main cause of the decomposition of eggs is the presence of small organisms which must have formed in the egg while in the oviducts of the fowl.

Mounting in Soft Balsam.—'Science Gossip' for March has an interesting note on this point by an American gentleman, Mr. W. H. Walmsley, of Philadelphia. He says that the following directions, if carefully followed, will invariably result in success:—Select the finest Canada balsam and slowly evaporate it until upon cooling it assumes a brittle resinous consistency. Break the mass into small pieces, and dissolve them in chemically pure benzole, until a saturated solution about the consistency of rich cream is formed. The specimen to be mounted, having been previously freed from moisture by drying, or by being passed through weak and absolute alcohol (the latter being by far the preferable method), is finally to be placed in oil of cloves, and carried from the latter to the slide, where, after being properly arranged with needles, a drop of the balsam is placed upon it, followed by a core in the usual manner, and the whole laid aside to harden, which will be accomplished in a few days. This will be facilitated if, after the lapse of twenty-four hours, the slide be slightly warmed, the core pressed carefully down with the forceps, and a small weight laid upon it. The best finish for the edge of the circle I have found to be made with a camel-hair pencil dipped in the same balsam that is used in mounting. It makes a very neat and handsome finish, with of course no tendency to run in and spoil the specimen, as is the case with all coloured cements used for this purpose. The oil of cloves is preferable to turpentine for mounting from, since it is more readily miscible with the balsam, and does not harden the specimens, which may be left in it for a long while unchanged.

Experiments on Septicæmic Blood have been, we understand, lately performed by M. Onimus, which are somewhat like those made some time since by Dr. Burdon Sanderson. M. Onimus enclosed septicæmic blood in a dialysing paper, and has plunged this into distilled water. At the end of twenty-four hours, the distilled water became milky, and swarmed with myriads of bacteria. He satisfied himself that these bacteria did not proceed from the septicæmic blood, but were formed outside the parchment pouch; for the dialysing paper, when examined by the microscope, did not show any bacteria. He injected as much as fifty or sixty cubic centimètres of this liquid, filled with living corpuscles, into a rabbit, without producing the slightest disturbance. But when on the other hand he injected a drop of the blood contained in the dialysing paper, the death of the animal was rapid. He repeated these experiments with albumen, injecting into the veins of a rabbit albumen, in which bacteria had developed, and the rabbit survived. M. Onimus concludes that: 1. The septicæmic virus is a non-dialysable substance; 2. Bacteria only, or bacteria developed in albumen, are not sufficient to produce the putrefaction of blood; the blood must be injected *in toto*.

The Professorship of Anatomy and Physiology in the Veterinary College of Edinburgh has been given to a most worthy candidate, lately a Fellow of our Society, Dr. James Murie, F.L.S., who will, we doubt not, be as energetic in the discharge of his duties in Scotland as he has been—judging by the published list of his numerous researches—in his various offices in this country.

A Chair of Microscopic Anatomy in Spain.—The 'Medical Record' (May 14) says that a chair of normal and pathological histology has been founded by the Spanish republican government in the University of Madrid, and endowed with a salary of 5000 pesetas (210*l.*). The medical faculty of the University of Valencia has protested against the establishment of a similar chair in that institution, on the grounds, *inter alia*, that the subjects are already taught by the several professors.

Man infested with Trichinæ through eating Pork.—We learn from a contemporary that there has been a number of people attacked by this worm through eating raw ham, a common practice in North Germany. It is said that about 200 persons, who had partaken of some raw pigs' flesh obtained from a butcher in Magdeburg, have been attacked with grave symptoms of the flesh-worm disease, due to the incision of their tissues by hosts of living trichinæ. One is dead. The living trichinæ have been found in numbers (as is usual), in small parts of the muscle, and removed by a little instrument devised for the purpose, from the arms of some of the patients (of whom twelve are in the hospital), among them being the butcher who sold the diseased pork. The swelling of the face and limbs, and the acute muscular pain characterizing the disease, have been observed in all the cases, and some are still considered to be in danger. The penalties of the Germanic custom of eating raw ham are severe.

A Grant for Geological Microscopy.—We understand that the Royal Irish Academy has given the sum of 40*l.* to Mr. G. H. Kinahan in order that he may continue his valuable researches into the microscopical structure of rocks, a subject on which for some time Mr. Kinahan has been engaged.

Sachs' Lehrbuch der Botanik.—We learn from 'Nature' that this book—which is of interest to microscopical students—is recommended by the Board of Studies in Natural Science of the University of Oxford to students preparing for examination at the University. For the benefit of those unacquainted with the German language, the Delegates of the Clarendon Press have arranged with Prof. Sachs and with MM. Engelmann, of Leipzig, for an English translation of this work from the third edition, just published in Germany, and containing a large amount of additional matter; the whole of the 460 woodcuts with which the original work is illustrated will be reproduced in the English edition. The translation has been entrusted to Mr. A. W. Bennett, B.Sc., who will also annotate the work on points where sufficient prominence does not appear to be given to recent researches, or undue prominence seems to be assigned to certain theories, in which part of the labour he will be assisted by Prof. Thiselton Dyer. The work is expected to be ready by about the end of the year.

A Prize for the best Essay on the Reproduction of the Lycopodiaceæ to the extent only of 10*l.* 10*s.* is offered by the Edinburgh Botanical Society. This prize is small in amount, and is alone to be competed for by students who have attended the botanical class of the Royal Botanic Garden, Edinburgh, during at least one of the three years preceding the award, and who have gained honours in the class examinations. The author is expected to give results of practical observations and experiments made by himself on the subject, illustrated by microscopical specimens. The essay and specimens to be given in on or before May 1, 1876, with a sealed note containing the author's name, and a motto outside.

A Ten Guinea Prize is also to be given by the Council of the Botanical Society of Edinburgh for the best essay on the structure and reproduction of the Frondose and Foliaceous Jungermanniaceæ. This prize is subject to all the conditions specified in the case of the former.

CORRESPONDENCE.

Measurement of Immersed Apertures.

To the Editor of the 'Monthly Microscopical Journal.'

Padnal Hall, Chadwell Heath, Essex, *3rd May*, 1873.

Sir,—I am willing to continue discussion so long as any true facts can be elicited from it, that may add to our stock of practical information. I have stated that with Mr. Tolles must be considered as ended, as my reasoning and experiments seem to be in a style utterly beyond his comprehension. I now make a few remarks on his method of measuring balsam apertures with immersion objectives, as I could only slightly allude to this in my last communications,—having then no precise knowledge of the means by which he obtained such wonderful results.

In the last 'Journal' for May (Plate XV., page 210) we have a plan of the arrangement. From this, it appears that Mr. Tolles cannot get beyond the idea of putting hemispherical or semi-cylindrical things of glass in front of the objective, so that the light may emerge "without sensible refraction"? It thus appears that the question stands hopelessly back at the very commencement. It began with this plan, and the causes of fallacy that I then figured and described, will serve still, and need not be repeated. Let us suppose that aperture is to be tested by this wretched adaptation (containing the seeds of a crop of refractive errors), strictly with the intention that the object-glass is in a fair adjustment for giving a correct defining aperture, as fixed by a known object, and not improperly set back so as to place the lenses as close as they will go. Now, the optical question is, what is the *loss* of aperture which must inevitably ensue, by partly or entirely destroying the refraction at the front surface. Well, bring up your semi-cylindric or hemispheric affair (which we will assume to be so exceedingly well made, that the centre or radius is on the flat surface), focus on the surface, precisely on a point in the centre of curvature. Now, let in your water—will the rays emerge "without sensible refraction"? By no means! the focal point has shifted its position, and to get an approximate measurement you must attempt to bring the focus again on to the surface. Further, the arrangement will not adapt to various conditions. The loss in water, *per se*, and other fluids of different refractive powers, cannot be truly ascertained, and every degree of refraction of fluids will again throw the focus away from the plane. The least alteration in the focus in too willing hands will readily favour the desired result in the way that it is expected to go, or give a remarkable facility for developing that unconscious self-deception,—so characteristic an element in the minds of mediums and spirit-rappers. Speaking plainly, I am astonished that any one having the slightest pretence to optical knowledge should put this forward as more correct, or in preference to simply immersing the objective in a glass tank containing the medium. It seems impossible for Mr. Tolles to comprehend this. With the tank nothing can go wrong. No focussing is required, and whatever the depth of immersion, the ray passes straight through the bottom, and fluids of any refractive power may be used with equal accuracy, the results remaining correct with all lengths of focus due to the refraction of the different fluids. Though this is certain, Mr. Tolles may undertake some unfathomable optical demonstration to disprove this, which may be amusing, but will need no answer.

Immersed apertures may of course be taken first, as employed on a balsam object, and the *increase* of aperture when dry, afterwards measured by the usual method. Fallacies have at times appeared of aperture measurements, with the lenses closed within the distance of definition. *Mere light* can be seen through a hemispherical lens up to 180°, far beyond the perception of distinct images.

Finally, I may assure Mr. Tolles that the inflexible laws of light will not bend to meet his wishes, and as he puts forward, without the pale of disinterested argument, some extravagant advantages in his own "peculiar objectives" when used as immersion, which he is "almost certain that no English objectives will be found to have," therefore, for my own part, I willingly accept the challenge, being rather sure that object-glasses made in this country intended to act as immersion, will keep their aperture, measure for measure, under similar conditions to anything that he can produce.

I have no doubt that the committee with any one else that Mr. Tolles might name, would act again—if possible, to settle this *aperture* question. Of course nothing need be stated concerning the relative performance of object-glasses unless he desires it.

F. H. Wenham.

The Angular Range of Objectives.

To the Editor of the 'Monthly Microscopical Journal.'

Boston, *April* 17, 1873.

Sir,—I desire to put on record the fact that I have, using the semi-cylindrical appliance under the stage of my microscope, somewhat in the manner indicated in the sketch sent you last month, put through and utilized successfully an angular pencil of 112° into cylindrical surface through its substance of glass, through balsam, slide, balsam-mounted object, glass cover, *water*, objective, infallibly to the eye, and of that angular dimension, at the *extreme* of oblique incidence giving symmetrical view of the object (*N. Amicii*), good definition and resolution.

The objective was one of three systems, having a cemented triple front. This is really my first attempt to exceed 100° of "image-forming rays" from (through) object in balsam, with only *three* separate systems. I hope very easily to exceed that angular range with the aid of the cylindrical condenser,—of which I am not prepared at present to give a more detailed description.

By putting the light of lamp-flame *down the open microscopic tube* through the objective, the angle of the *emergent* pencil was also easily measured at the cylindrical surface.

Cancelling so many reflecting surfaces of course increased the volume of the illumination considerably.

Respectfully yours,

Robert B. Tolles.

Desiccation of Rotifers.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—I have read the article by Mr. H. Davis in the May number of the 'Journal,' describing a new rotifer, and demonstrating by an elaborate series of experiments, the mode by which a rotifer left high and dry, is enabled to resist the desiccating influence of the atmosphere.

To my mind his discovery of the gelatinous envelope fully explains the phenomenon, and his experiments showing that this envelope, after a prolonged exposure to powerful desiccating agencies, still contains animal matter in a fluid state, finally disposes of a question which has been a fruitful source of controversy for many a long year.

Continuing to turn over the leaves of the 'Journal,' in the happy frame of mind induced by the feeling that in future it might be possible to take up a scientific publication without the dread of finding in every page an allusion to the inevitable dried rotifer, I was surprised to find a letter from Mr. Slack stating that Mr. Davis's discoveries were so far from being novel, that the question had been settled long ago.

Now, as I look over most of the periodicals devoted to microscopy, I began to think that, like a rotifer, I had just been revived after lying dormant for years, so I turned to Mr. Slack's quotations, with the determination of beginning at once to make up for lost time. But what do I find? The quotations resemble Mr. Davis's paper, only in the fact that they have to do with rotifers. In no one of them can I find the slightest allusion to the means by which a rotifer, out of its element, manages to preserve its moisture and consequently its vitality.

It should not be overlooked that the quotation from Dr. Pennetier (perhaps the greatest authority cited by Mr. Slack) is, in sum and substance, the very same as the quotation from the 'Cornhill Magazine,' given by Mr. Davis himself in his paper.

Whilst fully admitting the importance of carefully investigating every statement which is advanced as a scientific fact, yet I cannot help thinking that, in the present instance, an undeserved slur has been cast upon the result of what appears to have been a long and successful investigation.

I am, Sir, yours faithfully,

F. W. Millett.

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—When Mr. Slack, at the April meeting of the Royal Microscopical Society, promised to furnish evidence to justify his mitigated strictures and somewhat patronizing remarks on the supposed want of originality in my paper on desiccation, a vague uneasiness was left in my mind which, he may be pleased to hear, his letter at page 241 entirely dispels.

His quotations only show that the most recent writers on the subject go no further and prove no more than the older experimentists. The question being—can rotifers survive desiccation? was neatly "begged" by a method not altogether unknown to Mr. Slack,* for the creatures being baked and air-pumped, were only admitted to be dry when they did not revive, while, in the common event of the most protracted desiccating arrangements failing to kill, the drying process was pronounced imperfect. Pennetier, indeed, made a lame endeavour to account for the rotifers' preservation, but his explanation, and that a false one, had been anticipated by an anonymous English writer in 1860.†

Practically, Pouchet in 1859 closed the controversy so far, but lacking the true explanation of the phenomenon (furnished for the first time in my paper) only helped to a settlement of the question against his own theory. It is incredible that if in 1859 or earlier it had been proved or "settled" (as Mr. Slack affirms) that a perfectly dry rotifer was destroyed, such an author as Dr. Carpenter could be found to teach us in 1868 that a perfectly dry rotifer could be revived.‡ It is equally improbable that you, sir, would have invited your subscribers to make experiments and give you the results if celebrated writers had worked out and disposed of the subject at least ten years before.§ Further, if he believed in the finally "settled" facts, how could Mr. Slack himself, so recently as 1871, be found advocating opinions in direct opposition to them? ||

"The whole question" has never "turned upon the *amount* of desiccation," as the rival theorists always distinctly stated their opinions for or against "perfect desiccation," which can scarcely be a comparative term; and when in the 'Marvels' Mr. Slack shows the necessity for "*thorough dryness*" (his own italics), he surely does not mean even a very slight partial dampness.

The truth is that writers on the driest of all subjects just left it in a state ripe for re-opening whenever new facts could be elicited, and such facts (although most strangely ignored by Mr. Slack) I submit are to be found in my small contribution to the Society's 'Proceedings.'

To those Fellows who have kindly testified their unsolicited

* "No chemist would have expected to dry the rotifers by the process which did not succeed" (in killing them?).—*Vide* Mr. Slack's letter.

† 'Studies in Animal Life.'

‡ 'The Microscope and its Revelations,' 4th edition, pp. 477 and 480.

§ "Desiccation of Rotifers," 'M. M. J.,' May, 1869, p. 315.

|| 'Marvels of Pond Life,' 2nd edition, p. 132.

approval I return my best thanks, and join with them in the belief that no unfair criticism or semi-official opposition, although sufficiently annoying at times, will seriously impede our onward progress.

I remain, Sir, yours faithfully,

H. Davis.

The Figures in Mr. Stodder's Paper in the 'Lens.'

To the Editor of the 'Monthly Microscopical Journal.'

Sir,—Permit me to say to Mr. Slack that I agree with him, that the "figure" *all the figures*, intended to illustrate my paper in the 'Lens' on *Eupodiscus Argus*, is "not quite correct" or nearly so. The fact is, I am no draughtsman, and the sketches were only expected to give an approximate idea of the appearances, and the printed figures give but *an approximate* idea of the originals. I did not see any proof of the printed figures, or they would have been altered or suppressed. The "irregular hexagons" were distinctly irregular, not the effect of distortion—Figs. 4 and 5, perhaps, the best representation of the real appearance of any, but can be seen only by reflected light, and a high power. Fig. 2 is certainly the worst of all, and is unlike anything that I saw—as I did not think, and do not now, that the terms "areolæ" and "cellules" are confined to botany, but are words in common use; I use them as descriptive, without regard to any technical meaning as applied to other plants, if they have any such.

The real structure and constitution of the silicious shell of the diatoms is a study that will tax the skill, patience, *and instruments* of the best microscopists. I hope that some of the English workers will devote more time to solving the problems. It is of far more worth than making new species. My ideas of the structure are to be found in the 'Quarterly Journal of Microscopical Science,' vol. iii., N. S., p. 214.

Following up the investigation since with higher magnifying powers and immersion lenses, I have found but little reason to change my opinions. I do not believe that all diatoms are built up alike; I do not believe, notwithstanding all that has been written and published on the question, that the structure of the Pleurosigmata is settled. Recently I examined one with a Tolles' $\frac{1}{50}$th by sunlight. At one focal adjustment I got hexagons as sharp and distinct as those of *Triceratium favus;* a minute change of focus gave beads of all the prismatic colours. Which is true? Can the appearance of spheres be produced by transparent in any other shape? The answer to this question involves much more than the structure of a diatom; it affects almost all investigations with the microscope.

Charles Stodder.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

King's College, *May* 7, 1873.

Dr. John Millar, V.P., F.L.S., in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the Fellows were unanimously voted to the respective donors.

The Secretary read a paper by Dr. Maddox on a Cestoid Parasite found encysted in the lower part of the neck of a sheep, in which he described its general appearance and characteristics, and the results of microscopical examination. Drawings of the minute structure of the Cyst and Parasite accompanied the paper, which will be found printed *in extenso* at p. 245.

The thanks of the meeting were unanimously voted to Dr. Maddox for his communication.

The Secretary called the attention of the Fellows to the announcement made by circular of the scientific evening arranged for May 14th, and which he hoped would prove of much interest. The attendance and co-operation of the Fellows of the Society were requested on behalf of the Council.

A paper was then read by Mr. W. K. Parker, F.R.S., "On the Development of the Facial Arches of the Sturgeon," especially with regard to the formation of the mouth. The general characteristics of the Ganoid fishes and their relation to the osseous fishes and mammals, especially in the embryonic state, were explained and illustrated by drawings enlarged upon the black board, and the development and peculiar conformation of the Sturgeon's mouth were similarly described. The paper will be found printed at p. 254. The Chairman expressed his sense of obligation to Mr. Parker, and invited observations upon the paper.

A vote of thanks to Mr. Parker was then unanimously passed, and the proceedings terminated.

Donations to the Library, from April 2nd to May 7th, 1873:—

	From
Land and Water. Weekly	*The Editor.*
Nature. Weekly	*Ditto.*
Athenæum. Weekly	*Ditto.*
Society of Arts Journal. Weekly	*Society.*
Bulletin de la Société Botanique de France	*Ditto.*
Journal of the Quekett Club, Nos. 20, 21, and 22	*Club.*
West Kent Natural History Society's Report for 1872, with President's Address, &c.	*Society.*

The following gentlemen were elected Fellows of the Society:—

William F. Denning, Esq.
Frederick Hovenden, Esq.
Joseph B. Leslie, Esq.
Thomas Rogers, Esq.
Lieut.-Col. J. C. Salkeld.

Walter W. Reeves,
Assist.-Secretary.

Medical Microscopical Society.

The third ordinary meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital on March 21, at 8 P.M., Jabez Hogg, Esq., President, in the chair.

The Secretary read the minutes of the last meeting, which were confirmed, and the President then announced that the next meeting would be considered a Special General Meeting, for the purpose of electing two new members of Committee.

The papers promised for the present meeting having been unavoidably withheld by their authors, Mr. Schäfer described some of the "methods of observing tissues in the living state," illustrating his remarks by means of diagrams and instruments. Having dwelt briefly on the importance of the subject, Mr. Schäfer remarked that the investigation of a subject was not complete till it had been microscopically studied in the living state, and that such examination, at least for warm-blooded animals, should be carried on at the temperature of the body. Much was to be learnt from the investigation of tissues still attached to the living body, for thus had cell migration been discovered by Cohnheim in the frog's mesentery, and experiments on embolism had been made in that animal's tongue; while the tail of the tadpole had taught us much about connective-tissue corpuscles, and the development of blood-vessels. Muscular tissue was best seen in the living state, in the smaller crustaceæ.

Living tissues, removed from the body, allowed of being studied in many ways: some immediately without any addition whatever, as red blood corpuscles, and striated muscular fibre; while if any addition were necessary, a saline solution of 0·75 per cent., or serum would be best. For some purposes a moist chamber might be necessary, such as Recklinghausen's, in which frogs' blood had been preserved for days in a living condition (Schultze's 'Arch.,' 1866). Another form was Stricker's putty stage, which was also useful for the application of electricity in microscopical research by means of two electrodes of tin-foil, the points of which nearly meet in the centre of the stage. Mr. Schäfer finally described and exhibited various forms of warm stages, one kind of which, as Schultze's, was heated by means of a lamp applied to metal arms, which conducted the heat to the object-bearers; another kind, as Stricker's, in which a constant temperature was maintained by means of a current of warm water kept continually flowing through it; while another very ingenious form of stage, somewhat similar to Stricker's, was so arranged that a constant circulation of warm water was kept up in a closed system of tubes, the temperature of which was regulated by a mercurial gas-regulator, and measured by a thermometer, the bulb of which lay close to the central chamber. A discussion then took place, and

A vote of thanks was accorded to Mr. Schäfer for his interesting and instructive remarks, and it was then announced that all those gentlemen proposed at the last meeting were duly elected members of the Society, after which the meeting resolved itself into a conversazione.

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