NOTES AND CORRESPONDENCE.

The Circulation in Aqueous Plants.—In the 8th number of the Journal, published in July, 1854, there is an account of the circulation in the *Closterium lunula*, by the Hon. and Rev. Mr. Osborne, and Mr. Hogg. This circulation, according to Mr. Hogg, is "no new discovery," but to me, as a young microscopist, I must confess it was so, as until I applied the parabolic reflector of Mr. Wenham, with the assistance of direct sunlight, I have never suspected it to exist. By this means of illumination, however, it appears to me to be very distinct, although I have seen it to better advantage in *C. acerosum*.

Some time in April last I met with some good specimens of this plant, and with $\frac{1}{4}$-inch objective of Smith and Beck their No. 1 eye-piece, Mr. Wenham's reflector, and a prism instead of mirror, with the assistance of direct sunlight, I had repeatedly the gratification of beholding what Mr. Osborne appropriately calls a "godlike" sight of the most beautiful, undulating ciliary motion, magnificently illuminated with prismatic colourings. After a longer time than usual spent over one specimen, the water in the cage partially dried, and on the edge of the air-bubble being brought by this means in close proximity with the specimen, the usual effect of external ciliary motion was most distinctly visible to myself and a friend for some considerable time, although no cilia could be distinguished. The rapid and continuous passage of a stream of molecules in the direction of the extreme end showed beyond the possibility of any doubt that cilia were there.

A few days subsequently I met with a good sample of the *Chara*, and it struck me to examine the circulation by the same illumination I had so successfully employed with *Closterium*. Judge my delight when I found precisely the same appearances, the same rapid undulations, together with the same brilliant coruscations, that almost satisfied me *that herein consisted the phenomenon of circulation in aqueous plants.* I am not aware that this has before been noticed, or at any rate recorded, and hope some more practised observers will put it to the test; for whether I am correct in supposing the circulation in water-plants originates in ciliary movement or otherwise, they will be amply repaid for the trouble expended, in the glorious sight presented to them.—JAMES WESTERN, *Veterinary Surgeon, Madras Artillery.*

On the Starch Grain.—In the Botan. Zeitung for June 8, 1855, p. 407, is a short notice, by O. Maschke, on the starch grain. Adverting to a paper "On the Structure of the Starch Granule," by Mr. Grundy, which appeared in the 'Pharmaceutical Journal for April 1855,' the writer refers to his own researches on the subject, made in the years 1852 and 1853, and published in the 'Journal für praktische Chemie,' vol. 56, part 7-8, and vol. 61, part 1; and states that in these communications he endeavoured to show:—

1. That the starch-grains are enveloped with cellulose, and consequently that they represent vesicles or cells.
2. That the starch-grains examined by him were constituted of several cells, arranged one with the other in a pill-box fashion.
3. That the *amylon* exists between these cells in a soluble or insoluble state, in the latter condition presenting the form of extremely minute granules.
4. That the so-termed nuclear point of the starch-grain is a central cavity in the innermost vesicle, which is sometimes empty in consequence of desiccation, and sometimes filled with fluid.
5. That the "moss-starch" (moosstärke) is merely *amylon*, modified by the action of acids (modified starch).
6. The "staleness" of bread depends upon the circumstance that the soluble starch, which exists in new-baked bread, passes into the insoluble condition.
7. That what is termed "leiocom" is produced simply from the action of an acid; and that this acid is formed in consequence of the elevated temperature necessary for the demonstration of this substance."

As the author does not appear, when these observations were made, to have been in possession of a good compound microscope, he may perhaps, when so furnished, see reason to change his opinion in some respects as to the structure of the starch-grain.

Aperture of Object-glasses.—Professor Bailey having noticed in the last Journal my remarks bearing reference to the fact of his being able to discover the markings on the most difficult tests known, when mounted in balsam, I beg to state, that my observations were dictated by no other motive than the desire of establishing a correct fact, and that I was not prejudiced by any favourite theory.

Professor Bailey says, "It is appparent from the above that Mr. Wenham has convinced himself, both by reason and

experiment, that I *ought* not to have seen the markings on delicate test objects, when mounted in balsam." From this I infer that Professor Bailey had not seen a paragraph contained in my communication, in the 'Quarterly Journal of Microscopical Science' for January, 1855, page 162, or I feel assured that he would not have thought it necessary to make this form of reply, for I therein assert that subsequent experience had induced me to recall my remarks, and that I had lately succeeded in bringing out the striæ of some very difficult tests when in balsam. I will now corroborate this by saying that I am convinced that Professor Bailey is perfectly correct in his statement with respect to balsam tests, which must henceforth be recorded in the list of facts. Thus far we are quite agreed; but as Professor Bailey's allusions extend beyond this point, self-defence will be my apology for taking some notice of them. Referring to me, Professor Bailey says, "The error in his arguments will be sufficiently obvious to any one, who will trace the course of a divergent pencil of rays *out of* the balsam instead of *into* it, as in Mr. Wenham's experiments, and it will then be seen, that large angles of aperture are as useful for balsam-mounted specimens as for others." Surely Professor Bailey cannot have well considered this extraordinary, because extremely incorrect assertion, which is tantamount to saying, that a diverging pencil of rays from a luminous point, submerged in balsam, will in each case continue their course in the same right line, without suffering any refraction, *after* emerging from a plane surface of the medium. This is contrary to all reason, for in the trigonometry of optics where there are sufficient data connected with the position and direction of the rays, it comes to precisely the same thing whether they are traced *into* the refractive medium or *out of it*. But taking Professor Bailey on his own statement, I will explain what is the real effect in this case. Suppose a series of rays diverging from a balsam-mounted object; from the mean refraction of the balsam and glass cover (the indices being about 1·54 and 1·53) *total reflection* would take place from the upper surface of the latter at an angle of very nearly 41° from the perpendicular. This, therefore, at once limits the angle of rays collected by the object-glass to 82°, and as total reflection begins where refraction ceases, all rays beyond this point will be entirely reflected down again into the balsam, and lost by dispersion; and the extreme rays of the pencil of 82° that just exceed total reflection by passing through the glass, so far from continuing their course in a straight line, are brought down by refraction to the very level of the top surface of the cover

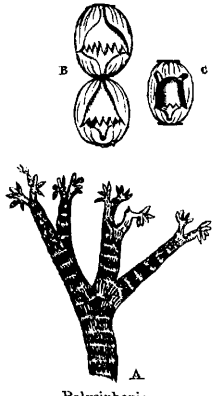
itself, so that if it were possible to use an objective of 180° of aperture, the effect of balsam-mounting would reduce it at once to 82°, and allowing for all possible variations of the refractive powers of the balsam and cover, I have no hesitation in affirming that any object mounted in the usual manner in this medium, has *never* been seen with an angle greater than 85°; but in all probability the extreme limit has been about 78°. This statement is not the result of mere hypothesis, but admits of ocular demonstration, by experiments that will prove it at least half-a-dozen different ways, and is so true in theory, that to endeavour to disprove it will be to take the difficult course, of attempting to undermine the ground upon which I have taken my stand, by denying the first laws of refraction upon which my assertion is based.

Professor Bailey has, no doubt, experienced the advantage of the utmost extent of aperture that can be obtained, in that particular department of investigation, in which he has so eminently distinguished himself; and I am willing to admit, that if the highest powers are to be used *only* for viewing thin and flat objects like the *Diatomaceæ*, the aperture may be as near to 180° as may be practically convenient for this especial purpose; but considering all the requirements, and perhaps more useful applications of the object-glass, I am still of opinion that beyond 150° there is no real advantage to be gained. I have expended much time, and taken special delight in the cultivation of the largest apertures, and possess an assortment ranging up to the greatest possible limit, and I can even now bring out striæ with 150° as readily as with anything beyond it, with the positive advantage of a greater distance between the front lens and object. Some of the phenomena described in my communication to the present Journal are extremely severe tests of all the good qualities of an object-glass, and yet I have had some, whose performance is unrivalled upon a difficult diatomaceous test, repeatedly break down and fail in their effective duty, when applied to the investigation of plant-circulation, from the fact of their possessing *too much* aperture.—F. H. Wenham.

On the Structure of the frond of Polysiphonia fastigiata.—The frond of *Polysiphonia fastigiata*, bearing antheridia, consists of a mass of transparent matter, in which are imbedded coloured, elongated cells or siphons. These are so arranged side by side in successive rows as to surround a central hollow passing through the whole extent of the frond. Each row of siphons with its hyaline matrix forms a kind of ring or section of a tube, and under pressure has a tendency to detach itself from

those next to it. These rings are articulated by some intervening dark matter laid transversely.

The tube thus formed is occupied by a series of clear vesicles of the same length as the siphons, which impress upon their outer surfaces a set of corresponding parallel depressions, and each vesicle contains an urn-shaped body of the same colour as the siphons. A row of spines is placed round the shoulder of this organ, and from either end a stem with a slightly-expanded termination passes out, by which all the vesicles and their contents are brought into connection. These urn-shaped bodies when immediately below a bifurcation of the frond are rather more squared than the rest, and give out a communicating process from each of the distal angles. The chain is in this manner continued upwards. The contents in the conditions in which I have seen them are mere granular matter. The same or corresponding structures have not been observed in other species of *Polysiphonia;* but in the frond of *P. fastigiata*, producing tetraspores, they are present.



Polysiphonia.

No description or representation of them has yet been published, and their functional relations remain unknown.

A. Terminal portion of a frond of *Polysiphonia fastigiata*, bearing antheridia.
B. Transparent cells containing urn-shaped bodies from interior of frond.
C. Urn-shaped body in cell from a part of the frond immediately below a bifurcation.

Further remarks on the Fly's Foot.—If Mr. Tyrrell's theory be correct, "That the Fly uses the hooks as levers to detach the foot," we should expect *à priori* that the Beetle did so: but the contrary is the fact. I placed one (not aquatic, or of the Curculio tribe) under the microscope, feet upwards, which was remarkably slow in its movements, and furnished with two circular pads, and one triangular, possessing trumpet-shaped hairs, and having the power of secreting fluid. When detaching the foot in walking, it raised the hooks first, and

kept them suspended for an appreciable length of time, *before* it raised the pads. I placed a blow-fly for examination, after having removed, under the influence of chloroform, the flap and two hooks of one foot, and about half the hooks of another: it could not attach the foot with one flap efficiently; but the one in which the hooks were so far shortened, that they extended only to the middle of the flaps, it used very well. Query, Would not the flap have been torn through, and half left on the glass, in this case, if the above theory were correct?

When the foot of the Midge (one of the *Tipulidæ*) is in action, it has the appearance of a horse's foot in miniature. I believe the Walrus, although it sometimes exceeds a ton in weight, has a similar apparatus to the Midge, by which it can support itself on the almost perpendicular sides of the immense icebergs it has to traverse.

The *Midge's* foot terminates in a single sucker, and has no hooks wherewith to detach itself.—J. HEPWORTH, *Croft's Bank.*

Microscopic Preparations.—From a notice in the Botanische. Zeitung for November 10, 1854, we perceive that Dr. J. Speerschneider, of Blankenburg, near Rudolstadt, in Thuringia, proposes, apparently with the co-operation of Professor V. Schlechtendal, to issue a collection of microscopical preparations, intended to exhibit the most important points with respect to the structure and development of plants. The entire collection will contain ten to twelve dozen preparations, and will be issued in five to six parts, each of which will cost only three Prussian thalers; and subscribers' names may be sent either to Dr. Speerschneider, as above, or to Professor Schlechtendal, at Halle.