EXPLANATION OF THE PLATES.

Plate XIII.

Fig. 1. *Pagurus ferrugineus* (Norman), slightly enlarged.
Fig. 2. Right arm, side view.
Fig. 3. Right hand, front view.
Fig. 4. *Pagurus lævis* (Thompson); right arm.
Fig. 5. *Hippolyte producta* (Norman); rostrum.
Fig. 6. *Doryphorus Gordoni* (Spence Bate, sp.); rostrum.
Fig. 7. Telson of the same.
Fig. 8. *Dennisia sagittifera* (Norman), twice the natural size.
Fig. 9. Rostrum of the same.
Fig. 10. Carapace viewed from above, showing the structure of the antennal scales and filaments.
Fig. 11. Pedipalp.
Fig. 12. Leg of first pair.
Fig. 13. Telson.

Plate XIV.

Fig. 1. *Palæmon minans* (Norman), slightly enlarged.
Fig. 2. Side view of carapace of the same.
Figs. 3 *&* 4. *Pandalus Thompsoni* (Bell); two forms of the rostrum.
Fig. 5. Internal antennæ of the same.
Fig. 6. Pedipalp.
Fig. 7. Leg of the first pair.
Fig. 8. Right leg of the second pair.
Fig. 9. Left leg of the second pair.
Fig. 10. *Cypridina teres* (Norman), magnified 50 diameters.
Fig. 11. *Philomedes longicornis* (Lilljeborg), magnified 50 diameters: the spine *a* is often absent.

Sedgefield, county Durham,
August 15, 1861.

XXX.—*Notes and Corrections on the Organization of Infusoria, &c.* By H. J. Carter, Esq., F.R.S.

As time progresses, we make new observations and detect errors in preceding inquiries; and he who would still pursue truth will publish both indiscriminately, wishing to record the results of his labours for the benefit of those who may come after him, and thus make some little return for the pleasure he himself has derived from the investigations of others.

Spherical Cells.

In my "Notes on the Organization of the Freshwater Infusoria" (Ann. Nat. Hist. vol. xviii., 1856), I have mentioned "spherical cells or biliary organs (?)" in the list of their contents. This was a mistake, so far as the Infusoria are concerned; for I have since ascertained that the bodies to which I have referred, instead of being homologous with the ciliated cells lining the stomach of the *Planariæ* and Rotatoria respectively, were merely ejected portions of the "abdominal mucus" of *Otostoma*, which, assuming a spherical form, and containing round pellets of half-digested vegetable matter, which had in consequence become of a bright brown colour, so simulated the bile-cells of *Planariæ*, that, at the time, I set them down as identical. No such cells occur in the Infusoria that I have since been able to see; and therefore "spherical cells," in my list of the organs of the Infusoria, should be erased.

Vesicula.

At page 126 *ibidem*, I have described the "contracting vesicle" under the name of "vesicula," as being a more appropriate and more convenient term, and have assigned to it an excretory function; but this has been disputed, and therefore I would now add more to strengthen my inference.

In their 'Études sur les Infusoires, &c.' (p. 42 *et seq.*), MM. Claparède and Lachmann have come to the conclusion that the vesicula is not an excretory organ; and in Pritchard's 'Infusoria' (4th ed. p. 432, 1861) it is stated that Cohn, in *Brachionus militaris* (which has a sinus attached to the vesicula), has "decisively proved, by mingling colouring matter in the water and witnessing a current inwards during each dilatation, and one outwards on each contraction, alternately, that it not only opens into the cloaca, but that it is a respiratory organ."

As regards MM. Claparède and Lachmann's conclusion, I would observe that, throughout their excellent article on the subject, while they endeavour to maintain that the fluid of the vesicula is returned into the vessels on the systole or contraction of the vesicula because the sinuses and vessels become filled immediately afterwards, they never once allude to the possibility that this may take place from the closing of the valves between the sinuses and vesicula at the moment the latter is about to eject its contents externally, which, after careful re-examination, would be my explanation of the phenomenon: viz. the fluid, still flowing on towards the vesicula, like that of the blood towards the heart, is ponded back for the moment in the sinuses and vessels, while the vesicula empties itself, like the ventricles of the heart, in another direction.

If we watch the vesicula in *Paramecium aurelia* as this animal gradually rotates itself upon its long axis, we observe that it is situated peripherally; that when distended and uppermost (nearest the observer) it is spherical in appearance, and presents a small papilla in the centre; and that when it is lateral it is bottle-shaped, the neck communicating with the papilla. Further, we observe that the sinuses are attached round the shoulders of the bottle, and that when the diastole or dilatation takes place, the fundus of the bottle is forced into the interior of the *Paramecium*, while, when the systole takes place, the fundus moves towards the papilla, or externally. Now, if the vesicula was not for ejecting the fluid which it receives from the sinuses and their vessels, but, on the contrary, for returning it into them, why should it have a distensible neck, this neck be attached to a papilla on the *surface* of the body, and the line of direction in which the vesicula contracts be from the fundus to the papilla?

MM. Claparède and Lachmann state that no current can be seen to pass out at the papilla in the Infusoria, and in this they are right; but why? simply because it is impossible to see this without adding some colouring matter to the water, and then the particles are whirled off the surface of the animalcule by its cilia with such rapidity and confusion, that it is equally impossible to discover among them the drop of fluid which may be ejected from the vesicula. But let us take an organism closely allied to the Infusoria, and not covered with cilia, that we may see what takes place there. This brings me to Cohn's statement above mentioned.

I must now assume that the vesicula or contracting vesicle, its sinuses and its vessels, are the homologue of this system in the Rotatoria; but not having been able to get *Brachionus militaris*, I must take *Brachionus urceolaris*, which has no sinus connected with its vesicula, a matter of no consequence here.

The system in *B. urceolaris* consists of a vesicula connected with a set of vessels on each side, which are more or less branched and terminate in blinded extremities, and a few monociliated appendages which are attached to them. In *Paramecium*, where the system is double, each part consists of a vesicula, sinuses, and more or less branched vessels, which also end in blind extremities. Thus the monociliated appendages do not appear in *Paramecium*, nor the sinuses in *Brachionus*. With these exceptions, equally matters of no consequence here, there is such a similarity in every part of these systems in *Brachionus* and *Paramecium*, that it seems to me no reasonable doubt can be entertained that they are homologous.

Now, what is witnessed when we place *Brachionus urceolaris* in water with which colouring matter has been mixed (say fine Indian ink), under a slip of glass which so compresses the *Brachionus* as to keep it stationary and prevent the coloured fluid from getting between it and the eye of the observer? We see that the vesicula becomes filled with a transparent colourless fluid, and that this fluid, as the vesicula contracts, is suddenly forced out of the cloaca into the surrounding medium, where, for a moment or two, it remains unmixed with the coloured water, and then disappears, while, the vesicula dilating again, the same phenomena may be repeated sufficiently often for the observer to feel certain that the fluid which accumulates in the vesicula is uncoloured and that it is ejected externally.

If, then, the water with which the vesicula is refilled comes from the exterior, as Cohn's experiment goes to prove, those who support this view will be inclined to state that, as it passes into the vesicula of *Brachionus urceolaris*, it is strained clear, and as it passes into that of *B. militaris* it is not so. But the sudden way in which it is ejected in *B. urceolaris* indicates a very large opening, and one which would admit the finest colouring matter that can be used, and which was used on the occasion of the experiment above-mentioned, viz. fine China-ink (or, as it is termed, "Indian ink"), very easily. Similar results were obtained by using carmine with a large *Notommata*.

The assumption that it is strained in one instance and not in the other, then, is not sufficient to lead to the conclusion that the vesicula is filled from without; and therefore, disregarding Cohn's statement, I still assert that the fluid is gathered from the interior, conducted into, and expelled by, the vesicula.

Thus we have further confirmation of the vesicula being an excretory organ in the Infusoria, as well as the fact of its being so not only in *Brachionus*, but probably in the Rotatoria generally, so far established.

Besides the monociliated tassel-like bodies which are attached to the vessels of this system in the Rotatoria, the vessels are accompanied by a fine granular substance, which more or less envelopes them; and this is particularly well seen in the segments of the *Naidina* (worms), where there is an organ of this kind in each segment, called by Dr. T. Williams the "segmental organ." (See my description of this, and figure, Ann. Nat. Hist. vol. ii. p. 27, pl. 2, 1858.)

It is not my intention to allude further to this granular substance now; but, as regards the 'tassel-like' bodies, I cannot help observing that they are as much indicative of the excretory nature of this system as any other part. Witness them in one of the largest species of Rotatoria, viz. in *Notommata*, where they exist in scores on one long vessel on each side, while the other long canals have none at all, but are surrounded by the granular matter. Here some of the tassel-like bodies are provided with the usual single cilium running through them, while others are destitute of it, but have in lieu expanded mouths *fringed* with cilia. Does not the cilium act after the manner of a "spiral pump" in raising the water from the abdominal cavity into the tubes, and has not the ciliated border the same effect as the single cilium of *Euglena*, &c., and the fringe of cilia round the disk of *Vorticella* respectively? while the granular substance around the other longitudinal and more tortuous canals, which are wholly without tassels, may perform another excretory function. Lastly, in *Nais fusca,* where this organ becomes the ovisac (see *l. c.*), if I mistake not, the spermatophorous cells are introduced into it through the ciliated opening,—thus still further showing that the current of this system is outward.

The only view, then, that I can take of the vesicula and its vessels is that it is an excretory system corresponding with the renal apparatus in the higher animals; and if I should be right, then the term "vesicula," which I have adopted for "contracting vesicle," will be still more applicable. It is remarkable, too, that in the *Naidina* it should be so intimately connected with the generative system, like these two systems in the higher animals.

Acineta *and its Metamorphoses.*

At page 236 of my "Notes," I have stated, respecting Stein's assumption of the gemmiparous reproduction of *Vorticellæ* from Rhizopods, that "Stein has described it in *Acineta,* and I have since observed it in a Rhizopod undistinguishable from *Amœba Gleichenii.* I have also seen *Vorticellæ* developed singly from *Acinetæ.*"

With the latter part of this assertion I have now nothing more to do, since it was an assumption, taking for granted that Stein was right, and I contradicted it as soon as I had an opportunity of watching the gemmæ which are thrown off by *Acineta* sufficiently long to know what became of them. Thus I stated (Ann. Nat. Hist. vol. xx. p. 37, 1857) that, with Lachmann and Cienkowsky, I never could find that the gemma thrown off from *Acineta* passes into anything but *Acineta.* It swims about rapidly by means of its cilia for some time, then becomes stationary, the cilia disappear, and the capitate tentacula are thrown out for catching its prey or food.

But with the assertion that *Vorticellæ* are thrown off from *Amœbæ,* which, on my authority, has been quoted in Dr. Carpenter's work on 'The Microscope' (I think), and in Pritchard's last edition of his 'Infusoria' (p. 364), the case is different. This has not been contradicted by me, although I now believe it to be just as much deserving of contradiction as the assertion that the gemma of *Acineta* becomes a *Vorticella.* However, as I did witness *Amœba* throwing off young living *Vorticellæ,* a description of the fact should accompany the view I now take of it.

Under date the 27th of March 1854, in my private journal, is a full description of this occurrence (with drawings which were then carefully made for publication), which took place in the following way:—

Some clear water from the main drain of Bombay (which is brackish from admixture with the sea, which flows into it twice daily), having been placed in a basin for the purpose of examining the great development of the common *Amœba* and *Vorticella microstoma* (mihi) which, after standing two or three days, takes place in it, some of these were swept off from the side of the basin with a hair-pencil, and having been transferred to a slide, were covered with a slip of thin glass, and placed under the microscope, when it was observed that some of the *Amœbæ* contained young *Vorticellæ*, in globular transparent spaces respectively, which, by their sudden contractions according to their custom, showed that they were alive and vigorous. Some *Amœbæ* contained one, and others two; and they were apparently in all stages of development as regards size, up to one-fourth of that of a full-grown *V. microstoma*, at which period they were seen to be thrown off by the *Amœba*, and with such indifference that the *Amœba* continued its course, and the rent made in its body closed up as if it was a natural occurrence.

Under the influence of Stein's assumption that the gemma of *Acineta* became a *Vorticella*, and having seen this gemma thrown off by *Acineta*, I immediately concluded that what has just been described was a similar production of *Vorticella* from *Amœba*. There was the young *Vorticella* in all stages of development as regards size, manifesting vigorous life by the activity of the vesicula and its peculiar contractions in totality, apparently encysted in the *Amœba*, and when ejected (which only took place with those which were largest) trimming itself for a few moments and then swimming off.

It was not unlikely, then, that I should have come to the conclusion that *Vorticellæ* were thus being developed from *Amœba*, instead of being previously enclosed for food, and only those thrown off which were so large and powerful that the *Amœba* could not retain them comfortably, which I now believe to have been the case.

But it may be asked, when and how did they become thus enclosed? We have only here to remember with what degree of voracity Infusoria feed when they are brought into direct contact with their food under a slip of glass (e. g. *Stylonychia* and *Chlamydococcus*), and that when they are first placed in this position they are frequently comparatively empty, although they have been taken from water abounding with their food, to come to the conclusion that it is being placed between the two slips of glass which enables them to entrap their food so easily, and therefore that it was the transfer of the *Amœbæ* and *Vorticellæ* together in great numbers from the side of the basin to this position that placed the active *Vorticellæ*, as it were, in the jaws of the slothful Rhizopod. Besides, the fact that the gemma of *Acineta* does not pass into a *Vorticella* having now been determined leaves us without a single instance of probability that *Vorticella* should be in any way developed from *Amœba.*

Having made this explanation, let us for a moment turn our attention to the other point of Stein's theory, viz. the transformation of *Vorticella* into *Acineta,* also witnessed by Udekem (Ann. des Sc. Nat. ix. p. 321, Zool. 1858).

Respecting this metamorphosis, much doubt, and even denial, has been expressed; but although all have not had the good fortune to witness it, yet it seems to rest now on evidence too good to be questioned, although perhaps the right interpretation has not been given to it by those who have seen and described it.

Now, no one can have observed a number of spherical *Acinetæ* (*Podophrya fixa,* Ehr.) adhering to the surface of *Paramecium aurelia* and carried about with it, without connecting it with the parasitic Rhizopods, which, attaching themselves to *Chlamydococcus, Eudorina elegans,* and the like, in a globular form, already convey their germs into their interior, and begin to devour the substance of their host while the latter are yet actively swimming through the water, and without thus coming to the inference that both organisms are of the same nature.

Again, no one can at first witness the change which, almost like a "dissolving view," takes place in the protoplasm and chlorophyll of *Chlamydococcus, Eudorina,* and that of the cells of Algæ generally, during which these pass from their original form into that of a Rhizopod, without inferring that the form produced is merely another one of that which preceded it, and no absolute change. Hence my description of the fancied passage of the vegetable protoplasm into *Actinophrys,* to which I shall more particularly allude by-and-by (Ann. Nat. Hist. vol. xix. p. 259, 1857), and which at the same time also I classed with that of *Vorticella* into *Acineta.*

Seeing, then, the great analogy, if not real identity, that exists between the nature of these organisms, I would suggest that the transformation of *Vorticella* into *Acineta* may be of the same kind as the passage of the vegetable protoplasm into rhizopodous forms—that, in fact, the germ of *Acineta,* like the egg of the Ichneumonidæ among Insects, becomes encysted with the *Vorticella,* eats up its host, and comes out an *Acineta,* as the larva of the Ichneumon-fly, which is hatched in the chrysalis of one of the Lepidoptera, lives upon its host, and comes out of the cocoon, not a butterfly, but a wasp.

That the stalked *Acinetæ* upon *Epistylis* are parasitic can easily be seen, because they are not upon the ends of the dichotomous branches of their host, but fixed to them by single smaller stems of their own. Not only that, but in a pool here (Bombay) where *Epistylis* abounds, the same kind of *Acineta*, apparently, which accompanies it, abounds also on the web of the frog's feet which live in this pool—thus assuming the position of an epizoon; while the cases described and figured by Stein and Cohn, in which the globular form (*Podophrya*) has been developed singly or in great plurality in the interior of *Stylonychia*, *Nassula*, &c., may afford instances of entozoic *Acinetæ*.

Directing our attention to the phases presented by the globular *Acineta*, one of the most striking phenomena exhibited by it is the projection of cilia which takes place round the half which is to swim about, just before its separation from the other half, during duplicative division, since it affords us an instance in which cilia can be put forth and retracted by a previously unciliated Infusorium, the cilia disappearing on the protrusion of the capitate tentacula, unless the same material can be put forth under different forms. Be this as it may, the cilia are present, and their retraction, &c., take place also in the gemma, and their presence in both instances appears to be for enabling the *Acineta* to seek for some living Infusorium upon which to fix itself for food. The swimming away of one while the other half remains stationary, in the duplicative division, seems to show that, when this mode of reproduction takes place in Infusoria, there is always one half older than the other, which is, as it were, the stock, and therefore the young half the bud.

Pythium entophytum, Pringsheim*.

Not altogether unconnected with the foregoing subject is the development which I have described and figured in different parts of the 'Annals,' since 1855, in the cells of the *Characeæ*, *Spirogyra*, *Œdogonium*, &c., and in the bodies of Rotatoria and in the egg of *Nais fusca*.

This development I at first tried to prove to be parasitic; then I thought it was a simple transformation of the protoplasm from one form into another; and, again, now I am compelled to view all these developments as originating from germs previously existing in the midst of the protoplasm when not obviously introduced.

Nothing, however, has impressed me with this belief so much as Prof. Pringsheim's descriptions and illustrations of the conjugation which takes place between the sporangium and filaments of the other species of this genus, to which he has given the name of *Pythium monospermum* (*l. c.*), while at p. 366 (*l. c.*) he states that, although he had not seen the fecundation of *Py-*

* Ann. des Sc. Nat. xi. p. 354, pl. 7. fig. 1 (Bot.), 1859.

thium entophytum, yet the presence of utricles filled with little straight tubes like the seminal bodies of *Vaucheria* and *Saprolegnia* in company with the sporangia of *P. entophytum,* indicates, with great probability, that they are its antheridia. Pringsheim, however, like myself, mistook the nature of this organism formerly, as may be seen by reference to the 'Annals' (vol. xi. p. 294, 1853), where he contended that it was not a parasite, but a reproductive element of *Spirogyra,* in whose cells it occurs, while in 1859 he makes it a *Saprolegnia.*

It was this organism to which I alluded in my communication to the 'Annals,' in 1857, entitled "The Transformation of Vegetable Protoplasm into *Actinophrys,*" where I described the contents of the sporangium as consisting of monociliated polymorphic cells which lost their cilium and put on the radiated form of *Actinophrys*; also that when within the cells of *Spirogyra,* they enclosed the protoplasm and its contents after the manner of *Amœba.* The *monociliated* bodies Pringsheim calls "zoospores;" and these would form the female, while his tubular cells ("tubes") would form the male element of the organism. Thus we have a being which brings us at once close upon the confines of the Animal, Algal, and Fungal divisions of organic life.

In the protoplasm of *Nitella,* as I have figured and described long ago, another form of these rhizopodous parasites abounds; and in the cells of *Spirogyra crassa,* circular nuclei may frequently be seen, which probably belong to *Pythium entophytum.* Thus for the future I would regard all those apparent transformations of the protoplasm as the development of parasitic germs previously existing in it (where they are not obviously introduced), which, under favourable circumstances, that is, where the specific vitality of the cell begins to ebb, begin to assimilate its protoplasm, &c., to their own form; for the protoplasm must be still fresh, as under sudden putrescency they do not appear, but probably as rapidly pass into decomposition as the protoplasm in which they have been living. It is difficult to realize the nature of these changes at first; for, like those of a "dissolving view," as before stated, they are inappreciable; but such, I am now persuaded, is the way in which they must be explained.

Eudorina elegans.

Lastly, at p. 10 of the 'Annals,' vol. iii. 1859, I have made a mistake in correcting what I fancied to be an error in my description of the "green cell" of *Eudorina,* viz. in trying to prove that what I had previously stated to be the "nucleus" was a "starch-cell." Subsequent observation has shown me that I was right in the first instance; for although, both in *Eudorina* and *Chlamydococcus*, the peripheral substance, and perhaps the interior of the nucleus itself, becomes purple and blue under the action of iodine, this cell must be considered the nucleus, while the "granules" in the protoplasm should be viewed as the analogues of the "starch-cell" in the plant-cell. Thus the "green cell" of *Eudorina* consists of the cell-wall with its two cilia, which contains the protoplasm and chlorophyll, the nucleus, the granules, or analogues to the starch-grains, the "red spot," and the two contracting vesicles. Such, too, is a list of the normal contents of *Chlamydococcus* and most of these green cells. The spore-cell of *Eudorina*, after impregnation (*Gyges*, Ehr. ?), I am led to think, has four cilia; but of this more hereafter.

Spongilla.

At p. 13 (*ibid.*) there is also a mistake made in a similar way, and arising from a similar cause, viz. a misgiving of the truth of an inference deduced from deliberate examination, corrected by a too short and hasty one. It is stated, respecting the "ampullaceous sac" of *Spongilla*, that it must have its cilia outside, instead of inside as in the first description. A still more recent examination compels me to state that the first description in this respect should stand as it is, and the cilia be considered *inside*, and not outside, the "ampullaceous sac."

XXXI.—*A Catalogue of the Zoophytes of South Devon and South Cornwall.* By the Rev. THOMAS HINCKS, B.A.

[Continued from p. 262.]

8. *Laomedea neglecta*, Alder.

Common: under stones between tide-marks; dredged on other zoophytes, &c., from in-shore to the Coralline region.

This species, in its Campanularian state, is very abundant on the stems of *Plumularia*, *Sertularia*, &c., from deep water. In such situations, so far as my experience goes, it is seldom and sparingly branched.

From a tide-pool at Meadfoot, near Torquay, I have it of large size (about an inch in height), much branched, and bearing capsules. The latter, which have not yet been described, are pyriform, and produced in the axils and on the pedicles which support the cells. The ova are developed into the perfect larval form within an external gelatinous marsupium.