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ORIGINAL COMMUNICATIONS.

On the Structure *and* Relation *of the* Corpuscula Tactus (*Tactile Corpuscles or Axile Corpuscles*), *and of the* Pacinian Bodies. By Thomas H. Huxley, F.R.S.

In February, 1852, Professor Wagner published in the Göttingen 'Gelehrte Nachrichten,' the results of some observations, made by G. Meissner and himself, the tendency of which was to establish the existence of peculiar bodies in certain of the papillæ of the fingers and palm of the hand, to which, from their relation to the nerves entering the papilla, he ascribed special functions, and thence proposed to confer upon them the name of *corpuscula tactus*—Tactile corpuscles. Wagner's principal positions are the following:—

1. The papillæ of the hand are of two kinds—nervous and vascular—the vascular papillæ containing no nerves, and the nervous papillæ possessing no vessels.

2. The nervous papillæ contain a peculiarly constructed oval mass, like a fir-cone, composed of bands or rows, arranged one behind the other.

3. The dark-bordered nerve-fibres enter the papilla, pass to this "tactile corpuscle," and terminate in it, either free or dividing into five branches.

4. These corpuscles are analogous to the Pacinian bodies.

5. They are specially subservient to sensation.

Professor Kölliker, whom this Memoir touched somewhat directly, replied in the 'Zeitschrift für Wissenschaftliche Zoologie' of the following June, by an essay on the same subject (Ueber den Bau der Cutispapillen und der sogenannten Tactkörperche R. Wagner's), in which, having carefully repeated and extended Wagner's observations, his general conclusions are:—

1. *a.* The corpusculated papillæ often contain vessels.

b. The vascular papillæ of the lip contain nerves.

c. In the lip and hand there are a few papillæ without axile corpuscles and with nerves.

2. The tactile corpuscle is not a peculiar body, but the ordinary embryonic connective tissue remaining as the axis of the papilla. Kölliker therefore proposes to call it "axile corpuscle."

3. The nerves do not enter and terminate in the corpuscle but wind round it and form loops.

4. The corpuscles are not specially subservient to sensation.

Besides the surface of the hand Kölliker found these corpuscles only in the red edges of the lips and at the point of the tongue.

Finally, in Müller's Archiv for 1852, Wagner, in a communication accompanied by very good figures (Ueber der Tactkörperchen, *Corpuscula Tactus*, Müll. Arch. II. 4), referring to the discrepancies between Kölliker and himself, considers the question as to the peculiarity of the structure of the corpuscles to be still open; he denies that the nerve fibres form loops on the axile corpuscles (quoting, in confirmation of his own views, Meissner, Ecker, Brüche, and Günsburg), and, also, that nerves enter any papillæ but those provided with tactile corpuscles. Wagner admits, however, that certain of the papillæ containing axile corpuscles also exhibit vascular loops, but these, according to him, always have nervous tissue at their extremities, and are in fact formed by the coalescence of a nervous and vascular papilla. Without pretending to decide, when two such eminent doctors in physiology disagree, I beg to lay before the reader the following results of my own examination of this matter, accompanied by some figures drawn on a larger scale, and with more attention to detail than those furnished by Wagner or Kölliker. I can best arrange what I have to say in the order of the points in dispute, as given above.

1. In the human finger I have met with corpusculated papillæ containing vascular loops, though rarely (Pl. I. fig. 2); but I have observed no papillæ without corpuscles, to present nerves. That there is not, however, necessarily an inverse relation between the presence of vessels and that of nerves, is shown by the fungiform papillæ of the sides of the base of the tongue in the Frog, in which very evident dark-contoured nerves may be seen terminating in papillæ, without any trace of a tactile corpuscle, and with a large vascular loop (fig. 6).

2. Everything I have seen leads me to believe with Kölliker, that the 'corpuscle' is not histologically, in any re-

spect, a special structure, but merely rudimentary connective tissue (areolated embryonic connective tissue of Kölliker), exactly resembling that which is to be found in the rest of the papilla. This consists in fact of a homogeneous matrix in which endoplasts (nuclei) are embedded, and which, in various directions surrounding and radiating from these, is metamorphosed into a substance more or less resembling elastic fibre. The sole difference from the surrounding substance presented by the corpuscle consists in this, that these elastic bands and filaments are more or less parallel to one another, and perpendicular to the axis of the corpuscle (fig. 1).

In one respect, however, I believe that the corpuscles are peculiar, and something more than the mere, imperfectly formed axis of the papilla. Kölliker has pointed out (*l. c.* p. 67) that the nerve-tubules which enter the papilla are accompanied by a delicate neurilemma, and I believe that the "corpuscles" are its continuation and termination. In structure, the neurilemma which surrounds the more delicate branches of the nerves in the human finger (fig. 7) is identical with the "corpuscles," except that in the former the elastic element is disposed parallel with the nerve fibre, while in the latter it is more or less perpendicular to it. In fact, I believe, that the "corpuscle" is simply the modified extremity of the neurilemma of the nervous tubules which enter the papillæ.

3. With respect to the extremely difficult question of the mode of termination of the nerves, I may state that, without having any reason to urge against the existence of loops (on the contrary, having observed them very distinctly in the cutaneous papillæ of fishes), I have never been able to convince myself of their presence, and frequently when I believed I had such cases before my eyes, the use of a higher power, or the causing the papilla to turn a little, would undeceive me.

On the other hand, it is by no means difficult to obtain the clearest possible evidence of the occurrence of the so-called free ends (figs. 3, 4, 5). The dark-contoured fibres pass, sometimes only a little beyond the proximal extremity of the corpuscle (figs. 4, 5), sometimes quite to its distal end (fig. 3), and here terminate by one or two pointed extremities, which appear to be continuous with the tissue of the corpuscle. I have never been able to obtain any evidence of the entrance of a dark-contoured nerve fibre into a "corpuscle." My belief that the nerves in the corpusculated papillæ of Man do really terminate in this manner, is strengthened by the ease with which this mode of termination may be demonstrated in the papillæ of the tongue of the Frog, to which reference has been made above (fig. 6). Here four or five coarse nerve-fibres enter the papilla, run to its very extremity, become pointed, abruptly lose their fatty nature, and terminate in the delicate reticulated fibres, which represent the elastic element of the connective tissue of the part.*

4. Wagner, as we have seen, compares the corpuscles to the Pacinian bodies, and I think with great justice. The Pacinian bodies are, as is well known, principally found attached to the nerves of the hand and foot in Man, to those of the mesentery in the cat, to the nerves of the extremities of many animals, to those of the skin and beak in birds, and to the intercostal nerves of the Boa constrictor. They are commonly said to be composed of numerous corpuscles of connective tissue, arranged concentrically, and separated by a clear fluid. The innermost contains, besides this fluid, a nervous fibre, which terminates in a free clavate or branched extremity.

In the human hand, however, I have invariably found that this description of their structure is not exactly correct. In fact, I find no interspaces filled with fluid, nor any central cavity. If the body be cut in two, each half remains as hard and uncollapsed as before; if it be torn, each layer of the "corpuscle" is seen to be united to its neighbour by a delicate, transparent, more or less granular, or sometimes fibrillated substance. Again, the nerve lies not in a cavity, but in a solid homogeneous substance; and, so far as I have seen, terminates more or less gradually in a portion of this mass, in which great numbers of endoplasts (nuclei) lie, and which has thence almost the appearance of cartilage.† The structure of the rest of the body

* Much has been said as to the possibility of confounding capillaries with nerves; but I conceive that such a mistake could hardly be made by any careful observer, unless perhaps strong alkaline solutions had been allowed to act unwatched upon the preparation. I have made use of both acetic acid and caustic soda, and I find the latter more available in discovering nerves, the former in making out vessels and the general structure of the papilla; inasmuch as it renders their "nuclei" more obvious, while soda makes them less so. It is very useful sometimes to use these re-agents alternately; and it is a good rule to apply them to the object only while under the microscope, so as to watch their gradual operation.

† According to Will (Reichert's Report, p. 69, Müll. Arch. 1851), the contents of the central capsule of the Pacinian body in Birds is formed by a dense cellular mass, and closely applied cells exist in the external neurilemma. From observations upon these bodies in the Pigeon and Duck I can confirm this statement: in fact, the Pacinian bodies of the birds are very like the young forms of those of Man. I have also noticed, as Wagner states (l. c. p. 499), that the internal cellular mass is occasionally transversely striated, somewhat like a tactile corpuscle. The Pacinian bodies in Birds are much more superficial than in Man, being situated in the superficial layer of the corium, close to the sacs of the feathers. In the Pigeon they are very small, frequently not more than 1-150th—1-200th of an inch

is, essentially, the same,* and the appearance of their concentric capsules is produced by the arrangement of their endoplasts in concentric layers in the outer part of the Pacinian body, and their connexion by the laminæ and fibres of more or less imperfect elastic substance.

The concentric lines in the Pacinian bodies are no more evidence that they are composed of capsules, than the parallel lines in the neurilemma of small nervous twigs (fig. 7) are evidence that it is composed of concentric tubes. In each case the appearances depend simply upon the disposition of the lines of elastic tissue. In fact, the Pacinian bodies are nothing more than thickened processes of the neurilemma of the nerve to which they are attached; and differ from the "tactile corpuscles" only in the circumstance that the thickening takes place on each side of the nerve fibril, while in the Pacinian body it takes place on both sides. The difference in the direction of the apparent layers is not so great as it seems, since, at each extremity of the Pacinian body, these are, as in the tactile corpuscle, perpendicular to the direction of the nerve.

5. The evidence with respect to the physiological functions of either the *corpuscula tactus* or of the Pacinian bodies is wholly negative; and it seems useless to enter upon the region of hypothetical suppositions. But I think that Comparative Anatomy promises to offer some assistance in this case by showing that these structures form the lowest terms, in a series whose higher members attain a very great development in certain animals, though their precise function is in many cases obscure. The homology of the tactile corpuscles with the Pacinian bodies appears, from what has been said, to be clear. What are the Pacinian bodies? Mr. Bowman, in his article on this subject in the Cyclopædia of Anatomy, will not decide upon their function, but points out their close similarity to certain bodies described by Savi in the Torpedo, and subsequently more fully described by Leydig (Beiträge zur Anat. d. Rochen. u. Haie.). These are capsules of homo-

in their long diameter, and possess only one or two "capsules," with a proportionally large inner mass. In the Duck they are to be met with in great numbers in the skin of the beak, especially in the ridged portion at its edges. Here the Pacinian bodies, often very small (1-400th of an inch), lie immediately under the epidermis, with their long diameters more or less parallel to the surface; and the nerves are related to them, just in the same manner as those of the fingers are to the tactile corpuscle. It is difficult to suppose that they have not here some special reference to the sense of touch.

* Compare Strahl. Müll. Archiv, 1848, who gives a similar account of the structure of the layers.

geneous connective tissue, containing a semi-solid gelatinous substance, and inclosing a knob-like process; the termination of the stalk of the vesicle. A nervous bundle passes through the stalk, accompanied by a vessel, and branches out in the knob; its fibres become pale and terminate here, not passing through as Savi stated. (Diag. C.)

In the Rays and Sharks, bodies precisely similar to these, open by a tubular neck upon the outer surface of the skin. In the Sharks they have no special external hard capsule, while in the Rays they are provided with such a capsule, composed of condensed connective tissue. (Diag. D. E.)

In the osseous Fishes, ampullæ, similar to these, connected together by a longitudinal tube open on the sides of the body along the so-called lateral line. The systems of each side are connected by a transverse tube which passes over the occiput. In the Sharks and Rays, organs of an exactly similar nature form a system of ramifying tubes in the head and over the sides of the body. These organs have hitherto been known as the "muciparous canals;" though, as Leydig has well shown, they contain a semi-solid gelatinous material, and have nothing to do with the mucus of the skin, which is formed by the altered epidermic layer. As Leydig has pointed out, then—the *Pacinian bodies*, the *Savian bodies*, and the so-called *muciparous canals* of osseous and cartilaginous fishes are homologous organs, and form a series, whose lowest term, if Wagner's conclusion be correct, is formed by the *corpuscula tactus*. What is the highest term? In the most complex ampulla, or muciparous canal, of a Ray or Shark, we find—1. externally a thick coat, composed of condensed connective tissue; 2, a nervous twig penetrating this, and passing to—3. an internal delicate sac, which contains a gelatinous matter, communicates with the exterior, and is lined by a layer of cells continuous with the epidermis: on the walls of this the vessels and nerves terminate. Now, we have only to conceive a single hair, developed within one of these ampullæ and taking the place of the clear gelatinous matter, to have a *vibrissa*, such as is met with in almost all the Mammalia about the lip and eyebrow (see Diagr. F); and I conceive that the *vibrissæ* are, in fact, the most complex and fully developed forms of this series of cutaneous organs.* Now, the *vibrissæ* are, without doubt, delicate organs of touch, and the mucous canals of Fishes appear to be very probably of the same nature; but when we

* The auditory labyrinth is constructed on precisely the same plan as the muciparous canals of fishes, and the eye on that of a *vibrissa*, as might readily be demonstrated; so that all the organs of sense are to be regarded as modifications of one and the same plan.

come to the Savian and Pacinian bodies, and to the Corpuscula tactus, two possibilities arise—either they may be still the instruments of a modified sense of touch, or they may be merely rudimentary representatives of the more completely formed organs. At present there appears to be no sufficient evidence to decide this point; and I would merely wish to draw attention to the fact, that these organs are not isolated structures, but form a series, with the function of whose highest members only, we are at present fully acquainted.

Some Observations on the ILLUMINATION *of* TRANSPARENT OBJECTS. By GEORGE RAINEY, M.R.C.S., Demonstrator of Anatomy and Microscopic Anatomy at St. Thomas's Hospital.

IT is observed by an excellent optician and writer on the microscope that "the manner in which an object is lighted is second in importance only to the excellence of the glass through which it is seen. In the investigating of any new or unknown object it should be viewed in turns by every description of light, direct and oblique, as a transparent object and as an opaque object, with strong and with faint light, with large angular pencils and with small angular pencils, thrown in all possible directions. Every change will probably develop some new fact in reference to the structure of the object." These remarks are so true that it is not too much to say that the power and perfection of the best modern lenses cannot be correctly estimated or fully appreciated unless employed in conjunction with the best modes of illumination; nor can the best methods of illumination be properly tested without the best lenses. But, in proportion as these optical inventions, like most other contrivances, approach perfection, so do the difficulty and care necessary in using them increase; and hence, to secure their full advantage, it becomes the more necessary to possess a certain amount of knowledge both of their construction and their action.

In the judging of optical instruments it is of importance that appropriate objects should be examined — namely, such as have upon them the most delicate, though distinct markings.

I know of no microscopic specimen better adapted for testing the excellency both of lenses and condensers than the one now generally in use for this purpose — the *Pleurosigma angulatum.*

I shall frequently have occasion in the following remarks to allude to Gillett's condenser and to Wenham's paraboloid, but as these excellent contrivances have been in use for some time, and therefore are generally known, it would be superfluous in me to describe their several parts, or to do more than simply to name them.

Nothing can show the advantages of the improved method of illumination better than a careful examination of the object just named, first as illuminated in the ordinary way, and then as illuminated in the improved one; afterwards contrasting the appearances which it presents under these kinds of illumination.

If the *Pleurosigma angulatum* be examined with a lens of 1-8th inch focus, and 150° angular aperture, by good daylight reflected upon it by a plane or concave mirror in the ordinary way, little more than the mere outline of the object will be visible. Nor will any advantage be gained by increasing the magnifying power of the microscope by the employment of the deeper eye-pieces. On the contrary, these are of more harm than benefit, in consequence of the diminution of light which they occasion. If, now, Gillett's condenser be substituted for the mirror, and the light be admitted only through the four or five smaller apertures of the revolving diaphragm of the condenser, so that the central rays only of the pencil of light pass through the object, no dots or lines will be seen upon it, but its appearance will be the same as when the mirror was used; nor will the deeper eye-pieces be of any use. Of course I am speaking of the condenser when properly centered and adjusted. The non-appearance of markings on the *Pleurosigma* under this kind of illumination is differently explained. That it is not due to a deficiency of light is evident from the following experiment. If one of these apertures be stopped, and the diaphragm be so placed that only a small quantity of light can pass obliquely through the condenser by the next hole, distinct parallel lines are made apparent, although the field of view is much darker than before. And that it is not in this instance caused by an excess of light, as imagined by some microscropists, is certain from the following fact. Turn the revolving diaphragm so that the pencil of light passing through the condenser is just sufficient to render the markings on this object visible; afterwards bring successively the larger apertures under the condenser, and it will be seen that the marking, in the place of becoming less apparent as the diameter of the pencil of light increases, becomes more so.

From these facts it is obvious that the appearance or non-appearance of lines on the *Pleurosigma* is altogether independent of the quantity of light, and due only to the direction in which the rays are made to fall upon this object. As it has been shown that the rays nearly perpendicular, called direct rays in contradistinction to the oblique ones, are of no use in the illumination of the object in question, and as its marking is rendered perfectly distinct by oblique light, it is evident that the most proper illumination in this case is that in which the central part of the pencil of light is cut off by a stop from the object, and only the oblique rays allowed to pass through it. These ends are fully attained by Mr. Gillett's condenser: and, as by this contrivance the oblique rays can be thrown equally on all sides of the minutest particles, shadows are prevented; and markings, which, when illuminated by oblique light only on one side, appear as lines, are in this way resolved into their component dots.

It now remains to show in what manner oblique light acts in developing structures which cannot be seen by nearly perpendicular rays. I may observe that it is not a question of degrees of distinctness that I am considering, but the fact that parts, which are perfectly distinct when examined by one kind of illumination, are totally invisible when examined by another kind.

The explanation of this fact seems to be deducible from the following considerations.

Suppose a part to be made up of two substances intimately connected, though distinct from each other, and both definitely arranged, whose refractive powers differ so little that they cannot be distinguished from one another under the microscope by the slight difference in their refraction of the light passing through them. This is, I believe, the condition of the *Pleurosigma angulatum* and other objects of a similar kind. Now, if the light, by any kind of illumination, can be prevented passing through one of these substances—the one having the greater refractive power—whilst it passes freely through the other, they will then become perfectly distinguishable, the one appearing dark, the other light. This is what seems to take place when such objects as the one in question are illuminated by oblique light; for a ray of light cannot pass out of a denser into a rarer medium if the angle of incidence exceed a certain limit, and this limit is different in substances of different refractive powers. Thus all rays incident on water, at an angle greater than 48° 36′, having to pass from it into air will not be refracted, but reflected. In the same way, all rays incident on glass, at an angle greater than 41° 49′, and passing from it into air, will not be refracted, but suffer total reflection. Hence, applying these facts to the *Pleurosigma*, I think that it admits of but little doubt that the dots appear dark only because the light beneath falling upon them at an angle greater than that at which all refraction ceases, and total reflection begins, cannot be transmitted, and hence these dots are seen as opaque bodies intercepting the passage of the light to the eye; whilst, on the contrary, the other material, having a lower refractive power, and therefore allowing all the oblique rays incident upon it at the same angle to pass through it, will necessarily appear bright.

The correctness of this conclusion will appear more probable when it is recollected that these two substances are distinguishable not by the one refracting the light differently to the other, but by the fact of one refracting it, and the other not; the former appearing bright and transparent, the latter dark and opaque.

As respects the non-appearance of the markings under direct illumination, it may be observed that, as the rays in this instance may nearly all be supposed to be incident upon the object at an angle less than that at which refraction ceases, they would be refracted by both substances nearly in the same degree, and therefore each would appear to be transparent, and the whole almost homogeneous.

These few facts show that, when all such objects as the *Pleurosigma* are illuminated by oblique rays, they must be examined by lenses which admit a large pencil of light, that is, have a large angle of aperture, in order that an allowance may be made for the diminished quantity of light which penetrates the object and enters the eye, in consequence of the total reflection from the lower surface of the dots.

Having considered the class of objects best fitted to display the effects of oblique illumination, I will now consider those which are best seen by light passing through them almost perpendicularly.

Although oblique light answers so well in the instances I have adduced, there are some structures and objects for which it is totally unfit, and which can only be successfully examined by rays passing through them almost perpendicularly, that is, by direct light.

Amongst this class of objects are all those which strongly refract light, either from their density or spherical figure, as, for instance, most recent structures, either animal or vegetable, these consisting chiefly of corpuscles, and highly-refractive particles of various forms and sizes.

The reason why such objects cannot be seen when illuminated by rays falling upon them very obliquely, but are distinct when illuminated by those which fall upon them almost perpendicularly, will, I think, be apparent from a few considerations respecting the undulatory theory of light. According to this theory, the phenomena of refraction are due to vibrations produced in an elastic medium occupying the intervals between the particles of all transparent substances by a force proceeding from a luminous body; and the elasticity of this medium is less in proportion to the refractive power of every transparent substance; or, in other words, the greater the refractive power of any substance the greater also will be the force required to excite undulations in the less elastic medium diffused through it. Hence, as the effect of the same force acting upon a resisting medium is proportional to the direction in which it acts, being at its maximum when the line of the force is perpendicular to that of the resistance, and at its minimum when the angle of inclination is upon the point of vanishing, it must be clear, that light falling obliquely upon a transparent surface will exert less power in producing the effects of refraction than if it fell perpendicularly; so that when the rays of light fall very obliquely upon a highly-refractive substance, their effect will be too feeble to excite its condensed vibratory medium into undulations, and therefore the rays will simply pass by it, producing at its border the effects of interference of light.

This is precisely what takes place when oblique light is employed to illuminate objects possessing a very high refractive power; whilst rays falling nearly perpendicularly upon the same objects, and thus acting upon them in a direction the most favourable for producing the effects of refraction, penetrate, as it were, their substance, and render their structure apparent in all its detail.

Though there are these two classes of objects, one requiring for their illumination oblique rays, and the other nearly perpendicular ones, yet the majority of compound organs require both kinds of light. Many of them are structures which, though they may appear most beautiful under direct illumination, will, by oblique light, be made to reveal something in their composition which would have remained concealed without this light.

Structures, if examined properly, should be subjected to a kind of microscopic analysis, in order that nothing in their composition may be overlooked.

Having shown some of the advantages of the present methods of illuminating microscopic objects, I will consider some defects in these methods, which have been in a great measure overlooked, and also the best way of obviating them.

This will be best done by carefully observing the effects of different modes of illumination upon those microscopic objects whose precise form and optical properties are known.

For this purpose I will first describe the appearances presented by microscopic globules of mercury of different sizes when illuminated by Gillett's condenser and Wenham's paraboloid.

Such globules can easily be obtained by condensing the vapour of boiling mercury upon a piece of glass, and then causing some of the particles to run together, with the point of a needle.

If one of these globules, about 1-300th of an inch in diameter, be examined by a lens of half-inch focus, and illuminated by Gillett's condenser, all rays coming from other sources being completely cut off, and the light admitted only through the smallest aperture of the revolving diaphragm, it will present, when the margin is in focus, a circular, darkish surface with an obscure ill-defined light in the centre; but when the nearest surface is brought into focus, the central spot of light will become distinct and well defined. If the diaphragm be revolved, so as to bring under the condenser the larger apertures, the size of the central spots of light will gradually increase in proportion to the size of the apertures. If, now, one of the stops be brought under the centre of the condenser there will be seen on this globule, in the middle of the illuminated circular space, a distinct image of the stop, which can be recognised by the cross-bar which joins the circular disk to the edge of the opening; and these can further be shown to be the image of the parts just mentioned by revolving the condenser, when they will be seen to move and to change their direction and position according to that of the condenser. If there be several globules of different sizes in the field of view, every one of them will have an image of the stop upon it. Of course in proportion as the globules are more minute, the images will be less recognizable, and on those about 1-1000th of an inch in diameter they can be distinguished only as a very minute circular spot with a dark point in its centre. Globules much smaller than these present only a minute point of light in their centre; and the smaller, those about 1-15000th of an inch, appear entirely dark. However, when higher magnifying powers are employed, an image of the stop can be distinguished on globules 1-4800th of an inch in diameter. If any of the globules have been a little compressed by the piece of thin glass placed upon them, to keep them from dust, so that their spherical figure is destroyed, no image will be formed on them; but the flattened central space, when the stop is under the condenser, will be faintly illuminated; if the place of the stop, however, be occupied by one of the apertures, it will be very brightly illuminated. Such are the appearances of globules of mercury under the higher power of the microscope. But if they are examined by a lens of lower power—one-inch focus, with one of the larger stops under the condenser—they will appear on a dark ground, as when illuminated by Wenham's paraboloid. There will be a well-defined ring of light around each globule, and at its centre an image of a stop; the only difference in their appearance as illuminated by these two instruments being this, that when the latter is employed, the light is a little brighter, and that in the place of the image of a stop in the centre of the globules there is a very distinct one at the end of the paraboloid, and of the cross-bar placed within the tube to support the central disk, which can be seen to move and change its direction when the instrument is made to revolve.

(*To be continued.*)

Paper on the Discovery of Quinine *and* Quinidine (*β. Quinine*) *in the Urine of Patients under medical treatment with the Salts of these mixed Alkaloids.* By W. Bird Herapath, M.D., Bristol.

It has long been a favourite problem with the scientific professional man to trace the course of remedies in the system of the patient under his care, and to know what has become of the various substances which he might have administered during the treatment of the disease.

Whilst some of these remedies have been proved to exert a chemical change upon the circulating medium, and to add some of their elements to the blood for the permanent benefit of the individual, others, on the contrary, make but a temporary sojourn in the vessels of the body, circulating with the blood for a longer or shorter period, but being eventually expelled and eliminated from it at different outlets and by various glandular apparatus.

Some of these substances being more or less altered in chemical composition, in consequence of having been subjected to the various processes of vital chemistry during their transit through the system; whilst others, having experienced no alteration in their constitution, but having resisted all the destructive and converting powers of the animal laboratory, have been by various means again separated from the excretions by the physiological and pathological chemist, in their pristine state of purity.

It has recently been somewhat more than a conjecture that, in common with others of the vegetable alkaloids, quinine may be included in the latter class of remedial agents; and several methods of discovering its presence have emanated from different scientific observers. It has been occasionally traced in the urine of patients suffering from intermittent fever, to whom large doses of quinine have been necessarily administered. However, the nature of the tests hitherto employed, and the various processes adopted, require large quantities of the fluid for examination, and the imperfection of the evidence resulting from the experiment still throws considerable doubt upon the value of the conclusions arrived at. It is merely necessary to allude to tannic acid and tincture of iodine as the usual tests employed, both being very far from efficient means of detecting minute quantities of quinine in organic fluids.

Having been struck with the facility of application and the extreme delicacy of the re-action of polarised light, when going through the series of experiments upon the sulphate of iodo-quinine (Phil. Mag., March 1852, and September of the same year), I determined upon attempting to bring this method practically in use, for the detection of minute quantities of quinine in the excretions or animal fluids.

After more or less success by different methods of experimenting, I have at length discovered a process by which it is possible to obtain demonstrative evidence of the presence of quinine, even if in quantities not exceeding the 1-100,000th part of a grain; in fact, in traces so exceedingly minute that they would be perfectly inappreciable by any other process. The same method (by a slight modification) has also enabled me to prove the fact, that quinidine escapes from the system by the kidneys in an unaltered state, which, as far as I am aware, has not hitherto been observed, although it might have been almost assumed, from the great analogical resemblance existing between these alkaloids.

The subject furnishing the urine for examination was a man suffering from tetanus, in consequence of an injury to the great toe. Amputation was performed at the Bristol Infirmary by Mr. Morgan. The patient's name was R. Alexander. My pupil kindly procured the specimen for me.

The tetanic symptoms were at first treated by the exhibition of 5 grains of the disulphates of quinine (and quinidine), with half a grain of canabis indica, every three hours. He consequently took 40 grains of the mixed disulphates in the period of 24 hours.

The urine had a greenish-yellow appearance, and, upon standing, it deposited a brownish-yellow sediment; the urine possessed a slightly acid re-action, and had a Sp. G. 1·032.

The sediment examined by the microscope showed prisms and lozenges of uric acid, together with amorphous urate of ammonia.

The deposit treated upon the field of the microscope with ammonia instantly became changed; the crystals of uric acid were rendered more clearly defined in consequence of the solution of the amorphous urates. The phosphate of ammonia and magnesia was subsequently deposited upon the slide as a cloudy mass when examined by the unassisted vision, but as a magma of very minute radiating aciculæ when magnified 60 diameters.

The fluid urine was cautiously decanted from the amorphous and crystalline deposits.

(A) Half a pint of this urine was treated with liquor potassæ until decidedly alkaline; it was then repeatedly agitated with pure washed ether; the ethereal solution having had time to separate by repose, was carefully removed by a pipette; and having been transferred to a counterpoised test-tube, it was evaporated to dryness in a warm-water bath; the residue weighed 0·79 grain after being kept at 212°, until no further loss of weight occurred.

(B) A magma of phosphates and adherent alkaloid still remained above the urinous substratum; this was also removed by a pipette, and transferred to a porcelain capsule; evaporated to dryness at 212°, and this residue exhausted by ether, the ethereal solution evaporated to dryness by a warm-water bath as before, and the residue dried at 212° gave ·61 gr. additional alkaloid.

Therefore $\cdot 79^{a} + \cdot 61^{b} = 1\cdot 4$ grains of alkaloids were obtained by these two operations from the 8 fluid ounces of urine.

Now, to determine if it contained quinine, the following process was followed:—

Test fluid.—A mixture of 3 drachms of pure acetic acid, with 1 drachm of alcohol, and to these were added 6 drops of diluted sulphuric acid (1 to 9).

One drop of this test fluid is to be placed on a glass slide, and the merest atom of the alkaloid added; time given for solution to take place; then, upon the tip of a very fine glass rod, a very minute drop of tincture of iodine added: if quinine be present, the first effect is the production of the yellow or cinnamon-brown coloured compound of iodine and quinine, which shows itself as a small circular spot, whilst the alcohol separates in little drops, which, by a sort of repulsive movement, drive the fluid away: after a time the acid liquid again flows over the spot, and the polarising crystals of sulphate of iodo-quinine are slowly produced in beautiful rosettes; this experiment succeeds best without the aid of heat.

To render these crystals evident, it merely remains to bring the glass slide upon the field of the microscope (having $\frac{1}{2}$-inch objective and lowest power eye-piece), with the selenite stage and single tourmaline beneath it: instantly the crystals assume the two complementary colours of the stage; red and green, supposing the pink stage is employed; or blue and yellow, provided that the blue selenite is made use of—all those crystals at right angles to the plane of the tourmaline producing that tint which an analysing plate of tourmaline would produce when at right angles to the polarising plate; whilst those at 90° to these educe the complementary tint, in the same manner as the analysing plate would have done if it had been revolved through an arc of 90°.

This test is so ready of application, and so delicate, that it must become *the test* for quinine.—(*Vide* Pl. I. figs. 1 and 2.) Not only do these peculiar crystals act in the way just related, but they may be easily proved to possess the whole of the optical properties of that remarkable salt of quinine, so fully described by me in the Phil. Mag. for March, 1852; and the chemical analysis of which was published in the number for September of the same year. In fact, these crystals are perfectly identical with the sulphate of iodo-quinine in every respect.

To test for quinidine it is merely necessary to allow the drop of acid solution to evaporate spontaneously to dryness upon the glass slide (before and without the addition of iodine), and to examine the crystalline mass by two tourmalines crossed at right angles, and without the selenite stage; immediately little circular disks of white, with a well-defined black cross very vividly shown, start into existence. should quinidine be present even in minute quantities. Fig. 3. This fig. was drawn from a slide prepared by the author from the urine of the same patient; about 1-20th part of a grain of the etherial extract was used by him in the manner described.

This is generally the case if "hospital quinine" or that of the British Alkaloid Company has been employed: these drugs severally contain a very large per centage of quinidine; the former at least 50, the latter about 20 per cent. But Howard's di-sulphate of quinine scarcely contains 5 per cent. of quinidine, according to my experiments. These substances are easily separated, in consequence of the much greater solubility of the di-sulphate of quinidine in cold water, thus—

One part of di-sulphate of quinine requires 740 parts water, at 60°.
One part of di-sulphate of quinidine requires 340 parts water, at 55°.

so that the latter salt is more than twice as soluble as the former.

If we employ the selenite stage in examining this object, depicted at fig. 3, we obtain one of the most gorgeous appearances in the whole domain of the polarising microscope —the black cross at once disappears, and is replaced by one which consists of two colours, being divided into a cross with a red and a green fringe, whilst the four intermediate sectors are of a gorgeous orange yellow: these appearances alter upon the revolution of the analysing plate of tourmaline. When the blue stage is employed, the cross will assume a blue or a yellow hue, according to the position of the analysing plate.

These phenomena are analogous to those exhibited by certain crystals of boracic acid, and also by the circular disks of salicine (prepared by fusion*), the difference being that the salts of quinidine have more intense depolarising powers than either of the other substances; and the mode of formation effectually excludes these from consideration. Quinine prepared in the same manner as the quinidine has a very different mode of crystallization; but it occasionally presents circular corneous plates, also exhibiting the black cross and white sectors, but not with one-tenth part of the brilliancy, which of course readily enables us to discriminate the two.

Having shown in my previous papers that none of the vegetable alkaloids, when treated with sulphuric acid and iodine, possess the power of forming crystalline compounds of similar properties, and these artificial quinine tourmalines being pre-eminent in their action on light, it follows that the existence of these crystals is a *demonstration* of the presence of quinine.

It has also been proved by me, that quinidine (β Quinine) cannot produce them, therefore we perceive that this alkaloid passes out of the system without experiencing any elementary change.

One subject is worthy of remark: the patient was taking 40 grains of the di-sulphates of quinine (and quinidine); there were found 1·4 grain of alkaloids, which would be equivalent to 1·884 of the di-sulphate; and if the patient voided three pints of urine in 24 hours, we should only account for 11·304 grains of the remedy employed, leaving a deficiency of 30 grains nearly, either to be assimilated by the body or to be

* I am indebted to my friend Mr. John Thwaites for this fact.

destroyed in its transit through the vascular system, or lost from other causes.

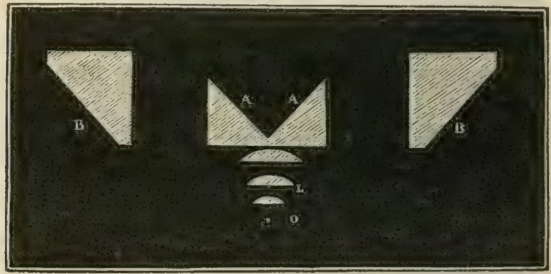
It would be interesting to undertake a series of experiments to determine whether other excretions of the patient contain this remedy, and also to discover what length of time elapses after ingestion, before all evidence of its elimination by the kidneys ceases: this being done, we may be in a position to say what the *medical equivalent* of quinine may be in a given disease.

On the BINOCULAR MICROSCOPE. By Prof. J. L. RIDDELL. Univ. La., New Orleans. Read before the American Association for the Advancement of Science July 30, 1853; Cleveland Meeting. (Communicated for the London Quarterly Journal of Microscopical Science, by the Author.)

IT is proper to premise that some brief notices of the binocular microscope (devised in 1851, constructed in 1852), have already appeared in Silliman's Journal and elsewhere. I now desire to submit a few remarks and explanations to the members of the Association; and at the same time to exhibit different forms of the instrument, so that the members interested in the microscope may form a definite opinion of the value and utility of the improvement.

The following diagram (fig. 1) will serve to illustrate the method first devised and put in practice. It shows as a longitudinal section of the position of the objective and the prisms for producing binocularity.

Fig. 1.



O, represents the object to be seen.

L, the objective combination, always brought as near as practicable to the prisms.

A, A′, two isosceles rectangular prisms of fine glass, in contact by edges somewhat ground away. The light entering the prism A through the objective, suffers internal reflection on the hypothenuse A, and emerges from the prism in the direction of B. Entering the prism B, it is restored to its original direction. So likewise that part of the luminous pencil entering the prism A′, emerges nearly parallel from the prism B′. The prisms B and B′ are adjustable to different distances apart, and have likewise an axial adjustment in the plane of the section represented; the first, that they may be made to correspond to the interval between the two eyes of the observer; the second, that the direction of the rays, travelling from each point of the object, through the prisms, may be such as will seem to the observer natural and unconstrained,—and with clear coincident fields.

In the smaller instrument before you this arrangement is observed. Used without eye-pieces, it gives a stereoscopic and perfectly satisfactory result. This instrument was constructed for a dissecting microscope; I use it with lenses whether plain, doublets, or achromatics, from ½ inch to 3 inches focal length.

The image is erect and orthoscopic. Objects can be viewed as opaque or transparent, and there is attached to it a flexible pipe, connected with a delicate cylinder and piston, which, in one respect, is made equivalent to a third hand. Tightening a screw, and taking the ivory termination of the flexible tube in the mouth, the focal distance of the instrument can be varied at pleasure with the breath. In very minute dissections, where the two hands are simultaneously employed with hook and needle, I have found this method of holding a focus of the greatest utility and convenience.

If over B and B′ single oculars be placed, the binocular vision is found to be pseudoscopic; that is, depressions appear as elevations, and elevations as depressions. With erecting or double eye-pieces, analogous to those of the terrestrial telescope, the vision again becomes orthoscopic.

On this account, I prefer to reserve this form of instrument for use without eye-pieces, in the manner explained, and to construct the compound binocular microscope on a plan which I will soon explain.

Binocular Magnifier.—I have found that for the magnifying glasses, used by artists and naturalists,—glasses having a focal length of one or two inches and more, a less complex and more economical arrangement can be adopted, namely:—

The reflecting surfaces A A′ and B B (fig. 1) can be substituted by pieces of common looking-glass, or plate glass silvered. The first surface reflections are too faint to interfere materially with distinct definition.

The two mirrors of the pair, on each side of the nose, are hinged together on the principle of the parallel rules. The whole arrangement is mounted something like a pair of spectacles, while the requisite lenses are adapted to be centrally attached when required. I regard the binocular magnifier as supplying a great desideratum to large classes of persons pursuing a great diversity of callings.

The effects, so often prejudicial to vision, of inordinately using one eye are thus avoided.

A perfectly natural relief, or definition of bodies in depth, as well as in extension, is thus attained.

Binocular Compound Microscope.—In the larger instrument before you, only two prisms are used for subdividing the light after its passage through the objective, and for directing the luminous pencils to the separate oculars. In this case orthoscopic vision is produced by the ordinary single oculars. The light suffers one instead of two reflections, as in the instrument before described. The arrangement of the prisms is shown in section below.

Fig. 2.

o, the object to be seen.
l, the objective, above and near to which is shown the two prisms.

The internal reflection takes place upon the two long sides, which are in opposition at a small angle, which admits of adjustment in the plane of the section shown, the lower termination always remaining in contact. The light through the objective, which impinges upon *a*, is that part of it which enters the prism, refracted to the left, so that it meets with the reflecting surface *b*. Suffering total reflection it emerges from the surface *c*, where, from the necessary identity of the immergent and emergent angles, it is refracted to the right, so as exactly to compensate for its previous refraction to the left. This implies that the upper and lower angles of the prism are equal.

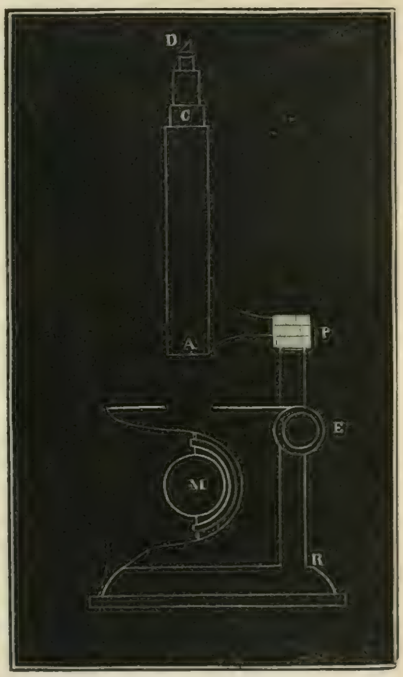
In the instrument before you, these equal angles are 45°. The ray of light, in pursuing the path *a*, *b*, *c*, suffers a minute chromatic dispersion, inasmuch as by the refraction and dispersion at *a*, the red, violet, &c., will be found somewhat separated at *c*; thereafter, in travelling in the direction *c d* to the ocular, the red and violet will move in parallel paths, so that no further dispersion will occur. Upon a close scrutiny into this matter, I find that it does not practically lessen the sharpness of definition, unless eye-pieces of unusually high power be made use of. The minimum limit of angular definition, perceptible by the human eye, is about 45 seconds of a degree (45″). The extreme dispersion occasioned by the prism as above, may be kept handsomely within this limit; this can be shown both by calculation and experimental demonstration. By making the equal angles of the prism 85° or 86°, so that the immergence and emergence shall be at right angles to the glass planes, this theoretical dispersion can be avoided. But practically, in this case, the usefulness of the prism would be destroyed by the interference of light directly transmitted through, without reflection.

Prisms with equal angles of 60° will probably be found as appropriate as any.

It would be improper to consume much of your time in explaining the mechanical details of this instrument. The following sketches will assist you to comprehend the essential peculiarities of a plain, firm, comparatively simple stand, and with all the most important adjustments.

Fig. 3 represents a side view of the instrument. The stage is immovable, being firmly supported, so as not to spring sensibly under considerable and sudden pressure: it extends 6 by 4 inches.

Fig. 3.



The optical parts are supported by a stout triangular gun-metal bar, bearing rack-work, and moving up and down by a cogged pinion, terminating in large milled heads, one of which is shown at E. For the convenience of changing objectives, the arm carrying the optical apparatus has at P nearly a half revolution, so as to carry it off the stage. The prisms are at the bottom of a brass box at A. One of the oculars is seen, as fitting into an adjustable tube C. A small rectangular, equilateral prism is so mounted in a brass cap as to be slipped at pleasure over the eye-glass. This little prism is adjustable in the plane of the drawing, on an axis transverse to the plane, so as to erect the image seen, and at the same time allow of its being viewed at any inclination between verticality and horizontality, which may be convenient to the observer. It will be seen that the prism at A has the effect of erecting the image in one plane, while the small prism at D can be placed so as to erect it in the plane precisely tranverse. Thus the movement upon the stage will be seen through the instrument to be natural or erect; a condition essential to the convenient manipulation or dissection of a microscopic object. M represents the position of a concave mirror or other apparatus for illuminating transparent objects. Two small mirrors will sometimes be found more satisfactory than one large one, as the operator can then easily secure a good light to each eye, which is sometimes difficult with a single mirror.

Fig. 4.

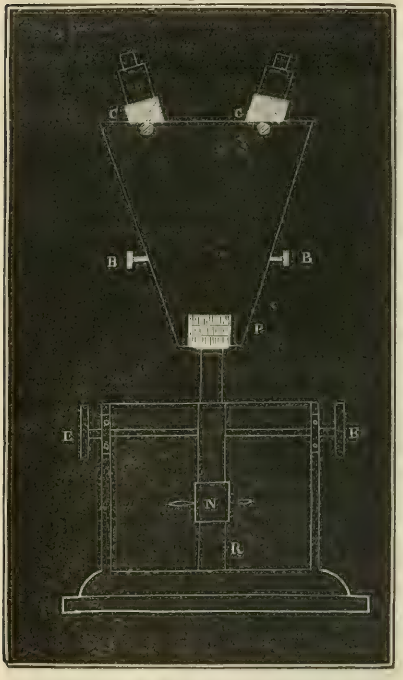


Fig. 4 exhibits a back view; the common letters, or letters *common* to both, referring to the same parts as in fig. 3. Thus C C are the adjustable tubes into which the oculars fit. These tubes are hung upon axes, so that their inclination to each other may be varied; and the whole arrangement slides at pleasure, horizontally, in order to adapt the distance to the eyes of different observers. B B are milled heads of screws for the adjustment of the inclination of the prisms, as explained in connexion with fig. 2. R is a brass tube surrounding the box in which plays the triangular gun-metal supporting bar, before explained. Concentric with R, and movable thereon, is N, a short brass tube carrying the illuminating apparatus.

Let the observer using the instrument carefully illuminate the object to be seen; then, after adapting the lines of vision to the natural requirements of the pair of eyes, duly alligning and superposing the corresponding images, and carrying them into corresponding parts of the two circles of light, as defined by the diaphragms of the oculars; and, lastly, regulating the focal position of the object in reference to the objective, all of which can be readily accomplished by the various adjustments: let him now place two good eyes of equal power in the proper position near the eye-glasses, and a magnificent field will present itself to his sight. He seems to look through a circular window or port-hole—say two feet off, and a foot in diameter; ten to twenty inches beyond which his microscopic objects, perhaps seemingly hung in mid-air, stand out in all the boldness and perfection of relief, and definiteness of position in width and depth, which he has been accustomed to realize without glasses in the natural objects around him.

It does not appear to him that any glass, or other artificial medium, is interposed between his eyes and the objects seen. The vision fatigues him no more than does a landscape, or the inspection of the implements and objects on the table before him. A drop of water teeming with algæ and living infusoria, looked into in this way for the first time, impresses upon the beholder, even though he be a veteran microscopist, a profound sense of the sublimity of Nature in her lesser spheres, and a vivid conciousness of beholding the microscopic world in a new and seemingly palpable condition.

By varying the inclination to each other, of the luminous pencils entering each eye, the objects can be made to appear definitely nearer or further off, at pleasure. In these cases the parallax or apparent angle subtended by an object remains constant, while the apparent size varies, of course, with the apparent distance.

Thus, a mite of a wheel animalcule, 1-100th of an inch long, will perhaps appear to be a foot off, and as large as a mouse; but bring the prisms nearer together, and tilt the oculars to correspond, and the image waxes marvellously immense, and taking a position perhaps apparently more than a hundred feet distant, the being, too small to be seen with naked eyes, vies with the great whale of the ocean in size; wearing an aspect more awful to behold than the savage beasts of the African forests; exhibiting a complex, transparent structure, more unique and wonderful than the mind of man can well conceive.

The instrument, with its firm stand, broad stage, and erect images, is pre-eminently adapted for use in prosecuting minute dissections, or the unravelling of minute structure of any kind. Opaque objects may be illuminated by the bull's-eye condenser; and transparent objects, by one or two concave mirrors, aided perhaps by two diaphragms or screens. At night two candles may be used conveniently with one mirror. To illuminate for the higher powers a single achromatic condenser suffices.

Almost any model or form of monocular compound microscope extant can be modified to become binocular, on the principles here explained in connexion with fig. 2. In one respect it would be convenient to adopt the trunnion mounting; the whole instrument might be tilted, so as to use conveniently the Wollaston camera lucida in drawing. But this would detract from the firmness and simplicity of structure, so essential in a dissecting microscope, and add materially to the cost, a circumstance of importance to some who might wish to possess it. As it is, the instrument can be readily braced up at an angle near 45°, at which angle Natchet's camera lucida works in drawing with perfect satisfaction.

If the same object be drawn as seen through each ocular respectively, a difference between the two drawings is perceptible, similar to that between match stereoscope pictures; so that if these two drawings be viewed, each with the appropriate eye, the natural relief of the object is reproduced. I have already suggested in 'Silliman's Journal' the propriety of publishing such drawings, appropriately placed upon paper, in illustration of natural history and histology.

Additional Observations on the Diatomaceous Deposit *of* Mull. By William Gregory, M.D., F.R.S.E., Professor of Chemistry.

In the paper published in the last No. of the Journal, I gave a list of 60 species of Diatoms observed in the Mull deposit, or rather in one portion of it, by the Rev. W. Smith, adding that I had myself, in other portions, seen not only all these, but others which Mr. Smith did not meet with. Since then I have examined with care several different portions of the deposit, which, as I mentioned, varies a good deal in the relative proportion of species in different parts, and have detected upwards of 30 additional forms, most of which I have been enabled easily to identify by the aid of Mr. Smith's Synopsis. In mentioning my observations, I shall use the names given in that work; not, however, meaning by this to assert, that all Mr. Smith's species are true and permanent species, for this is not the opinion of that author himself; but only with a view to the subsequent identification of the observed forms with those so beautifully figured by Mr. West, and their comparison with forms from other localities.

But before proceeding I have to apologize for, and to correct, several errata in the former paper, chiefly in the list of species. My absence on the continent prevented my seeing a proof; and for the same reason, the figures of the new form were not seen by me after they were engraved, and owing to their having been taken from hurried sketches never meant for publication, they are not such as I could have desired.

Perhaps the best plan will be to repeat here the list of forms given in the former paper, and then to subjoin my new observations. These forms were—

1. Pinnularia major
2. „ acuminata
3. „ oblonga
4. „ viridis
5. „ divergens
6. „ acuta
7. „ radiosa
8. „ mesolepta
9. „ interrupta
10. „ Tabellaria
11. „ gibba
12. „ gracilis
13. „ lata
14. „ alpina
15. Navicula serians
16. „ rhomboides
17. „ ovalis
18. „ dicephala
19. „ firma
20. „ angustata
21. Gomphonema acuminatum
22. „ *var.* coronatum
23. „ Vibrio
24. „ capitatum
25. Amphora ovalis
26. Stauroneis Phœnicenteron
27. „ gracilis
28. „ linearis
29. „ anceps
30. Cymatopleura elliptica
31. Cymatopleura apiculata
32. Cocconeis Thwaitesii
33. „ Placentula.
34. Surirella Brightwellii
35. „ biseriata
36. Cymbella Helvetica
37. „ maculata
38. „ Scotica
39. „ affinis
40. „ cuspidata
41. Himantidium gracile, Kütz.
42. „ bidens, W. Sm.
43. „ pectinale, Kütz.
44. „ Arcus, Kütz.
45. „ majus, W. Sm.
46. „ undulatum, Ralfs
47. Tabellaria fenestrata, Kütz.
48. „ ventricosa, Kütz.
49. Epithemia turgida
50. „ gibba
51. Eunotia gracilis
52. „ tetraodon
53. „ Diadema
54. Synedra capitata
55. " biceps
56. " " *var.* β recta*
57. Fragilaria capucina, Kütz.
58. Orthosira nivalis, W. Sm.
59. „ orichalcea, W. Sm.
60. Nitzschia sigmoidea.†

With respect to the new form previously described, Mr. Smith has proposed, since my former paper was written, to call it, provisionally, *Eunotia incisa* (No. 61), the notches which it exhibits forming a very well marked character. The figures formerly given of the two modifications or varieties of this form not being satisfactory, I here give such as will better indicate their true character.‡ My reasons for including the two forms together are, first, that both invariably exhibit the notches; secondly, that the number of striæ appears to be the same in both, while the general aspect undoubtedly is so; and thirdly, that many specimens occur in which one apex is narrow, as in fig. 1, while the other is rounded, as in fig. 2. I would add a remark with respect to a point which appears

* I have added this variety as it was noticed in the former paper.

† This species was accidentally omitted from the former list.

‡ These figures will be given in the next number of the Microscopical Journal.

characteristic ; that while the form No. 1 is quite symmetrical, No. 2 very rarely, if ever, is so, one end being always broader than the other. From the greater width of No. 2, the striæ are much more easily seen in that form than in No. 1.

Since my paper was written, I have detected this form in one more deposit, besides the present one and that mentioned in my former paper as said to be from the banks of the Spey; namely, in the Bergmehl of Lillhaggsjön in Lapland. It is not so abundant there as in the Mull deposit; and while, in the latter, No. 1 is the more frequent form, in the former, the Lapland earth, No. 2, is more common.

I shall now give a list of those additional forms observed by me, in a careful study of several different portions of the deposit, which can be readily referred to figures in Mr. Smith's Synopsis. These are—

62. Pinnularia nobilis
63. ,, viridula
64. ,, cardinalis
65. Navicula tumida
66. ,, gibberula
67. ,, Semen
68. ,, obtusa
69. Gomphonema dichotomum
70. ,, constrictum
71. Stauroneis dilatata
72. Tryblionella marginata
73. Cymatopleura Solea
74. Surirella splendida
75. ,, nobilis
76. ,, minuta
77. ,, Craticula
78. Surirella linearis
79. Cymbella Ehrenbergii
80. Epithemia rupestris
81. ,, ocellata
82. ,, Sorex
83. Eunotia triodon
84. ,, diodon
85. Synedra Ulna
86. Cocconema lanceolatum
87. ,, cymbiforme
88. ,, Cistula
89. Nitzschia Amphioxys
90. Cyclotella Kützingiana
91. ,, antiqua
92. ,, Rotula.

Of the above 27 forms, the only ones which do not entirely agree with Mr. Smith's figures are those I have termed *Navicula obtusa* and *Epithemia Sorex*. The latter is nearer in form to *E. Musculus*, but as that is a marine form, I have preferred the other name, provisionally. In my specimens this form is very scarce, but may be found more abundantly in others. As to *N. obtusa,* the form so named by me, of which I give figs., Nos. 3 and 4, is rather more like *N. affinis*, but is one half larger than either of the two as figured in the Synopsis. As, however, *N. obtusa* is said to occur in the Lough Mourne deposit, and as I find there the form here figured, precisely as in the Mull earth, and no other resembling it, I have chosen the name *N. obtusa* in the mean time. It is clearly distinct from all the Naviculæ figured in the Synopsis, except perhaps the two just named.

We have thus 88 distinct Diatomaceous forms (whether in all cases true species or not, is a matter for future decision) in this remarkable deposit. The study of it has led me to think it probable that several forms, at present separated, will have to be united; but our knowledge of Diatomaceous forms and of their modifications is not yet sufficient to enable us with certainty to classify them all. There is nothing, therefore, to be done, but to describe and accurately to figure, all such forms as appear distinct, and we shall thus in time be able to trace out the relations among them. This very deposit appears to me rich, not only in distinct forms, but in modifications of these, in several cases exactly intermediate between the figures of recorded species. To this part of the subject I shall return at a future time; for the present, I confine myself to mentioning the forms which agree with the published figures.

It will be observed, that 27 species and 2 genera, *Cocconema* and *Cyclotella*, have already been added to the former list. But I am quite sure that the number is not yet exhausted, for I have observed several well marked forms, which I cannot securely refer to any of those figured in the Synopsis, and which may probably, therefore, prove to be new to Britain. Among these are one or two *Pinnulariæ;* one or two *Naviculæ;* one, perhaps two, *Synedræ;* one *Nitzschia*, possibly two; one which I take to be a *Melosira*, but for want of the 2nd volume of the Synopsis I cannot compare it with figures of the British species. The same remark applies to the genus *Himantidium*, of which Mr. Smith identified 6 well marked species, but of which, or of some allied forms, I have reason to think two, perhaps three more are present. I shall hereafter describe and figure all such doubtful forms.

For the present, I shall conclude with the description of a very distinct and well marked species of *Pinnularia*, on the specific character of which no doubts can be entertained, and which is therefore new to Britain, if not to science. This form I early noticed, but it was not till I had compared it with the figures in the Synopsis that I felt sure of its being different from all the forms in that work, as well as from all figures of Pinnulariæ known to me. In the Mull deposit, it occurs in all the different portions I have yet examined, but invariably very widely scattered, so that a good slide, rich in forms, seldom yields more than one or two individuals, and occasionally contains none at all. Hence, and from its small size, it is apt to be overlooked, except in a very minute and careful search. It is of course impossible for me to say whether this form have been already described as occurring in foreign countries, but as yet I have seen neither figure nor description to which it can be referred. I would propose, therefore, provisionally, to name it *Pinnularia hebridensis;* and if any of your readers should recognise it as one already named, of course the earlier name must be adopted.

Pinnularia hebridensis.—V. elliptical, narrow, almost rectangular, with rounded ends, sometimes very slightly constricted in the middle, and sometimes very slightly acuminate at the apices. F. V. rectangular, with the corners slightly rounded. Length, from ·00125 to ·0025. Costæ strong, distant, radiated at the middle, not nearly reaching central line, 10 to 11 in ·001. Habit stout, notwithstanding its narrowness, so that it seldom occurs fractured. The figures 5 and 6 will give some idea of its aspect.

The small size, for this is one of the smallest *Pinnulariæ*, combined with the strong distant costæ, at once distinguish it from all those figured by Mr. Smith. I find in the late edition of Pritchard's 'Infusoria,' a description of *Stauroptera scalaris*, Ehr., which has some points of agreement with the above, such as the small size and the distant costæ. But the figure of the valve (Prichard, pl. xv., fig. 10) is very much broader, and the number of costæ is said by Ehrenberg to be 12 in 1-1200, which is = 14 in ·001, whereas my form has usually 10 only, sometimes only 9·5. Besides this it has not the pseudo-stauros which marks the genus *Stauroptera* of Ehrenberg, and the nature and form of the nodules and median line correspond exactly to those of *Pinnularia alpina*, while the arrangement of the costæ is also very similar to what is seen in that species, only on a very small scale; the form, however, is quite different.

I have only to add that, hitherto, I have been unable to detect the presence of this form in any other deposit which I have had an opportunity of examining; and that if any of your readers can throw light on the subject, or has observed any other well marked species in the Mull deposit, I shall feel deeply indebted to them if they will make known their observations. I shall also be happy to supply observers with the material for their researches.

TRANSLATIONS, &c.

On Cutaneous Diseases *dependent upon Parasitic Growths.* By Dr. B. Gudden. Abstracted from the 'Archiv für Physiolog. Heilkunde,' Heft II. 1853.

Porrigo appears under numerous external forms, to which special names have been assigned by many authors, and which have in fact been distributed in different classes, as if they had no mutual connexion. They all, however, have one common characteristic by which, as respects their origin, they are distinguished as a group from other cutaneous diseases; this characteristic is the existence in them of *Fungi*, which were discovered by Schönlein (in *Porrigo lupinosa*), and their presence subsequently confirmed by all observers.

"We shall show, by a series of observations, that the medium, in which these *Fungi* find their nutriment, is the normal *epidermis*, and that those spots in it, which are more especially favourable for the reception of foreign particles, are, almost exclusively, the situations in which the vegetable formations germinate. From the borders of the organic life of the epidermis we shall trace the progressive growth of the parasites to the site of their development—the *cutis*—and in thus tracing them shall be able to explain all the phenomena which occur in *Porrigo*, whether as the direct effect of the devouring parasitic growth, or as the consequence of the reaction set up on the part of the *cutis* in opposition to it. We shall, moreover, find reason to be convinced, that the *Fungi*, when transplanted into a perfectly healthy man, take root; and ultimately prove, that with their removal, in simple cases, the entire disease is cured.

"We will consider the two factors of our disease—*Fungus* and skin—in the first instance apart, although not without reference to their mutual relations:—

"I. Oval, transparent corpuscles, presenting sharp, dark outlines, and whose length and transverse diameter vary very much in proportion to the abundance of their nutriment, constitute the primitive form of the *Fungus* we are considering. In the air they readily dry up, but again swell out with considerable rapidity on the addition of water, although they do not burst. No nucleus is visible in these corpuscles. Whilst, not unfrequently, especially at a later stage, "chlorophyll-granules" of a more or less yellow colour are developed within them, from a differentiation of the fluid contents; and at the same time the outlines become paler. They vary, also, considerably in number, size, and shape. When they occur isolated, they present a deceptive resemblance to nuclei, and I fancy it is they which have been described as such by Fuchs and Bennett.

"On the above described vesicles, there are developed, for the most part at the ends, but not unfrequently also on the sides, one or perhaps two bulgings, which increase in size, become constricted at the base, sprout out again in a similar way, and constitute moniliform, dichotomously branched rows of cells. In the meanwhile the older cells do not remain stationary. They grow, and, occasionally with a diminution in their width, increase in length, the outlines become pale, owing to the flattening the constrictions are removed, and the moniliform strings are transformed into elongated, round filaments which continue to undergo greater and greater attenuation. At the same time they are capable of propagating, throw out spores on the sides, but ultimately becoming imperceptibly minute, and quite colourless, are lost in a molecular detritus.

"This is the mode of growth of the *Fungus*, when it is in no way interfered with. And the correctness of the exposition will be most readily shown by the assiduous examination of the root-sheaths of the hairs, but most convincingly when the *Fungus* in its earliest stages is successfully brought under the microscrope. Circumstances, however, will, of course, produce many varieties.

"In the common form, upon examination of the crumbling substance of the scabs of *Porrigo lupinosa*, it is well-known that the cells are seen to be scattered and separate, or only united by two or three together into short series In most cases, perhaps, this is owing merely to the preparation, for the less pressure and crushing is employed, and especially when very fine vertical sections are made, from scabs not too old, the more numerous and the longer are the rows of cells brought into view. Specimens also are not rare, in which the individual cells remain in contact with each other at the angles. Nevertheless, I would not altogether deny the occurrence of a spontaneous separation. The supposition that such an occurrence does take place, is supported both by the readiness with which the cells may be separated, and also by the capability of each when isolated to propagate itself independently. This capability, of which I have satisfied myself by the examination of individual specimens, and the observation of preparations in which, among elongated pale cells, several rounded, yellowish, sharply defined ones appear, as it were, intercalated, or, in the reverse case, where among some of the latter kind one or two of the former sort are interposed—proves that each cell is an individual plant, the development of which under favourable circumstances proceeds to the production of a mycelium such as has been described."

The author then proceeds to give a lengthened description of the hairs and follicles, and to point out their correspondence with the nails and their bed, but this it is needless to transcribe. From the nature of the fungus and from considerations drawn from the formation of the hair follicles, he deduces the following conclusions:—

"1. The more dry the skin, the more secure is the person against the invasion of the parasite.

"2. With a similar condition of the epidermis, healthy and diseased persons are equally disposed to the attack; different morbid conditions coming under consideration only so far as they increase or diminish the conditions favourable to the growth of the fungus.

"3. No part of the skin is, under all circumstances, entirely secure against these plants, but the hair follicles, and especially those on the head, are peculiarly adapted for their reception and propagation."

The author therefore denies that there is proof of the scrofulous nature of *Porrigo*, and that its appearance is always preceded by a scrofulous exudation, and is of course still more opposed to the extraordinary assertion of Neukrantz of the identity of tubercle and *Favus*, as well as to its having any direct connexion with several other forms of strumous disease, with which it has often been associated.

Simon states, that, in *Favus*, he never observed the *Fungus* extending to any distance within the hair follicle, in which Gruby also agrees with him. Whilst the latter observer in the hairs of the beard noticed between the root-sheath and the shaft, *Fungi*, the spores of which were minute, commonly round, and the *mycelium* furnished with granules. He designates this form, analogous to his *Porrigophyta*, under the name of *Mentagrophyta*. He also saw *Fungi* in *Alopecia circumscripta* of the scalp. In his *Phyto-alopecia* the hair at its point of exit from the follicle is surrounded with a sheath composed of the *fungi* which extend to the height of $\frac{1}{2}$-$1\frac{1}{2}'''$ up the shaft, and thence spread themselves over the neighbouring hairs. They consist of *mycelium threads* and spores. The latter are tolerably minute, the round $\frac{1}{2220}$ - $\frac{5}{2220}'''$; the oval $\frac{1}{1100}$ - $\frac{4}{1100}'''$ long. In his *Rhizophyto-alopecia* the fungi are said to be developed in the root of the hair itself, to grow within its medulla and to occupy its interior. They consist only of spores of about the same diameter as the oval ones of the *Phyto-alopecia* and form for the most part moniliform strings, lying parallel with the axis of the hair, and causing the hairs to become grey and thick, and from the loss of their elasticity to be easily broken. *Fungi* in the interior of the hair are also described by Malmsten (Miller's Arch. 1845).

Dr. Gudden then describes a case of *favus* which he treated first by the removal of the scabs by soap and water, and afterwards by the application of a mixture of olive oil and croton oil followed by blistering with cantharides, and in which he at first thought he had obtained a cure, but was disappointed by seeing, after about 12 days from the healing of the vesicated surface, the disease reappear in the form of minute yellowish rings around each hair, which rings as shown by the microscope were composed of the *fungus*. The only part of the scalp which remained free from the growth was about the vertex where the hairs had been removed during the suppuration caused by the blister over the extent of about 1½ square inch.

When once acquainted with the appearance of the hair-sheaths with the *Fungi* within them in *Porrigo lupinosa* (Favus), the author says that a look is sufficient to show the identity, with the latter, of the parasitic growth, found also in the hair-sheaths in *Porrigo furfurans* and other forms of *Porrigo* (not *Porrigo decalvans*, in which the author was unable to detect any parasitic growth). In these forms of *Porrigo* the fungus appears, from one reason or another, to be limited almost wholly to the root-sheath of the hair, and can only be detected when the root-sheath is extracted together with the hair. In ordinary cases where there is no inflammatory action present, the sheath does not usually come away with the hair when the latter is plucked out and the parasitic growth is therefore not to be seen, but Dr. Gudden states that by rubbing the part with croton oil so as to excite some degree of inflammation around the hairs, the root-sheath will come away and within it the fungoid growth will be readily perceived. The detection of the fungus is facilitated by immersing the scurfy scales together with the adherent hair follicles, removed from the scalp in *Porrigo furfurans*, in oil of turpentine, which acts more slowly in rendering the *fungus* transparent, than it does upon the horny tissue of the epidermis, &c.

The various forms of *Porrigo* depend upon the individuality of the skin, and this is proved not only by the identity of the *Fungus* in the different forms, but also by their frequently observed coexistence, and the transitions from one form to another in the same individual.

The *diagnosis*, however, is, in all cases rendered certain by the finding of the *Fungus* as above described. *Pityriasis*, which is also caused by a parasitic growth, is an entirely different disease, and its *fungus*, as the author shows in a subsequent chapter, is not to be confounded in any way with that of *Porrigo*. The contagious property of *Porrigo* is shown to depend upon the *Fungus* alone: in proof of which the author describes numerous experiments made by himself and others.

The cure proposed by the author, and which appears to have been successful in his hands, consists, first, in the loosening of the hair-sacs by the use of croton-oil frictions and the after application of an oil-poultice, and subsequently the plucking out of the hairs by means of tweezers, a prolonged and not very pleasant occupation, as it requires to be done with extreme care. Whether his plan possess any advantages over the old one of a pitch cap, this is not the place to decide; it appears, however, that the disease cannot be cured, in the form of *Favus* at all events, without the eradication of the hairs together with their *root-sheaths*.

On a Species of FILARIA *found in the Blood of the* DOMESTIC DOG. By MM. GRUBY and O. DELAFOND. Abstracted from the 'Comptes Rendus.' Tom. xxxiv. p. 9. 1852.

AFTER noticing that several observers — Schmitz, Baer, Valentin, Vogt, and Remak, from 1826 to 1842—had indicated the existence of *Filariæ*, *Monostomata*, *Distomata*, and *Infusoria* in the blood of Frogs, of certain Fishes, and of some *Mollusca*, the authors proceed to state that, in the year 1843, they were the first to announce the discovery of entozoa, of the genus *Filaria*, living in the blood of certain domestic dogs, and circulating with the globules of that fluid in all the vessels. Since that communication to the Academy, MM. Erdl and Mayer, in 1843; Hyrtl, Gros, and Ecker, in 1845; Chaussat and Wedl, in 1848; and M. Guérin Méneville, in 1850, have established the fact of the presence of *Hæmatozoa* in the blood of the Field-Rat, of the Black Rat, of several Birds, and Fishes,—of the Crab, the River Mussel, the Earth-Worm, and the Silk-Worm. The present memoir contains more particularly the researches to which the authors had devoted themselves for the last nine years relative to the worm living in the blood of certain domestic Dogs. The results at which they arrived may be shortly stated in their own words as follows:—

1. The number of microscopic *Filariæ* inhabiting the blood of certain dogs may be estimated approximately at from

11,000 to about 224,000. The mean number, deduced from twenty dogs, was more than 52,000.

2. The microscopic *Filariæ*, having a diameter less than that of the blood discs, circulate in the most minute capillaries where the blood discs can find entrance. A drop of blood taken from these vessels, it does not signify at what part of the body, nor at what season of the year, contains these minute *Hematozoa*.

3. The chyle and the lymph of dogs, whose blood contains microscopic *Filariæ*, present none.

4. Nor do any of the secretions or excretions.

5. Nor in the dissection of twenty-eight dogs of different sorts and ages, and whose blood was known to have been verminous for periods varying from several months to more than five years, and made with the utmost care, were any *Filariæ* ever discovered in any of the tissues. Their proper habitat seems to be exclusively in the blood-vessels.

6. The authors calculate, from the examination of 480 dogs, that the blood in about four or five per cent. is verminous.

7. It is so more frequently in old and adult dogs than in young ones.

8. The verminous condition seems to be irrespective of race, sex, or general habit of body.

9. Even when most abundant, this condition of the blood does not seem to interfere with the instincts or muscular force of the animal.

10. Nor is the constitution of the blood itself altered.

11. Transfusion of verminous blood, deprived of fibrin, into sound animals, was not followed by any result. But,

12. When unaltered verminous blood was thus injected, *Filariæ* were found living in the animals experimented on, for more than three years, or until their natural death.

13. *Filariæ*, transfused with defibrinated blood into two Rabbits, lived in the blood of those animals for 89 days; after which time none could be found.

14 In a similar experiment with six Frogs, two of which already had *Filariæ* in their blood, the canine *Filariæ* lived for eight days, during the whole of which time the blood discs of the Dog appeared unaltered among those of the Frog. On the ninth and tenth days the Dog's blood discs having become changed, the *Filariæ* had disappeared, and the Frogs died of a scorbutic malady. (!)

15. Injected together with the blood into the serous cavities or cellular tissue of Dogs, in good health, the *Filariæ* could not live in their new domicile.

16. A verminous Dog, of one race, with a female not so

affected, of another, had offspring of which those belonging to the paternal race were verminous, and the others not.

17. When the conditions were reversed, so was the result.

18. But the *Filariæ* in the blood of the descendants could not be detected till the dogs were five or six months old.

The authors have also succeeded in finding in the verminous blood of a dog which died in consequence of its being fed exclusively on food composed of gelatin, large worms, visible to the naked eye. They found six, of which four were females and two males, and they were lodged in a large clot occupying the right ventricle of the heart. The worms were white, from 0·5 to 0·75 inch long, and from 0·039 to 0·058 inch in diameter. They propose for this Hematozoon, the name of *Filaria papillosa hæmatica canis domestici.*

On the Existence of SPERMATOZOIDS *in certain Freshwater* ALGÆ. By Dr. H. ITZIGSOHN. Abstracted from the Annales d. Sciences Nat. Tom. xvii. p. 150.

HITHERTO, among the Algæ, the spores of which exhibit spontaneous movements (Alg. Zoosporées, *Thur.*), the genus *Cutleria* was the only one known to possess "antheridia:" this genus, however, belongs to the group of Phæosporées (*Thur.*), whilst those noticed by M. Itzigsohn, as presenting spermatozoids, all belong to the Chlorosporées (*Thur.*). On the other hand, the active corpuscles, contained in the "antheridia," either in *Cutleria*, or in the Fucaceæ, very closely resemble the spores, properly so called, of those Algæ, and, except in their cilia and their motility, have no analogy with the long known spermatozoids of *Chara*, of the Muscineæ, and those of the Ferns, and Equisetaceæ. Should M. Itzigsohn's observations prove to be correct, the Algæ will present three distinct types of antherozoids:—1. Ciliated and motile zoospores (sporomorphes ciligères et mobiles): 2. those of the Florideæ, also resembling spores, but in which the presence of cilia and the existence of motility are still disputed points; 3. lastly, the vermiform antherozoids, without cilia, but very active, forming the subject of M. Itzigsohn's communication, which is in the form of a letter to Mr. L. R. Tulasne.

"The object of my communication," he says, "is to announce a discovery which I have recently made, of the 'spirozoids' (spiralfäden) of the freshwater Algæ." "My researches have been especially directed to *Spirogyra arcta*, Kütz. About the time in which the well-known phenomenon of conjugation is observed in this Conferva, the band-like endochrome of some of the filaments becomes condensed into quaternary globules. These minute spheres are at first of a bright green colour, which afterwards becomes paler, and finally turns into a greyish white. They frequently exhibit very distinct movements within the tube in which they are contained, and this motion becomes much more active when, from any cause, they have escaped from the parent cell. If one of these utricles be gently crushed between two glasses a mucous material is seen to escape, from the midst of which, after the lapse of a quarter or half an hour, are disengaged an infinity of spiral filaments, each of which was originally contained in a parent cell. These spirozoids are for the most part agglomerated or grouped into minute rounded masses, to which I propose to give the name of 'spermatosphères.' At the end of eight to fifteen days, should they have been preserved alive, the spirozoids have become much longer and larger, having at the same time retained the faculty of performing the most active movements. I have not, however, hitherto been able to discover any cilia, nor even an appreciable terminal enlargement. The development of these bodies within parent cells appears to me an indisputable fact; there can, therefore, be no question as to their not being Vibriones.

"Long ago I observed the formation of spermatosphères in *Vaucheria;* in that genus they frequently occupy distinct compartments of the filaments of which the plant is constituted, and are very large. M. Karsten has published a figure of them in the 'Botanische Zeitung;' but he is wrong in regarding them as a morbid product of the *Vaucheria.* In form, the 'spermatozoids' of *Cladophora glomerata*, on the contrary, are in all respects analogous to those of the *Spirogyræ.* My observations upon this interesting subject are not yet concluded, although I feel fully assured of the exactitude of the results now transmitted. Spermatosphères are found also in *Closterium* and *Œdogonium.*" M. Itzigsohn concludes by saying, "it is manifest that the generation of spermatozoids in *Spirogyra*, *Vaucheria*, and other analogous Algæ, casts considerable light upon that which takes place in the *Lichens* and *Fungi.*"

Note on the MUSCULAR STRUCTURE *in* PALUDINA VIVIPARA, *and other* GASTEROPODA, by LEYDIG.

ACCORDING to Leydig (S. and K. Zeitsch., Vol. ii., p. 191), the structure of the muscles in *Helix*, *Bulimus*, *Caracolla*, *Paludina*, and other gasteropoda, is the following:—The special elementary tissue of the muscles is a tube which is derived from a successive series of coalesced cells. The nuclei of these cells are, even in full-grown animals, to be seen in many of the muscles, occurring in some more numerously than in others; thus they are abundant in the muscular tubuli of the heart in *Paludina*, where they are 0·004‴ long, and besides this, in the red-coloured portion of the oviduct; they are rare again in the muscular tubuli of the foot. The form of the primitive muscular tubuli is always more or less cylindrical and slightly compressed; the former shape is seen in the muscular tubuli of the fleshy body of the excitant organ, the latter in those of the foot. Moreover that they really are of a tubular nature, is convincingly shown when suitable transverse sections are treated with acetic acid, by which the contents are dissolved, or at least rendered more transparent, only the membrane of the tubes remaining. A peculiarity of these muscular tubes consists in this, that they not unfrequently divide, as may very frequently be seen in the heart of *Paludina vivipara*. The contents of these tubuli is, in most cases, a transparent, colourless, gelatinous substance, soluble in acetic acid, or, in other words, the contents of the tubuli are distinguishable into a homogeneous cortical substance and a finely granular colourless axial substance; in other muscular tubuli again, as for instance in those of the pharynx and of the heart, the minutely granular axial substance becomes farther developed so that the granules acquire sharper outlines, and what should be specially noted, are so disposed that muscular tubulus of this kind, as in the heart of *Paludina*, is so much the more like an immature, transversely-striped primitive muscular fasciculus of a higher animal, as the granules in the tubulus may be more numerous.

In this way might a parallel be drawn between the primitive muscular tubuli of the Mollusca and the striped primitive muscular fibrillæ of the Orthropoda and Vertebrata; their inferiority consisting only in this, that their contents are not developed into fibres as they are in the latter. A very faint longitudinal marking is evident in many, especially of the compressed muscular tubuli of *Paludina vivipara*, for instance in the foot, which disappears under acetic acid, and of which it is doubtful whether it is situated in the membrane or in the contents of the tubulus.

No muscles corresponding to the smooth muscles of the higher animals exist in *Paludina vivipara*; that is to say, no muscular fibres representing a single elongated nucleated cell. All their muscles are tubuli, and are produced by the coalescence of a series of cells.

On a FUNGOID GROWTH *in the* NAILS. BY C. MEISSNER. Abstracted from the 'Archiv f. Physiol. Heilkunde,' Jahrg. 12, H. 1., p. 193, Pl. 1.

THE numerous observations which have been made respecting the growth of *Fungi* in and upon the human body, both in the mucous membranes and on the skin, may be divided into very distinct classes,. having reference respectively to the real significance, in a pathological point of view, of the fungoid growths. In the one case the production of the lowest vegetable organisms may be wholly and merely accidental, and be connected with the co-existing pathological processes only in so far as the latter may afford the conditions, or the appropriate soil for their vegetation. In the other set of cases, which are very much more infrequent, these morbid changes of the organs are more intimately connected with the fungoid growth; so that, in fact, the *Fungus* is, to a certain extent, the disease itself. The number of affections referable to this category—so far as they are as yet known—is very small; for, perhaps, the fungus of *Porrigo lupinosa* (favus), and that of *Pityriasis versicolor*, are the only ones which can be included in it. The fungoid growth which occurs in aphtha must probably remain doubtful and improbable.

In the course of the summer (1852) the author had an opportunity, in the Clinic of Professor Baum, of observing a species of *fungoid* vegetation, of which no mention, according to him, had previously been made, and which would probably belong to the second class of cases above mentioned. The subject of the observation was an old man of 80, who had been admitted into the hospital for a tumour of the *testis*.

The finger-nails presented an extraordinary appearance, and abnormal form. They were about 1-12th of an inch thick, nearly throughout, and at the same time much arched above, so that the anterior portion was incurved like a claw over the point of the finger: the anterior edge was thick and broad, but presented in no respect the conformation of the nails which is usual in *phthisis* and *cyanosis*. Their colour was, for the greater part, a peculiar yellowish-white, in places passing into brownish, and quite opaque; but this abnormal and very remarkable discoloration was not uniformly spread over the whole nail, frequently forming merely streaks running from the root of the nail as far as the free border, and between them were spaces more or less numerous in different nails, which, except in the abnormal thickness, appeared sound, red, and transparent. The nails, moreover, were more moveable in their bed than is natural; they were not, however, although in a state apparently of dying off, full of cracks and soft, but, on the contrary, were hard and brittle under the knife, like wood. All the nails were thus affected, except that of the right index-finger, which was quite sound.

The author's attention having been directed to the subject by Professor Baum, he examined a piece of the nail, in order to ascertain whether any air was contained between its cells, upon which the abnormal appearance might depend. On a longitudinal section the surface exhibited streaks, which ran from behind forwards, and were frequently of a yellow or brown colour; and while the surface of the nail, as above said, was smooth internally, it appeared disintegrated, easily separating into thin lamellæ, all of which were quite opaque. One of these lamellæ, treated with caustic soda, and examined microscopically, was composed of the well-known cells of which the nail is constituted; but, as these were rendered transparent, and swollen under the influence of the re-agent, there was apparent a rich plexus of variously convoluted filamentary fungi, which spread upon and between the cells, and frequently projected beyond the free edge of the object. All the affected nails presented the same appearances. The fungus is very similar to that of *Porrigo lupinosa*, and *Pityriasis versicolor*. The author noticed a *mycelium*, composed of long, much branched, jointed filaments. They appeared greenish from the diffraction of the light; had a width of 1-900th—1-700‴th, many even only of 1-1000‴th, and consisted of successive joints, which were not of uniform size, though, on the average, from two to four times as long as broad, but were frequently also scarcely perceptible. This growth, in many places, formed a very thick felt, which was very beautifully seen when the cells of the nail were almost dissolved under the prolonged action of the soda, by which the fungus was in no way altered. Besides these, there occurred broader, shorter, unbranched filaments, of a clavate form, which were much more distinctly jointed, and consisted of short, square, or rounded segments; these were the *sporangia*, containing the spores disposed circularly, and consequently having a double outline, the outermost delicate line being that of the sacculus itself, the inner that of the spores. Imbedded in the network formed by these filaments, and those of the *mycelium*, there were, lastly, large masses of free, detached spores in vast quantities. The latter were round, also of a greenish hue, and of very various sizes; the smallest measuring only 1-1000‴th, up to 1-900‴th; the largest as much as 1-450‴th.

The *fungus* above described differs from that of *Pityriasis versicolor* in the jointed structure of its *mycelium*, and the greater size of its filaments and spores. It has the greatest general resemblance to the fungus of *Porrigo lupinosa* (*favus*); the *mycelium* of the latter, however, is also unjointed, and the author states that he did not find the spores so large as in the ungual fungus.

The disposition of the growth in the substance of the nail could be very well observed in vertical sections. In thin sections made in this direction, treated with soda, it was evident that the fungus extended through the entire thickness of the nail, forming *strata*, running in streaks from the root of the nail forwards, and parallel with the surface. The cells of the nail were separated from each other by the fungoid strata, and the thickening of the nail was perhaps exclusively owing to them. The above described yellow and brownish lamellæ and streaks were seen to be due to the fungus, consisting, in fact, almost entirely of masses of innumerable spores. The peculiar discoloration of the nail was produced by them, just as the brown colour of scales in *Pityriasis versicolor* is ascribable to the fungus.

The toe-nails were thickened, fissured, and whitish, as is usual in old people, but presented no trace of the fungoid growth; nor did any part of the skin, which was dry and scurfy.

When asked respecting his nails, and the cause of their alteration, the old man stated, that about thirty years previously, a heavy weight had fallen upon his fingers, in consequence of which, the nails were broken and had come off; that they subsequently grew again, but had gradually become thick and white; he did not remember whether the right forefinger had escaped at the time of the accident or not.

On the Multiplication of Chara by Division. By M. C. Montagne. Comptes-Rendus, Tome xxxiv., No. 24, p. 898.

The object of this Memoir is to demonstrate what had previously been merely suspected, viz., that certain species of *Chara*, but especially *Chara stelligera*, Bauer, were capable of propagation in another way than by spores, that is to say by *bulbilli*, a mode of reproduction analogous to what takes place in several of the Liliaceæ, and many other plants, both vascular and cellular.

The multiplication of this species, which is rarely and widely distributed, and which very seldom indeed produces spores, is insured by the mode above adverted to. The author proceeds to describe the bulbules in the following manner:—

"The apparatus in question is formed by the agglomeration of cells, developed in a circular manner around the principal tube, on a level with the septa, or endophragms. These cells, disposed like the sides of a melon or gourd, spring from the wall itself of the tube, according to the laws of the multiplication of vegetable cells, observed by MM. Mirbel and Hugo Mohl, confirmed by the more recent observations of others, and among them of M. W. P. Schimper, who, in a Paper on 'Red Snow,' gives to this mode of multiplication the very suitable name of 'exogenous cell formation.' The cells which constitute the star-like nodules multiply in two different ways: the division takes place either in an excentric manner, and on the same plane, in which case the stellate concretions are produced, to which the plant is indebted for its specific name,—or the division is effected in a direction parallel to the axis of the central tube, and in this case sometimes as many as four rows or whorls of super-imposed cells may be counted, but no rays. The number and arrangement of the cells in these bulbules is best made out in sections of them, to which has been added a drop of solution of iodine, which colouring blue the grains of starch, and not so affecting the tissue of the dissepiments, renders the cells in question evident, and allows of their being counted. When mature they are filled with starch, whence they acquire an ivory-white colour and polished surface. In the younger state the cells are green, and contain no starch. The interior of the starch-bearing cells is occupied by a reticulated cellular tissue in the utricles of which the starch is generated."

Bulbules of a similar kind, but of different aspect in the different species, have been observed in *C. hispida*, *C. aspera*, *C. alopecuroidea* var. *Montagnei*.

On SARCINA *in the* LUNGS.—Zeitsch. f. rat. Medizin, von Dr. J. Henle, and Dr. C. Pfeufer.

DR. Zenker, of Dresden, found *Sarcina* in the lungs of the body of a female aged 38, dead of medullary cancer. This was the second occasion on which this production had been found in those organs. The first case was observed by Virchow, but it would seem that the more recent instance noticed by Dr. Zenker differed from the former in many material respects.

The following account is extracted from the history of the *post-mortem* examination. Both lungs were free, with the exception of a loose adhesion at the inferior border of the right; everywhere highly emphysematous, not collapsing, and pale. In both lungs there were scattered through all the lobes a moderate number of whitish-grey, or greyish-red medullary *tubera*, varying in size from that of a pin's-head to that of a hazel-nut, of a rounded form, placed on the surface, and somewhat projecting above it, soft and readily enucleated, leaving a smooth surface. The superior lobes of both lungs everywhere contained air, and were in various spots more or less œdematous. The inferior lobes were, for the most part, also filled with air, very œdematous, and, towards the base, rather friable; their posterior surface, to some extent, of a uniform, dark blue colour; the tissue bordering this surface (on the right to a greater depth than on the left) without air, of a dark grey red colour when cut, and on pressure affording an abundant, serous fluid.

In each lower lobe, both in the parts containing and in those without air, were presented, on a section being made, numerous, distinctly yellow coloured, ill-defined spots. When the substance of the lung was squeezed, in these spots, a few minute yellow flocculent particles were obtained mixed with the *Sarcina*, and which, from their colour, which the author believed must have been derived from altered hæmatin, attracted his attention. Upon his subjecting them to microscopical examination, he found, to his astonishment, both in the flocculent particles, as well as in their neighbourhood, tolerably numerous specimens of the *Sarcina* in the most distinct forms.

They showed division up to the third stage (64 squares), were in part quite colourless, in part of a light yellowish colour or greenish, and with respect to their figure differed in no way from the well-known forms. Upon further investigation, the *Sarcina* was found not only in all the above-mentioned yellow spots, but also, in greater or less numbers, in the serous fluid, expressed from other parts of the lungs which did not present the yellow spots and flocculi.

The author concludes, that the *Sarcina* in this case was not *primarily* developed in the lungs, but rather that it had reached them from the stomach. (It may be observed, that in the stomach also, there was found a great abundance of *Sarcina*.) The author thinks, that some of the contents of the stomach during life had been brought up by eructation, and had got into the air-passages from the pharynx.

With reference to the above, Heller (Archiv f. phys. u. path. Chemie u. Mikrosk. H. 5, 1853, p. 197) remarks, that he assigns no importance to the occurrence of the *Sarcina* in parts beginning to putrify and consequently in dead bodies, because the *Sarcina*, like other *fungi*, may not have been formed in them till after death—an opinion which he attempts to confirm by his observations on luminous dead fish, marine animals, sausages, &c.—(Heller's Archiv, l. c.).

(This opinion of Heller's, however, is obviously quite inadmissible, as there is not the most remote connexion between the organism in question, and any known form of *fungus* occurring in dead animal or vegetable tissues.)

Contributions to the Knowledge of the Hairs of 'Collomia coccinea' by Dr. Oudemans (Botanisch Zeit., Part 24, June 17, 1853), with a plate.

The author describes two kinds of hairs:—one conical and pedunculate, consisting of 2 or 3 cells, and with tolerably thick walls; these cover particularly the under surface of the leaves; and a second, capitate (glandular hairs as they are termed), with which the sepals more especially are clothed, but which are also found upon the upper surface of the leaves. His observations were devoted more particularly to the latter form of hair, and he states that they are distinguished by the following characters:—

1. Their compound structure.

2. By the presence of a distinct *cuticula*, which invests the whole hair.

3. By the cells containing green matter, belonging to the capitulum.

History of the Development of the Flower and Fruit of 'Manglesia cuneata' Endl., by Dr. Herman Schacht (Botan. Zeit., June 24, 1853), with a plate.

Contributions to the Knowledge of the Nostachaceæ, and an attempt at their natural arrangement. Inaugural dissertation, by Dr. L. Fischer, Dr. Phil. Bern, Hulier & Co. 1853. pp. 24. 4to, with 1 lithographic plate.

On the Cuticle of Ligneous Plants, by Dr. Th. Hartig. (Botan. Zeit., June 3, 1853).

The author says—

1. That a simple cuticle, which is to be regarded as a distinct organ, exists as the most external investment of the plant, of which it constitutes the outermost closed boundary, whence to a certain extent it is the primordial cell of the individual, from the first appearance of the embryo until it is destroyed, and on older parts of the plant, is replaced by cork-tissue.

2. That in most plants, between the cuticle and epidermis-cells, a substance is deposited in larger deposit-layers, which differs from the cuticle as well as from the epidermic cells by its solubility in caustic potass; and which, at least in all cases, cannot arise from a transformation of the outer cell-walls, since it is also apparent upon such epidermic cells, as have their walls thickened either not at all, or uniformly throughout, as, for example, in *Ruscus* and *Salisburia*, or as

in the leaf of the *Coniferæ*, between the epidermic cells resembling bast-fibres and the cuticle.

3. That the epidermis is also closed over the stomates.

He then proceeds to describe the mode in which his investigations were made.

De plantarum generatione sexuali Dissertatio inauguralis physiologica, by Dr. ANTON DE BARY. Berlin, 1853. pp. 30. 8vo.

THIS essay affords a complete review of all the discoveries and opinions respecting the sexuality of plants, having reference to the actual process of impregnation in the vegetable kingdom, from Kölreuter to the present time.

IN the 'Bulletin Scientifique' of the September number of the 'Bibliothek. univ. de Genève,' is contained, in the botanical section, a list of various observations respecting the disease of the vines in the year 1851. And besides the papers by Professor Mohl enumerated in this list are the following Essays, for the most part in the Transactions of Societies :—

Lettre de M. le Dr. Desmoulins, au Congrès scient. d'Orléans sur la Maladie des Raisins. Orleans. 1851.

Lettre de M. le Dr. León Dufour à M. le Président de la Soc. Lin. de Bordeaux relativement à la Maladie des Raisins. 18 April, 1852, in the Act. Soc. Linn. Bord., v. 17, liv. 1.

Ch. Laterrade, Maladie du Raisin, et de la Pomme de Terre, en Suisse, en 1851. In the Actes de l'Acad. de Bordeaux.

R. Blanchet, la Maladie du Vigne dans le Canton de Vaud en 1851. In the Bull. de la Soc. Vaudoise des Sc. Nat.

The conclusion which may be deduced from these observations is the following:—1. The *Oidium Tuckeri* is the true, principal, and extrinsic cause of the disease. 2. This disease is probably not new in Europe, but has not hitherto been so general or so active. 3. It is not proved that the same *Oidium* occurs on other plants. 4. When insects are found upon the diseased vines, either internally or externally, they constitute only a local and accidental phenomenon, and may induce either an aggravation or a diminution of the disease.

REVIEWS.

The Microscope in its Special Application to Vegetable Anatomy and Physiology. By Dr. Hermann Schacht. Translated by Frederick Currey, Esq., M.A. With numerous Illustrations. Highley's Library of Science and Art, London.

In the first number of the 'Microscopical Journal' we called attention to this little work of Dr. Schacht's on the Microscope, and we are glad now to be able to introduce it to our readers in an English dress. We stated our opinions of the work so much at length in our first notice, that we must now confine ourselves to the translation. Mr. Currey has, we find, omitted most of the remarks made by Dr. Schacht on foreign microscopes, which would have been of but little advantage to the English reader. We would here repeat what we have often before stated, that the best foreign microscopes are inferior to those of our three great firms, and that the English student can derive no advantage from the purchase of the former. It is true our best microscopes are expensive, but we are glad to find that efforts are being made to place in the hands of the English student instruments which, while they are as convenient in their construction and cheap as foreign ones, will possess the excellent glasses of English makers.

Dr. Schacht's remarks on the use of the microscope are chiefly illustrated by reference to the vegetable kingdom, but the general principles on which the instrument must be employed are the same in every department of nature. At the same time the botanical student will especially find in this work a number of admirable remarks on the structure and nature of vegetable tissues. As an illustration we give the following account of the development of the flowers in *Cleome arborea*:—

"The first rudiments of the flower appear, in the usual manner, in the form of a cellular cone in the axil of the bract; shortly afterwards appear the rudiments of the four sepals, and next to these come the four petals alternating with the sepals. Figs. 40 and 41 represent a flower in a young state seen from above. After this, however, follows a whorl of six elements. Fig. 42 shows a flower at a somewhat later stage than is represented at fig. 41. In fig. 42 the sepals are removed, and only two of the petals drawn. Fig. 43 represents a very perfect transverse section, showing all the parts of the flower. It might, perhaps, be supposed that there were two whorls of anthers, each whorl containing four elements, and that two of the elements of these whorls were abortive; but that

Fig. 40.

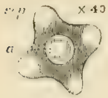


Fig. 41.

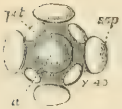


Fig. 42.

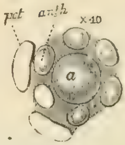
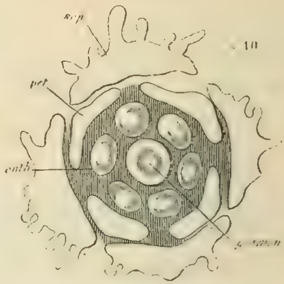


Fig. 43.



this cannot be so is shown by the regular position of the six anthers in one whorl, which always occurs in good specimens. The long-stalked ovary, which is subsequently developed, appears in the form of a solid column; at its apex there is formed a small depression which at first is very flat; the depression subsequently increases, and the ovary assumes the form of a cup. The edge of this cup afterwards increases in thickness, its walls approximate to one another, and form the stigmas and the style. The ovary is unilocular with two parietal placentæ. The ovule has two integuments; at a later period it exhibits a peculiar curvature; the anther is quadrilocular, but at the time of dehiscing it is bilocular.

"Fig. 44 represents a longitudinal section corresponding to the stage of development, represented at fig. 43. Figs. 45 and 46 represent longitudinal sections of the ovary in different stages of development, the sepals

Fig. 44.

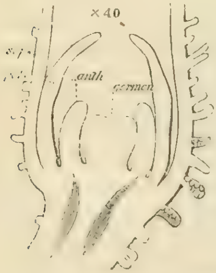


Fig. 45.

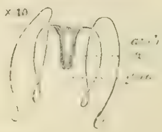
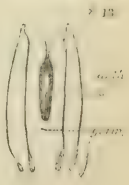


Fig. 46.



and petals being removed; (x) is the hollow of the ovary. Figs. 47 and 48 represent transverse sections of the ovary in different stages of development. Fig. 49 represents a transverse section of the young anther; it is quadrilocular; (a) is the vascular bundle of the connective, (b) one of

Fig. 47.

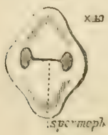


Fig. 48.

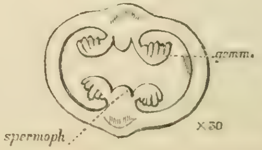
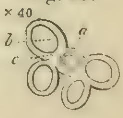


Fig. 49.



the chambers of the anther, (*c*) the cellular tissue of the connective, which is absorbed shortly before the dehiscence of the anther."—p. 111, 112.

The drawings, which in the original were on separate plates, are here introduced into the text in the form of woodcuts. The work is neatly got up, and sold at a low price, and forms one of a series of works forming a library of science and art, published by Mr. Highley, of Fleet-street.

A Naturalist's Rambles on the Devonshire Coast. By Philip Henry Gosse, A.L.S., &c. London, Van Voorst.

Every naturalist should possess a microscope: in fact it is becoming every day more obvious that a man cannot be a naturalist without one. At any rate, those naturalists who wish for a notice in our pages must use the microscope or we shall be compelled to pass them over. Mr. Gosse had a true eye to the all-embracing nature of the study of natural history, when, in reply to the question of where he should go for the benefit of his health, "Now where shall it be? Leamington—Tonbridge Wells—Clifton? No, none of these; since I must go, it shall be to the sea-shore; *I shall take my microscope with me;* and get among the shells and nudibranchs, the sea-anemones, and the corallines." And we will stake our medical reputation upon the success of such occupation in such districts in hundreds of cases like Mr. Gosse's, where blue pill, hydrocyanic acid, and the most approved dietetical regimen have utterly failed. What can be more healthful than the exercise of body and mind in the fresh air of the sea-side, whilst seeking for the animal and vegetable treasures on the shore or in the waters of the ocean?

But let us follow Mr. Gosse to the sea-shore. He went to Devonshire, and was not long before his microscope was unpacked; and, as it is impossible for us to describe all that he has recorded of what he saw under the microscope, we must content ourselves with giving a few extracts, as indicative of the pleasant things to be met with in his very pleasant volume. He has caught a pecten, and of course kept it alive for some days in a marine aquavivarium, by which term is to be understood a hand-basin of sea-water; but his pecten, as pectens and other sea creatures will do, dies:—

"The death of my little pecten gave me the opportunity of submitting some of the gemmeous specks to the microscope. With a power of 220 diameters, I distinctly perceived a large lens, a glassy coat investing this, which itself was buried for more than half its volume in an investiture apparently granular, of a yellowish brown colour, having an ill-defined circle near its anterior side, of a blackish hue. Under pressure with the compressorium, the lens was manifestly circular; the coloured socket discharged dark granules, and from the darkest part a deep crimson pigment, which did not appear to be granular.

"I submitted portions of the gills also to the same magnifying power. Each of the four laminæ consists of a vast number of straight slender transparent filaments, evidently tubular, and about 1-1500th of an inch in diameter, arranged side by side; or rather of *one filament*, excessively long, reverted upon itself again and again, at both the free and the attached end of the laminæ, throughout its whole extent. This repeated filament is armed on each of two opposite sides with a line of vibrating cilia, the two lines moving in contrary directions; by the action of which a current of water is made continually to flow up and down each of these delicate filaments: so that the blood which circulates in their interior (for they are doubtless blood-vessels) is continually exposed throughout this its long and tortuous course to the action of oxygen.

"Like all organic functions, the action of these cilia is not under the will of the animal. It is said that if, during life, a small portion of the gills be cut off, the motion of the cilia will convey the fragment swiftly away, with a smooth easy motion, through the surrounding fluid, in a definite direction. It does not cease even with the life of the animal. The specimen which I examined had been dead at least fifteen hours, yet when I placed the torn fragments of the branchiæ, one after another, beneath the microscope, the energy of the ciliary action, as the wave flowed with uniform regularity up one side and down the other of every filament, filled me with astonishment. Even the next morning, twenty-six hours after death, when the tissues of the filaments were partially dissolved, the ciliary motion was still going on, on portions that preserved their integrity."—p. 52-54.

The capture of a madrepore affords our naturalist an opportunity of examining its structure under the microscope. Nothing is more marvellous to the observer of the tissues of these animals for the first time than the filiferous capsules or thread-cells with which many of these creatures abound. In the madrepore certain salmon-coloured bands are observed, sometimes regarded as ovaries:—

"Having detached a minute portion of one of the bands, I submitted it to an uniformly graduated pressure on the stage of the microscope, when I found that in its substance were imbedded a great number of filiferous capsules, exactly resembling, in essential points, those of certain Medusæ.

"The capsules are transparent and colourless, in shape a long oval from 1-650th to 1-800th inch in length, and are seen to contain a thread closely coiled. When the pressure reaches a certain point, the capsule shoots forth from one end the elastic thread, which in a moment starts out like a spring to a length thirty times as great as that of the capsule: sometimes in a straight line, sometimes in a serpentine, or (as I rather believe) a spiral form. The capsules do not *burst*, yet, at the instant of the propulsion of their filament, there is a distinct crack heard.

"I now cut off carefully, with fine-pointed scissors, two or three *tentacles* from one fully expanded, and submitted *them* to the same scrutiny. The rounded head of the tentacle appeared rather rough or hairy at first, but, as pressure began to flatten it, filiferous capsules were seen to be protruding from the outline, which increased in number as the pressure proceeded, until an amazing multitude appeared, and the whole substance of the tentacle-head was seen to be literally composed of these capsules, as thick as spiculæ in any sponge, with only a slight quantity of gelatinous matter to hold them together. To see these thousands of little vesicles discharging their missiles in rapid succession, like the flights of arrows in ancient battles, was an astonishing sight. When the propulsion could be distinctly followed by the eye, there was always seen a little zigzag line on each side of the thread, reaching to a considerable distance from the base, which I at first thought indicated a delicate membrane pushed out from the orifice of the capsule by the projected thread, until it at length burst, and shrank back in folds around the base. The form of the capsules differed from that of those described above, in that they were proportionally longer and more slender, being in fact almost linear. I could not discover any capsules *in the body* of the tentacle, but only *in the head.*

"If, indeed, these projected bristles are so many darts injected into the bodies of those minute animals which are the prey of the Madrepore, accompanied, as we must suppose each puncture to be, to insure its effect, with a fatal poison,—does not their presence in the convoluted bands of the interior militate against the supposition that these bands are ovaries, especially as I have seen the curious manner in which these are appressed to the swallowed morsel? Is it unreasonable to conjecture that their office may be accessory to that of the tentacles, destroying what may remain of life in the victim, after it has been enclosed by the lips, and is consequently out of the reach of the tentacles?

"This inference was confirmed by the results of further investigation; for, examining in the same manner other minute portions of the frilled bands, as I could detach them with the point of a pin, I at length found a piece in which the capsules were much more numerous, and vastly larger than any that I had yet seen, whether in the bands or the tentacle-heads. They were fully 1-300th inch in length, long-oval, but somewhat curved. Their size enabled me, with a power of 300 diameters, to see their structure much more distinctly.

"At the larger end is situated a lozenge-shaped body reaching to the middle: from the inner end of this, partly coiled round it, but extending through the remainder of the capsule, is the thread, lying in an irregular, rather loose spiral, the appearance of which differs considerably in different capsules. When it is projected, the *whole* contents of the capsule disappear from the interior, in a manner which induces me to believe, strange as it seems, that the lozenge-shaped body at least, if not the whole thread, is turned completely inside-out; for the extended thread is attached, *not to the smaller, but to the larger end*, without the least appearance of rupture.

"Now for the structure of the thread, or *wire*, for it is as elastic as steel. This is marvellously elaborate, especially when we consider its excessive tenuity, the threads of the largest capsules being less than 1-7000th of an inch in diameter, and those of the smallest perhaps 1-20000th of an inch. The basal part of the thread, to a length about half as great again as that of the capsule, is clothed with alternate series of triangular plates, laid one over the other, or imbricated, like the scales of an artichoke. About half of this portion is furnished with an armature of hairs rather closely set, standing out at right angles, like a bottle-brush; they are twice or thrice as long as the diameter of the thread, in the middle of the brush, but diminish to each end; the individual hairs taper to a point.

"I have offered a conjecture that the projection of the thread is an evolution of its interior, and I believe that it is a complete one through its whole length. I have, even since I wrote that conjecture, seen an example of the process, which I can scarcely describe intelligibly by words, but the witnessing of which left on my own mind scarcely a doubt of the fact. It was effected not with the flash-like rapidity common to the propulsion, but sufficiently slowly to be watched, and *by fits or jerks*, as if hindered by the tip of the lengthening thread being in contact with the glass. In consequence, probably, of this impediment, it took a serpentine, not a straight form, and *each bend of the course was made and stereotyped* (so to speak) *in succession, while the tip went on lengthening*; and the appearance of this lengthening tip was exactly like that of a glove-finger turning itself inside out.

"The brush of hairs, I think, is originally enclosed in the lozenge at the large end of the capsule. Both the lozenge and the brush are wanting in the small filiferous capsules; when I observed them in the large ones, the suggestion occurred that I might have overlooked them in the smaller, on which I examined some afresh with the utmost care, but in each case, the thread, which at first occupied the whole cavity of the capsule without any lozenge, was simple when evolved.

"The capsules appear confined to the thickened edge of the frilled band, in which they are set side by side, pointing outwards."—122-126.

We had marked many other passages, but the length of our last extract precludes further quotation at present. We may draw on Mr. Gosse's pages at a future opportunity. Now all we have left ourselves to do, is to commend Mr. Gosse's volume to the attention of our readers. It is not alone those who are about to visit the Devonshire coast that will find it of interest, but all who are fond of nature and natural scenery as they present themselves by the sea-shore. The volume is illustrated by twenty-eight plates, most of which are coloured, affording lively illustrations of the objects observed. Many of these contain representations of the microscopic structure of the higher animals, and not a few are devoted to microscopic objects alone.

NOTES AND CORRESPONDENCE.

On the best Form of Micrometer for the Microscope.—Now that the achromatic microscope is an instrument found in the hands of so many persons, and investigations are carried on generally with such extreme accuracy, it has become absolutely necessary that a mode of micrometrical measurement should be universally adopted, which, while it admits of easy application, shall at the same time be capable of as extreme minuteness and accuracy as modern art can achieve. I am afraid it cannot yet be said that this is the case, inasmuch as not one person, perhaps, in ten uses the *cobweb micrometer*, which, in my opinion, as far exceeds in point of exactness the common eye-piece apparatus as the modern achromatic microscope surpasses the old-fashioned instrument. It is a matter of astonishment to me to find two such acute and accurate observers as Mr. Jackson and Mr. Quekett advocating the use of the slip of ruled glass in the eye-piece, and disparaging the cobweb micrometer on such fallacious grounds as the following: viz. that as the value of the divisions in each apparatus must be calculated by the ruled lines placed on the stage and based on their supposed correctness, therefore the one form must be as correct, or nearly so, as the other.

The object of this short paper is to point out this fallacy, and to prove, at the same time, if indeed it needs proof, first, that the cobweb micrometer is, at the least, equally easy to use as its rival; secondly, that the measurements made with it far exceed those made with the latter in point of accuracy.

In the first place no one will deny that the one form of instrument is as readily placed *in position* for measurement as the other; if so, any advantage in point of facility in use that the one may have over the other must be in reading the divisions; and I appeal with confidence to any impartial person whether (setting aside the superior ease with which the cobweb lines give the exact diameter of the object) the notches of the comb and the divisions of the milled head are not as easily read off and referred to the table of values as the lines on the ruled glass plate. I think I may at once assume that, as far as regards *manipulation*, one form of micrometer is just as simple as the other.

Now, with regard to the second point in question, let us see whether the two forms are also equal in point of accuracy.

In every glass plate, which has been ruled with lines either 1-100th, or 1-500, or 1-1000th of an inch apart, there will be discovered, upon close examination, *some* inequality in the spaces. I at least have never yet seen one in which I could not detect an error, while in many it has been very considerable. Now an error to the amount of 1-1000th of an inch is easily made, and easily passed over by the observer; and if he uses such a plate in his eye-piece as a micrometer, many of his measurements will be affected by that error; while some again may not, owing to the use of a different portion of the ruled space; and thus an inequality or irregularity will run through his recorded measurements. This source of error cannot be avoided except by taking a number of measurements of the same object *on different lines*, and then striking a mean of the whole. Who is there, I may ask, who ever has or ever would take this trouble?

With respect to the cobweb micrometer the case is different; here there can be no possible error arising from the micrometer eye-piece itself, provided it is as good in point of workmanship as those of Messrs. Powell and Lealand. The only source of error lies in the stage micrometer used to determine the value of its divisions; and the mode of obviating this is sure and perfectly simple. My plan has been to procure three ruled slips of glass, ruled to 1-500th, 1-1000th, and 1-5000th of an inch respectively. I first take a mean of the values of the divisions in the eye-piece, observed *on three different portions* of the ruled space in *each one* of the three ruled slips; this gives me three values, or one for each slip; then a mean of these three will, I conceive, approach as near to absolute correctness as any one could wish to attain.

The value, then, of the divisions in the cobweb micrometer (which should be drawn out in a table for reference) will be *a mean of nine observations with each object-glass.* And when once this table has been drawn up I need scarcely say that every single measurement that may be made hereafter must be *correct*, without the necessity of any further trouble; or, if there be an error, it rests with the observer, and not in any part of the instrument he uses.

It is obvious to remark that in order to arrive at *equally* accurate measurements with the common positive or negative eye-piece micrometer (even granting that such could be made at all, which I dispute) at least *nine different measurements* must be taken of every object measured, and *three different* slips of glass used for the purpose.

In respect to *minuteness* of measurement of course the cobweb micrometer carries off the palm; here the eye-piece apparatus cannot attempt to compete with its rival. Mr. Jackson has stated that a space so small in amount as 1-800,000th of an inch may be appreciated with the former instrument when used with a 1-8th object-glass. I find, however, that the value of each division of the milled head with that object-glass is 1-300,000th of an inch, and that, in practice, a turn of the screw less than four of these divisions is not appreciable by the eye; giving, therefore, 1-75,000 of an inch as the smallest measurable space with the 1-8th, and 1-112,000th with the 1-12th object-glass. Whatever may be Mr. Ross's opinion, I do not hesitate to say that it is quite possible with a modern 1-12th object-glass and a good cobweb micrometer to estimate a quantity as small as the 1-100,000th of an inch with ease and precision.

Mr. Jackson is of opinion (' Trans. Microsc. Soc.,' vol. ii. p. 136) that, although so accurate a measuring instrument as the cobweb micrometer may be necessary in the case of the telescope, yet it is not so with the microscope, alleging, as a reason, the advantage the former instrument possesses in having the uniformity of *time* for a basis of measurement. I think, however, that I have shown that the value of the divisions in this micrometer, when applied to the microscope, may be easily ascertained to such a nicety that the amount of error is reduced to very small dimensions indeed, so much so, as to be *quite inappreciable*, if it be not entirely destroyed; and if this be so, then I cannot see why a more accurate means of measurement should be applied to the telescope (when the magnifying power with such observations rarely exceeds 500 times) than to the achromatic microscope, whose definition is infinitely superior to that of the former instrument, and whose magnifying power, with the micrometer, amounts often to 900 diameter, and occasionally to as much as 1200.

It is said that the measurements of the cobweb micrometer are unnecessarily fine, and that the expense of the instrument must prevent its ever coming into general use. This doctrine I confess I cannot understand. I can conceive no measurements to be too accurate; nor do I think that the difference between 4*l*. and 1*l*. would be any obstacle to the more extended use of the more expensive instrument, provided its superior qualities were thoroughly known and appreciated.

The names of Mr. Jackson and Mr. Quekett, both of them noted microscopists and accurate thinkers, must of necessity carry great weight, and will, I fear, have induced many persons to adopt that form of micrometer which is most economical in point of expense, though, as I think I have shown, least accurate in practice; and it is with the view of counteracting what I believe to be a fallacy in their writings, and with the desire that the greatest possible accuracy should attend everything connected with our favourite instrument, that I have been led to make these few remarks.—H. C. K., *Stretton Rectory, near Hereford*, July 16, 1853.

On Rotation in the Cells of Plants.—Last summer I ascertained the occurrence of this rotation in a very distinct form, in a plant recently added to the British flora—*Anacharis Alsinastrum.* This is an aquatic plant, the new "water weed," as it is called, which has recently appeared in great abundance in the rivers and streams of various parts of England, so much so as to interfere with navigation and fishing, and especially to put a stop to regattas and sailing matches,—it has completely blocked up some of the canals and locks of the English fens. It is supposed by some botanists (and with very good reason) that the *Anacharis* has been accidentally introduced with timber or other material from North America, where a species (*Udora canadensis*), apparently identical, is of common occurrence in the rivers. As elsewhere, this plant has made its mysterious appearance in the pond of the Edinburgh Botanic Garden, where it is not known to have been planted.

The leaf of the *Anacharis* is composed of cells of an oblong form, but in some parts of the leaf becoming much elongated. At the margin of the leaf, which is toothed (each tooth consisting of a single somewhat triangular cell), the tissue of the leaf consists of a single layer of cells, the latter being more elongated in form than those towards the centre of the leaf. In these marginal cells, the green granules (chlorophyll) which they contain may be readily seen in rotation, thus indicating the currents of cell sap. The phenomenon is best seen, however, in those cells (very much elongated) which form the midrib of the leaf. Granules are seen scattered about in the cells, a few in the centre of each cell are fixed. But there will be observed another set of spherules, forming a continuous line around the margin of each cell; these are in rapid motion, flowing along one side of the cell, generally with great regularity, till they arrive at the end, where they cross over and return by the other side, thus forming a continuous "rotation" in the cell. The arrows show the direction of motion. Although the granules generally move on in this way, without interruption, closely following each other, still a casual interruption occasionally takes place, and crowding ensues; this is most frequent at the ends of the cells, at the "crossing." But the granules are gifted with even a greater share of politeness than is usually to be found at a London crossing,—for when a crowding takes place, there is never seen an obstreperous granule trying to gain the precedence of his fellows, to get over first. When stagnation occurs, the granules behind continue to move up in regular order, crowding in rear of those that preceded them, and not one will move till the one that first stopped begins to go on; nor is it crowded upon and pushed forward by those behind, as is usual in human society. With the greatest regularity each waits till the way is cleared for him, and then politely resumes his course to make way for his patient followers! Slow movements of a similar nature are seen in the tubular cells of the stem of *Anacharis*.

The movements in *Anacharis* very much resemble those seen in *Vallisneria*, but from the nature of the leaf they are much more easily observed in the former. I have been much interested in the subject, and have made a few observations from time to time, but defer their publication until I can find leisure to enter into a more complete examination of the phenomenon; this especially, lest I incur the censure of those who regard the description of it given by Schultz as a "pattern of imperfect observation, and unfortunate conclusions."

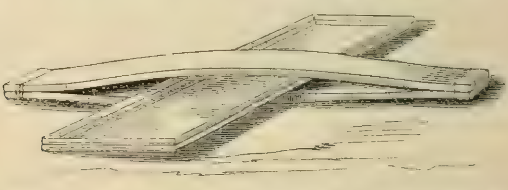
Rotation has been observed in the cells of the following, as well as of other, plants:—

Chara and Nitella.
Sagittaria sagittifolia.
Stratiotes aloides.
Vallisneria spiralis.
Zannichellia palustris.
Hydrocharis Morsus-Ranæ.
Potamogeton.
Equisetum.
Anacharis Alsinastrum.
Loasæ (in stinging hairs).
Urtica do.
Tradescantia virginica (hairs of filaments).
Campanula Medium (corolla hairs).
Marchantia (radical hairs).
Mosses.
Ceratophyllum.
Podostemaceæ.
Lemna.
Lichens.
Argæ.
Fungi.

As a hint to those who may wish to examine the rotation in cells, it may be mentioned that heat seems to increase the movements, and thus to lead to their more easy detection. It is therefore desirable to pursue such examinations in a warm room. — G. Lawson, *Scottish Florist and Horticultural Journal.*

Rotation in the Vallisneria Spiralis.—It is stated in botanical works (Schleiden's 'Principles of Scientific Botany,' English translation, p. 94) that the circulation in the *Vallisneria* continues for months after the leaf has been separated from the plant. I have recently had an opportunity of verifying this interesting fact. A portion of the leaf was given to me on the 15th of February last, and having kept it, changing the water occasionally, on the 2nd of August, *after a lapse of five months and a half*, I had the pleasure of showing the circulation to several gentlemen (microscopists), though the leaf was then perfectly yellow. It may be as well to state, that up to about the conclusion of four months and a half there was no difficulty in seeing the circulation in any portion of the fragment; but, at the expiration of that period, it began to be more confined to single cells, so that, on the 2nd of August, I was some time in finding a cell which exhibited the phenomenon. I looked with great care on the 10th, and again on the 11th, but could not detect any circulation; the leaf appears now to be perfectly dead, and is much decayed.—G. HUNT, *Liverpool.*

Holder.—Those who are engaged in mounting objects in the dry way for the microscope are aware that, for various reasons, it becomes necessary frequently to separate the slides which contain them, in order to examine the specimens before they are finally mounted; more especially if they are undergoing the drying process after maceration. The slides are usually bound tightly with pieces of string or tape after each examination, or sometimes they are pressed with weights; but, when time is of value, each of these plans is open to objection. I have, therefore, substituted a very simple contrivance, which, while it maintains a constant pressure upon the glasses, can be removed or replaced without trouble or loss of time. It consists of two pieces of whalebone three inches and a half long tied or riveted together at each end; and which, for convenience sake, may be called a *holder*. It is well to have several of these holders at hand, and for this purpose, as well as to save the trouble of making them, I have requested Mr. Pritchard, of 162, Fleet-street, to prepare and keep some always ready for sale; where they can be had singly, or by the dozen, at a moderate price.



Brass Cementing Pencil.—When it is required to *cement* the thin glass cover to the slide, the usual method consists in melting a portion of the cement in a ladle. It is then painted round the edge of the cover and the contiguous surface of the slide with a camel's hair brush, and smoothed off with a heated wire. The whole of these objects are effected by using the little instrument which I propose to call the cementing pencil. It is a brass tube, six inches long, with a conical bore, having a lid to screw on. When a small portion of the cement, crumbled into fragments, is shot into the tube, it is ready for use.

In using this instrument the extremity is gently heated in the flame of a spirit-lamp, and when the cement begins to ooze out, holding the pencil like a pen, the point is traced along each side of the cover leaving a line of cement in the angle. It is thus laid on much easier than with a brush, and after a little manipulation it will be found that the point will suffice to polish off instead of using the flattened wire.

I beg to recommend this to those who, like myself, pressed by professional avocations, are anxious to make the most of snatched intervals. And, I think, after resorting to various expedients, that, where it is desirable to use cement this will be found the cleanest and most expeditious method.

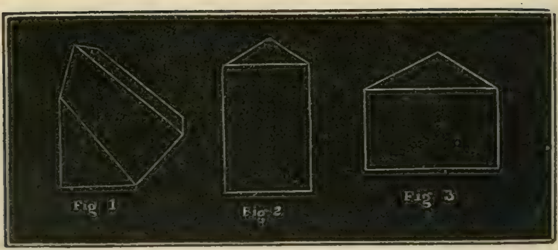
The cement suggested by Quekett is as follows :—

Electrical Cement.—" A very excellent cement is made by melting together two ounces of black resin, one ounce of bees-wax, and one ounce of vermilion."

This instrument can be procured at Mr. Pritchard's.— JOHN GORHAM, *Tunbridge*, *Kent*.

Binocular and Stereoscopic Microscope.—The attention given in the Microscopical Journal to the subject of binocular vision induces me to submit to your readers an account of a prism or solid of glass, which, with the least possible loss of light, will enable one microscopic object-lens to furnish either a pair of stereoscopic pictures for the photographic camera, or two separate pencils of rays for the eye-pieces of a binocular microscope. The prism or solid to which I refer has been already employed as an erecting camera-lucida for the compound microscope and for the astronomical telescope, and is roughly represented in the accompanying diagram, fig. 1.

The nature of solid will be readily understood, if two common rectangular isosceles prisms be placed with their largest faces in contact and with their axes perpendicular to each other. The prisms should have their largest faces of exactly



the same size, but should differ in other respects, as shown in figs. 2 and 3.

The effect, which a solid of this description produces upon a pencil of rays incident centrally and perpendicularly upon either of its largest faces, is to divide it into two separate pencils which, after two internal and total reflections, emerge so as to form two inverted stereoscopic images of the object from which the pencil first originated. This inversion is precisely that which is required for the binocular microscope, and enables a photographic camera to produce a pair of stereoscopic pictures by one operation.

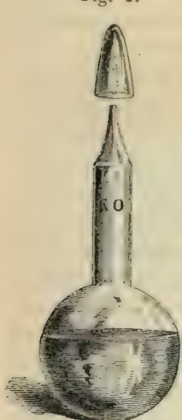
The mechanical modifications of Chevalier's microscope (figured by Hanover, plate II., fig. 2), or of Highley's camera, represented in your last number, will readily suggest themselves to those who desire either a binocular erecting microscope or a stereoscopic photographic camera.—W. Hodgson, *Old Brathay, near Ambleside*, August 11, 1853.

Method of applying Chemical Re-agents to Minute Quantities of Matter.—The usual method of testing a small quantity of a deposit or a drop of a solution with chemical re-agents, consists, as is well known, of allowing a drop to fall from the bottle containing the test, or of removing a small quantity of the solution upon a stirring-rod or pipette. In practice both these plans are often found inconvenient, particularly in those instances in which the quantity to be tested is very minute, as often occurs in microscopical investigations.

For some years past I have been in the habit of using little bulbs, terminating in capillary points for containing the tests, and from these the smallest quantity of the re-agent that can possibly be required, is readily expelled by the expansion of the enclosed bubble of air, when the bulb is inverted and held in the warm hand.

The solution or deposit to be examined may be divided into several small portions, and placed upon one of the ordinary glass slides employed for mounting microscopical preparations. Each portion is then tested with a separate re-agent, as the nature of the examination may require. About a dozen bulbs will contain all the re-agents required by the microscopist, and these may be conveniently arranged in a smal lcase.

Fig. 1.



The bulbs may be made with the aid of the blow-pipe upon a piece of ordinary glass tube; they should be about an inch in diameter. The tube may then be drawn to a capillary point, and a small cap, also of glass tube, adapted to the top. This may afterwards be ground to the neck in order to make it fit accurately. Or a cap is very readily made of a small piece of gutta percha; and this will be found to answer very well for those bulbs which are not intended to contain acids. (Fig. 1.)

Fig. 2.

The name or formula of each test may be written upon the neck of the bulb with a writing diamond, or painted upon it with a little black varnish.

Mr. Highley has suggested an improvement in the form of these little vessels, which possesses many advantages. He has had little bottles made from thin glass tube, about half an inch in diameter, with capillary openings and ground glass tops. These have flat bottoms, so that they stand very well, and are more convenient for packing.* (Fig. 2.)

Of filling the Bulbs with the Test Solutions.—This is very readily effected by the application of a moderate heat to the bulb, the capillary orifice of which is inverted under the surface of the liquid which it is wished to introduce. The included air becomes expanded by the heat, and much escapes from the opening. Upon removing the spirit-lamp the fluid rushes into the bulb to occupy the place of the air which has been expelled. The bulb should not be more than two-thirds full of the solution, in order that sufficient air may be included to admit of considerable expansion when the warmth of the hand is applied to the bulb. This method of applying re-agents will be found very convenient in microscopical examination, in testing for carbonates, urinary calculi, &c., and with a very few drops of the test solutions a qualitative examination can be readily effected upon a quantity of substance far too minute for the ordinary methods of examination.—LIONEL BEALE.

* These may be obtained, arranged in cases, of Mr. Highley, 32, Fleet Street.

New Microscopical Society.—On the 20th March, 1853, a Microscopical Society was founded at Dresden by Drs. Günther, Pieschel, L. Rabenhorst, H. Richter, Stein, G. Struve, Zeiss, and Zenker. One of the provisions in the Rules of the new society declares, that there shall be no inactive members, so we ought to expect a good deal from its labours.

Powell's Condenser.—Those who are interested in observing the improvements in microscopic apparatus, will be pleased to know, that with the new Condenser made by Powell and Lealand (a modification of Gillot's), and an $\frac{1}{8}$th object-glass, the *Ceratoneis fasciolata* can be demonstrated to be covered with dots similar to those of *Navicula Hippocampus*, though of course much smaller. I have used the No. 8 aperture, and the No. 4 stop. The dots are most easily demonstrated by daylight, and No. 2 eye-piece.—THOMAS INMAN, M.D., *Liverpool.*

Query.—In what work may there be found a description of the exceedingly beautiful structure of the proboscis of the fly, more especially, of what are termed, in the explanation of the plate, in the third number of this Journal, "divided absorbent tubes"?—G. H., *Liverpool.*

PROCEEDINGS OF SOCIETIES.

British Association for the Advancement of Science.

The meeting was held this year at Hull. Several papers, involving microscopical research, were read.

Professor Allman read three papers. 1. On the presence of Endochrome, developed from the interior of cells in a species of *Conferva*. 2. On the cellular structure of *Hydra viridis*, in which he maintained, in opposition to the views of Kölliker and Ecker, the distinctly cellular origin of the tissues of this animal. 3. On the structure of *Bursaria* and the nature of its nutrition.

Dr. Redfern, of Aberdeen, made some remarks on the relation of bone and cartilage, and drew attention to some observations which he had made which led him to conclude they were more intimately related than was generally supposed, and that cartilage was an incipient condition of bone.

J. D. Sollitt, Esq., and R. Harrison, Esq., read a joint paper on the Diatomaceæ, which, from the interest of the subject at this time, we present entire.

"*On the Diatomaceæ found in the vicinity of Hull.*

"No part of the kingdom is richer in Diatomaceæ than the vicinity of Hull; the low situation, and as it were terminations of the various waters as they come to the Humber, cause a deposit of these minute animals which have had their first being perhaps at a distance, and are then brought down to us by the currents, there to increase and multiply to a great extent; while at the same time the Humber, rich in the salt-water Diatomaceæ, deposits these in the various pools and holes into which the spring-tides reach, where they are left until the next spring-tide without being disturbed, and thus they are easily collected for the microscope. It may be justly observed that the Diatomaceæ are, of all others, the most beautiful of microscopic objects; their almost unlimited number, and the beautiful variety of their forms, are alone sufficient to fix the attention and raise the admiration of the microscopist; but in addition to this, they form a most useful resource in trying the excellence of various lenses, and as *Test Objects* for the microscope they are invaluable. The modern improvements in achromatic object-glasses have placed all the old test-objects in the background and rendered them almost useless; but the Diatomaceæ form a series of objects for this purpose, adequate for every improvement that has been or may be effected by the practical opticians either of this or of any other country. As Test Objects they have now been adopted by the scientific in all parts of the world, and for this adoption the optical world are wholly indebted to the Hull Microscopists. We in Hull first discovered the delicate markings on their silicious coverings, and pointed them out to others as the proper tests for lenses. The first of the Diatomaceæ on which the lines were seen was the *Navicula Hippocampus* of Ehrenberg, Pritchard, and Quekett: this discovery was made early in 1841, when specimens were sent to the Microscopical Society in London; but the London microscopists not being able to bring out anything but the longitudinal markings, a remark was published by Mr. Harrison in the Microscopical Journal for June 1841, stating that we had discovered cross striæ on the *N. Hippocampus*, but that these were only visible on some of the specimens; on this they were immediately written to, and told that we meant to say there were both longitudinal and cross striæ on the specimens sent. They, after labouring for nearly six months and frequently asserting that we were mistaken, at length saw the cross striæ, and an account of our discovery then appeared in the Microscopical Journal for January 1842. The next specimen on which the markings were seen by us were the *N. liniata* of Harrison; this Navicula was unknown until 1843, at least it had not been described by any author before that time. In 1844 Mr. Soleitt was in London and showed the lines on this Navicula to Mr. Ross, with a 1-8th of Nachett's, although Mr. Ross at that time could not bring them out with a very fine1-12th which he had just finished; this little circumstance, trifling as it may appear, caused that eminent optician to alter the construction of his microscope, and to bring it into its present superior form. We afterwards discovered the lines on the *N. angulata*, the *N. Strigosa* and, after much labour, those on the *Ceratoneis Fasciola* and the *Navicula sigmoidea*, and afterwards on the *N. Arcus*; which last is so extremely difficult, that, in order to even catch a glimpse of its delicate markings, the observer must be in possession of glasses of a very large angle of aperture and the finest definition, have the most careful management of oblique light, and in addition be possessed of a large share of patience.

"The delicacy of the markings in these objects will be easily judged of from the number of striæ on each which would be required to make an inch: these are as follow:—

		Striæ.		
$\frac{1}{2}$	N. strigilis, Marine . . .	34,000	in the inch	40
$\frac{1}{4}$	N. Hippocampus, fresh water	42,000	,,	60
	N. Spencerii Quekett . .	50,000	,,	70
	N. liniata and angulata, large	60,000	,,	80
	N. angulata, small . . .	70,000	,,	90
$\frac{1}{8}$	N. strigosa, large . . .	70,000	,,	
	N. strigosa, small . . .	80,000	,,	95
	Ceratoneis Fasciola . . .	90,000	,,	110
	N. sigmoidea	105,000	,,	120
$\frac{1}{12}$	N. Arcus	130,000	,,	150

"We have dwelt longer than we intended on these particular species on account of their value to the microscopist as test objects. We may observe that they are equally tests for defining power as they are for angle of aperture: take for illustration the *N. fasciola*, with a 1-12th object-glass and angle of aperture of 150°; adjust the object-glass for the proper thickness of glass covering the object, then by proper oblique light you will have the markings brought out beautifully sharp and distinct; alter the adjustment for the thickness of the glass, so as to deteriorate the defining power, and the markings will then scarcely be visible.

"In addition to the riches of our neighbourhood in recent living specimens of these minute forms, we must not forget the very large bed of fossil Diatomaceæ which was discovered in Holderness whilst the workmen were employed in making a drain for carrying off the water from the low lands near Keyingham. Throughout the part which the drain intersects these fossil shells lay in some places so numerous as to form a bed of about two feet in thickness, many yards in breadth, and, for anything that we know to the contrary, it may be miles in length. In this bed of fossil Diatomaceæ are to be found almost all the various forms of smaller *Navicula*, *Eunotia*, *Cocconeïs*, *Bacillaria*, &c. The *Eunotia granulata* are very numerous in this bed, at least we found them so in several slides in our possession.

"We may likewise state that amongst the remains of a forest buried in the sands on the Holderness coast, near Roos, we have found a very large number of fresh-water Diatomaceæ in a fossil state, particularly those common to our fresh-water ditches, and which are never found in salt water, although the sea now flows over this submarine forest at every tide.

"But to return to the subject of the immense numbers of beautiful Diatomaceæ found in our neighbourhood. It would be a vain attempt to try to give a description of each of them. I may observe that we have several species of *Gallionella*, almost every kind of *Navicula* Pritchard named, and a great many more which are not either in plates or named in his list. We have also several species of *Eunotia*, *Cocconeïs*, *Bacillaria*, *Tessella*, *Fragilaria*, *Meridion*, *Isthmia*, *Synedra*, *Podosphenia*, *Gomphonema*, *Echinella*, *Cocconema*, *Achnanthes*, and *Frustulia;* also the several species of living *Xanthidia*—mentioned in Pritchard. If we take Smith's classification, of course the number of genera will be considerably increased, and even his species are not nearly sufficient to take in all that we have in this part of the country. We have seen in our waters nearly every one he has figured in the first volume of his work, and the few we have not seen is no proof that they are not to be found in this vicinity.

"The places where we find the fresh-water Diatomaceæ are Spring Ditch, which is the old aqueduct that used to supply the town with water; the Cottingham Drain; and other similar runs of water, of which there are many in the neighbourhood. We may observe that on the other side of the Humber, at Burton, about five miles from hence, there is a stream of water very similar to our Spring Ditch, in which the fresh-water Diatomaceæ are sometimes so numerous that the whole surface of the mud is covered with them to the thickness of an inch or more. With regard to the salt-water species, as we have observed before, they are found in large quantities in holes and ditches into which the spring tides reach. *N. Fascicola* is found in large quantities in the Garrison Moat, into which the tide occasionally finds its way; and in some of the stagnant waters by the side of the railway, along the South Humber Bank, as it is usually but improperly called, we find large numbers of the *N. liniata* (*attenuatus* of Smith), and also the *N. angulata* and *strigilis*.

"We would just here observe that, when we first discovered the lines or markings on the various test shells which we have named in this paper, we sent specimens of them not only to the members of the London Microscopical Society, but also to Mr. Smith, Mr. Ross, Messrs. Powell and Lealand, M. Nachet in Paris, and Professor Baily in America, the whole of which at once saw the excellency of those objects as tests for the microscope. Indeed they, are without doubt, to the microscope what the close double stars are to the telescope."

The paper was illustrated with diagrams, and a list of the species discovered in Hull was given.

TRANSACTIONS OF MICROSCOPICAL SOCIETY.

DESCRIPTION OF PLATE I. VOL. II.

Fig.

1. Bacteriastrium furcatum.
2. ———— curvatum.
3. Euphyllodium spathulatum.
4. Triceratium sculptum.
5. ———— arcuatum.
6. ———— arbiculatum.

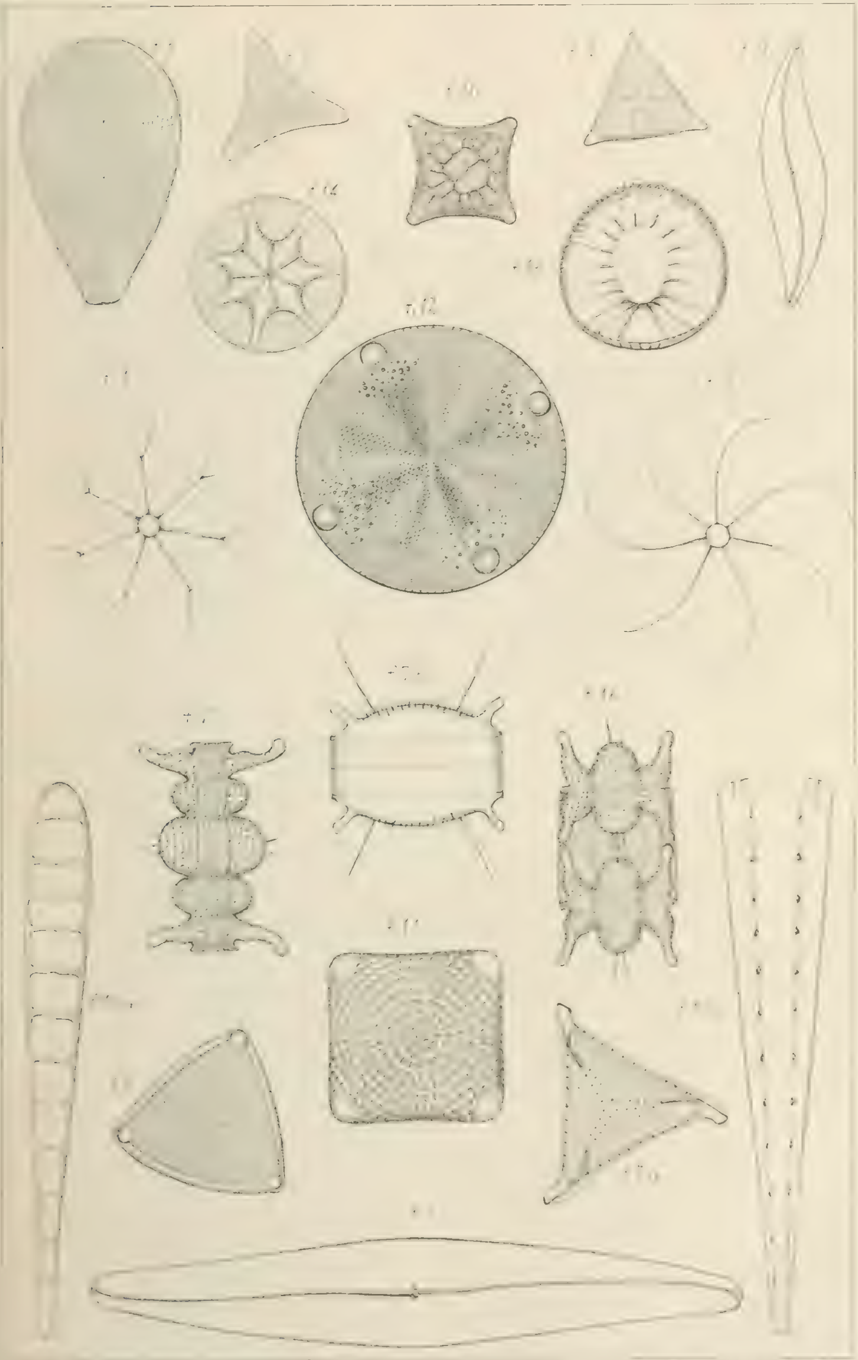
7*a*. Triceratium contortum.

*7*b*. ———— do. showing spine.

8. Pleurosigma vallidum.
9. ———— inflatum.
10. Amphitetras arisata.
11. ———— tessellata.
12. Eupodiscus crucifer.
13. Campylodiscus latus.
14. Asterolampra impar.

15*a* & 15*b*. Climacosphænia catena.

16. Denticella simplex.
17. ———— margaretifera.



Tuffen West ad nat. sculp.

DESCRIPTION OF PLATE I. VOL. II.

Figs.

1.—Four papillæ from the point of the finger; the largest containing a tactile corpuscle with its nerves, while the others possess capillary loops. Acetic acid added—*a*. Nerves. *b*. Neurilemma. *c*. "Nuclei." *d*. Capillaries.

2.—A papilla from the finger of a Tahitian, with a small tactile corpuscle. Letters as above. Acetic acid added.

3. 4. 5.—Termination of nerve-fibres against tactile corpuscles. Caustic soda added. 600.

6.—Extremity of one of the papillæ at the base of a Frog's tongue, the epithelium being stripped off.

7.—A nerve, consisting of a single, dark contoured fibril in its neurilemma, from the human finger.

8.—Portion of the wall of a Pacinian body from the human finger.

9.—Section perpendicularly through one of the ridges on the beak of a Duck.—*l*. Horny layer of epidermis. *m*. Mucous layer. *n*. Derma. *p*. Pacinian bodies.

10. A single Pacinian body of the same.

Diagrams.

A.—Of a Tactile corpuscle.

B.—Of a Pacinian body.

C.—Of a Savian body.

D.—Of the "Muciparous Canals" of Fishes.

E.—Of a Vibrissa of a Rat.

PLATE II.

Illustrating Dr. Herapath's Paper on Quinine in the Urine.

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DESCRIPTION OF PLATE I. VOL. II.

Figs.

1.—Four papillæ from the point of the finger; the largest containing a tactile corpuscle with its nerves, while the others possess capillary loops. Acetic acid added—*a*. Nerves. *b*. Neurilemma. *c*. "Nuclei." *d*. Capillaries.

2.—A papilla from the finger of a Tahitian, with a small tactile corpuscle. Letters as above. Acetic acid added.

3. 4. 5.—Termination of nerve-fibres against tactile corpuscles. Caustic soda added. 600.

6.—Extremity of one of the papillæ at the base of a Frog's tongue, the epithelium being stripped off.

7.—A nerve, consisting of a single, dark contoured fibril in its neurilemma, from the human finger.

8.—Portion of the wall of a Pacinian body from the human finger.

9.—Section perpendicularly through one of the ridges on the beak of a Duck.—*l*. Horny layer of epidermis. *m*. Mucous layer. *n*. Derma. *p*. Pacinian bodies.

10. A single Pacinian body of the same.

Diagrams.

A.—Of a Tactile corpuscle.

B.—Of a Pacinian body.

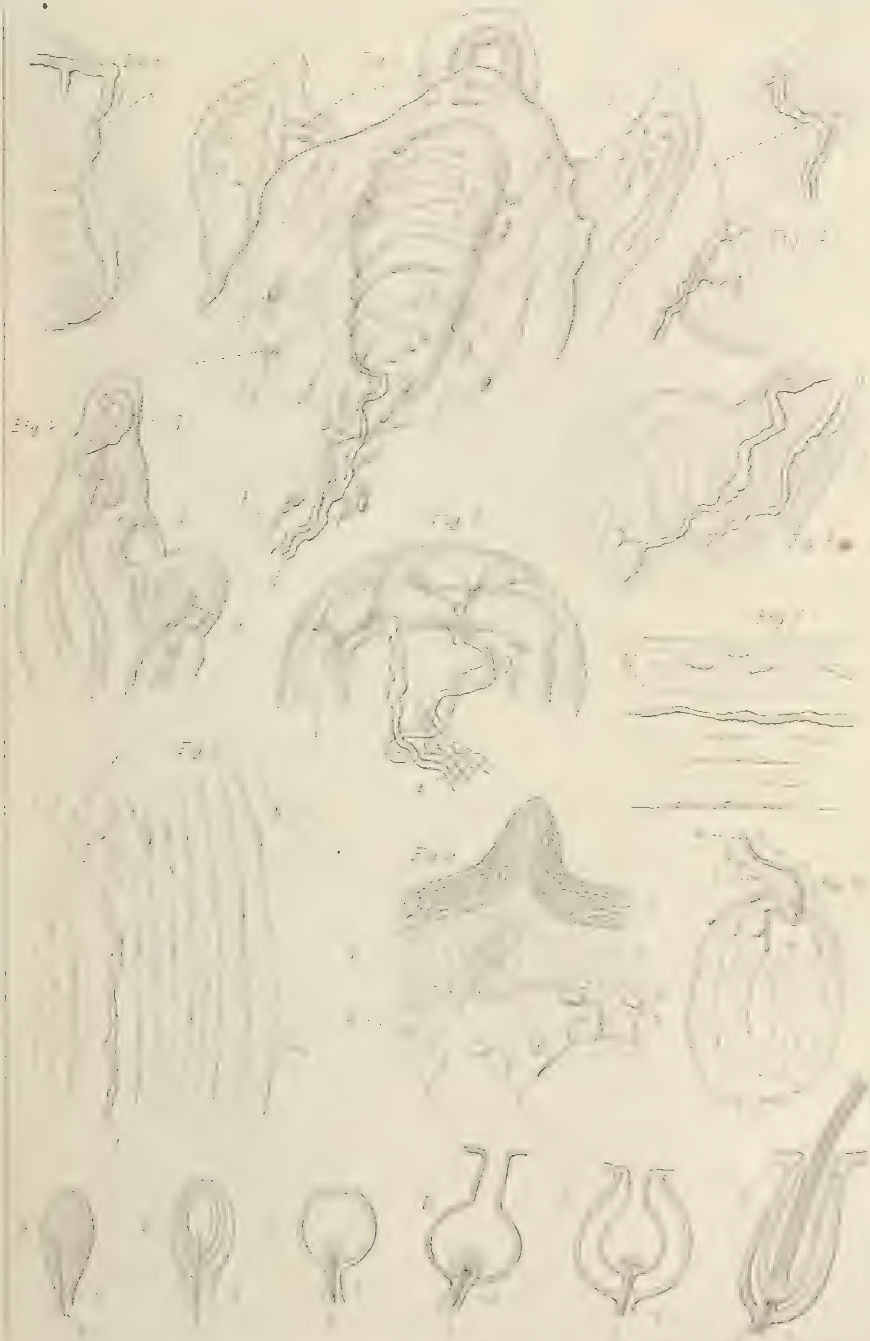
C.—Of a Savian body.

D.—Of the "Muciparous Canals" of Fishes.

E.—Of a Vibrissa of a Rat.

PLATE II.

Illustrating Dr. Herapath's Paper on Quinine in the Urine.



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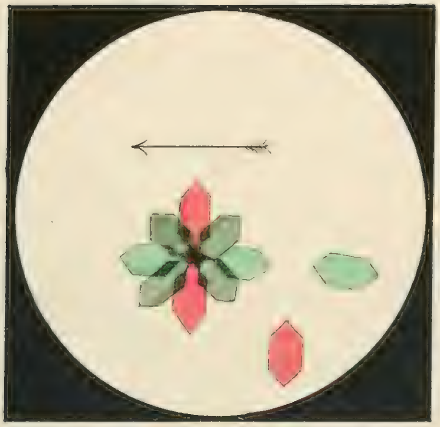
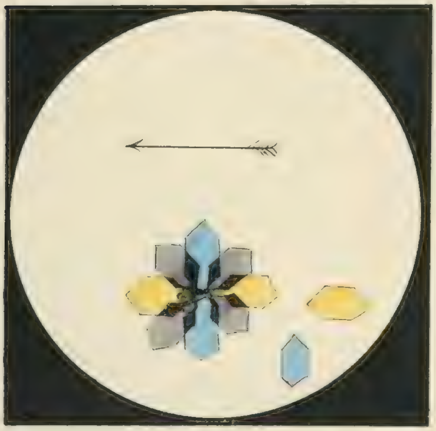
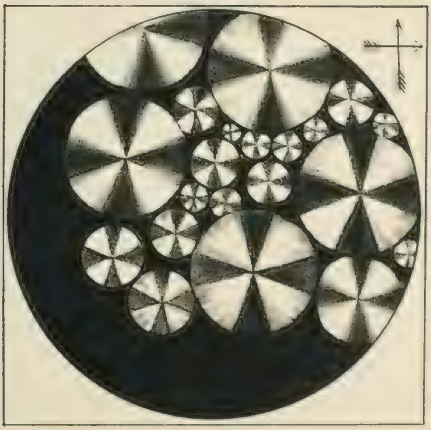






ORIGINAL COMMUNICATIONS.

Some Observations on the Illumination *of* Transparent Objects. By George Rainey, M.R.C.S., Demonstrator of Anatomy and Microscopic Anatomy at St. Thomas's Hospital.

(*Continued from page* 13.)

I may observe, that I was rather surprised to see these images on globules of mercury even as large as 1-70th of an inch in diameter, especially as they occupied a part of the globule exactly opposite to that on which the light coming directly from the condenser fell. The illumination of this part of a globule cannot be attributed to radiated light, as none of the rays coming from below can reach it, and a mere image on a reflecting surface cannot be supposed capable of absorbing light, and therefore it cannot radiate it

I may observe also that I found considerable difficulty in determining the cause of this fact, chiefly because the different circumstances under which it was examined gave such contradictory results. I first thought that an image was formed in the microscope which was reflected upon the upper surface of the globule, which I afterwards found to be only partially correct. My friend Dr Bristow suggested the probability of its being a reflection from the lower surface of the object-glass, which was also in part correct; but as neither of these suppositions satisfied all the conditions under which these images were seen, I made the following experiments with a view to simplify, and therefore to facilitate, the examination as much as possible.

I removed all the lenses from Gillett's condenser, and employed only the diaphragm with the flat side of a mirror, when I found, on examining globules of mercury with achromatic lenses of different magnifying powers, that images of the stops were formed as before, though very much more minute, and much less perfect. Hence I concluded that the appearances above described, although much exaggerated by condensing lenses, are not entirely produced by them.

I next examined with a simple lens, of about one-inch focus, globules of mercury illuminated by Gillett's condenser, and I found that when they were *not covered* by a piece of thin glass no image was formed upon them, but when they were covered, the thin glass being 1-200th of an inch above them, that a distinct image of the stop was seen upon their upper surface.

Hence in this instance, no doubt could exist as to the cause of the image, as it could only have been produced by the reflection of the rays proceeding from the source of light, and impinging obliquely upon the under surface of the cover backwards upon the upper surface of the globule, the figure of the stop being occasioned only by the quantity of light which it intercepted, and thus prevented from falling upon the glass, and consequently from illuminating the globule.

I next substituted for the simple lens achromatic lenses of one and two inch focus, and the result was precisely the same; that is, when no cover was placed upon the object there was no image, but when it was, there was a distinct one. In all these cases the cover was 1-200th of an inch from the object: if this distance be increased beyond a certain limit, images will not be formed on the globules. The greatest distance for perfectly flat glass is about 1-30th of an inch; but if the glass be convex and the globule be placed beneath the centre of its convexity, the distance is very much increased, the amount of increase depending upon the degree of the convexity of the glass cover.

I then examined an uncovered globule of mercury with a lens of 1-4th of an inch focus, on which I could perceive two imperfect and distorted images of the stop of the diaphragm. They were of different size, and did not appear to be exactly upon the same plane; the smaller one was the nearer and seemed to be much darker than the other. As in this case the distance of the object-glass from the object was less than 1-30th of an inch, I concluded that the glasses composing it had reflected the images on the globule, especially as there were two images, in the same way as the glass cover had done in the experiment with the simple lens and the low powers.

Lastly, I substituted a half-inch for the quarter, and the result was the same as when the latter was used, excepting that one of the two images was very distinct and permanent, whilst the other one was fugacious, appearing and disappearing with the slightest movement of the head. The distance of this lens from the object exceeded the 1-30th of an inch, the limit at which images are formed on flat glass; but as it did not exceed the limit at which images are formed on convex glass, I concluded that these images were reflected by the glasses composing the objective, in the same way as when the quarter of an inch lens was employed, especially as two images were apparent.

The inference, then, to be drawn from these experiments is, first, that the image of the source of light is larger with condensing lenses than with a plane mirror; secondly, that when low powers are used to examine covered objects the images are produced by the reflection of the rays coming from the source of light, and falling obliquely upon the cover, backwards upon the object; but that, when high powers are used, those coming near to the object, the lenses themselves reflect the images on the object.

But the formation of images like those above described is not confined to opaque metallic globules, since all transparent substances of a globular figure are well known to have the property of reflecting the images of objects thrown upon them by a reflector. However, the explanation just given does not apply to transparent but only to opaque globules. Among transparent bodies I have particularly observed this fact in some minute spherical calcareous bodies which were found on the capillaries of the brain; also in globules of oil, provided they are not too much flattened; in air bubbles, starch granules, spherules of glass, &c. &c. There is one circumstance respecting all transparent globular objects worthy of notice, which is, the position of the image on the globule. If a globule be of a refractive power greater than that of the medium in which it is examined, the image of the source of light will be seen on its anterior surface, or a little in front of it: if these conditions are reversed it will be seen on its posterior surface, or a little behind it. Hence, if globules of oil be examined in water, the image of the stop of Gillett's condenser will be seen on their anterior surface, whilst, on the contrary, if globules of water be examined in oil, the image will be seen on their posterior surface. If a minute spherule of plate-glass be examined in water, the image of the stop will be seen on its anterior surface, but if the same spherule be examined in oil of cassia it will be seen behind it; whilst if the spherule be examined in Canada balsam, which has nearly the same refractive power as glass, no image will be visible.

This fact furnishes the means of distinguishing one body from another by its refractive power, and is particularly applicable to very minute particles. If, for example, there were suspended in water very minute spherules of calcareous matter, such as those I have mentioned, which look more like air than anything else, and globules of air, these would be readily distinguished by the position of the image of the stop, the former having the image on its anterior surface, the latter on its posterior.

As the distinctness of these images will depend upon the degree of transparency and homogeneousness of the bodies on which they are formed, and as their form will be very much influenced by the greater or less perfection of the spherical figure of the surface which reflects them, the strange appearances which, from this cause, may mask and disfigure the true characters of microscopic objects, will be almost endless. And it must be remembered that these defects are common to all kinds of illumination of transparent objects in some degree or other.

Notwithstanding these defects, from which I have shown that the most modern methods of illumination are not free, we have in the latest improvements the means, not only of rendering them of but little account, but of converting them into advantages as useful aids in microscopic analysis.

In the experiments on the globules of mercury it was observed that when a number of globules were in one field of view, an image of the stop of Gillett's condenser was seen on all of them. Now, it must be further observed, that as all these appearances are secondarily the reflection of only one magnified image of the stop, which can be seen by a low power, situated on a plane posterior to that on which objects are visible, and exactly in the axis of the microscope, only that globule whose axis coincides with that of this image can have upon it a perfectly symmetrical figure of the stop; upon all the others this image will be distorted, the degree of distortion being proportional to the remoteness of the globule from the axis of the microscope.

This applies equally to all objects, transparent as well as opaque. Hence we see, that all appearances of an object which are not the same when it is placed in the centre of the field of view as when it is placed near its margin are spurious. It must be remembered that the difference of focus, although never so slight, consequent on removing the object to different parts of the field, must be taken into consideration, and that when the object is removed from one part of the field to another each position requires a fresh focus.

I will give one example. If one of the pale shells of the guano be illuminated by Gillett's condenser, the smallest stop being under the lenses, and examined by a lens of one-eighth focus, it will present a reticulated appearance, the true nature of which is not very evident; however, as thus illuminated, the meshes will present different appearances, according to the focus at which they are seen, and these appearances will vary as the object is being removed from one part of the field to another, showing them to be the reflection of something situated in the axis of the microscope. If, now, the object is so focused that the dark spots in those meshes which are situated in the centre of the field is made as distinct as possible, and the condenser is rotated, the spots occupying these meshes will be seen to move, and be recognisable as the images of the stop of the diaphragm of the condenser.

The appearance, although much less distinct, will be precisely the same as that presented by the compound cornea of a very minute insect, when examined under the same circumstances. Hence the true structure of such shells as these is manifestly lenticular, that is, each space is filled up by transparent material in the form of a convex lens.

I might give many other examples, but this will probably suffice to show to what use the facts which I have mentioned may be applied. The use of Mr. Gillett's condenser in this kind of analysis might be extended, if something more characteristic than a stop were placed in one of the perforations of the diaphragm, as, for instance, a small cross.

I will now conclude this paper by some observations upon dark-ground illumination, as shown by Mr. Gillett's condenser, and Mr. Wenham's paraboloid.

If a globule of mercury, illuminated by Gillett's condenser, with one of the stops under the condensing lens, be examined by a very low power—a one or two inch lens—it will have the appearance of a dark disk surrounded by a circle of light, and if it be covered with thin glass, there will be an image of the stop at its centre, but not otherwise. In this case the object is seen on a dark ground, which is the magnified image of the stop interposed between the object and the light, and thus all the central rays of the illuminating pencil are cut off; and, as the rays which are thrown immediately upon it are considered to have a degree of obliquity given to them by the margin of the stop, too great to allow of their entering the microscope, the object is thought to be rendered visible only by the light which it radiates as if it were self-luminous. As this mode of illumination is precisely the same as that with the paraboloid, I will defer the further consideration of radiated light until the action of that instrument is explained.

For this purpose it will be necessary to repeat the examination of the globules of mercury, when illuminated by the paraboloid, first when uncovered, and afterwards when covered with a piece of thin glass, situated about 1-200th of an inch from the object. In the first case the globules will appear to be surrounded with a circle of light, in which the cross-bar contained in the tube of the paraboloid, or any other object reflected upon the tube by the mirror, can be seen. In the second case there will be two circles of light—an external one, which is the same as that just described, and an internal one; the latter is the image of the end of the paraboloid reflected upon the mercury by the glass cover. The cross-bar and other objects are also seen, as in the first case.

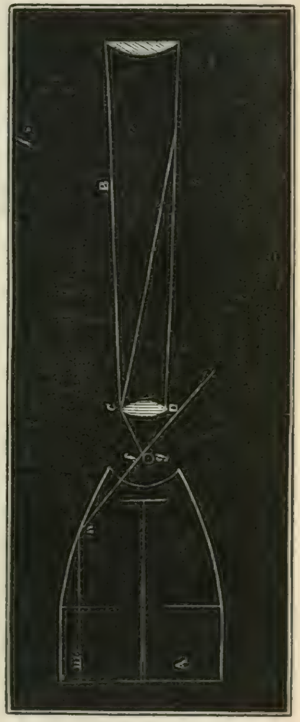
I may observe that in all these experiments I have not thought it necessary to notice the various appearances produced by the reflection of different parts of the microscope, as the extremity of the object-glasses, &c., upon the objects, as these can be easily recognised.

Now, according to the theory of the illumination of microscopic objects by radiated light, "all objects, either transparent or opaque (excepting white), absorb some of the rays of light falling upon them," and are rendered visible by the portion which they radiate. Hence Mr. Wenham observes, "that no rays from the source of light should enter the object-glass by passing through or around the object, which must be illuminated by very intense light, thrown on it, in all or in opposite directions, at an angle exceeding the aperture of the object-glass, so that the light which enters the microscope should be that which radiates only from the object as if it were self-luminous."

As a great part of the luminous appearance presented by the covered globule of mercury has been shown to be due to the light first reflected from the glass parabola upon the glass cover, and then by the latter upon the upper surface of the globule, that much of the appearance cannot be the effect of radiated light. The glass cover having in this instance served the purpose of a Lieberkühn, has made the object appear in a false light; and as nearly all microscopic examinations are made on covered objects, the paraboloid is in this respect defective, and the error must always be allowed for. It is true that all objects are not spherical, and, therefore, do not possess the property of reflecting perfect images, yet they will derive some portion of the rays by which they are illuminated from the reflection of the source of light upon them, in the same way as if they were globular.

With respect to the illumination of uncovered objects, experiment is equally at variance with a leading postulate of the radiated-light theory, which is, that no rays from the source of light passing around the object enter the object-glass; for if this were true, the image of objects reflected by a plane mirror up the tube of the paraboloid could not be seen by a two-inch lens of 12° angle of aperture.

This will be best understood by referring to the adjoining diagram, in which A is to represent the paraboloid, B a compound microscope, CD an object-glass of two-inch focus and 12° of angular aperture. Now, suppose (m, n) a ray of light coming from a part of the window-frame, or from close to the outer side of the cross-bar within the tube of the paraboloid, to be thrown upon the point (n) of the parabolic reflector, it will be so reflected as to pass through the focus (f), and continuing in the direction (n, f, g), it cannot, under the conditions specified, enter the microscope, and consequently the part from which it emanated would not be visible. But these are not the sole conditions: for, suppose a globule of mercury to be placed at *f*, then it is very easy to see how this same ray (*n*, *f*), by being reflected in the direction *f*, *c*, and making the angle of reflection equal to the angle of incidence, would pass through the object-glass, CD, and form an image of the point from which it had proceeded, according to the common laws of refraction; and it is in this way the object is seen. Now, as what is true with respect to this ray applies equally to every other ray thrown by the parabola upon the margin of the globule, we may conclude that it is the light which is reflected from its entire circumference which produces the appearance of the circle of light before described; and so also if there were any number of globules, each would be surrounded by a ring of light reflected from it in the same manner.



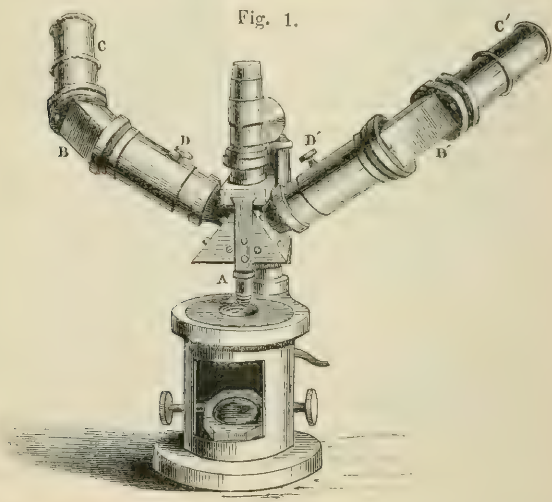
But if we suppose any other substance, possessing in a much inferior degree the property of reflecting light, to be situated in the place of the globule of mercury, it will be illuminated in the same way by the rays concentrated upon it by the parabolic reflector, which rays it will reflect also according to the same law; and such an object would no more be seen by radiated light than the picture of a portion of the window-frame on the upper surface of a globule of quicksilver would be. This explanation of the manner in which objects are seen as illuminated by the paraboloid, is equally applicable to the same objects when illuminated by Gillett's condenser, and examined by a low power. I may further add that this explanation is in accordance with the simplest laws of optics, that it agrees in every respect with experiment, and that it assumes no endowment of material substances with properties which they are not well known to possess.

On a Microscope *adapted for* Anatomical Demonstrations; *and on a* Binocular Microscope. By M. Nachet, of Paris.

All micrographers have felt the difficulty of defining the nature and situation of an object in the field of a microscope so as to make it apparent to several persons. I need not explain that this arises from the number of different objects visible at the same time, and often also from the moving of the objects themselves in fluids, particularly *infusoria*, which it is impossible to show in the same state in which one has observed them oneself.

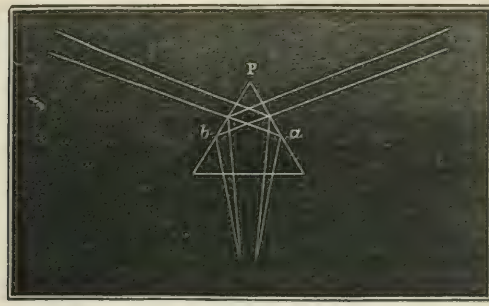
These difficulties are experienced still more in the demonstration of microscopical dissections; for the observer, to avoid losing considerable time, exhibits only the result of his operations, and not the method by which he arrived at it, though that is also very important. The problem then has been, how to enable two or more persons to observe at one time the same object. I believe I have resolved this difficulty by the construction of the instrument represented in fig. 1, with which

Fig. 1.



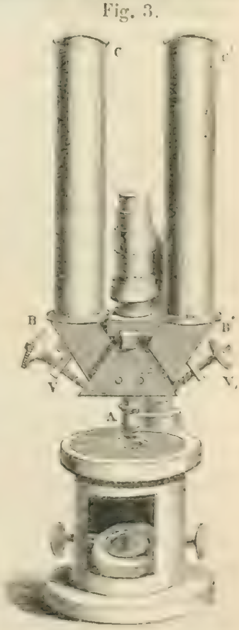
two persons looking through the eye-pieces, C C′, obtain each an erected image of the object; the division of the pencil of light formed by the objective, A, is accomplished by a prism, P, fig. 2, the section of which is an equilateral triangle; the image reflected upon the face *a* emerges normally upon the face *b*, that reflected upon *b* also emerges upon the face *a*; there is thus formed on the right and left of the instrument an image erected in a certain sense; if upon the transit of these rays the prisms, B B′ fig. 1, are placed, similarly to the separating prism, but so disposed that their reflecting surfaces are perpendicular to the central prism, the object will then be completely erected on each side, which is a considerable advantage for the demonstrating of dissections; moreover, the two observers can place themselves on the same side of a table,

Fig. 2.



for the eye-pieces are brought nearly into the same plane as regards the prisms B B′, and are placed at a very convenient inclination for avoiding fatigue to the head. If the two have not the same focus, one is first adjusted by the pinions and screws, and the other by drawing out and pushing in the tube carrying the oculars. If a vertical view be desired, the screw-heads D D′ have only to be turned and the prisms B B′ to be revolved a quarter round, to obtain an effect exactly opposite to the preceding one, that is to say, that the images are reversed as in an ordinary microscope, for the faces of B B′, fig. 1, becoming parallel to those of the central prism, destroy the first erection caused by it.

Fig. 3.



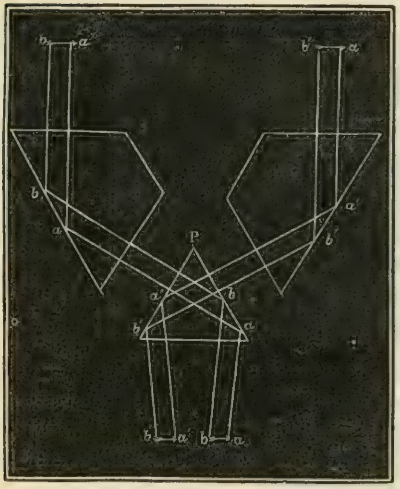
It will be seen that by leaving B inclined and by bringing B′ into the perpendicular, a person looking in C will have an erected image, whilst another looking vertically in C′ will have a reversed image.

The parallelism of the oculars C C′, when they are in a vertical plane, brings to mind Professor Riddell's valuable method of simplifying the construction of binocular microscopes, for if the prisms

B B′ are brought towards the central prism, as in fig. 3, the oculars C C′ are placed so as nearly to suit the distance between the eyes.

Fig. 4 shows sufficiently well the course of the rays, and requires no further explanation; I shall only remark that the rays, *a b a′ b′*, always emerging normally on the terminal faces, there is no chromatic dispersion; and the superior angle of the prism being very acute, and the surfaces perfectly even (which is not difficult of execution), the loss of light is almost inappreciable. There is a great advantage attached to this arrangement in the impossibility of pseudoscopic effects arising, as in Professor Riddell's last and definite arrangement, or in Mr. Wenham's very ingenious refracting prism. When the distance between the oculars requires to be modified, the screws V V′ are used for moving the lateral prisms nearer or further off at pleasure.

Fig. 4.



The phenomena of complementary colours produced by polarization can be examined with this instrument in the most simple manner; and with regard to crystals, it is certainly one of the most beautiful spectacles to behold a crystal in relief, appearing tinged with complementary colour to the single eye, while to binocular vision it becomes white, as if it were not seen under polarized light.

On the Ultimate Structure *and* Relations *of the* Malpighian Bodies *of the* Spleen *and of the* Tonsillar Follicles. By Thomas Huxley, F.R.S.

The first account of those peculiar whitish corpuscles, discovered by Malpighi and to be met with, more or less distinctly marked, in the spleen of every animal, which at all satisfies the requirements of modern anatomical science, was given by Professor Müller, in his Archiv. for 1834. Müller describes with great accuracy the mode in which these bodies are supplied by minute arteries, and explains that they are, in fact, outgrowths of the adventitious tunic of those arteries. He states that, by means of fine injections, he found that "the arterial twigs sometimes passed by the side of the Malpighian bodies without giving off any branches to them—sometimes went straight through the whole body or a part of it, in which case, however, no portion of the arteries terminated in them. These fine arterial twigs appear less to pass through the middle of the corpuscles than to run on their walls and then to leave them. When an arterial twig divides into many minute branches in the Malpighian body, which never takes place upon its surface, but always in the thickness of its walls, these arterioles pass out again to be distributed as very minute branches in the surrounding red pulp: in fact, the ultimate termination of all the finest penicellate arteries is in this red substance. From all this I have become convinced that the white bodies, as mere outgrowths of the *tunicæ adventitiæ*, have no relation with the finest ramifications of the arteries."

With regard to another important point,—whether the Malpighian bodies are hollow or solid—Professor Müller's statements are less definite. In the commencement of his article he affirms, in opposition to Malpighi and Rudolphi, that they are solid, but at the end he qualifies this opinion: "I was long of opinion that the white bodies are not hollow, but merely filled with a white pulpy substance, which might indeed be pressed out of them, but was not distinctly defined from the walls of the bodies. Further observations recently made, however, have instructed me that the white granular substance which is contained in the Malpighian bodies is too fluid, while on the other hand their walls are too solid, not to oblige us to regard them as a kind of vesicles with tolerably thick walls. The white clear fluid (breiige) matter which they contain consists for the most part of equal-sized corpuscles, which are about as large as the blood-corpuscles—not however flat, like these, but irregularly globular. These corpuscles present exactly the same microscopic appearance, and are of the same size, as the granules of which the red substance of the spleen is composed." Pp. 88, 89.

Although the Malpighian bodies have been the subject of frequent and repeated investigations since 1834, I think that more has been done to confuse than to improve the above (in its general outlines) very accurate account of their structure.

Giesker, in a work which I have not seen (Splenologie, 1835, cited by both Henle and Kölliker), appears to have been the first to diverge from Müller's views. He states that there is a delicate membrane investing the proper membranes of the

Malpighian bodies in which arterioles ramify—and thus the latter never enter the Malpighian bodies at all (Henle, Allg. Anat. p. 1000); and Kölliker, Gerlach, and Sanders (On the Structure of the Spleen, Annals of Anat. and Phys. 1850), agree with Giesker on the latter point.

In the meanwhile, however, Günsburg (Zur Kenntniss des Milz-gewebes, Müll. Arch. 1850) had confirmed and extended Müller's observations with regard to the distribution of the vessels in the Malpighian bodies. He says, p. 167, "Their framework is a vascular plexus. The larger vessels (*cylinder*) are longitudinally triated, in consequence of the regular arrangement of the nuclei upon their walls, the smaller are simple tubes." These observations were made on persons who died of cholera.

In January, 1851, Dr. Sanders read a paper 'On the connexion of the minute Arterial Twigs with the Malpighian Sacculi in the Spleen,' before the Edinburgh Physiological Society, in which he describes a peculiar method of preparation of the pig's spleen, whereby arterial twigs may be demonstrated "passing diametrically across the area of the sacculi." "Stains of blood also, often in linear arrangement, indicating capillaries, were seen in the interior of the sacculi." Kölliker ('Mik. Anat.' and 'Handbuch,' 1852), while denying the entrance of the arterial twigs into the Malpighian bodies, states that he had just succeeded in once observing a network of fine capillaries in those of a cat, and he supposes that they will hereafter be discovered in other animals. Finally, Mr. Wharton Jones speaks doubtfully of having observed a single capillary tube in the Malpighian bodies of the sheep. (On blood-corpuscle-holding cells.—Brit. and For. Med. Chir. Review, 1853). The existence of a special continuous membrane investing the Malpighian bodies is affirmed by Ecker, Gerlach, Kölliker, and Sanders. On the other hand, it is denied by Henle (Allg. Anat. 1001), and by Wharton Jones (l. c.)

With regard to the contents, Müller's statements, as we have seen, waver. Henle, Gerlach, Kölliker, and Sanders say that they are composed of corpuscles suspended in a fluid. The quantity of the latter is however, according to Kölliker, small.

I may now proceed to communicate the results of my own observations upon the structure of the Malpighian bodies in Man, the Sheep, Pig, Rat and Kitten, and I will arrange what I have to say under the three heads of—1. The distribution of the vessels of the Malpighian bodies. 2. The structure of their substance (so-called contents), or the Malpighian pulp. 3. The structure of their peripheral portion, or so-called 'walls.'

1. *The Distribution of the Vessels of the Malpighian Bodies.*

In all the animals above mentioned, I find it very easy to demonstrate, in almost every case, that one or more minute arterial twigs enter and frequently subdivide in the substance of the Malpighian body, making their exit on its opposite side, to terminate, finally, by breaking up into minute branches in the pulp. Indeed, it is so easy to convince oneself of this fact, if a thin section of a fresh spleen be examined under the simple microscope, that it is difficult to understand how two opinions can exist upon the subject. The method I have adopted is simply this: to such a section I add some weak syrup, so as to retain the colouring matter in the blood-corpuscles contained in the vessels, and thus to have the advantage of a natural injection; then, I either trace out the vessels into the Malpighian bodies with needles, under a $\frac{1}{4}$-inch lens; or, placing a glass plate over the section, I apply a gentle and gradual pressure, just sufficient to render the bodies transparent. It is then easy, by sliding the plate with a needle, to cause the bodies to roll a little upon their axes, and thus convince oneself, by the relative positions which the vessels and the bodies assume, that the former do really pass through, and not merely over, the latter. In Plate III. (figs. 1, 2, and 7) I have represented the ordinary modes in which the arterial twigs are disposed in the Malpighian bodies of the sheep (figs. 1, 2) and of Man (fig. 7). It should be observed, however, that the Malpighian bodies have by no means always the well-marked oval outline which is here represented. On the other hand they are very frequently diffuse and irregular, sending out processes along the efferent and afferent twigs.

The application of a high power, either to the compressed Malpighian body, or to one which has been torn out with needles and its vessels isolated, fully confirms the results obtained by the previous methods. In Man, the structure of the minute arterial twigs within the bodies does not differ from that which they possess elsewhere (fig. 7). Both the transversely (smooth-muscular) and longitudinally fibrous coats are well developed, neither being in excess; and the addition of acetic acid produces a clear line external to the former, representing the innermost portion of the *tunica adventitia*, which passes into, and is continuous with, the Malpighian pulp. The artery, therefore, is not only surrounded by, and in immediate contact with, the indifferent tissue of the pulp, but the latter, as Müller pointed out, is really the representative of a part of its *tunica adventitia*. In fact, the indifferent tissue so completely forms an integral constituent of the coat of the artery, that I could not, in any way, obtain the latter free from it.

In the Sheep, the arterial twigs have precisely the same relation to the Malpighian pulp, but the intimate structure of their walls is different, the circularly fibrous layer becoming almost obsolete, while the longitudinally fibrous coat acquires proportionally increased dimensions, and takes, at the same time, the structure of organic muscle. In the small arterial twigs of 1-800th inch in diameter, represented in fig. 3, the cavity of the vessels did not occupy more than one-third of their diameter, and, like the efferent ramuscules, unless they contained blood, they resembled mere *trabeculæ*, consisting of organic muscle.

The vessels within the Malpighian bodies are, however, not arterial ramifications only: I find that there invariably exists, in addition, a tolerably rich network of capillaries connecting the arterial ramuscules. These capillaries are vessels of 1-1000th to 1-3000th of an inch, or even less, in diameter, which can hardly be said to have parietes distinct from the surrounding indifferent tissue of the pulp (figs. 3 and 8); unless they are filled with blood, indeed, they are not distinguishable with certainty; and in the figures 2 and 7, I have, therefore, only represented those fragments of the capillary network in which blood corpuscles were clearly distinguishable, their colouring matter being retained by the syrup. After the addition of water, it is often impossible to recognize the capillaries at all; but using syrup, I have readily enough seen them in all the animals above mentioned.

It may then perhaps be fairly concluded that, in mammals, the Malpighian bodies are traversed by minute arteries, and contain, in addition, a network of capillaries.

2. *The Structure of the "Contents" or Pulp of the Malpighian Bodies.*

Almost all writers have agreed in stating that the interior of the Malpighian bodies is filled by a liquid, consisting, as Kölliker says, of a small quantity of fluid with a large proportion of corpuscles. However, I have been quite unable to convince myself of the existence of any fluid matter at all in the interior of the perfectly-fresh Malpighian bodies of any of the animals I have examined. On the other hand, the Malpighian pulp appears to me to be as solid as any other indifferent tissue, *e. g.*, that which constitutes the lowest layer of an epidermis or epithelium, or as the most superficial portion of any dermal structure. It is, indeed, like these, soft and capable of being crushed into a semifluid substance, which becomes diffused in any surrounding liquid, like mud in water; but that it is a soft solid and not a fluid, results, I think, from what I have stated with regard to the difficulty of completely detaching it from the arterial twigs.

The essential structure of the Malpighian pulp appears to me to be that of every other indifferent tissue which I have yet examined; it consists, in fact, of a homogeneous, transparent, structureless matrix, or periplast, containing closely-set rounded or polygonal vesicular endoplasts: these vary in diameter from less than 1-5000th inch up to 1-2500th, or a little more, and contain usually one to three, but frequently many, minute granules* (fig. 4). On the addition of acetic acid, the periplast often becomes granular and less transparent, while the endoplasts are rendered darker and more sharply defined, undergoing a certain wrinkling. There are neither cell cavities nor cell walls distinguishable around these endoplasts, and therefore the Malpighian pulp cannot be said to be composed of 'nucleated cells;' resembling, in this respect, all the primary, unmetamorphosed tissues with which I am acquainted.

True cells are, however, to be met with here and there in the Malpighian pulp. There is first to be observed a clear area, as of a cavity, surrounding an endoplast; the periplast forming the outer limit of this clear area then acquires a more distinct definition (fig. 5), and becomes recognizable as a cell-wall, from the remaining periplast. Such complete cells measure from 1-2500th to 1-1500th of an inch in diameter. A further change is undergone by the periplast within and around some of these cells; granules are deposited, which are sometimes minute and colourless, sometimes, on the other hand, have a deep-red colour and a considerable size, constituting the well-known 'pigment-globule-cells' of the spleen; but I may remark, that I have never been able to observe any blood corpuscles in such cells.

If the Malpighian pulp be pressed out or torn with needles, it is very readily broken up and diffused through the surrounding fluid. We then find in the latter free endoplasts—endoplasts surrounded by definite cell walls and cell cavities—and granule and pigment cells, corresponding with the elements which were observed in the uninjured tissue. That the free cells were not primarily independent structures, but have simply resulted from the breaking up of the periplast along its lines of least cohesion, is evidenced, in a very interesting

* These therefore correspond with the "nuclei" and "nucleoli" of authors. The reasons for not so denominating them are contained in an article 'On the Cell Theory' ('Brit. and For. Med. Chir. Review, October 1853'). I may observe that I know of no tissue better calculated to illustrate the view which I have there taken of Histogenesis, than the Malpighian pulp.

manner, by such forms as are represented in fig. 6, where two cells may be observed still connected by a bridge of periplastic (or as it would here be called, in the language of the cell theory, 'intercellular') substance, while the outline of a single isolated cell is still irregular and granular, from the adhesion of particles of the periplast of which it once formed a portion. Such bodies as these are quite undistinguishable, structurally, from pus, mucus, or colourless blood corpuscles.*

3. *The Peripheral portion, so-called "Wall," of the Malpighian Body.*

In the human spleen, the Malpighian bodies cannot be said with any propriety to possess walls. Their structure remains, as we have described it, up to their junction with the surrounding red pulp. At the line of junction, a somewhat more condensed tissue, which breaks up, like a great deal of the red pulp, into spindle-shaped bodies, and those fibres with one-sided endoplasts, described by Kölliker, may be found; but this tissue belongs as much to the red pulp as to the Malpighian body.

In the Sheep, on the other hand, I find, to quote Mr. Wharton Jones's words, that—

"Examined with a low magnifying power, the Malpighian corvesicles present the appearance of thick-walled, glandular vesicles, with contents. The thick walls are not defined and homogeneous, but are, on examination with a high power, found to be composed of nucleated fibres and nucleated corpuscles, similar to those of the red pulpy substance, between which, indeed, and the exterior surface of the Malpighian corpuscles there is no very distinct line of demarcation other than is produced by the condensation of the wall of the Malpighian corpuscles and the absence in them of coloration."

In addition to this, however, I find upon the exterior of the Malpighian bodies in the Sheep the mesh-work of pale fibres, (fig. 2, *d*,) like very young elastic tissue, or the fibres of the zonule of zinn, to which Kölliker and Sanders have referred; and I have occasionally met with such fibres in the interior of the bodies themselves, traversing the Malpighian pulp. They appear to me to belong to the original *tunica adventitia* of the arteries. The existence of any distinct structureless limitary

* The above account of the structure of the Malpighian bodies is essentially identical with that given by Mr. Wharton Jones, l. c. pp. 34, 35, but was drawn up before I had the good fortune to become acquainted with his article. He describes the wall of the nucleated cells as being "not very smooth," and the periplast as a "diffluent intercellular substance," whence I presume that I may quote him as an authority for the absence of fluid in the Malpighian bodies.

membrane may, I think, be very decidedly denied; and with regard to the "granular membrane, the internal surface of which is lined by a layer of large nucleated cells, while free nuclei or corpuscles, with a homogeneous or granular plasma, fill its interior" (Sanders, l. c., p. 35); all I can say is, that I cannot give any opinion as to what it may be, never having met with a Malpighian body presenting any such structures.

It may be said, then, that the Malpighian bodies of the mammalian spleen are not closed follicles, and have no analogy whatever to the acini of ordinary glands, but that they are portions of the spleen, everywhere continuous with the rest, but distinguished from it—*a*, by immediately surrounding, and as it were replacing, the *tunica adventitia* of the arteries; *b*, by containing no wide venous sinuses, but, at most, a network of delicate capillaries; and *c*, by being composed of absolutely indifferent tissue, *i. e.* of a structureless periplast with imbedded endoplasts—or of a tissue in which the periplast has undergone no further metamorphosis than that into cell-wall and rudimentary fibre.

For a demonstration that each of these propositions holds good of the Malpighian bodies in the other three classes of the Vertebrata, I must refer to Remak's very able essay, 'Ueber Pigment-kugel-hâltige Zellen,' in Müller's 'Archiv.' for 1852, and to Leydig's recent 'Untersuchungen über Fische und Reptilien,' in which ample evidence of the fact will be found; and my limits oblige me to allude, with equal brevity, to another important doctrine which many recent writers have maintained, but which is especially enunciated and illustrated by Leydig, namely—that there is no line of demarcation to be drawn between the spleen, the lymphatic glands, Payer's patches, and the glandulæ solitariæ, the supra-renal capsules, the thymus, and the pituitary body, but that these form one great class of glands characterized essentially by being masses of indifferent tissue contained in vascular plexuses, and which may therefore well retain their old name of Vascular Glands.

The primary form of these is represented by the solitary gland of the alimentary canal, which is nothing but a local hypertrophy of the indifferent element of the connective tissue of the part, and possesses no other capsule than that which necessarily results from its being surrounded by the latter.

A number of such bodies as these, in contiguity, constitute, if they be developed within a mucous membrane, a Payer's patch; if within the walls of the splenic artery and its ramifications, a spleen; if within the walls of lymphatics, a lymphatic gland; if in the neighbourhood or within the substance (as in Fishes) of the kidney, a supra-renal body; if in relation with a part of the brain, a pituitary body.* All these organs agree in possessing nothing that can be called a duct. To those, however, which are in relation with mucous membranes, Kölliker has already justly shown ('Handbuch' and 'Mikr. Anat.') that the 'follicular' glands of the root of the tongue and the tonsils must be added; the former of which possess rudimentary, and the latter a tolerably perfect, system of ducts, formed by diverticula of the mucous membrane, around which the elements of the vascular gland are arranged, though they are not directly connected with them. I can fully testify to the general accuracy of Kölliker's account of the structure of the tonsils; but I must add that I have been unable to find 'closed follicles' either in Man or in the Sheep; and, on the other hand, that the indifferent tissue of the so-called 'follicles' is permeated by a network of capillaries, which have exactly the same relation to the indifferent tissue in which they are imbedded, as in the Malpighian bodies (figs. 9, 10). So far as its structure is concerned, in fact, the tonsil exactly represents a lymphatic gland, developed around a diverticulum of the pharyngeal mucous membrane; its 'follicles' precisely resembling the 'alveoli' of the latter, in being constituted by imperfect septa of rudimentary connective tissue, containing a solid mass of indifferent tissue, traversed by capillaries.

Can this series of '*vascular glands with false ducts*,' as they might be called, be extended by any further addition? I venture to think that it may, and that no one can thoroughly comprehend the structure of the tonsils without perceiving, at once, that there is but a step from them to the liver. A mass of indifferent tissue contained in a vascular plexus and arranged around a diverticulum of mucous membrane, is a definition which would serve as well for the liver as for the tonsil; it is, further, perfectly in accordance with that theory of the relation of the biliary ducts to the hepatic substance, which is due to Dr. Handfield Jones, and which all recent researches, both anatomical and physiological, tend to confirm, viz., that the liver is essentially a double organ, consisting of two elements, an excretory and a parenchymatous, different homologically and functionally. It seems odd that, from being a sort of histological and physiological outcasts, the Vascular Glands should turn out, if this view be correct, to be the most important and extensive class of organs in the whole body, claiming *the* gland *par excellence*, the liver—as one of their family.

* I purposely abstain from including in this series the thyroid and pineal glands, because I think it certain that the former, and probably, the latter, have a different import.

On the Manufacture *of* Large Available Crystals *of* Sulphate *of* Iodo-Quinine *(Herapathite)*, *for* Optical Purposes *as* Artificial Tourmalines. By William Bird Herapath, M.D., Bristol.

Having been repeatedly applied to by various parties for the details of my process for the manufacture of these useful crystals, I have been induced to enter into numerous experiments to obtain greater certainty in the results, and to study the conditions necessary for the production of broad foliaceous plates. Permit me to make the formula known to science, together with the precautions necessary for adoption to secure the crystals when obtained, and to mount them so as to be available as polarisers or analysers for the microscope, or even to enable us to perform all the experiments in the polariscope.

The success which I have obtained is so great, that there is no doubt tourmalines and Nichol's prisms will be soon completely superseded by these new crystals, since the scarcity of the one and the difficulty in manufacturing the others render them very costly apparatus. But a little practice in the following process will soon enable any one to make them large enough for every purpose; and so superior are they in power to the best tourmaline, *that two plates scarcely thicker than gold-leaf* may (by a slight modification of my formerly published method) be rendered *totally* impervious to light when they are crossed at right angles.

[Herewith are enclosed two marvellously thin plates of considerable size; one being six-tenths of an inch long and three-tenths of an inch broad, the other the same length but one-tenth of an inch broader. Upon crossing them you will perceive that they are optically perfect as polarisers, and as useful as plates of tourmaline for which you would be charged four guineas each plate. I have succeeded in getting much larger ones by the same process and equally good.*]

The materials employed are the same as before, the chief modification being in the proportions of the ingredients, and the care taken in the method of crystallization.

It is necessary to procure pure disulphate of quinine, and for this purpose none approaches so thoroughly to the standard of absolute purity as that manufactnred by Messrs. Howard and Kent.

I dissolve it in pyroligneous acid, having a specific gravity of 1·042, and dilute the solution with an equal quantity of

* We have examined Dr. Herapath's crystals, and have no hesitation in pronouncing them equal to any polarising arrangement we have ever employed.—Eds., Mic. Journ.

proof-spirit, made by adding rectified spirit of wine, spec. grav. 0·837, to equal bulks of distilled water.

The spirituous solution of iodine is made by dissolving 40 grains of iodine in 1 fluid ounce of rectified spirit of wine.

I can, after these explanations, give the formula.

Take of disulphate of quinine 50 grains,
,, ,, pyroligneous acid, 2 fluid ounces,
,, ,, proof-spirit, 2 fluid ounces,
,, ,, spirituous solution of iodine, 50 drops;

dissolve the disulphate of quinine in the pyroligneous acid mixed with the spirit; warm the solution to 130° Fahr., and directly add the solution of iodine by drops, agitating the mixture from time to time.

This formula gives to the mother-liquid, after crystallization at 52° Fahr., a specific gravity of 0·986, which appears highly favourable to the deposition of the majority of the crystalline production, and yet allows only the very broad and thinner plates to float—thus getting them *perfectly free* from all interfering and adhering plates.

It is necessary to perform this operation in a *wide-mouthed* Florence flask or matrass, and to take care that the temperature is maintained for a little time after the addition of the iodine, so that the solution should become perfectly clear, dark, sherry-wine colour; then set it aside to crystallize, under the following conditions:—

1st. It is essential that the apartment should be tolerably equable in temperature, about 45° or 50° Fahr., as a slight variation in the temperature produces currents in the crystallizing fluid which destroy the parallelism of the crystals, and of course negative all the efficiency of the manufacture; and a greater rise, if only to 60° Fahr., redissolves the thinner plates.

2nd. It is equally necessary that the liquid should be kept in a perfect state of repose during the whole act of crystallization—even the *common vibration* of the apartment must be counteracted, for the same important reason as the last.

The best method to adopt is one which my friend Mr. John Thwaites employs, namely, to suspend the flask by the neck with strong twine, and attach this to a similar string stretching across from one wall of the apartment to the other. This certainly gives the most uniform results, and offers other advantages.

The plan I had usually employed was to set aside the flask on the steadiest support to be found, a wall, pillar, or table; and imbed it on a feather, cotton, or tow pillow, to act as a non-conductor, and at the same time destroy vibration.

3rd. It is also necessary that the surface of the fluid should not be exposed to too rapid evaporation, as the temperature would fall too quickly, and various currents and intestinal motions would result; therefore the flask or matrass answers better than the evaporating dish.

4th. It is also decidedly an advantage to have a broad surface in proportion to the depth of liquid; the reason being, that the thinner and most easily reached plates *form on the surface*, and float there until the time arrives to remove them; and the greater the surface, the more numerous are the plates.

5th. These broad plates are not always formed; but if after six hours none make their appearance, it is merely necessary to apply a spirit-lamp to the bottom of the flask and warm the liquid to dissolve all the deposited crystals, then add a little spirit and a few more drops of iodine solution, and again wait for crystallization.

6th. Supposing that we obtain a crop of these broad floating plates, which generally occurs under the aforementioned conditions, we permit them to remain from twelve to twenty-four hours to complete their disc and fill up all crevices, &c., and to attain a sufficient degree of thickness; for if too thin, they do not stop the red or purple-violet rays, as Haidinger has beautifully shown and admirably explained (*vide* Phil. Mag., Oct. 1853, and Poggendorff's *Annalen*, for June last). If the crystals are allowed to remain too long in their mother-liquid, we run the risk of loss and injury; for after some time a dissolving or disintegrating action appears to occur, and considerable disappointment is occasioned. I have lost several batches of beautiful and magnificent plates from inability to secure them at the nick of time.

Having by these means obtained the object of our best wishes, it now remains to secure the prize. This requires a little patience and a tolerable amount of care; the following plan is the most ready, and requires but little practice and a steady hand to insure success.

The first stage of the process is to procure a table as near as possible to the crystallizing spot, furnished with the following apparatus:—

1. A gallipot or small mortar, to hold the flask on as a support.
2. A supply of perfectly clean circular glass discs, small enough to pass down the neck of the flask with ease.
3. A glass rod of sufficient length to descend to the bottom of the flask, if necessary.
4. A little marine glue or sealing-wax.
5. A spirit-lamp and matches.
6. A quantity of blotting-paper cut in strips about an inch

wide and two inches long, and also a folded sheet of the same to act as a pad or support.

Now remove the flask with the greatest amount of care from its attachment to the horizontal string; this is best done by holding the perpendicular twine in the left finger and thumb, at the same time cutting the upper end of it with a pair of scissors to avoid all disturbance. It will now swing easily and steadily, and may be carried and gently deposited upon its gallipot support. Then attach the edge of one of the circular glass discs to the end of the glass rod by a little of the wax or marine glue, and let it, when cold, be carried *flatly* down the neck of the flask, which should be very gently inclined, as nearly horizontally as possible, to admit of this being easily accomplished. Having selected the largest crystalline plate, pass the glass circle gently beneath it, raise the plate by depressing the hand, and the little crystalline gem is at once caught on its surface.

If this operation be neatly accomplished, it appears spread out as a thin uniformly-coloured film upon the glass; if any black patches appear, they are occasioned by the accidental crossing of some interposed crystals, or from some on the under surface of the glass disc. These last must be at once wiped off by the blotting-paper, the others will sometimes float out upon raising the edge of the glass disc to a perpendicular position; if they are near the edge, they may sometimes be gently drawn out from under the large crystal by a little dexterity on the part of the operator; frequently there are no such precautions necessary.

Now rapidly dry the plate by imbibing all the fluid most scrupulously by blotting-paper. This must be done without *touching* the crystalline surface, for the least contact destroys its beauty, symmetrical arrangement, and optical usefulness. Having done so, let it dry by exposure to the air in a cool room, say at 40° to 50°; this is to prevent resolution and disintegration of the crystal in its own mother-water, a little of which must remain attached after all our care.

It is sometimes necessary to float them on, or dip them *for an instant* only in a little *cold distilled water*, somewhat imbued with iodine. This serves two purposes; it removes all mother-liquid, and prevents crystals of sulphate of quinine subsequently forming and interfering with the perfect polarization of the new tourmaline, as every crystal of this substance interposed between the plates would, of course, rotate the polarized beam as far as its influence extended, and *depolarize it.* The iodine acts also in preventing the solution of the new crystals in the water. They must again be dried by imbibition and by exposure to air, as before, and then placed under a cupping-glass, having a watch-glass, with a few drops of tincture of iodine in it. This gives a decidedly black tone to the field; and, if the crystal were before too thin to obstruct all the light, and thus give a red or purplish violet-tint, its power of polarization will be very materially improved by following the above simple directions.

It is essential in iodizing the plate that the exposure to the vapour be not too long continued; the time necessary will, of course, depend on the temperature of the apartment; about three hours at 50° Fahr., being generally necessary. The reason of this precaution will be at once evident upon making the experiment, for the crystals assume a rich golden yellow colour, both by reflected and transmitted light; the field will, therefore, when the two plates are *parallel*, be intensely yellow, a most objectionable colour for the examination of objects. The crystals have lost the power of stopping the yellow rays, and the complementary relation of the body to the superficial colours appears to be lost also—a very remarkable fact.

The rationale of the periodizing process appears to be the addition of as much iodine to the crystal as will be sufficient to communicate the exact complement of yellow to neutralize the red and blue rays of the purple "body-colour." These rays are now absorbed by the plates as they would be by yellow glass.* If too much iodine be added, an intense yellow light becomes transmissible when the crystals are parallel, and the plate becomes rotten and brittle, and will be almost certainly destroyed in mounting, even if it be exposed to the air for some time before attempting to do so; by which process the superadded iodine again volatilizes, clearly showing that no chemical union could have existed.

Having so far prepared the "artificial tourmalines," it merely remains to cover them by another plate of thin microscopic glass, interposing some highly refractive cement or varnish between the two plates.

Several cements offer themselves to our notice, but some selection is necessary. Canada balsam is one of the best: however, in using this it is necessary to have it very fluid, and not to employ much heat in the process; in fact, I believe it best to use it so as to be fluid at the ordinary temperature. I have found, however, that it appears to attack the crystals and dissolve out the iodine. In order to correct this destruc-

* I have since found that yellow glass has no effect in absorbing these red or violet-red rays—the only absorptive media which I have found possess any power in stopping them, are, copper solutions, glass charged with copper, or a thin crystal of sulphate of copper.

tive tendency, it is essential to saturate the fluid Canada with iodine, at the ordinary temperature. This is best done by warming some small quantity of the balsam in a test-tube or thin bottle, and dropping into it some crystals of iodine, agitating them well together by a glass rod: giving time to cool, and the excess of iodine to subside, it is fit for use. It is merely necessary to take a small drop of this fluid on the end of a glass rod, place it on the larger and clean glass circular disc, then invert the disc, carrying the crystal upon it, press the two together gently and steadily with the finger or a glass rod, or piece of stick, taking great care not to use much force, in case the circles or crystal may sustain injury. Now remove all the extra Canada balsam from around the edge, and expose the little apparatus to the air, so that the balsam may become dry; it is then fit to mount in the brasswork of the microscope in the same manner as a tourmaline.

I have found it best to employ an æthereal solution of Canada balsam in this process, made by dissolving the hard old balsam in washed pure sulphuric æther, afterwards adding a little iodine to it as before. This dries more rapidly, hardens quicker, and more perfectly than the usual fluid Canada, and it does not attack the crystals—a very great advantage.

These directions may appear very prolix, tedious, and excessively troublesome; but, however, when set in practice, the whole operation resolves itself into the utmost simplicity; habit soon reconciling oneself to the routine, and the different precautions appear to offer themselves unconsciously to us as we proceed. I have frequently prepared a dozen good tourmalines in an hour, as far as the catching and drying part of the operation; the others, of course, require longer time, but for these we must wait, and occupy ourselves with some other stages of the same process.

When it is absolutely necessary to obtain a *perfectly black* field with a total stoppage of all the incident rays (upon "crossing" the two crystals), it is much the better plan to employ a thicker plate of this substance: such a crystal will be generally found in the flask at the bottom of the mother-fluid. There is more trouble requisite in obtaining perfect plates, free from all intervening crystals, but the experimenter is generally repaid in the end by the perfection of the polarizing medium.

When the selenite stage is employed, the thinner and violet-coloured crystals are far preferable to those which give a black tone to the field; as the colours are more brilliant and the flood of transmitted light much greater, so that we are enabled to use a less illuminating power. I am not in the habit of using an achromatic condenser with my polarizing apparatus, which probably accounts for some discrepancies in the results of observations made by different experimenters upon the same crystalline plates: those crystals which will transmit the violet rays when strongly illuminated will not do so when the instrument is used in daylight, or with a plane instead of a concave mirror, and without the achromatic condenser.

If it be necessary to obtain a most decidedly black field, the violet rays may be readily absorbed by interposing a thin plate of sulphate of copper beneath the polarizing plate of Herapathite and the source of illuminating power.

The author has recently employed a plate of this substance, 1-20th of an inch thick, cut on a hone, polished and mounted between two plates of thin glass in Canada balsam, as a means of correcting the defects of the thinner plates of his new tourmalines*—this substance possessing the power of absorbing the violet rays of the spectrum in a pre-eminent degree. In order to succeed in this experiment it is necessary that the sulphate of copper should be inclined at a certain angle to the plane of primitive polarization, as it is a substance possessing two neutral axes or planes of no-depolarizing power; the position of which may be easily found, and their direction marked upon the support, so that the intervening plate may be always inserted at the angle of its greatest activity.

Professor Stokes has lately, in a letter to me, suggested the employment of a glass laden with the oxide of copper as a means of attaining the same end: having, therefore, prepared a boracic glass, coloured by the black oxide of copper, I have used it effectually as an absorbent medium for counteracting the violet-red colour of the polarized beam. But although it offers great and manifest advantages when the new tourmalines are crossed at right angles, yet, upon revolving the superior crystal, and therefore bringing the two plates into a parallel position, we have a blue colour in the field, which must assuredly alter the colours of depolarizing media: it is, however, a very agreeable light to work by, as the intense yellow of gas-light is much mellowed down and counteracted by it. This corrective medium would be inadmissible when the selenite stage is employed, as the tints would be materially changed by its absorptive agency.

The mode of making this glass is simply to dry powdered biborate of soda in a crucible by the heat of an ordinary fire; again reduce the effloresced mass to powder, and mix it with

* A solution of the sulphate or nitrate of copper in water will equally succeed in producing a black field.

a small quantity of the oxide of copper, such as is generally used in organic analysis, then introduce the mixture into a platinum crucible, and with a steady, long-continued heat, thoroughly vitrify it, pour it out upon a flat slate, clean metallic, or Wedgwood-ware surface, and press it while still soft into a flattened plate. Upon cooling, a portion must be quickly ground down on a hone, polished, and then mounted in Canada balsam between glass: the unmounted boracic glass may be kept for any length of time in turpentine without change, but in the air it effloresces, and becomes opaque and useless.

There is not the least doubt that, before long, these splendid and useful crystals will be offered for sale by opticians at as many shillings as tourmalines now cost pounds, and certainly of equal value and practical utility—in my own opinion, of even greater, for less light is lost by these than by any of our polarizing apparatus at present in use.

I have invariably used, in this description, the original terms employed by me, namely, "artificial tourmalines," and "crystals of sulphate of iodo-quinine." Professor Haidinger's term of "Herapathite" is certainly a highly complimentary one to myself; but as it does not give either an idea as to the optical properties or chemical characters of the substance in question, it does not appear to me so suitable as those I originally attached to them.

Notice of the New Forms and Varieties of Known Forms occurring in the DIATOMACEOUS EARTH *of* MULL; *with Remarks on the* CLASSIFICATION *of the* DIATOMACEÆ. By WILLIAM GREGORY, M.D., F.R.S.E., Professor of Chemistry in the University of Edinburgh.

THE two notices which have already appeared in the Journal have made known the occurrences, in this deposit, of about ninety distinct forms, of which two were noted as being new to science. But the continued and diligent examination of it, which I have carried on during my residence abroad last summer, has led to results so much more remarkable, that I have to beg of the readers of the 'Journal' to regard those papers as merely introductory to a more satisfactory and complete account of this interesting deposit. This I shall now attempt to give; but the limits of this paper, and of the illustrative plate, will not allow me to complete it at this time, and a large portion of my materials must therefore be reserved for a subsequent number of the 'Journal.'

My observations have been made on a very large number of excellent slides; and the method of search which I have found to answer best is the following, which I briefly notice here, because, by a slight alteration in the usual mode of keeping notes of what is seen, it is easy afterwards to find any required object, by the help of these notes alone:—

I always begin, then, the examination of a slide at the right-hand side, and carry it on by successive vertical sweeps, the first and all the odd numbers being downward sweeps (apparently, really upward), and the even numbers upward ones. I find this a great help to the memory. If in the first sweep I notice nothing remarkable, it is simply recorded thus | 1 |. If I see in it a peculiar or very fine specimen, I note this after the number, prefixing certain abbreviated signs to show whether it be above, or below, or near the middle horizontal line, or near the top or bottom of the sweep. Thus, | 1. B m, *P. latestriata* S. V. fine. N b, *S. Craticula* | signifies that in sweep 1, below the middle line (really, not apparently), I find a fine S.V. of the new *Pinnularia*, and near the bottom, a *Surirella Craticula.* Having made the first sweep, I now shift to the second, and the extent of shift is, as nearly as possible, half a diameter of the field. I use for searching, either Ross's 1-4th or Smith and Beck's 1-5th, with the 2nd eye-piece. This high power is necessary on account of the numerous small forms; of course I note the object-glass used, or by means of the draw-tube make the field of the 1-4th equal to that of the 1-5th. I go on in this way over the whole slide, noting every remarkable form, and as the number of sweeps varies, according to the diameter of the cover, from 60 to 80 and upwards, it is easy to see that the exploration of a full slide is a matter of considerable time and labour. But the notes, being once made as above recommended, serve not only as a record of the remarkable contents of the slide, but as a means of finding any object. If the object be in a sweep near the right side, say in 4, I make four shifts from the side onwards, and move up or down according to the record, and am sure to find the object instantly. If it be further on, I look for some very conspicuous object, such as a fine *P. alpina*, &c., not far from it (with a low power), and, replacing the high power, count the shifts from that; or with the high power I take the first striking object in that part of the slide, and, referring to the notes, use it as a point of departure. I may, however, in every case, count from the beginning or end of the sweeps, that is, from the right or left sides of the slide, and if the shifts have been well made, the object is always very soon found; and if not, it must be very near, on one side or the other, and by trying a little, it is sure to appear.

There is really no more trouble in keeping the notes in the above-described way than in any other. But in the case of any very remarkable form, which requires further examination, I move to the nearest edge of the circle, and note carefully any marks there, by which I can find the object in a moment, at any time. If there are no marks there, I look on the opposite side, or on one of the two other sides, and if none have marks naturally, which very rarely happens, I place a spot of ink, and note it. In this way my notes serve as finders, without the annoyance, incidental to the finders recently proposed, of having continually to change the object-glass. At the same time, the application of a scale to one side of the slide, as recommended in a recent number of the 'Journal,' answers well for finding single objects, although its use is too troublesome to allow it to be employed in a case like the present, where hundreds of forms have to be marked.

I have just said that the exploration of a single slide demands both time and labour to no small extent; and I must add, that a single exploration, however careful, is never sufficient. A second or a third will invariably detect interesting or even new forms, overlooked on the first, as I have very often experienced.

And this leads me to remark, that the results hitherto obtained from a careful exploration of the Mull deposit have been such as to convince me that none of the known deposits have yet been fully investigated. Indeed, few are willing to devote to them the time and labour necessary for this purpose. At the suggestion of the Rev. Mr. Smith, therefore, I propose to examine all the fossil Diatomaceous deposits I can procure, which I the more readily undertake because, being lame, and unable to walk far, I cannot attempt the collection, personally, of living species. I beg therefore to mention that I shall feel extremely grateful to observers for any portions of such deposits, from any part of the world, which they can spare for examination; and that I shall be happy to supply them with the Mull deposit, which it will soon, I fear, be impossible to obtain in situ, as I understand a great part of it has been removed in the course of agricultural improvements, and employed as manure. I have, fortunately, sufficient for microscopical purposes.

I proceed now, in the first place, to lay before the reader a list, corrected to the end of November, 1853, of the known forms which I have detected in the Mull deposit. In the second place, I shall briefly describe some of the new forms which have occurred, leaving the remainder for the next part of this paper; and thirdly, I shall notice certain striking varieties of known and figured forms, in which the deposit is remarkably rich. I shall conclude with remarks on the classification and nomenclature of the Diatomaceæ, on which subject the study of this deposit promises to throw much light. As the first volume of the 'Synopsis' of the Rev. Mr. Smith is, or ought to be, in the hands of every student of the Diatomaceæ, I shall adopt the names employed in that work, in order to facilitate reference. The 'Synopsis' is the only work on the subject, so far as I have yet seen, in which the figures are really calculated to assist the observer. In organisms, such as the Diatomes, in which the markings constitute essential characters, and in which, also, the number of forms having a great general resemblance, and differing only in small but important particulars, is very great, nothing short of the utmost attainable accuracy in the figures is of the smallest value. The attempt to find one's way through the labyrinthine mass of Diatomaceous forms, in the absence of actual specimens of all the described forms, by the help of the kind of figures often given, is an utterly hopeless one. Such figures actually tend to confuse the young observer. But the beautiful figures of Mr. T. West, in the 'Synopsis,' as I can testify from ample experience, are precisely such as the student requires for his guidance. They combine minute accuracy in form and markings with a very remarkable and very rare quality, that, namely, of presenting to the eye the true general aspect or character of the forms, a point of the utmost importance, because many species, and even genera, are easily recognized by their aspect alone. There is nothing at all to be compared to these figures, for practical utility, anywhere to be found: on the contrary, in some works, not only are the markings inaccurate, or altogether omitted (evidently because inferior objectives have been used), but the character or general aspect of the surface is often so entirely missed that the reader fails to recognize forms with which he is familiar. Although the 'Synopsis' is not yet completed, it fortunately happens that most of the genera occurring in the Mull deposit are treated of in the volume already published; and of course, where I can refer to figures so accurate, it is unnecessary to figure the species about to be enumerated. I shall only, therefore, give figures of such forms as are new or newly distinguished, or such as exhibit important varieties not figured in the 'Synopsis.'

The following is the list of forms observed and identified with species figured or to be figured in the 'Synopsis,' and in the order of that work. Those marked with *a* are so abundant as to be characteristic of the deposit; *f* is attached to such as, although less abundant, are frequent, and occur in every slide; *r* indicates such as are less frequent, or perhaps rather scarce, but may usually be found; while *rr* denotes that the form is hitherto of extreme rarity in the deposit.

1. Epithemia turgida, *f*
2. ,, Zebra, *r*
3. ,, Argus, *r*
4. ,, ocellata, *rr*
5. ,, rupestris, *r*
6. ,, gibba, *f*
7. ,, ventricosa, *rr*
8. Eunotia gracilis, *f*
9. ,, triodon, *r*
10. ,, tetraodon, *a*
11. ,, Diadema, *a*
12. ,, bigibba? *a*
13. Cymbella Ehrenbergii, *rr*
14. ,, cuspidata, *f*
15. ,, affinis, *f*
16. ,, maculata, *a*
17. ,, Helvetica, *a*
18. ,, Scotica, *a*
19. Amphora ovalis, *r*
20. Cocconeis Placentula, *f*
21. ,, flexella (Thwaitesii), *f*
22. Coscinodiscus excentricus, *rr*
23. Cyclotella Kützingiana, *f*
24. ,, Rotula, *rr*
25. ,, antiqua, *r*
26. Surirella biseriata, *f*
27. ,, linearis, *f*
28. ,, splendida, *rr*
29. ,, nobilis, *rr*
30. ,, Craticula, *rr*
31. ,, minuta, *rr*
32. ,, ovata, *rr*
33. ,, constricta, *rr*
34. ,, Brightwellii, *r*
35. Tryblionella marginata, *rr*
36. ,, angusta, *f*
37. Cymatopleura apiculata, *f*
38. ,, Solea, *rr*
39. ,, elliptica, *r*
40. Nitzschia Amphioxys, *r*
41. ,, sigmoidea, *r*
42. ,, Sigma, *rr*
43. ,, linearis, *f*
44. ,, minutissima.
45. Navicula rhomboides, *a*
46. ,, serians, *a*
47. ,, dicephala, *f*
48. ,, affinis, *f*
49. ,, ovalis, *f*
50. ,, firma, *r*
51. ,, gibberula, *r*
52. Navicula angustata, *r*
53. ,, obtusa, *rr*
54. ,, Semen, *rr*
55. ,, Crassinervia, *r*
56. ,, tumida, *rr*
57. ,, pusilla, *rr*
58. ,, inflata, *rr*
59. Pinnularia major, *a*
60. ,, viridis, *a*
61. ,, acuminata, *a*
62. ,, oblonga, *a*
63. ,, divergens, *a*
64. ,, acuta, *a*
65. ,, interrupta, *a*
66. ,, Tabellaria, *f*
67. ,, mesolepta, *f*
68. ,, nobilis, *f*
69. ,, gibba, *f*
70. ,, lata, *f*
71. ,, alpina, *f*
72. ,, radiosa, *r*
73. ,, viridula, *r*
74. ,, gracilis, *r*
75. ,, cardinalis, *rr*
76. ,, stauroneiformis, *f*
77. Stauroneis Phœnicenteron, *a*
78. ,, gracilis, *a*
79. ,, anceps, *f*
80. ,, dilatata, *rr*
81. ,, acuta, *rr*
82. ,, linearis, *r*
83. Pleurosigma attenuatum, *r*
84. Synedra biceps, *a*
85. ,, ,, *var.* β, recta, *f*
86. ,, radians, *f*
87. ,, Ulna, *r*
88. ,, capitata, *f*
89. ,, Vancheriæ, ? *r*
90. ,, delicatissima, *rr*
91. Cocconema lanceolatum, *r*
92. ,, cymbiforme, *f*
93. ,, Cistula, *f*
94. ,, parvum, *f*
95. Gomphonema coronatum, *a*
96. ,, acuminatum, *f*
97. ,, tenellum, *f*
98. ,, dichotomum, *f*
99. ,, capitatum, *f*
100. ,, constrictum, *f*
101. ,, Vibrio, *f*
102. Himantidium Arcus, *a*

103. Himantidium majus, *a*
104. ,, pectinale, *a*
105. ,, bidens, *a*
106. ,, undulatum, *a*
107. ,, gracile, *a*
108. Fragilaria capucina, *f*
109. Odontidium Tabellaria, *rr*
110. Denticula tenuis (?), *r*
111. Tetracyclus lacustris, *rr*
112. Tabellaria fenestrata, *a*
113. ,, ventricosa, *f*
114. ,, flocculosa, *f*
115. Diatoma vulgare, *r*
116. Melosira varians, *r*
117. ,, arenaria, *rr*
118. Orthosira nivalis, *a*
119. ,, aurichalcea, *a*

The predominance of the forms marked *a*, and the frequency of most of those marked *f*, give its peculiar character to this deposit. Perhaps the group of the *Himantidia*, all of which are most abundant, constitutes the most striking feature. But several *Pinnulariæ*, the two first-named *Naviculæ*, several *Eunotiæ*, the *Tabellariæ*, the *Orthosiræ*, *Synedra biceps*, and *Gomphonema coronatum*, are all very prominent. Of the *Pinnulariæ*, *P. major* is found in very fine specimens and of great frequency, but *P. divergens* (?) is the most abundant, and exhibits numerous and interesting varieties. In some portions of the earth very fine specimens of *P. lata* and *P. alpina* occur frequently. I am not aware that the latter beautiful species, which is rare in the living state, has yet been observed in any other deposit. Some specimens of it, as well as of *P. lata*, attain to twice the length of those figured in the 'Synopsis.'

Having now noticed those forms which agree with the species in the 'Synopsis,' I have next to mention those which, although known abroad, are not given as British species in that work, and such as appear to be altogether new. As most of them, so far as they are now to be mentioned, are figured on the accompanying plate (Pl. IV.), perhaps the best way will be to notice them briefly in the order in which they occur in the plate, being that of the 'Synopsis.' I should add, that several of the figures are also varieties of forms already mentioned.

Fig. 1. This is apparently a modification of *Epithemia Argus*, and calls for no special remark.

Fig. 2. This represents a valve of an *Epithemia*, not very rare in the deposit. It differs both in form, aspect, and markings from *E. rupestris*, when we compare the same parts, and I am disposed to refer it to *E. gibberula*, Ehr., so far as I can judge from the separate valves, which have not yet occurred united. 120, *r*.

Fig. 3. These figures show a few of the forms of *Eunotia bigibba?* which I have introduced into the foregoing list because I believe Mr. Smith now admits it as a distinct form. It varies most remarkably in length and in the form and proportions of the dorsal prominences, which in some cases seem to be blended into one; but, in all its variations, the square

apices are constant, and at once distinguish it from *Himantidium bidens*. (Pl. IV., fig. 20.) In some individuals, there are two ventral as well as two dorsal prominences. (I use the terms dorsal and ventral merely to designate the convex and concave sides.)

Fig. 4. These figures represent the new form, which Mr. Smith, to whom I pointed it out, has named *Eunotia incisa*. One shows the more frequent, probably the typical form, with acute apices; another, the variety β, with rounded apices, which is also broader; and a third exhibits an intermediate form, with one acute and one obtuse apex. All have the notches, close to the terminal puncta, from which the specific name is taken. The striæ are fine, 44 in ·001″, and require good object-glasses, with careful adjustment, to render them visible, especially if in balsam. Those on β, however, are more readily seen from its breadth. I find this form, in balsam, an excellent test-object for ascertaining the adjustment and the general performance of an object-glass. Ross's 1-4th and Smith and Beck's 1-5th, if duly adjusted, bring out the striæ perfectly, even without the condenser. I see that this species resembles in form *Himantidium Veneris*, Kütz; but the latter, as figured, has no trace of the notches, nor of striæ, and Professor Kützing, to whom I sent a portion of the deposit, regards mine as a new form. This species must be added to those above named as being most abundant, and characteristic of the deposit. I have detected it in a deposit labelled from the banks of the Spey, in that of Lillhaggasön in Lapland, and in that of the Lüneburg heath; but it has not yet been observed in the living state. (I may state here that the two last-named deposits, although widely distant in locality, exhibit a perfect agreement in the nature and relative proportions of the species. I shall take an early opportunity of describing a form which I find in both, but of which I can see no mention in any work on the subject.) 121, *a*.

Fig. 5. The next form is either a *Cymbella* or a *Cocconema*, these genera, when fossil, being undistinguishable, if indeed they are really distinct. It is abundant in the deposit, and the reader, on comparing it with the figures of *Cymbellæ* and *Cocconemata* in the 'Synopsis,' all of which occur along with it, will see that it differs from all. It approaches nearest to *Cymbella Helvetica*, but is much shorter, and also broader, in proportion, and is very permanent and uniform in these characters. It is possibly a variety of that species, but one which certainly ought to be figured. Regarding it, for the present, in this light, I shall not give it a separate name. 122, *f*.

Fig. 6. This is a very remarkable and puzzling form. It has the form of a large and broad *Navicula serians*, with the strong median line and large central punctum of that species; but it has also bars resembling those of *Surirella craticula* (qu. *canaliculi?*), and beneath these fine, but very distinct, cross striæ. There are also indications of longitudinal lines. This strange medley of characters makes it very difficult to class it properly. For the present, I shall consider it as *Surirella craticula*, with abnormal or sportive development of the median line and central punctum, and some variation in form.

Fig. 7. These three figures represent a *Tryblionella*, frequent in the deposit, which would appear to be a form of *T. angusta*. It is in some cases narrower than the figure given in the 'Synopsis,' and in all these the apices are more acute and more produced. It also occurs shorter and broader. There are individual specimens still shorter and broader than the left-hand figure here given. The striæ are very fine, and, I rather think, are more numerous than Mr. Smith states them to be in *T. angusta*.

Fig. 8. This is *Navicula affinis*, as it occurs in the deposit, perhaps nearer the typical form than that figured in the 'Synopsis.' It will be seen that in size, as well as form and aspect, it approaches to *N. firma*, and should, perhaps, form but one species with it.

Fig. 9. This is a remarkably long and narrow *Pinnularia*, which appears to me distinct from all others. It is of delicate aspect and very fragile, has a strongly-developed median line, and sides parallel, except just at the apices. The striæ are radiate in the middle, and distinct. Its form and aspect, as well as the much finer striation, and the very delicate margins, seem to me to distinguish it from *P. acuta*; but even if it be a variety of that species, it is so marked a one, that it requires to be named. I therefore propose for it the provisional name of *P. tenuis*. 123, *f*.

Fig. 10. This is a remarkable small capitate *Pinnularia*, which is perhaps allied to *P. mesolepta*, but differs from all the figures of that species which I have seen. The sides are slightly undulated, the striæ very delicate, and the rounded heads are of almost the same width as the very narrow body, giving it a peculiar aspect. It never exceeds, and frequently falls short of, the size here shown. (In all the figures the power used is 400 linear.) I propose to name this form *P. undulata*. *P. mesolepta* also occurs in the deposit. The present form is at once distinguished by its much smaller size, and very much finer striation, from the latter, as figured in the

'Synopsis,' as well as from another somewhat analogous form, *Navicula nodosa*, Kütz. 124, *f.*

Fig. 11. This is a small *Pinnularia*, with very distinct striæ. There are several forms nearly approaching it, which want of space prevents me from figuring at present. This one may be supposed to have some relation to *P. gibba*, but is, certainly, not gibbous. The others, which I hope to figure hereafter, are still more distinct from *P. gibba;* and, on the whole, I am satisfied that they, or some of them, ought to constitute a new species, to which I would give the name of *P. parva*. 125, *f.*

Fig. 12 represents a very elegant *Pinnularia*, which is, perhaps, allied to *P. radiosa;* but yet, as may be seen by referring to the 'Synopsis,' has a different character, almost intermediate between those of *P. radiosa* and of *P. peregrina*. I figure it, that it may be compared with those in other forms, but do not venture to name it as certainly distinct, *r.*

Fig. 13. This is the new species described by me in the last number of the 'Journal' as *Pinnularia Hebridensis*. But as Mr. Smith, before seeing my form, found it living at Grasmere in August last, and has named it *P. latestriata*, I adopt his name as the better of the two. It exhibits two varieties: one, the type, as I believe, elliptico-lanceolate; the other more linear, and frequently very slightly constricted in the middle, as in fig. 13, *β*. The costæ, 10 to 11 in ·001″, are divergent, not reaching the middle line. It is very remarkable that this beautiful species should have been detected living so immediately after I had found it in this deposit, and that, in fact, it must have been thus observed by Mr. Smith, even if I had not noticed it in the Mull earth, where it is rather scarce. In the gathering made by Mr. Smith, at Grasmere (which has yielded two other new and beautiful forms), this *Pinnularia*, so long overlooked, is the most frequent of all the *Pinnulariæ* present. I am informed that it has since been found living near Ipswich, a neighbourhood so well searched that one would have thought so marked a form could not have escaped notice. These facts strongly confirm what I have above said in regard to the necessity of strict exploration, and prove that it is not only the fossil deposits, but also the gatherings of living forms, which have been imperfectly studied. 126, *r.*

Fig. 14. This is a very pretty small form, either a *Navicula* or a *Pinnularia*, which does not agree with any figures known to me. Its form is broadly elliptical, with acute but not produced apices. The striæ are distinct and radiate. It is, possibly, a form of *P. gracilis*, but differs very much from that

species, as figured in the 'Synopsis,' or as it occurs in the deposit. It may be called, if new, *N.* or *P. exigua.* 127, *r.*

Fig. 15. These figures represent some of the varieties of *Pinnularia divergens.* Even the typical form, in this deposit, varies considerably from that figured in the 'Synopsis,' being smaller, and having about 21 costæ in ·001 instead of 11, as in that figure. It farther occurs broad and elliptical, without terminal constriction; with parallel sides, and very narrow, and in many cases with a very slight but perceptible constriction in the middle. These varieties are seen in the figures, but there are many more. I may here mention that the normal form, which is not here figured, although in shape and arrangement of markings wonderfully close to the figure in the 'Synopsis,' yet differs from it so materially in the number of striæ, that I am led to doubt if it be truly *P. divergens.* If a distinct species, then this species will include not only that normal form, and the four varieties here figured, but also *P. stauroneiformis* (fig. 16), and, as I have reason to believe from the occurrence of forms to be hereafter figured, *P. interrupta*, W. Sm. also.

Fig. 16. This figure represents *P. stauroneiformis* as it often appears in our deposit. There are other modifications, which, as mentioned in the last paragraph, may have to be united to fig. 15.

Fig. 17. This figure represents a new species of *Stauroneis*, which is not figured in any work I have seen. Its form is nearly rectangular, the stauros distinct, not reaching the margin, the striæ so delicate that it is very difficult to see them. I name it *S. rectangularis.* 128, *r.*

Fig. 18. This is a large *Gomphonema*, which was included under *G. acuminatum*, but appears to have been recently distinguished. I cannot ascertain whether this be the form called *G. Brebissonii*; but as it has not yet been figured, I give it. It is supposed by some to be merely the sporangial frustule of *G. acuminatum.* This is a point I am unable to decide. 129, *f.*

Fig. 19. This I consider to be a *Gomphonema* not yet described. It is so abundant in the deposit as to be one of the characteristic forms. It seems to stand between *G. tenellum* and *G. Vibrio*; its striæ are finer than in most species of *Gomphonema.* It is possible that it may be a *Pinnularia*, and without the F. V., which, as I do not yet know, it is not easy to pronounce. But it has so much the aspect of a *Gomphonema*, that I shall consider it as such, and propose for it, in the meantime, the name of *G. Hebridense*, from its abundance in the deposit. 130, *a.*

Fig. 20. This is one of the forms of *Himantidium Arcus*, at

least I conjecture it to be so. It is possible, from the character of the striæ, that it may belong to *H. majus.*

Fig. 21. These figures represent several varieties of *Himantidium bidens*, in which the two dorsal prominences are seen, as the valve lengthens, to sink, and finally to disappear, leaving a nearly straight line. But the apices never vary. The modifications of this, as of all the *Himantidia* in the deposit, as well as of *Eunotia bigibba* and *Pinnularia divergens*, above figured, are quite endless.

Fig. 22. These are remarkable, I believe, sporangial frustules of *Odontidium tabellaria*, *β.*

I have now noticed all the figures in this plate; and, adding to the former list such of those now mentioned as are different, we have in all 130 distinct forms in these two lists. But this is not all, for I have not been able to figure or describe at this time a considerable number of forms still undetermined, several of which I believe to be new. These must be reserved for another occasion. Moreover, I have now to mention a few species which I have not introduced hitherto, because they are not here figured, and do not occur in the 'Synopsis,' unless as included under other names. Thus I have observed—

131. *Navicula trochus*, new to Britain, *rr.*

132. *Navicula lævissima*, new to Britain, *rr.*

133. *Navicula apiculata*, one of the new forms lately found at Grasmere by Mr. Smith, *rr.*

134. *Cocconema gibbum*, *f*, new to Britain, at least not in the 'Synopsis.'

135. *Eunotia Camelus*, Kütz: this, although not in the 'Synopsis,' I cannot but regard as a distinct form, *f.*

136. *Eunotia depressa*, Kütz: the same remark applies to this form as to the last, *f.*

137. *Himantidium exiguum*, Bréb: this is certainly distinct.

138. *Orthosira punctata:* this is another new form, observed, I believe, living, by Mr. Smith, who showed it to me. I had seen it in the deposit, but had neglected it, not having seen any good figures of *Orthosiræ*, *rr.*

I should now proceed to make some remarks on the classification and nomenclature of the Diatomaceæ, but must postpone these to the next number of the 'Journal,' when I shall also describe the remaining forms. Some, I believe, will have to be added to each of the divisions of this paper, but chiefly to those new as British species, or new to science. I cannot, however, conclude without expressing my obligations to Mr. West, for the trouble he has taken in producing the very beautiful figures in the Plate.

TRANSLATIONS, &c.

On a Substance presenting the Chemical reaction of CELLULOSE, *found in the* BRAIN *and* SPINAL CORD *of* MAN. By RUDOLPH VIRCHOW. (Sept. 4, 1853.) Virchow's Archiv. B. VI., H. 1, p. 135.

IT is well known that Carl Schmidt * was the first to discover in the Ascidians the presence of a principle previously known to exist only in plants, viz. *cellulose*, and to show that it was a constituent of the animal tissue. The researches of Kölliker and Löwig,† of Schacht,‡ and of Huxley,§ have established this important fact. The occurrence of this substance, however, was limited to a comparatively very low class of the Invertebrata, and the further discovery made by Gottlieb, in *Euglena viridis*, viz. that this *infusorium* contains *paramylon*, a body isomerous with starch, also had reference only to a creature in the lowest class of the animal kingdom.‖ Nothing of the kind, on the other hand, has been known as existing in the vertebrata, and it is only since the discovery by C. Bernard—that the liver produces sugar—that we have had reason to suppose that substances belonging to the *amylum* series may also have a representative.

From histological considerations, it had struck me that the umbilical cord of man presented a great resemblance in structure to the cellulose tissue of the Ascidians (Wurzb. Verh. 1851. Bd. II., p. 161. *note*), and I was only the more confirmed in this notion by Schacht's observations, so that I have since directed my researches with care to the subject. But in many instances this was in vain, as, for instance, in the *ova* of Amphibia and fishes, the remarkable vitelline plates of which I described (Zeitsch. f. wiss. Zoologie. 1852. Bd. IV., p. 240).

* 'Zur Vergleichenden Anat. d. Wirbellos.' Thiere, 1845, p. 61.

† 'Ann. d. Sci. Nat.,' 1846, p. 193.

‡ 'Mull. Archiv.,' 1851, p. 176.

§ 'Quart. Journ. Micr. S.,' vol. i. p. 22, 1853.

‖ The pertinacity with which German naturalists cling to the animal nature of Euglena, we must confess, is very surprising to us, who are equally satisfied that it is as much a subject of the vegetable kingdom as the motile zoospores of any Alga, such as *Volvox*, *Hydrodictyon*, *Protococcus*, &c.

I was more fortunate, when a short time since, I directed my attention to the so-termed *corpora amylacea* of the brain, upon the precise nature of which, contrasted with the other kinds of amyloid bodies in man, I had not previously arrived at any accurate notion. (Wurzb. Verh. 1851. Bd. II., p. 51.) It was now apparent that these bodies assumed a pale-blue tinge upon the application of iodine, and upon the subsequent addition of sulphuric acid, presented the beautiful violet colour which is known as belonging to *cellulose;* and which in the present instance appears the more intense from the contrast with the surrounding yellow or brown nitrogenous substance.

I have repeated this experiment so often, and with so many precautions, that I regard the result as quite certain. Not only have I instituted comparative researches in different human bodies, and in the most various localities, but I have also noticed the action of the reagents under all possible conditions. The experiment is best made in the mode adopted by Mulder and Harting, with regard to vegetable cellulose (*vide* Moleschott 'Physiologie des Stoffwechsels,' p. 103), viz., by causing the action of diluted sulphuric acid to follow that of a watery solution of iodine. The iodine solution should not be too strong, for the observation may then be impeded by its precipitation; and, on the other hand, care must be taken that the iodine exerts due action upon the substance. Owing to the volatility of the iodine, and its great affinity for animal substances, its action is usually very unequal, so that the border of the object and not the centre may be penetrated by it; or perhaps, of spots in close contiguity, one will contain iodine and the other not. It is, consequently, always advisable to repeat the application of the iodine several times, but to avoid the addition of too much. Upon the subsequent addition of sulphuric acid, if the action have been too powerful, the result is a perfectly opaque, red-brown colour. The most certain results are obtained if the sulphuric acid be allowed to act very slowly. In fact, I have procured the most beautiful objects in allowing a preparation covered with the glass to remain undisturbed with a drop of sulphuric acid in contact with the edge of the covering-glass for 12 to 24 hours. Under these circumstances, the most beautiful light violet-blue was occasionally presented. Lastly, I would just intimate that accidental mixtures of starch or cellulose may readily happen, seeing that very light fibres or minute particles from the cloths with which the object and covering-glasses have been cleaned, may very easily be left upon them, which would afterwards exhibit the same reaction as the above.

Every precaution having been taken, the following results will be obtained :—

1. The *corpora amylacea* (Purkinje) *are chemically different from the concentric-spherical corpuscles, of which the brain-sand is composed*, and with which they have hitherto usually been confounded. The organic matrix of the brain-sand granules is obviously nitrogenous ; it is coloured of a deep yellow, by iodine and sulphuric acid. This is true not only of the sabulous matter in the pineal gland and chorioid plexuses, but also of that of the Pacchionian granulations and of the *dura mater*, as well as of the dentate plates in the spinal arachnoid. In all these parts I have, in general, nowhere obtained the blue reaction, except in a few spots in the pineal gland. It would, therefore, for the future, be convenient to restrict the name of 'corpora amylacea' to the bodies containing cellulose.

2. These bodies exist, so far as I have at present found, only in the substance of the *ependyma ventriculorum* and its prolongations. In this I include especially the lining of the cerebral ventricles and the transparent substance in the spinal cord described by Kölliker, as the *substantia grisia centralis* (Mikrosk. Anat. Bd. II. 1, p. 413). With respect to the cerebral ventricles, I have already repeatedly stated, that I find them to be lined throughout with a membrane belonging to the connective tissue class, upon which rests an epithelium. This membrane contains very fine cellular elements, and a matrix sometimes of more dense, sometimes of softer consistence, and is *continued on the internal aspect without any special boundary between the nervous elements.* In the deeper layers of this membrane, and in immediate contiguity with the nerve fibres, the cellulose corpuscles are found most abundantly, and they are also especially numerous where the *ependyma* is very thick. They are consequently very abundant on the *fornix*, *septum lucidum*, and in the *stria cornea* in the fourth ventricle. In the spinal cord, the substance corresponding to the *ependyma* lies in the middle, in the grey substance, in the situation where the spinal canal exists in the fœtus. It there forms evidently a rudiment of the obliterated canal, such as it presented in the obliteration of the posterior cornu of the lateral ventricle, which is so frequently met with. In a transverse section of the cord, it is easily recognised as a gelatinous, somewhat resistant substance, which may be readily isolated. Its cells are much larger and more perfect than those of the cerebral *ependyma.* This *ependyma spinale* forms a continuous gelatinous filament, which extends to the *filum terminale*, and might therefore, perhaps, be most suitably described as the *central ependymal filament.* In it the cellulose

granules are also found, though, as it would seem, more abundantly in the upper than in the lower portion. In other situations I have sought for these bodies in vain, and in particular I have been unable to find them in the external cortical layer of the cerebrum, or anywhere in the interior of the cerebral substance.

3. Since, from the experiment of Cl. Bernard, who produced saccharine urine by wounding the floor of the fourth ventricle in the Rabbit, there appeared to be reason to conclude that the existence of cellulose was connected with that phenomenon. I sought for it also in Rabbits, but in vain: I found in that situation both in the fourth, and the third, and in the lateral ventricles, a very beautiful tessellated *epithelium* with very long vibratile cilia, but no cellulose.

4. The cellulose granules, therefore, appear to be everywhere connected with the existence of the *ependyma-substance* of a certain thickness, and might perhaps be regarded as a constituent of it. They occur of excessively minute size, so that the *nuclei* of the ependyma scarcely correspond with them. Can they be formed out of the latter? The larger they are the more distinctly laminated do they appear. But there is never any indication in them of a nitrogenous admixture, recognizable by a yellow colour. The centre only is usually of a darker blue, and consequently perhaps more dense than the cortical laminæ.

5. As to an introduction of these bodies from without, such a supposition is the less probable because a similar substance is nowhere else known. We are acquainted with a series of varieties of vegetable cellulose, but the substance now in question appears to be distinguished above all by its slight power of resistance to reagents, seeing that concentrated acids and alkalies attack it more powerfully than is usually the case with the cellulose of plants.

6. In the child I have as yet sought for it in vain, so that like the "brain-sand," it appears to arise in a later stage of development, and probably may have a certain pathological import.

Since writing the above, Professor Virchow has repeated and confirmed his observations, and ascertained in addition that similar bodies also occur in the higher nerves of sense. He found them most abundantly in the soft grey interstitial substance of the olfactory nerve, less frequently in the acoustic, although the observations of Meissner (Zeitsch. f. rat. Med., N. F., Bd. III., pp. 358, 363), would indicate a proportionately great disposition to their formation in that situation. Rokitansky appears to have seen them in the optic nerve, and from an oral communication the author has learned that Kölliker has found them in the retina.

Having already stated that the *ependyma* is continued without special limitation among the nervous elements, the author goes on to observe that it is now apparent that there is a continuous extension of the same substance in the interior of the higher nerves of sense. From a series of pathological observations, he concludes that a soft matrix referrible mainly to connective-tissue substance, everywhere pervades and connects the nervous elements in the centres, and that the *ependyma* is only a free superficial expansion of it over the nervous elements. The opinion, that the epithelium of the cerebral ventricles rests immediately upon the nervous elements, appears to have arisen from a confusion of this interstitial substance with the true nerve-substance.

The isolation of the *corpora amylacea* in larger quantity, in order that they should be subjected to chemical analysis, the author has not yet succeeded in effecting. Nevertheless it seems impossible to entertain any doubt as to their cellulose nature. No other substance is known which affords the same reaction; and although the author has examined the most various animal tissues, and has accurately investigated, particularly, the concentric corpuscles occurring elsewhere, as in the *thymus* in *tumours*, &c., nothing of the same kind has presented itself.—(*Sept.* 25, 1853).

An abstract of the above observations also appears in the 'Comptes Rendus,' for the 26*th Sept.*, 1853, p. 492, but containing nothing additional.

Being desirous of verifying the above observations, I have examined the brains of one or two individuals; and, as my results differ in some respects from those of Professor Virchow, I will here briefly state them, leaving a more detailed account of the matter to a future opportunity, my observations at present having been too scanty to justify the expression of any settled opinion. The first case I examined was that of a young man who died of the consecutive fever of cholera, after an illness of five or six days, during the whole of which period, the renal secretion was completely suppressed. What I noticed in this case, was:—

1. The enormous abundance of the *corpora amylacea* in certain situations, as the *ependyma ventriculorum*, particularly on the *septum lucidum*, and more especially also on the choroid plexuses, upon gently scraping the surface of which a fluid was obtained containing these bodies in the most surprising quantity.

2. That they existed in immense abundance in the olfactory bulbs and in the superficial parts of the brain, both cortical and medullary, contiguous to the tract of the olfactory nerves. But scarcely any part of the *cerebrum* and *cerebellum* could be examined, at all events towards the surface, without meeting with some or more; and they occurred abundantly in the very middle of the *cerebellum*. Their distribution, however, was very irregular, inasmuch as they abounded in some spots and were nearly, if not altogether wanting, in others. I could find none in the *corpora striata*, where they seemed to be replaced by "brain-sand," of which more will be said afterwards.

3. The cerebral substance, in immediate contiguity with the *corpora amylacea*, appeared quite natural.

4. The corpuscles were starch and not cellulose, and possessed all the structural, chemical, and optical properties of starch, as it occurs in plants, as the following few details will show:—

They were of all sizes, from less than a blood-disc up to 1-500th inch or more—generally more or less ovate, but many irregular in outline, and apparently flattened, as all the larger kinds of starch I believe are. Many of the larger ones showed the appearance which, in starch, has been erroneously described as indicative of a laminated structure; whilst in others this appearance under any mode of illumination certainly did not exist. The point that would correspond with the so-called *nucleus* of a starch-grain was, unlike that of most kinds of starch, central, and consequently the laminated marking was concentric to the grain, which is rarely the case in the starch of plants. This apparent lamination depends, as I believe, upon the same circumstances as in other starch (*vide* Trans. Micr. Soc., Quart. Journ., vol. i., p. 58), that is to say, upon the corrugation of a thin *sacculus*. That this was the case I satisfied myself by the use of sulphuric acid and of Schultz' solution (chloride of zinc and iodine), in the mode described in my paper above quoted. By these means, but more readily and conveniently by far by the latter, the *corpora amylacea* could be seen to unfold into empty, flaccid, thin-walled, blue sacculi, six to eight times larger than the original grain. Their structure thus appearing to be identical with that of starch, the identity of their chemical composition was rendered evident with equal facility. Simple watery solution of iodine coloured them deep blue, which ultimately became perfectly black and opaque. They were soluble after swelling and expanding in strong sulphuric acid, and by heat; and, moreover, they acted upon polarized light in the same way as starch does. Some of the smaller grains exhibited a distinct and sharply-defined black cross, of which the lines crossed at angles of 45° in the middle of the grain, but in the majority, there was only a single dark line in the long diameter of the grain, and which seemed always to correspond with an irregular fissure or hilus, as it might be termed, in the same direction, which was presented in a great many of the grains, and seemed to be the indication of a partial inrolling of them, as in the starch of the horse-chestnut. This longitudinal fissure was not unfrequently crossed by a shorter one at *right* angles. When the covering-glass was closely pressed, the grains were easily crushed, breaking-up in radiating cracks around the margin ; and sometimes, when thus compressed, a concentric annulation would become evident, which was before inapparent.

In the *corpora striata*, as I have mentioned above, I could find few or no starch-grains, but here an appearance presented itself which seems to be connected with their formation. Many particles of sabulous matter or crystalline corpuscles of the ordinary "brain-sand," were met with, all of which, instead of lying like the starch-grains, in the midst of unaltered nerve-substance, were lodged in irregular masses of what appeared a fibrinous or immature connective tissue-substance; and, in this instance, upon the addition of iodine, each mass of crystals was found to be immediately surrounded by an irregular thickness of a transparent matter, which was turned not *blue*, but a light *purplish pink* by that reagent — a substance, in fact, closely resembling in that respect the very early condition of the cellulose wall; for instance, in *Hydrodictyon*,—an immature form, as it may be termed, of cellulose.

In a second case, that of an old man—dead of chronic dysentery, and who died comatose —I found the ventricles distended with about three ounces of clear fluid. The surface of the *ependyma* throughout all the continuous cavities was studded like shagreen with minute transparent granulations, which on microscopic examination, appeared finely granular and homogeneous, or sometimes faintly fibrillated. In this case there were, I think, no *corpora amylacea* in the *ependyma* (at least I found none), nor in the central substance of the brain : a few were met with in the peripheral portions, especially on the summits of the hemispheres, and still more in the much-developed Pacchionian granulations, and there commingled with *other* concentrically-laminated bodies, which formed botryoidal masses imbedded in a stroma of immature connective tissue: these bodies, which might, to distinguish them, be termed the 'chalcedonic corpuscles,' were rendered yellow by iodine. In this case also, I did not notice the *quasi* cellulose-deposit around the particles of 'brain-sand,' but in several instances I saw minute amylaceous particles (coloured blue by iodine), contained in cells which they only partially occupied.—GEO. BUSK.

NOTE.—In the 'Comptes Rendus,' No. 23, (Dec. 5, 1853,) are some further observations on the "Animal Substance analogous to Vegetable Cellulose," by R. Virchow, in which he announces the discovery of corpuscles presenting the same reaction as the *corpora amylacea* of the brain, in the Malpighian corpuscles of diseased human spleens—in the condition termed "waxy spleen" (Wachsmilz).

On the Irritability of CILIATED CELLS. By RUD. VIRCHOW. (Virch. Archiv.) Vol. VI. Part I., p. 133. 1853.

AMONG the many extraordinary circumstances with which the phenomenon of ciliary motion is surrounded, it has not been the least, that we have scarcely been acquainted with a proper excitant of it. Whilst we were able to call the contractile substances into activity by mechanical, physical, and chemical agencies, the founders of the doctrine of ciliary motion, Purkinje and Valentin, could, as regards the cilia, find nothing but means to impede or destroy it. And the influence of mechanical agitation, the only means by which they had seen the failing vibration again become more vigorous, was doubted by Sharpey. Can it be said, therefore, that the ciliary substance is wholly and entirely different from the other contractile substances?

A short time since, in examining a human *trachea*, I chanced to hit upon the discovery of a chemical excitant for the *cilia*. Upon the addition of a solution of potass to an object in which the ciliary movements, which were at first very lively, had begun to slacken, I noticed a renewal of the motion in every part, and that it lasted until the parts were destroyed by their solution in the caustic menstruum. I have since repeated this experiment under various conditions, and always with the same result. In a *trachea* from a human body, in which the ciliary movements had quite ceased in places, and was universally very faint, and in which the ciliated epithelium itself was readily destroyed upon the mere addition of water, I was still able to recal the phenomenon in great intensity, by the application of the potass-solution, although but for a short time. In better preserved and more recent mucous membrane, on the other hand, the revival of the motion could be maintained for a pretty considerable duration.

I have usually allowed a microscopic preparation, in which

I had ascertained the existence of ciliary motion, to remain in contact with water until the motion ceased. Not unfrequently I was obliged to wait until large clear drops appeared on the surface of the cells between the cilia, which indicated the commencement of the splitting up of the contents. Under the prolonged action of the potass-solution, isolated cilia would first begin, here and there, to exhibit irregular, jerking movements. By degrees, more and more would begin to move, but in such a way that their movements were in opposite directions, and showed no correspondence either in direction or amount. But gradually the phenomenon acquired more and more regularity, force, and uniformity, until at last the rapid, rhythmical, sweeping movement of whole series of cells would be seen to be restored.

That we have, in this case, to do with a chemical influence, is clear; but that the entire phenomena of the motion cannot be the result merely of the corrosive action of the alkali, is evident from the perfect correspondence in the course of the phenomena with what is seen when the movements are spontaneous. It is only when the action of the potass is too powerful and rapid that the excitation is seen to be limited to a short, active agitation of the *cilia*, which is immediately followed by the solution of the substance; and, in this case, it has very much the appearance as if it were the sudden swelling up of the substance in conjunction with the consequent undulation of the fluid which produced the motion. But even in such instances as this, it may be satisfactorily shown, by comparison with other similar minute corpuscles, that a moment of activity exists, in which the contractile substance produces a movement independent of the swelling up, and of the currents.

Soda acts in the same way as potass; whilst the effect of ammonia, which at once causes chemical decomposition, is quite different. Nor have I been able to find any other substance having the same effect, although I would by no means preclude farther researches, my own inquiries having been too restricted to decide the matter. When the long list of chemical substances with which Purkinje and Valentin (*De phenomeno gen. et fundam. motûs vibratorii continui*, Vratisl. 1835, pp. 74—76) have unsuccessfully experimented, is surveyed, no great hopes certainly can be entertained; and I consider myself particularly fortunate in having chanced at once to hit upon two substances overlooked by those careful observers. Nor indeed can they be justly blamed for this oversight, when it is considered that they had tried fifty different reagents, and each in six different degrees of concentration.

No further proof, perhaps, is requisite, to show, that the *substance of the vibratile cilia, from their irritability, as proved in the above-described experiments, approximates the contractile substance of the muscles* (*syntonin*, of Lehmann).

On the Germination of the Spores *of the* Uredineæ. By M. L. B. Tulasne. Extracted from the Comptes Rendus. Tome xxxv. June 1853.

The author had previously shown that the spores of the Uredineæ, like the pollen-grains of phanerogamous plants, had a variable number of pores, from which afterwards tubular filaments arose, apparently analogous to those which are the first result of the germination of the spore of a fungus.

In addition, he has proved that the so-called *Æcidiolum exanthematum* (Unger), may, very probably, be correctly regarded as equivalent to the *spermogonia* of the other fungi, so that in all probability it is not a sexual. According to new researches the germinal filaments of the spores do not all retain the simple, continuous condition, which was formerly assigned to them, and probably do not represent the commencement of the true *mycelium.*

When sown, the spores of *Æcidium Euphorbiæ sylvestris*, D. C., did not retain their continuity, but were subdivided into four or six unequal-sized cells, by means of transverse septa; there then appeared upon each of these cells, and particularly of the upper ones, a lateral short process (*spicula*), which soon supported an obovate and rather oblique tubular process. These tubes were the last vegetative effort of the spores; they became free (?), and produced only very slender filaments. Upon the separation of these bodies, the jointed tube from which they arose is emptied, and, like the spores, is destroyed; so that this sacculus or filament represents a sort of *promycelium,* a vegetation which intervenes between the primary spore or fruit, and those lesser follicles which are either secondary spores, or rather, perhaps, the only true and actual producers of the *mycelium.*

The same thing takes place in *Puccinia*, the spores of which are capable of germination while yet upon the parent plant. The spores of *Puccinia graminis* throw out tubes by which they lengthened two or three times, divide into cells, and again produce reniform spores, which soon germinate. It is exactly the same in *Phragmidium incrassatum.* Link.

The *Podisomata*, also belonging to the *Uredineæ*, throw out from their two-sided fruits (sporidia) as many as eight tubular processes, crossing each other in pairs and superimposed one upon the other, which invest the fungus with a sort of pile; each of these produces several obovate spores, which may be collected in vast quantity.

In several Uredineæ (*U. Rosæ suaveolens*, *Tussilaginis crassum*), the tubular processes are capable of branching, and bear a still closer resemblance to a normal, fungoid *mycelium*.

The spermogonia of the Uredineæ are highly aromatic. It is to them that is due the odour of *Uredo suaveolens*, &c.

M. Tulasne has not yet accurately investigated the germination of the spores of the Ustilagineæ. The elongated cell, which proceeds from the spores of *Ustilago antherarum*, Tal., is probably analogous to the secondary spores of *Æcidium* and *Puccinia*.

In *Ustilago receptaculorum*, Fr. organs, without doubt analogous to these secondary spores, are produced from a slightly-developed *promycelium*, consisting only of a few cells, but resembling that of *Æcidium Euphorbiæ sylvestris*, D.C.

On the Structure and Vital Properties of the CONTRACTILE SUBSTANCE of the LOWEST ANIMALS. By Professor ALEX. ECKER. Abstracted from Siebold and Kölliker's Zeitsch., vol. i., p. 218, pl. 18.

THESE observations relate principally to the nature of the substance of which the body of the *Hydra viridis* is composed, but the conclusions are applied also to many other of the lower animal forms. It is stated that the entire body of the *Polype* consists of a homogeneous, sometimes clear, sometimes granular, soft, extensible, elastic, and contractile substance, which is reticulated with clear spaces, containing a more or less clear fluid. Ecker denies that cellular structure is found in any part of the animal, of which structure Corda and Baumgärtner would make it entirely to consist. The three layers which are presented in the body and arms of the *Hydra*, are in immediate connexion, and are distinguished: 1. The external—by the presence in it of the thread-cells and the greater rarefaction of the tissue. 2. The middle—by the green granules and a less broken-up tissue: and 3. The innermost layer—by the brown excretion-granules, and during digestion by various absorbed matters, oil-drops, &c. From this similarity in the structure of the three layers, it is probable they are all equally contractile; though it is perhaps the middle layer in which this property resides in greatest activity.

This contractile substance above-described, from its want of definite form, cannot, according to Ecker, be termed muscular. It is distributed throughout the whole body, and not formed into filaments or fasciculi; nor any more is the sensitive substance yet collected into nerves, but must be assumed to be dispersed through the whole body. The one is always most intimately connected with the other, as the investigation of all the lower animal forms teaches us. It is not until nerves are developed, that even scattered muscles are assigned for any given purpose in the economy. Muscles are not possible without a connecting nervous system.

The author then proceeds to show the exact similarity between the contractile substance of the Hydra and that of other lower animal forms—such as the Infusoria and Rhizopoda. The properties of this substance in its simplest form are seen in the Amœba, the body of which, as is known, consists of a perfectly transparent, albumen-like, homogeneous substance, in which nothing but a few granules are imbedded, and which presents no trace of further organization. This substance is in the highest degree extensible and contractile; and from the main mass, are given out, now in one part and now in another, perfectly transparent rounded processes, which glide over the glass like oil, and are then again merged in the central mass. There is no external membrane. In the body of the Amœba there occur, besides the granules, clear spaces with fluid contents, which are sometimes unchangeable in form, and sometimes exhibit rhythmical contractions. Exactly the same particulars are exhibited in the other Infusoria.

Thus it is evident that the body of the Infusoria is always formed of a sometimes perfectly homogeneous and transparent, sometimes minutely granular, soft, elastic, and contractile substance, which is more or less extensively penetrated by spaces containing fluid. This substance was termed "sarcode" by Dujardin. Ecker proposes to call it "amorphous contractile substance." The clear spaces are termed by Dujardin "vacuoles."

The contractile substance is, according to Dujardin, insoluble in water, though gradually destroyed by it (in dead Infusoria); soluble in alkali, though not so readily as albumen; coagulable by alcohol and nitric acid.

Having satisfied himself of the identity of the contractile substance of the Infusoria with that of the *Hydra*, the author goes on to inquire whether the same substance occurs in other of the lower animal forms? He believes that Quatrefages is in error when he describes muscles in *Synhydra* and *Eleutheria*, explaining the appearances noticed by that author, in accordance with his own views. Reaching the Anthozoa, however, and the Bryozoa (Polyzoa), we find abundantly undoubted muscular fibres. In the Rotifera and Tardigrada the contractile substance is perfectly homogeneous, soft, without the least trace of further organization, and exactly like the "sarcode," with which it furthermore corresponds in the circumstance that in the dying animals it forms "vacuoles." This substance forms sometimes (as in the Rotifera) at the anterior extremity, wart-like, ciliated masses, sometimes in the interior (especially evident in the Tardigrada) muscle-like strings, of definite and permanent form and arrangement, which have been actually described by Ehrenberg and Doyère as muscles. But in the definition of a tissue it is necessary to consider, not the external form, but its histological relations. A perfectly homogeneous, soft, structureless substance cannot be called "muscle," without a complete change in the idea of the latter, and connecting the term merely with contractility. This substance (in the Tardigrada) appears to form a well-marked transition from the amorphous contractile substance, such as we find in the Hydra, to the true muscular substance.

As in this instance the transition from the "amorphous" to "formed" contractile substance is shown in different animals in an ascending scale, it remains to inquire whether a similar transition also takes place in one and the same animal. That this is the case would appear to be proved by Dujardin's ('Observ. au Microscope,' p. 78, Pl. V., fig. 3, 10, 11) observation of the very early appearance of amorphous contractile substance or sarcode in the ovum of higher animals. He saw in the yolk of the egg of the *Limax*, which has no vitelline membrane, that the diaphanous substance, surrounding and holding together the vitelline granules, exhibited movements exactly like those of Amœba.

In the larvæ of Insects again, even after they have escaped from the egg (as, for instance, in *Chironomus*), the muscles consist of a perfectly homogeneous, non-fibrillated, very contractile substance, precisely resembling the so-called muscles of the Tardigrada, whilst at a later period these same muscles exhibit distinct transverse striation. It would appear, therefore, from this and the former observation, that the amorphous contractile substance, both in the animal kingdom and in separate individuals, gradually passes into the "formed," that is, into muscle.

Accurate chemical examination of the two forms of contractile substance is highly desirable; it may, in passing, be observed, that both are hardened by carbonated alkalis. In the next place, a comparative history of the development of the muscular and of the amorphous contractile substance is a necessary requisite; and, lastly, an experimental inquiry into their vital properties, particularly into their behaviour under galvanic excitement, which would show whether very different substances possess the contractile property, or whether this property, with a varying histological condition, is not connected with a determinate chemical constitution.

The following are the forms in which the contractile substance is met with:—

1. A transparent, homogeneous, structureless substance, reticulated with clear spaces, contractile in all directions, and continuous throughout the whole body, or even constituting the greater mass of it; no nervous system. "Amorphous Contractile Substance." (Infusoria; *Hydra;* Hydroida.)

2. A transparent, homogeneous, structureless substance, non-fibrillated, but divided into isolated, muscle-like portions. An appearance of nerves. (Systolida; young Insect larvæ.)

3. A substance composed of fibres, and contractile in the direction of these fibres. "Formed contractile substance, or muscular substance."

4. Contractile cells, leaving out of the question the Gregarina and the ciliated cells, the appendages of which are contractile, appear to occur only in the embryonic condition. (*Planaria;* Heart-cells of the embryo in *Alytes* and *Sepia;* Caudal vesicle of the *Limax* embryo.)

[With respect to the last observation, if the author, by contractile cell, means a cell with a wall distinct from, and of a different material to, its contents, his assertion may be correct, except in as far as it applies to certain unicellular animals—and which will constitute a large exception. But if under the same name the so-termed "protean cells," of which, in fact, the substance of most, if not all, sponges is mainly composed, he is manifestly greatly in error in limiting their occurrence so closely as he does. Similar cells, as has been pointed out by Mr. Huxley, abound in the tissues of the *Medusæ* (in which are also found distinct striated muscular fibres); in *Hydra* itself; and in many other instances, not only in the lower, but even in the higher animals, for the protean changes of form have been observed even in the white corpuscles of the human blood. The zoospores, again, of many of the lower Algæ may be said to be composed of the same protean substance, and many, if not all, of these may be regarded as unicellular—applying that term in a general sense, and not restricting it merely to those organisms which present a distinct wall. These monadiform spores, or gonidia, are, in fact, well named by Cohn primordial cells, by which he means cells composed wholly of vegetable protoplasm, which in its essential nature seems to differ little, if at all, from the "sarcode" or "amorphous contractile substance" of the animal kingdom.—G. B.]

REVIEWS.

ON THE STRUCTURE OF THE MUSCULAR FIBRIL AND THE MUSCULARITY OF CILIA. By Dr. BARRY.

DR. M. BARRY's "dominant idea" is a twin spiral; and, inspired by this perverse and pertinacious spirit, he has again presented us with a repetition of his views with respect to the structure of the muscular fibre and vibratile cilium, in an attempt to assign the "Main cause of discordant views on the Structure of the Muscular Fibril," together with some "Further Remarks on the Muscularity of Cilia," in the November number of the 'Philosophical Magazine.'

The latter paper, it is needless to say, states that every *cilium* is a twin spiral; and, to confirm this discovery, there is a wonderful figure of a cilium from the gill of the common Oyster. This monstrous object appears to be partly of the nature of plant and partly animal, having roots whereby it grows, and the faculty of "spinning-up after threads" "by its twisting and untwisting." It is not, however, very clear how this primitive "jenny" performs this task, nor are the threads, as their name would imply, to be regarded as the tail of the creature, but rather, if it had one, are they to be considered as its head.

The paper then proceeds to give a notice of a model in lead-wire of the muscular fibril, which Dr. Barry has presented to various colleges in this country, and to the University of Prague, we presume, for the edification of his sole disciple, Professor Purkinje. This model, which is intended to afford a complete elucidation of the mode of action of striped muscular fibre, will doubtless be found extremely useful to the venerable Professor, and to any others who may hereafter be misled into a belief in the inventor's doctrine. But Dr. Barry has omitted to give a model to explain the mode of action in the contraction of unstriped and smooth muscular fibres, and in that of amorphous contractile substance. Professor Purkinje has also been gratified with the presentation of the model of a young *cilium*, and at the same time with this information :—"From analogy, it appears extremely probable that the heart arises, in like manner, out of the *nucleus* of a cell, being originally such a double spiral as in the *cilium* aforesaid. If so, the spiral form of the heart may be explained by the continued division of what was originally a double spiral fibre."

It is satisfactory to find that Professor Purkinje has not kept this information to himself, but had the consideration to put it into German for the benefit of his compatriots. How they will have received it, it is not very difficult to surmise.

The former paper of those here cited assigns as the "main cause of the discordant views" respecting the structure of things in general, and especially of muscle, entertained by Dr. Barry on the one side and the rest of the world on the other, the fact, "that observers, in their endeavours to reach the ultimate structure of the muscular fibril, have actually gone too far. Not content with the examination of the mature fibril, they have arrived at what almost defies the microscope—its *embryo* [whose?], mistaking and delineating for the fibril a row of quadrilateral particles, the mere elements thereof; mistaking for the chain, as it were, a row of half-formed links destined to compose it." "I cannot," he very properly goes on to say, "wonder that in a row of quadrilateral particles no one could discern my twin spirals,—" nor, we conceive, can any one. He maintains, therefore, in the first place, "that it was impossible for them to agree with one another; and secondly, as our attention was directed to two different things, that it was still less possible for any of them to agree with me. Hence, a main cause of discordant views on the "structure of the muscular fibril."

This extraordinary declaration requires no comment; but we would remark that not only in 1842 did Dr. Barry particularly recommend muscle from the tail of the very minute Tadpole (4 to 5″ long.), and has all along maintained the existence and asserted the demonstrability of what he calls a coiled fibre in the blood discs (concerning which, *see* Microscopical Journal, Vol. II., p. 257, 1842), beyond which one need scarcely go in search of the phantom; but actually on the page immediately preceding that from which the above assertion is taken, he expressly and far more truly says, "in order thoroughly to understand the structure of this tissue, it is essential to see it in its most incipient state, and patiently to follow it through every stage, for," as he properly observes, "at that early period its elements are very large."

We have no purpose of discussing a question, which ought long ago to have been consigned to the limbo of oblivion, and notice this paper merely to admonish our readers that they must not expect to find in it any additional evidence or reasoning in support of Dr. Barry's singular views. The reasoning being of a character of which the congruity between the two quotations above given may serve as a sample; and the facts consisting merely of some diagrammatic figures of muscular fibre taken from preparations which had been preserved in weak spirit or glycerine for three or four years. The author also cites a very curious conclusion at which Dr. Allen Thompson had arrived, from the inspection of some of the well-known thread-cells of an *Actinia*, viz., " that if these double spiral, prehensile filaments of the *Actinia* are contractile, they may fairly be used as an argument in favour of Dr. Barry's views." That is, Dr. A. Thompson is of opinion that the structure (in this case altogether mistaken) of one tissue can be employed in demonstration of that of another with which it has no relation whatever.

But, that these unfortunate thread-cells should be brought into the argument at all is not a little surprising to us, who remember with what triumph they were received in Edinburgh, as an undoubted proof of the correctness of Dr. Barry's views, on the supposition, *mirabile dictu!* in that city of naturalists, that they were portions of muscular fibre! We were promised, at the same time, some specimens to the same effect from the Lobster or some other Crustacean, which, however, does not as yet appear to have been caught.

Beitrage zur Mykologie (Contributions to Mycology). By Dr. George Fresenius. First and Second Parts, with 9 Plates. Frankfort, A. M., 1852. (With Plates.)

Dr. Fresenius commences his work with an expression of regret that Mycology has up to the present time been so little studied; and he attributes the neglect partly to the unsightliness (unscheinbarkeit) of the objects of the study. We think, however, that the charge of unsightliness is one which cannot be maintained against fungi in general. The *Agaricus muscarius*, with its brilliant crimson *pileus* and snowy gills, the *Agaricus rutilans* with a cap of more sober crimson and gills of the richest yellow, the *Agaricus psittacinus*, or Parroquet Agaric, varied as the plumage of the bird after which it is named;—these, and many others which might be mentioned, fairly rival flowers in beauty. In fact, a well-arranged group of Agarics, *Clavarias*, &c., gathered from the first wood into which the botanist may wander, would form as pretty a decoration for a lady's boudoir as any bouquet from the greenhouse. Those persons therefore who, without troubling themselves with scientific details and distinctions, look only to the external charms of the productions of nature, would find ample employment in searching out the beauties of the Fungi. We find them assuming the forms of clubs, mitres, bowls, cups, stars, and even of birds'-nests, and lanterns;* and their colour is as varied as their shapes, and growing as they principally do, upon the mouldering remains of other organisms, they seem intended, as Mr. Lee has happily expressed it, to deck even decay and ruin with beauty.

To the microscopist the study of the Fungi affords opportunities for the use of the highest powers of his instrument, and for the exercise of the utmost of his manipulative skill. The organs of fructification, even in the largest plants, are so minute as to render careful preparation and the aid of a good microscope indispensable for their examination, and the structure of many of the more minute genera is so imperfectly understood, and the opinions of mycologists with regard to them so much at variance, that a vast field is open for elucidation and discovery.

With this introductory recommendation of the Fungi, we will proceed to notice the contents of the work before us, which will be found full of interest. It contains particular descriptions, accompanied in most instances by plates of about eighty different kinds of Fungi.

The first genera which the author discusses are *Mucor* and *Ascophora*. He differs from Link, Wallroth, and others, in regarding *Ascophora* and *Mucor* distinct, stating that his late observations have convinced him of the propriety of their being united in one genus, and he thinks there is no real ground for considering the genus *Rhizopus* as distinct either from *Ascophora* or *Mucor*. He alleges that the construction of the *sporangium* is precisely the same in all three genera, and that the mode of its disruption does not, as has been supposed, distinguish *Ascophora* from others of the Mucoroideæ. He has not, however, made any remarks as to the difference in the arrangement of the spores. In *Rhizopus* they are concatenated, which may afford sufficient ground for generic distinction.

Five different species of *Botrytis* are described and figured in this volume, and of these five, three afford a striking instance of the diversities of opinion amongst mycologists, to which we have referred. Dr. Bonorden, in his "Handbuch der Allgemeinen Mycologie," states that he considers the *Botrytis plebeja* of Fresenius to be *Botrytis elegans*; *Botrytis interrupta*, Fres. to be *Botrytis bicolor*, and with regard to the third, *Botrytis furcata*, he is unable to come to any decision.

The following remarks on the *flocci* of the *Hyphomycetes*

* The *Diamphora bicolor*, a minute Brazilian fungus, has the appearance of two small lanterns attached to a forked branch.

may be usefully extracted. The appearances referred to will be familiar to those who have paid any attention to the microscopic examination of moulds :—

"In diagnoses and descriptions we often meet with the expression *flocci æquales*, in opposition to *flocci strangulati*, or with the expression *articuli alterni constricti, alterni compressi.* This characteristic, however, is of such frequent occurrence amongst those *hyphomycetes* which have passed their earliest stage of growth, that it is hardly worth mentioning in any particular case. The flocci are almost universally compressed, and they are twisted in a manner which gives them a jointed appearance; they may have septa or they may not. In the former case the compressed cells stand in regular alternation, one over another, cutting each other at right angles,* so that we see the surface of one cell and the edge of the adjoining one. In order to observe this appearance, the object must be examined dry."

We may add here, that all moulds should be examined dry in the first instance, as the spores are immediately dispersed by contact with water. They should then be moistened in order to enable the observer to ascertain the mode of attachment of the spores.

The author ridicules, and as it appears to us with some reason, the frequent use by mycologists of the expression, "*sporæ inspersæ.*" He says—

"What would be thought of a botanist who, in giving the generic character of a phænogamous plant, were to say that its seeds, after being shed, remain hanging, partly about the stem and partly about the leaves? and yet can it be said that the expression '*sporæ dein floccis inspersæ*' implies anything else?"

The *Asterosporium* (*Hoffmanni?*), of which a figure is given in this work, is peculiar, from the form of its spores, which have four conical rays proceeding from them; the rays do not lie in one plane, but diverge in different directions from the middle point of the spore. The author says that the spores have been compared to a man-trap. The man-trap, however, must have been of foreign construction, as there is no resemblance to the English form of this (now illegal) instrument. It is somewhat singular that Chevallier, in his "Flore des environs de Paris," took upon himself to deny the existence of this plant, and to state that its *supposed* existence originated in an optical delusion; and yet Dr. Fresenius remarks that it does not require the assistance of a modern achromatic microscope to discern the peculiar formation of the spores.

We extract the following remarks upon the genus *Discosia* :—

* "In planes at right angles to one another" would have been a more nearly correct expression, although not *mathematically* accurate, a *compressed cell* not being strictly a *plane*.

"Much confusion has arisen from the circumstance that in the observations hitherto made, oil-drops have been mistaken for spores, and spores for *asci*; thus Libert, in speaking of the genus *Discosia*, talks of *ascidia fusiformia* and *sporidia globosa*. Corda, who had not examined the plant, adopted this diagnosis, and Bonorden has lately attributed to the genus the possession of round spores and *asci*, and moreover has inaccurately united it with *Dothidea*. It is evident that the whole subject must be re-examined, and that it is unsafe to rely upon the early observations."

One of the most striking plants described by Dr. Fresenius is the *Peziza macrocalyx*. The family of the Pezizæ is a very interesting one. It is so numerous, also, that specimens of one kind or another are to be met with in every locality. The *Peziza virginea*, a minute plant, the cup of which is of the purest white, and fringed with hairs which are frequently tipped with dew-drops, is a most beautiful object under a 2-inch glass. It is so common, also, that there is no difficulty in procuring specimens. The *Peziza calycina* (*pulchella* of Greville), which we have frequently met with in the neighbourhood of London, is also fringed with white hairs, but the interior of its cup is of a bright yolk-of-egg yellow. The *Peziza xanthostigma*, forms little golden dots upon fallen branches in moist woods, and there are many others too numerous to mention of equal beauty. The *Peziza macrocalyx* here described is peculiar from its large size and from its mode of growth. It is found in pine-woods, sometimes solitary, sometimes in groups of as many as four or five together. The cup is buried nearly to its middle in the earth. The *sporidia*, it is said, spring out actively upon the plant being shaken. This latter circumstance, however, is not peculiar to *Peziza macrocalyx*, but is common to other Pezizæ. We have seen the sporidia of *Peziza macropus* rising like a white cloud from the surface of the *hymenium*, looking as if some fairy were burning incense in the hollow of its beautiful cup.

At the close of the volume we find some interesting observations upon the peculiar red spots which are occasionally seen upon articles of food, and which Dr. Fresenius, in common with most other observers, considers to be of vegetable origin. These spots (called by the Germans *Blut im Brode*) have been the subject of much difference of opinion amongst scientific men. Ehrenberg attributes the appearance to an animalcule which he calls *Monas prodigiosa*; Montagne considered the spots to be Algæ, of the genus *Palmella*; Dr. Sette, of Padua, expressed his opinion that the substance was a fungus, and called it by the somewhat unpronounceable name of *Zoogalactusa imetropha*.

A late writer in the 'Gardener's Chronicle' is of opinion that the matter in dispute is a fungus allied to the yeast fungus or *Torula cerevisiæ*, as it was formerly called, which latter fungal is now supposed to be only a myceloid state of the common mould *Penicillium glaucum*.

We have lately had an opportunity of examining a fragment of bread affected in this manner, which has been kept for several years. It has the appearance of having been soaked in plum-juice; and our observations of it under the microscope, so far as we have yet been able to carry them, accord in the main with those of the author. The latter procured some potatoes which were affected with these red spots, and he gives an account of his experiments and observations, from which the following is an extract:—

"I took four boiled potatoes and placed them in a drawer, having previously rubbed two of them slightly here and there with the red substance. After about twenty-four hours, the two potatoes which had not been rubbed, and which had not been in immediate contact with the other two, were affected with fresh spots of the red substance, whilst the spots upon the two which had been rubbed had increased in extent. The spots showed themselves in the form of irregular groups of blood-red drops of different size, which in some places were distinct, and in others had run into one another. The individual bodies of which the spots consist are mere molecules, their diameter varying from 1-2000th to 1-4000th of a line. They are mostly round, occasionally oval, and sometimes slightly constricted in the middle by way of preparation for increase by division into two small round cells. By far the greater number of them, when brought under the microscope in a drop of water, remain at rest; they lie close together in large numbers; when they are more dispersed in the fluid they have a motion which is not distinguishable from ordinary molecular motion. When the drop of water moves they are carried mechanically over the stage like other molecules, and when this motion ceases, they remain at one spot in a sort of quivering state until a fresh current carries them in another direction. * * * * If the eye is kept carefully upon a part of the stage where the small bodies are thinly dispersed, it will be observed that they passively follow the current of the water, nor, when the current has become sluggish, or has even altogether ceased, are individual bodies ever seen to detach themselves from the group and take a contrary direction, which real monads would do with great activity."

With this extract we must conclude our notice of this interesting book. We strongly recommend such of our readers as have good instruments and time at their command to turn their attention to the subjects treated of in it. We can assure them from experience, that they will find the study a most engrossing one, and the objects of it *not* unsightly; and their assistance may render good service to Mycology, by helping to dispel some of the mists and clouds which still envelope many parts of this most interesting science.

BOTANICAL LETTERS TO A FRIEND. By DR. F. UNGER. Translated by DR. B. PAUL. London, Highley.

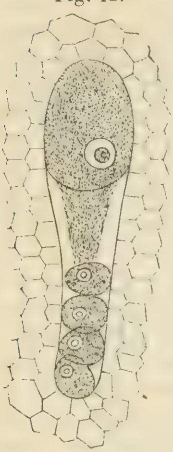
NOTHING distinguishes our age more than the tendency that exists on the part of those who cultivate Science to diffuse its truths as widely and extensively as possible. The age of conservation in science is over, and the dream of those who would confine it to our universities, colleges, or royal societies, is for ever dispelled. Much as we are indebted for this condition of things to the practical sagacity of Englishmen, and can boast of a large popular scientific literature in our own language, we have yet to offer a large meed of praise to the professors of science in the German universities, for their attempts to teach beyond the limits of their college walls, and to gain an audience amongst the hitherto despised Philistines. To Humboldt, Liebig, Schleiden, Buff, Moleschott, Schacht, we are indebted for treatises tending to diffuse a knowledge of the highest truths of science, and to these we may now add the name of Unger. These Botanical Letters are an indication of the progress of botanical science. Let any one compare the manuals and introductions formerly put into the hands of students with this volume, and they will see how great has been the advancement in correct observation and generalization within the last few years. Instead of the mere dry details of the forms of the organs of plants, we are introduced to a knowledge of their intimate structure and the laws of their developement; instead of an absurd comparison of plants and animals, and rude guesses at the functions of the former from a study of those of the latter, we have the physiology of the plant, established upon observations made upon its own structure alone. To all who have attended to the study of the physiology of plants it is known, that the use of the microscope, and that alone, has produced this change and tended to this advancement. The structure of organised bodies and their functions can no longer be studied without this instrument; and it is only as it is skilfully used that we can expect to attain a true knowledge of the laws which govern the existence of organised beings.

In noticing Dr. Unger's work, we can only refer to the more especially microscopical department of his work; and here, as in all others, he has displayed the knowledge of one who has faithfully kept up with the progress of science, and has a claim to be heard as a teacher. We give two extracts as examples of the style and matter of the work. Our first is from the history of the developement of the plant.

"Let us now examine, somewhat more minutely, this production of building-stones by the plant. As we have already seen, these building-stones of the plant are, properly speaking, not solid homogeneous masses, but variously-shaped membranous utricles, vesicles, etc., filled with soft substances and liquids of all kinds. Each vesicle which is employed in the building up of the plant is, without exception, formed in the interior of an already-existing cell: when its formation is complete, it is at once pushed out, and laid in the place which it is destined to occupy. Neither windlass nor pulley is requisite; the whole operation takes place so readily, and, as it were, spontaneously, that we may well be astonished that such a thing is possible. We will now examine how this is accomplished.

"First, the old cell swells up considerably, increases in circumference, grows; but it must be remembered that it is not a mere growth that takes place here. As in a pregnant animal, new cells are formed in its body; when these have advanced so far in developement as to possess all the organs requisite for their independent existence, they are set at liberty; and the mother-cell, which, during the continuance of these processes, not only devotes the whole of its contents to the formation of the brood of daughter-cells, but likewise suffers a diminution of its membranous envelope in consequence of the progressive enlargement, continues in a kind of dream existence, and is at last entirely consumed. Fig. 12 represents a remarkably large bag-shaped cell from the seed-bud of the biennial (*crepis biennis*). It is situated between parenchymatous cells which do not any longer enlarge. This mother-cell contains five secondary cells, of which the uppermost is further developed than the others. The daughter-cells originate, therefore, altogether at the cost of the mother-cells: their existence involves the death of the latter. Something very analogous is presented in the propagation of certain insects; the pregnant animals gradually increase in size to such an extent that they appear more like bladders. All the organs, all the functions of the mother, are directed to the production of her young, and after their birth there remains scarcely anything more than a dry, rent membrane. May not, therefore, the formation of cells by the plant likewise be termed a generation? And what else is the entire plant formation, with its myriads of cells, than the result of a continued generation of its elementary parts?

Fig. 12.



"After this insight into the progressive developement of the stones, how different becomes the aspect of the masonry of the plant perpetually being renewed, and, as it were, growing out of itself! Here all kind of analogy with architectural operations ceases; we are unacquainted with any work of human hands or human invention which is even in the remotest degree similar to the building up of the plant temple. It is an invisible hand which inscribes upon its walls words as mysterious as those once written in the palace of Belshazzar. Nevertheless, we will follow up the formation of the cell still further."

The author then proceeds to describe the further developement of the cell, and subsequently the tissues which it forms. In the concluding chapter he again recurs to the functions of the cell, and some of those recondite properties it possesses, of the nature of which we have yet but an indistinct notion.

"When the uninjured cell is observed in full operation, as it appears in its youth, no difference can yet be detected between its contents and boundary; but in the content itself, there very soon appears a vital centre, produced in the form of a tiny vesicle. This leaflet, named nucleus, causes, very soon after its first appearance, a remarkable separation of the half-liquid contents. A tough, liquid, granular substance, detaches itself from the residue, which displays a more watery nature. This, called protoplasm, unites itself as well to the vital centre as to the periphery, and thereby binds both together with many radiating, simple, and branching threads.

"It is a charming spectacle to observe in the so-far perfected cell, the active flow of this vital sap from the centre to the periphery and back.

"The most manifold motions, even in the most opposite directions, are seen in close proximity in the same thread-currents. All is activity and motion in this protoplasm; nevertheless, the remaining part remains motionless, and is only here and there drawn into the current of the stream. These streams are moved by no pulsating veins; there is no pumping apparatus which forces them from the centre of the cell and back again. This marvellous substance, this self-moving wheel, is a protein substance, consisting of the same nitrogenous compound that is present in every animal.

Fig. 37.

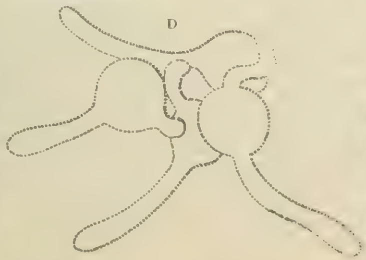
B

C

"In some instances (but, as far as our experience yet extends, only in the lower plants,) the developement of this protoplasm advances beyond the exterior boundaries.

"It is not a mere motion of the liquid mass which goes on, but a developement of half-solid thread-like processes, capable of performing other motions than those of circulation. When such cells are set at liberty by the opening of the mother-cells in which they have been formed, they commence motions entirely independent of the latter, and when they are in water, swim about freely in it.

"Fig. 37 is a young plant of *Vaucheria clavata*, Agdh, at the period of the ripening of the fruit; that is, when the first germ-cell is pushed out. B is the germ-cell after being detached from the mother-cell, and floating freely. The extremely delicate ciliate processes of the membrane by whose vibrations the motion is effected are shown at C, magnified a thousandfold. They will be seen to be of equal size, and to cover the whole surface of the egg-shaped cell. D is a group of young germinating plants of the same kind, less highly magnified.



"The thread-like ciliæ upon their surface serve at this time the purpose of rudders, in the same way as the ciliæ and hairs of infusoria. Neither the form, the chemical nature, nor the power of contraction, without which the ciliate or quivering motion would be inconceivable, distinguish these vegetable cells from analogous animal forms, and, to connect them even still more closely with the latter, dots of colour present themselves as indications of organs sensitive to light.

"But this burst of life in the plant-cell is of short duration, and ceases in the ciliate swarming cells sooner than in others. After a short time these feelers, with which they strive to penetrate further into the exterior world than by the roots, are drawn in, the cell again becomes smooth, and cellulose is soon deposited upon its surface, which renders the incarceration complete. The vigorous play of motions does indeed continue for a longer or shorter period, even under the rigid envelope of the cell-membrane, and it is at this time especially that the reproduction of the cell goes on by the formation of new vital centres; but this last spark of life is soon extinguished, and the victorious forces of molecular attraction, affinity, &c. reduce the cell gradually within the domain of inorganic nature.

"Nevertheless, it appears that the manifestation of this vigorous vital action is reserved to only a few cells in the body of the plant, although not for its entire duration, at least for some time. These are the reproduction cells. While all permanent cells are capable of manifesting their higher nature only in the motion of their juices, the reproduction cells break all the bonds which govern the former, and, even although only for a few moments, enter into the most unrestrained activity.

Fig. 38.

B A C

"In some series of the vegetable kingdom, in which, as we have seen, the duality of sex does not appear to be fully developed, such reproductive cells become swarming cells in other series; the one reproduction cell does not acquire such a degree of freedom, but the second one moves the more unrestrictedly, extended lengthways, as spermatozoids, whose motions are far from being understood in respect to their relation to fructification (fig. 39).

"A, fig. 38, represents a spermatozoid of *Asplenium septentrionale*, detached from the mother-cell in which it was formed, and moving rapidly in water by means of the ciliæ. It is magnified × 1200-fold. B, spermatozoids of *Equisetum arvense*, magnified × 500-fold. C, a spermatozoid just escaping from the mother-cell.—(*W. Hofmeister*.)"

From these extracts it will be seen how well Dr. Paul has performed his task of translation. The work is illustrated with numerous carefully executed woodcuts, and, like the series to which it belongs, is cheap and very creditably got up; and we know of no better general introduction to the biology of plants than these Botanical Letters of Dr. Unger.

Anatomisch-Histologische Untersuchungen ueber Fische und Reptilien. Von Dr. Franz Leydig. (Anatomical and Histological Observations upon Fishes and Reptiles. By Dr. Franz Leydig), Berlin, 1853.

We have more than once had occasion ('Quar. Jour. of Micros. Science,' Nos. IV., V.) to refer to the labours of Dr.

Leydig, who has done more than any one, of late years, towards extending our knowledge of histology to the lower Vertebrata and to the Invertebrata.

His essays are characterized not only by the accuracy and elaborateness of their details, but by their breadth of view and the extensive knowledge of Anatomy and Physiology which they indicate. We are acquainted with no more important contributions to the departments of Zoology with which they are concerned than his Memoirs on *Lacinularia;* on the larva of *Corethra;* on *Argulus;* on the anatomy and development of *Paludina;* and on the so-called muciparous organs of Fishes, which have appeared in Müller's Archiv, and in Siebold and Kölliker's Zeitschrift, in the course of the last five or six years. Of equal value are his 'Contributions to the Microscopical Anatomy and Development of the Sharks and Rays,'* published as a small independent work, in 1852; followed up in the present year, by that now under notice. Dr. Leydig must be a hard, as well as an able worker; for his present effort is more considerable than any which have preceded it, and assuredly does not fall below them in scientific value or interest.

It is divided into two sections; the first of which treats of the histology of the Sturgeon, the second, of that of reptiles in general.

Dr. Leydig's book requires and deserves careful study, and we must be satisfied with drawing attention to a few of the chief matters of novelty and interest which it contains.

Verifying Ecker's discovery, that the pituitary body of fishes is a vascular gland, he finds that in the Sturgeon, the pineal gland is of a similar nature, resembling the thyroid, however, rather than the other vascular glands.

As might have been expected, from the zoological position of the Accipenseridæ, the 'muciferous' tubes and *ampullæ* of the Sturgeon, are intermediate in structure between the corresponding organs of cartilaginous and osseous fishes.

A new light is thrown upon the nature of the supra-renal capsules by Leydig's discovery, that in Sturgeons, and other cartilaginous fishes, Batrachia, and Reptilia, they belong to one series of organs with the so-called 'axillary hearts;' with certain peculiar appendages, discovered by Leydig, upon the ganglion of the sympathetic; and with the fatty-looking yellow bodies on the blood-vessels and in the kidneys themselves.

In Plagiostomes, in *Chimæra*, and in the Sturgeon, the net-

* 'Beiträge zur Mikroskopischen Anatomie und Entwickelungsgeschichte der Rochen und Haie.' Leipzig, 1852.

works of the hepatic cells lie within canalicular cavities in a kind of connective tissue, by which they are supported. Leydig considers this to be evidence that the liver essentially resembles other glands; a conclusion to which, however, the evidence he adduces would not lead us.

The author's investigations into the structure of the spleen of fishes and reptiles are of the highest importance: they fully confirm Remak's views, and tend to the conclusion that the difference between a spleen, a Peyer's patch, and a lymphatic gland, is but one of degree; while they greatly extend our knowledge of the relations between the blood, vascular, and lymphatic systems.

The position and anatomical characters of the *Thymus* and *Thyroid glands* are determined for fishes and many reptiles. Histologically they invariably present the same differences as in man.

Leydig doubts the existence of cæcal terminations in the *tubuli uriniferi* of fishes. His inquiries into the development of the genito-urinary system of Amphibia and reptiles are of the utmost value, and tend to clear up many difficulties in the morphology of these organs, not only in these but in the higher Vertebrata.

Our space prevents us from further analyzing Dr. Leydig's work; but we trus twe have said sufficient to justify the high opinion of its merits which we have expressed.

Der Baum. Studien uber Bau und Leben der hoheren Gewächse. Von Dr. H. Schacht. Berlin, 1853.

We have already noticed Dr. Schacht's previous works on the microscope as applied to botanical subjects, and his more important one upon the Plant-cell—and have again the pleasing task of directing our readers' attention to another from his indefatigable pen—which, like the former, contains a vast amount of matter interesting especially to the microscopic observer.

Like the previous works by the same writer, the present is the result in great measure of independent research, and is therefore the more interesting and useful, and contains the results of valuable investigations which were made during a prolonged residence in the forests of Thuringia.

It gives a considerable series of detailed, recent inquiries respecting the germination and growth of Forest trees, the anatomical conditions of the wood, the formation of cork and bark, on the structure and growth of the leaves and roots, on the formation of branches and of the buds, and lastly, on that of the blossom and fruit of most of our common useful trees.

NOTES AND CORRESPONDENCE.

On the best Form of Micrometer for the Microscope. — In the 'Journal' for October last, page 51, is a paper signed H. C. K., on the best form of Micrometer for the Microscope, in which Mr. Quekett and myself are said to have disparaged the use of the cobweb micrometer in advocating that of ruled glass. As Mr. Quekett's time is fully occupied, perhaps you will allow me to make a few remarks. The author is correct in saying that, " as far as regards *manipulation*, one form of micrometer is just as simple as the other," but the statement that 1-1000th of an inch is a common amount of error in the ruled glass can only be explained by supposing that the printer has omitted a cipher in the denominator of the fraction ; for the mean of nine observations on such glasses could at best only reduce the error to 1-9000th of an inch—a quantity wholly inconsistent with the accuracy which he advocates. The error in a carefully-ruled glass rarely amounts to 1-10,000th of an inch, and therefore the plan of finding the value of the cobweb micrometer by a mean of several observations will give a sufficiently near approach to absolute accuracy ; but it does not seem to strike H. C. K. that the same means are equally applicable to the glass micrometer when placed in the eye-piece.

In stating that 1-800,000th of an inch can be *read* by a cobweb micrometer and a 1-8th object glass, I assumed that the eye-piece was of Ramsden's form, that the screw had 100 threads in an inch, the divided head 100 divisions, and that the body of the microscope was 10 inches long. Messrs. Powell and Lealand I believe use a negative eye-piece and a shorter body, and they may probably have adopted a coarser screw, but the reading still appears to exceed the power of observation four times, and justifies the assertion that the *readings* (not the *measurements*) of such an instrument are " unnecessarily fine."

The finest eye-piece micrometer that I use is ruled to the 1-250, and with the 1-12th object-glass reads 30-1000ths of an inch. Now, as one-third of a division is easily estimated, the power of reading approaches very near to the limit of observation ascertained by H. C. K. Let us suppose that an error of 1-1000th of an inch, or one-fourth of a division (an amount that should never be tolerated), exists in such a micrometer, it will only affect the measurements made with the 1-12th object-glass to the 1-120,000th of an inch—a quantity scarcely appreciable, and much less than the errors which are unavoidable in determining the value of the readings, even when a mean of more than nine observations is taken.—H. C. K. seems to have forgotten that, when a micrometer is applied to the magnified image in the eye-piece, its errors are diminished in proportion to the amplification effected by the object-glass.

The inquiries for a cheap form of microscope, which I constantly hear, make me think that the difference between 4*l.* and 1*l.* for an adjunct to the instrument, would in many instances be a serious obstacle to the use of any means of minute measurement; and it is with the view of placing these means within the reach of all observers that I have advocated ruled glass. I cannot admit that I have disparaged the more expensive instrument, having merely asserted that the readings obtained by it are far beyond the power of observing; and that, therefore, it is not so very superior to the cheaper form as it appears to be. Its superiority consists principally in this—that there is nothing between the object-glass and eye-piece to injure the definition; whereas the ruled glass produces a perceptible (though very slight) deterioration in the sharpness of the image, and therefore *to that extent* diminishes the accuracy of observation.

To induce observers to make accurate measurements, which is the aim both of H. C. K. and myself, it is not sufficient to place an instrument in their hands, they must be taught to use it *with little trouble.* The following directions, which are applicable to every form of eye-piece micrometer, will tend to promote that object.

Having screwed on an object-glass, lay a micrometer on the stage, and get a distinct view of it. Apply the eye-piece micrometer; and taking the divisions on the stage, which occupy the middle third of the field, draw out the draw-tube until the corresponding reading in the eye-piece amounts to some convenient decimal number, or at least can be readily translated into one by a simple mental process, such as multiplying or dividing by a single figure. Then examine the divisions on different parts of the stage micrometer, and if you please, on different micrometers. Should they be found to differ, which they probably will in a trifling degree, adjust the draw-tube on that part of the scale which gives the mean value. The draw-tube being graduated, the point at which it now stands, with the reading indicated, should be entered on a card in a line with the object-glass used.

If the fine focal adjustment moves only the object-glass, as is usually the case, it should be placed about midway during this operation, that its subsequent movements may have as little effect as possible in altering the reading.

The adjustment for the thickness of covering glass, which all objectives of large aperture must have, changes the value of the eye-piece micrometer so much that it requires a special provision. The method which I adopt is to adjust the draw-tube in two positions—one when the object-glass is arranged for an uncovered object, and the other when screwed to the full extent of its motion. These two positions of the draw-tube being entered on the card, it is easy to set it so as to correspond with any intermediate point that the lens may be adjusted to.

When this has been done for each object-glass, the operation of measuring is an exceedingly simple one. Draw out the tube to the point corresponding to the object-glass in use, and the measurements are at once obtained in decimals of an inch, without the trouble of referring to a table of values. By this means the micrometer above mentioned (250 in the inch), when inserted into a negative eye-piece, is made to read 5000 with the 4-10, 10,000 with the 1-4, 20,000 with the 1-8, and 30,000 with the 1-12th object-glasses. By merely multiplying the first of these numbers by two, and dividing the third and fourth by two and three respectively, the measurements are at once set down in decimal fractions.—GEORGE JACKSON.

Structure of Anacharis alsinastrum.— In the last Number of the 'Microscopical Journal,' appeared an interesting account of the rotation in the cells of the new water-weed—*Anacharis alsinastrum.* Mr. Lawson there pointed out the particular cells in which the current may readily be seen, viz., the elongated cells around the margin of the leaf and those of the midrib. On examining the leaf with polarized light, these cells, and these alone, are found to contain a large proportion of silica, and present a very interesting appearance. A bright band of light encircles the leaf and traverses its centre. The teeth-cells at the edge of the leaf do not contain silica, but are firmly planted upon the silica band: this evidently gives great support to their clinging property. In fact the leaf is set as it were in a framework of silica.

As the silica is only found in the cells in which rotation is visible, may not its deposition be in some way connected with the circulatory movement? The large amount of silica will account also for the brittleness and weight of the plant, which sinks to the bottom of the water on being cut, and does not float like other water-weeds. The large proportion of silica may also render "the pest," a useful manure for some kinds of land.

The specimen sent with this communication has been boiled for a few minutes in equal parts of nitric acid and water: by this means a portion of the vegetable tissue has been got rid of, and the silica rendered more distinct, without at the same time destroying the form of the leaf.—FERGUSON BRANSON, M.D., *Sheffield*.

Rotation in Anacharis.—In an extract given in the 'Microscopical Journal' (vol. ii. pp. 54, 55), from my paper in the 'Scottish Florist,' on the "Rotation of the Cell-sap in Plants," a remark occurs which may be inexplicable to your readers. In describing the movements seen in the cells of *Anacharis alsinastrum*, it is stated, "*The arrows show the direction of motion.*" I do not suppose that any reader of the Journal will be so inexperienced as to look for arrows in the plant's cells; but it may be as well to mention that the remark applied to a wood-cut which appeared in the 'Florist,' but was not repeated in the 'Microscopical Journal.' I may also take this opportunity to add to the observations quoted, that while nuclei are generally found in the cells of *Vallisneria* and *Chara*, which exhibit rotation (and in *Chara* the nucleus is also in motion), there are no nuclei to be seen in the cells of *Anacharis*, at least I have not met with any.—G. LAWSON, 7, *Hill Square, Edinburgh*.

Binocular Microscope. — Since the publication of the last 'Journal' which contains the papers of Professor Riddell and myself on the 'Binocular Microscope,' I have received so many inquiries relating to this instrument that I feel myself called upon to make some further remarks on the subject, as I find that many are under an erroneous impression of its utility and effectiveness.

At the request of some members of the Council of the Microscopical Society, I was induced to bring forward the substance of my experiments, and at the time my paper was read these were still in progress; for this reason, such information as related to the effects of the instrument was somewhat indefinitely expressed, and allowed different observers to form their own judgment of its merits. This has tended to some extent to place the matter in a false position, which has no doubt been confirmed by the rather glowing account which the American Professor gives of the performance of his microscope in page 23 of the last Journal, and the description of the formidable aspect that our innocent wheel animalcule bears when viewed by his own instrument.

The binocular microscopes up to the present time have done but little else than afford a glimpse of the splendid and substantial appearance that nearly all microscopic objects may be expected to bear when the instrument is brought near to a state of perfection; but this remains to be effected, for all the attempts that have been made have failed in giving that degree of definition and distinctness which can alone satisfy the eye of a microscopist. This arises from reasons that I shall now briefly enter into:—

First, as regards a system of prisms for bisecting the emergent pencil close behind the objective: in every way that this has hitherto been carried out, the object is seen with only half the object-glass, and therefore the undue preponderance of oblique pencils thus obtained in the formation of each image is the cause of very unsatisfactory vision even of the easiest tests. If the minutiæ of structure cannot be seen with each eye separately, they will not be rendered visible by using them both together.

When the principle of the dividing prisms is directly followed, some improvement may be expected by using object-glasses which will give an equally-distinct image, with every portion of their acting surface. I believe the construction of such objectives to be within the pale of possibilities, as I have tried some (by traversing a small stop over the back lens) which approach near to the desired standard. Such a glass would also be of a superior quality for all the general purposes of observation. Where two prisms are employed for dividing the pencil behind the object-glass, some of the most valuable portion of the surface of the latter, straight across the diameter, is lost at the junction of the prisms. M. Nachet has most ingeniously remedied this defect, by using only one isosceles prism for splitting the pencil in both his binocular and duplex microscope.

If, instead of equally dividing the diameter of the object-glass, we can obtain a further portion past the centre both ways, or say two-thirds of the diameter in each eye, we should still have stereoscopic vision combined with greatly improved definition, for the objective would bear to lose the one-third portion without materially injuring its defining power. I have not yet discovered any optical contrivance that will effectually accomplish this: it appears to be almost a mathematical impossibility.

If a modification of the double-image prism were to be placed close behind the object-glass, having a sufficient separating power to bring the object into each eye, both images would be alike and would consequently produce binocular but not stereoscopic vision; but the latter might be obtained by cutting off some portion of the opposed sides of the separated pencils: I think that this experiment is worthy of a trial. I also believe that the loss of light arising from double refraction might be supplied by increased intensity of illumination.

I am convinced that a perfect result can never be obtained by bisecting the pencil behind the object-glass, unless it is improved in the way that I have above referred to. On the other hand half of the top lens of the eye-piece may be cut off without losing sight of the markings on test objects, or otherwise materially injuring the definition; but the greatest objection to separating the images there, is the contraction of the field, which is caused by reasons I have explained in my paper.* I am now seeking a remedy for this evil with some prospect of success. I have abandoned all attempts at making a binocular microscope with two objectives, as I found that I could not get even a pair of $1\frac{1}{2}$s to bear upon the object together.

Having now stated distinctly that the binocular microscope is at present far from being a perfect thing, and in what direction greater perfection is to be sought for, I trust that others will tax their ingenuity in effecting improvements; for even as it now is, all must agree, on seeing the magnificent perspective of some objects (particularly living and infusorial) that is obtained through its means, that no efforts should be spared in attempting to improve it. In this country I have worked at it almost single-handed, and have found the subject teeming with practical difficulties to an unusual extent; for which reason I think it is of importance that the instrument should be benefited by the ideas of others, in particulars of construction wherein I may have been found deficient. — F. H. Wenham.

On Measuring the Aperture of Object-glasses; and Remarks on their Adjustment.—The usual method of measuring the angle of aperture of object-glasses, suggested by Mr. Lister, and figured and described in Quekett's 'Treatise on the Microscope,' is sufficiently accurate for all apertures up to 100° or thereabouts, and I think for general usefulness cannot be superseded; but within the last few years the aperture of our highest powers has been increased to such an extent as to

* See 'Quart. Journal of Micros. Science,' for Oct. 1853, p. 9.

render their measurement by means of this instrument in some respects unsatisfactory. I have therefore to propose an addition which will enable us to obtain a more definite result.

On measuring an object-glass of very large aperture (say 170°) by the usual method, the extreme light becomes very faint, and disappears so gradually as to render it difficult to define the boundary between light and darkness, and any interior reflection from the rim of the stop, &c., may readily be mistaken for aperture. I have now before me a 1-12th in which light appears visible up to near 180° when measured by the ordinary means. In this glass every precaution has been taken to prevent internal glare: it is provided with two stops, and the whole of the inside of the tube blackened with smut lacquer; yet with this object-glass I can see through moderately thin glass. As it would be impossible to accomplish this with such an aperture, I consider it is a sufficient proof that there is something radically wrong in the method of measurement.

It occurred to me that it would be preferable to obtain a distinct image of a distant object through the microscope, and then to rotate it horizontally (taking the focus of the object-glass as the centre of motion), and stopping at the point when the object appears bisected, or half of it, is observed at both extremes, the range passed through will represent the angle of aperture. This will also have the advantage of showing the state of correction of the oblique pencils, by the degree of distortion appearing in the object, when seen at the exterior of the pencil of rays.

By the mode of measurement hitherto practised, the extreme light may appear sufficiently strong; but there is no means of judging at the same time whether this is consistent with a definite image or is really indicative of effective or well-corrected aperture.

There are several optical contrivances which would give an image of distant objects through the object-glass, but I have preferred the one here to be described because it does not interfere with the instrument commonly used. It consists simply of a biconvex lens of about one-quarter of an inch focus, set in a tube made to fit over the top of the lowest power Huygenian eye-piece, in a similar manner to that which Mr. Ross supplies with his microscopes, under the name of an examining-glass. The single lens should be in such a position that its focus is coincident with that of the emergent pencil from the eye-piece. As the focal distance of the latter will vary with every different power of object-glass employed, and also with the distance of the object, the single lens must have a sliding adjustment.

Having fixed the single lens over the eye-piece of the ordinary instrument for measuring apertures, with the object-glass to be tried, attached, place a candle in front in the direction of the axis of the tube at the same level, and at a distance varying from three inches to as many feet, according to the power of the object-glass employed, the highest requiring the candle to be very close, in order that the image may be of a sufficient size to be readily seen; next adjust the single lens by means of the sliding tube till the flame of the candle is most distinctly visible, then set the instrument at zero, and move it bodily round till the wick or centre of the flame is exactly bisected, and when this occurs again, on moving the index alone in the opposite direction the number of degrees passed through will indicate the aperture. If the object-glass is a good one, the image of the flame when seen at the extremes of the aperture suffers a remarkably small degree of distortion; but when this appears considerable, I have ascertained by direct experiment that the performance of the glass is improved by reducing the diameter of the stop, and so cutting off the exterior or unsuitable rays. I think that it will be easy to judge what portion of these are serviceable in assisting the definition, merely by the appearance of the flame at the extremes.

With this method of testing aperture, it is not absolutely necessary to be possessed of a special instrument for the purpose; for if the single lens is fitted over the lowest eye-piece of the ordinary microscope, and two candles be placed in front of the object-glass, and moved asunder till the half of each flame is cut off, a line drawn from the focal point to the centre of each flame will represent the angle of aperture. The axis of the microscope and the two flames should be in the same plane.

It is perhaps not generally known, that the position of the glasses for adjustment makes a considerable difference in the aperture (sometimes to the extent of 15° or more), this being always at a minimum when at the mark "uncovered." This fact should of course be remembered in measuring apertures. The 1-2nd and 1-4th may be placed midway between the two ranges, but the 1 8th and 1-12th should be taken when at the mark "covered," as we most generally use them when near to this point.

I have only lately used this method of testing the aperture of object-glasses, and therefore do not pretend to say that a better may not be found; but I have for a long time been of opinion that the ordinary mode is insufficient, and trust that some attention will be given to the subject.*

There is one very important point which I have seen almost totally neglected by some of the most experienced observers—I refer to the adjustment of the object-glass for the aberrations caused by the various thicknesses of glass used for covering different objects; for in order to obtain correct definition it is quite as necessary that this should be attended to as the focussing of the objective itself, and, unless performed with particular nicety, the late improvements that have been made in the way of increasing the aperture will be rendered *worse than useless*, not only in the highest powers but in the lower also. For instance, I have a very fine 4-10th, made by Smith and Beck, with a working aperture of 90°, which will not well show the markings on a severe test object such as the *P. angulatum*, until it is brought to the exact point of adjustment: they then become very distinct, almost instantaneously. I mention this because many suppose that in a power as low as this, an adjustment is not at all requisite.

The majority of observers with the microscope, generally content themselves, before screwing on the object-glass, with placing the vernier at the mark "covered," or in such a place between the two extremes as they think may chance to be the right one. But this practice is not to be depended upon, for even admitting it to be possible that a correct judgment may be acquired as to the thickness of the glass cover and the corresponding position of the adjustment, the latter oftentimes requires to be altered for different parts of the same object, for, frequently, the covers are not of uniform thickness, and various portions of the structure under view may be more or less deeply immersed in Canada balsam or fluid. The chief use of the marks "covered" and "uncovered" are, to enable us, by turning to the right or left, to see in which direction the lenses are either separated or brought closer together.

Although very easy in practice, there is some difficulty in giving any definite rules for effecting the adjustment of object-glasses with certainty and despatch; but when the indication of being out of adjustment can be clearly seen, one hour's practice will be more instructive than pages of description. For an *uncovered* object no directions are required;

* Since writing the above, I have been informed by Mr. De La Rue, and subsequently by Mr. Ross, that a similar method of measuring apertures to that here described has been used by Professor Amici for some years past, but I am not aware whether it has ever been published.

all that is necessary is to set the glass to this mark, which our first opticians are always very particular in placing correctly.

The following may serve as a guide for adjusting the object-glass for a general object that is covered. Select any dark speck or opaque portion of the object, and bring the outline into perfect focus, then lay the finger on the milled head of the fine motion, and move it briskly backwards and forwards in both directions from the first position. Observe the expansion of the dark outline of the object both when within and when without the focus. If the greater expansion or coma is when the object is without the focus, or furthest from the objective, the lenses must be placed further asunder, or towards the mark "uncovered." If the greater coma is when the object is within the focus, or nearest to the objective, the lenses must be brought closer together or towards the mark "covered." When the object-glass is in proper adjustment, the expansion of the outline is exactly the same both within and without the focus.

On the *Podura* and other marked tests, we may learn to adjust from a different indication. If the dots have a tendency to run into lines when the scale is placed without the focus, the glasses must be brought closer together. On the contrary, if the lines appear when the object is within the focal point, the lenses must be further separated.

I may state generally, that if there is a difference in the expansion of the coma surrounding an object when quickly brought equally within and without the focus, the glass is not in adjustment. This is more remarkable under opaque illumination, but most particularly with my parabolic condenser, which requires the object-glass to be very accurately adjusted; for if not so, the whole field of view sometimes seems filled with a kind of haze, and the outline of the object surrounded with a thick chromatic border, both of which immediately disappear when the adjustment is perfect, and leave the field intensely black.

The foregoing remarks apply only to our first-rate object-glasses, for if unskilfully constructed, or badly corrected, but little improvement can be derived from the adjustment.

I have for a long time objected to the mechanical method, by which all our opticians apply their adjustment, as there is oftentimes excessive friction, and that to such an extent as to endanger the centering of the object-glass, or to risk the displacing of the object in the endeavour to make use of it; and the range between "covered" and "uncovered" is performed in from one to two revolutions of the external collar. For this reason the motion is too slow, and the contrast between good and bad not sufficiently immediate to insure an accurate and quick adjustment. I think that the practical inconveniences of working this adjustment prevent its being more generally used by observers. As a remedy for these evils, I have always constructed my own object-glasses with an adjustment of the following description: instead of moving the outer lens to or from the others, as hitherto, I make the former a fixture, and move the two inner combinations, which slide easily in the interior of an accurately-bored external tube, similar to a pencil within a pencil-case. The motion of the inner tube and its lenses is effected by means of a pin fixed thereto, and first passing through a longitudinal slit in the outer fixed tube, and then in an inclined slit cut in an arc of 120° in the circumference of an exterior revolving milled collar; the whole range of adjustment being thus performed in one-third part of a revolution. I consider that this plan has these advantages: the front lens (which magnifies more than the others together), being fixed to the outer tube, retains the same distance from the object, which is, consequently, not entirely lost sight of during the adjustment, as in the other cases where this lens is moveable. The motion is performed with scarcely any friction, and the step from good to bad or better definition, is so immediately palpable that the right point of adjustment can be hit upon very readily. I can generally adjust my own glasses in a few seconds, whereas I sometimes most uncomfortably spend several minutes in wrenching round the adjusting collars of our professional makers, to obtain the same result. —F. H. Wenham.

Note to Mr. Huxley's paper on 'the Corpuscula Tactus' in the preceding Number.—My communication was in type before the publication of the last part of Siebold and Kölliker's 'Zeitschrift für Wissenschaftliche Zoologie' (Band. V. H. 1), which contains two important memoirs on the Pacinian bodies, the one by Dr. Franz Leydig (who deserves to be better known in this country as one of the ablest histologists of Germany), 'Ueber die Vater-Pacinischen Körperchen der Taube; the other, by Professor Kölliker, 'Einige Bemerkungen über die Pacinischen Körperchen.' Leydig points out several new localities in which he has met with Pacinian bodies in birds, particularly the interosseous spaces of the fore-arm and leg. His description of the structure of these bodies agrees very closely with that which I have given, but he lays a greater stress upon the transverse striation of the middle substance, and describes a narrow cavity in the middle of the so-called "central capsule," which he shows to be, as I have stated, otherwise solid, and which he regards as the expanded termination of the nerve itself. He further points out that his observations on the termination of the nerves in the invertebrata are in accordance with Wagner's doctrines, and takes occasion to explain at greater length his views respecting the analogy of the Pacinian bodies with the follicular organs and mucous canals of fishes.

Professor Kölliker admits that what he and Henle had previously named the "central cavity" of the Pacinian body in mammalia is solid, but still maintains the existence of a fluid between the outer layers, at least in the cat. He further points out that, in the mammalia at any rate, the central mass is not the expanded nerve fibril. He considers that the Pacinian body of birds is essentially different from that of mammals; a conclusion which is, I think, strongly opposed by the comparison of young mammalian Pacinian bodies with those of birds.

I must also express my regret that, having overlooked a paper by Dr. Waller, in the 'Philosophical Transactions' for 1848, I neglected to mention him as the original describer of the distribution of the nerves in the papillæ of the frog's tongue.—T. H. H., December, 1853.

Stirrup's Microscope.—I am particularly interested in the inquiry after a microscope, by a Thomas Stirrup, of the date, if I remember rightly, of the early part of the 17th century.

May I beg your kind assistance, to make an editorial inquiry in your next Number, whether any of your subscribers or readers possess the apparatus in question, and to solicit the name and address of the possessor?

About January, 1851, one was sold by auction in Stoke Newington, to a broker, as I have been credibly informed; he, again, states, that he disposed of it to a medical gentleman, in Bayswater, but here all trace of it has been lost.—F. G. S.

Is Coal a Mineralogical Species?—The question of the nature of coal having recently excited so much scientific interest, we give insertion to the following note.*

It has long been a vexed question with mineralogists as to what should be regarded as a mineralogical *species*, there being many difficulties in drawing an exact definition; they are unanimous, however, in regarding a crystal as the true natural-history *individual;* but as minerals more frequently occur in a massive state, they have extended the idea to those homogeneous bodies which exhibit the same chemical constitution throughout their mass. From the time of Werner to the latest works on

* See also, for further information, article "Coal," in English Cyclopædia.

mineralogy, Coal, though never found otherwise than in a massive state, has been classed in all systems as a mineralogical species. Glocker, in his Synopsis, the latest attempt at a precise systematic arrangement of the Mineral Kingdom, divides Coal into three species—*the non-bituminous* or *Anthracites*, and *the bituminous*, including two species, the *Black Coals* and *Brown Coals*, or *Lignites*. In his arrangement of the *varieties* there is a gradual passage from turf and semi-fossilized wood to the most perfect forms of coal.

Professor Quekett has examined, by the aid of the microscope, a vast number of sections of coal from various localities, as well as the Torbanehill mineral; and in concluding his paper at the last meeting of the Microscopical Society, December 21st, he proves, that whilst the Torbanehill mineral is a resinous-looking body, nearly homogeneous, and devoid of vegetable structure, Coal, on the other hand, is almost entirely made up of woody fibre, and not homogeneous in aspect, the interstices between the fibres being filled with a resinous body similar in appearance to the Torbane mineral. Having examined many varieties of coal taken at random from Professor Quekett's extensive series of horizontal and vertical sections, I come to the conclusion, that Coal is nothing more than fossil wood mineralized by bituminous or resinous matter; and that it holds the same relation to such bodies as wood opal does to *Opal*, and is, in fact, similar in character to any organic remains mineralized by silex, carbonate of lime, or other substances. Viewed in this light, *Coal has no claim to a place in any Mineral System as a species*; if this be admitted, it will prove the necessity of submitting massive minerals to microscopical examination, before their true character can be acknowledged, and before they can be admitted into any natural-history classification.

The Torbane mineral seems, from the homogeneous nature of its structure, to have greater claims to a position in a Mineral System; and, so far as our present knowledge as to its other characters extends, is probably a mineral species *sui generis*.

As it is well known that some minerals contain a vast amount of extraneous matter in *mechanical* combination, for example, the so-called Fontainebleau Sandstone, which consists of rhombohedral crystals of *Calcite* greatly impregnated with sand, may not the Torbane mineral be a true mineral species, whilst ordinary coal is the same, mechanically mixed with woody matter? Under the microscope, the eye cannot detect any difference between the matter of which the Torbane mineral is composed, and that which occurs between the interstices and pores of the woody fibres of coal. I incline, however, to the former opinion, that coal is nothing but a fossil wood; therefore, *not a mineral species*.—S. HIGHLEY.

Rainey's Light Moderator.—Several persons having applied to me desiring to know where they can obtain the apparatus described in the last Number of the 'Quarterly Journal of Microscopical Science,' for moderating the intensity of artificial light, I shall be obliged if you will have the goodness to say in your next Number, that the apparatus in question, can be obtained of Mr. Ross, optician, at a moderate cost.—GEORGE RAINEY.

PROCEEDINGS OF SOCIETIES.

Microscopical Society. *October 26th*, 1853.

George Jackson, Esq., President, in the Chair. A paper by Dr. Hobson, of Leeds, "On the Development of Tubular Structure in Plants," was read.

The author stated, that the object of this paper was to afford a practical illustration from Nature herself, in proof of the reality of that which has hitherto been but a theory in histology. For this purpose the structure of the hairs growing on the spurred petal of *Viola tricolor*, or Heartsease, is described as seen under the microscope, each hair affording in itself examples of the three processes considered by the author as requisite for the formation of tube.

A linear arrangement of united cells was described as forming the first stage. In the second, or transition stage, the passage from cell into tube was shown, which was considered as produced by the progressive change by absorption of the axis, or primary point of union of the cells with each other, constituting the hitherto impervious state of hair. The third stage was described as consisting of the tubular condition of the tube itself, gradually and continually increasing during the life of the plant by the absorption of cells. Thus in one and the same tube, and at the same moment, a beautiful example of Nature's simple mode of formation of tubular structure may be exhibited in this instance, consisting of a perfect cone.

A paper by R. S. Boswell, Esq., on *Actinophrys Sol*, was read (Transactions, vol. ii., p. 25).

A paper by Tuffin West, Esq., on the disease affecting the vine and other plants, was read.

The following gentlemen were elected Fellows of the Society :— Dr. Alphonse Normandy ; E. E. Lloyd, Esq. ; F. George, Esq.

November 23.—George Busk, Esq., in the Chair. A paper was read by the Chairman, On the Avicularian and Vibracular Organs of the Polyzoa (see Transactions, vol. ii., p. 26).

A paper was read by Professor Quekett, On the Microscopical Structure of the Torbanehill Mineral, illustrated with numerous specimens and diagrams.

T. D. Dyster, Esq. ; Dr. E. Smith ; J. B. Bevington, Esq. ; Conrad Loddiges, Esq. ; J. A. Beaumont, Esq., were elected Fellows.

December 21.—George Jackson, Esq., in the Chair. The conclusion of Professor Quekett's paper was read (Transactions, vol. ii., p. 34). Dr. Bristowe and W. Travis, Esq., were elected Fellows.

Physiological Section of the London Medical Society.

In the absence of any Society in the metropolis, for the discussion of purely physiological subjects, the London Medical Society have determined to set aside one evening in the month, for the discussion of physiological subjects. Three meetings have been held this

Session, and papers by Mr. Edwin Lee, Dr. E. Crisp, and Dr. Snow, have been read. These papers have been on subjects not necessarily involving the use of the microscope, and, therefore, not demanding further notice from us. We make no doubt, however, that this section of the London Medical Society will be made subservient to the increase of our knowledge of the functions and structure of organic beings, by the aid of microscopical research, and we hope to be often called upon to report its proceedings.

Royal Society of Edinburgh.

Tuesday, December.—Sir Thomas Brisbane in the Chair. Professor Traill read a paper on the Torbanehill "Mineral." He gave a detailed account of the local situation and geological position of the mineral in question; but these did not seem to supply any evidence as to the true nature of the substance. There could be no doubt but that it occurred in a coal formation, although he considered this as no argument in favour of its being coal, as many other minerals are found under exactly similar conditions. He then gave a description of the substance, dwelling particularly upon those points which seemed to distinguish it from household coal and cannel coals: in its colour it was stated to differ, as well as in its scratch, changing colour and not being lustrous. Its thin edge, when held betwixt the eye and a candle-flame, was shown to transmit light. It also possessed very considerable elasticity, causing the hammer to rebound, and differed in its fracture from ordinary coals. Specimens were handed round the room to illustrate the various statements made by the learned Professor, who also threw a block of the substance into the fire, to show that it did not burn like ordinary coal, but produced a bright flame, never going into a red heat, such as would render it suitable for the cooking of meat and other culinary operations. He also lighted a piece of it at a candle, showing that it burned freely in this way, with a bright flame, and produced a large quantity of smoke, the carbonaceous matter being deposited in part upon the piece itself. Organic structure had been traced in the substance, but he did not deem this of much importance. His various researches led him to the conclusion that it was not a coal, nor a parrot-coal, nor asphaltum, nor a bituminous shale, to which it nearly approached, but a distinct *mineral*, for which he proposed what he considered to be the most appropriate name—*Bitumenite.*

Professor Gregory stated, that he did not consider the substance in question similar to a bituminous shale, as the bitumen could not be extracted from it by any solvent with which he was acquainted. He did not see any chemical evidence for regarding it as differing from coal.

Professor Fleming contended that the substance, which he preferred to call the "Boghead gas coal," was a coal, and a candle coal. What is a candle coal? asked he, and why does it get that name? Because it burns like a candle, which *this* substance does. And what is a parrot-coal, and why does it get that name? Because in burning it makes a noise, "chatters like a parrot;" and the block burned in the Royal Society's grate demonstrated that the so-called mineral did the same. He did not consider it a mineral at all. It was not formed of certain elements in certain proportions, but varied much in different specimens; and from these and other considerations, it was not a mineral but a *rock;* and it did not differ more from coal than the Craigleith sandstone did from the Hailes sandstone, or the Redhall did from the Granton, or the sandstone of Salisbury Crags did from all four. With respect to structure, he had seen specimens of fossil wood wherein the slightest trace of organic structure could not be detected under the microscope.

Professor Bennet exhibited drawings, from the microscope, of true coal, and the substance in question. The latter exhibited markings, which he believed were regarded by the botanists engaged in the inquiry as cells. Now, they wanted the character of cells. The vegetable cell consisted of—1, a central nucleus; 2, a primordial utricle; 3, a proper cell-wall; with also included granules. Now, these appearances he could not find in any specimens of the substance submitted by him to the microscope. It was of great importance, he considered, to determine whether the markings shown in his drawing were cells or not, because the whole question hinged upon it, and if they were cells, then this substance showed a structure so profuse in cells as to be unparalleled in any other instance of coal.

Dr. Greville stated, that although the markings in the drawing looked very like cells, still he was not prepared to say that they were cells in their normal condition. They might have been once cells, and become altered.

Professor Balfour entered at length upon the question in its botanical bearings. There could be no doubt but that organic structure, both vascular and cellular, occurred in the disputed substance. He alluded to the opinions of Quekett and others who had examined it with this view, and thought that it was wrong to draw any argument against the substance being coal, from the fact that the structure seen was not of a certain kind; coal might be formed of coniferous wood, in other cases it might not. We know well that various plants differing from the *Coniferæ*—the Stigmarias and others—occurred in the coal formations, and it was but reasonable to suppose that these contributed to the coal deposits, as well as the *Coniferæ.* It was impossible, therefore, to set limits to the kind of structure to be found. As for finding the nuclei and primordial utricles of cells, we could not expect that. So far from the Torbanehill mineral being distinguished in general appearance from some kinds of coal, he had placed specimens of it and of certain kinds of coal, side by side, before a very competent authority, who could not decide which was the one and which the other. On the whole, therefore, he was satisfied that it was vain to attempt to draw any distinction between the Torbanehill substance and coal.—*Condensed from the "Commonwealth."*

DESCRIPTION OF PLATE II.

Figs. 1, 2, 3, 4, 5.—Avicularium of *Notamia bursaria.*

Fig.
1.—Avicularium in the closed state.
2.—Avicularium open, exhibiting the tactile (?) brush.
3.—Avicularium viewed in front, to show an opening below the beak.
4.—Avicularium in the early stage of developement.
5.—Closed avicularium, more magnified, showing the muscular structure and the internal organ.

Figs. 6, 7, 8.—Avicularium of *Bugula plumosa.*

6.—In the closed state.
7.—Open, showing the tactile (?) brush.
8.—Open, to show the diaphragm.

Figs. 9, 10, 11.—Avicularium of *Bugula avicularia.*

9.—Open, to show the tactile (?) brush (more magnified).
10.—Two avicularia grasping each other.
11.—One partially open (less magnified).
12.—Avicularium of *Scrupocellaria scruposa.*
13.—A small vermicule captured by two avicularia in *Scrupocellaria scruposa.*
14.—Avicularium of *Bugula plumosa*, in the early stage of developement.

DESCRIPTION OF PLATES III., IV., & V.

Illustrating Professor Quekett's Papers on the Torbane-hill Mineral.

PLATE III.

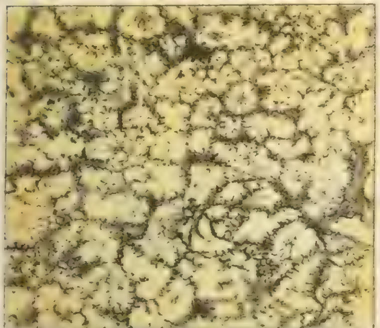
Fig.
1.—A section of the yellow variety of the Torbane-hill Mineral, as seen under a magnifying power of 130 diameters.
2.—A section of the dark variety of the Torbane-hill Mineral, as seen under a power of 130 diameters. The yellow circular masses exhibit a radiated structure; they form the combustible portion of the mineral, whilst the dark matter is the earthy ingredient.
3.—A section of the Torbane-hill Mineral, in which a specimen of *Stigmaria ficoides* is imbedded: every part of this plant can be readily distinguished from the mineral by its rich brown colour. Magnified 6 diameters.
4.—A portion of the same specimen magnified 50 diameters, showing how easily the smallest portion of vegetable tissue can be distinguished from the substance of the mineral.
5.—A section of the Torbane-hill Mineral, through which a thin layer of coal ran, which may be readily recognised by its brown colour. The yellow particles of the mineral in contact with the coal are of more or less oval figure.
6.—The powder of Torbane-hill Mineral, showing the yellow bituminous particles, and fragments of vessels.
7.—Ash of the Torbane-hill Mineral.

PLATE IV.

1.—Transverse section of the Brown Methil Coal.
2.—Longitudinal section of the same.
3.—Transverse section of the Black Methil Coal.
4.—Longitudinal section of the same.
5.—Transverse section of the Lesmahagow Cannel Coal.
6.—Longitudinal section of the same.

PLATE V.

1.—A section showing the Mineral and Coal in juxtaposition; magnified 3 diameters.
2.—Representations of the comparative sizes of the transverse sections of the brown elongated cells from various Coals, drawn by means of the camera lucida, by Dr. Adams, 70 diameters.
3.—Chippings of Newcastle Coal, showing dotted woody tissue.
4.—Ash of common domestic Coal, exhibiting the remains of a transverse section of wood.
5.—A longitudinal section of Coal from Lochgelly, showing its identity with a similar section of wood, from a drawing in the possession of Dr. Adams.
6.—Ash of Coal, exhibiting portions of siliceous cuticle and other fragments of vegetable tissue foreign to the coal.
7.—Powder of Breadisholme Coal, from a drawing by Dr. Adams.



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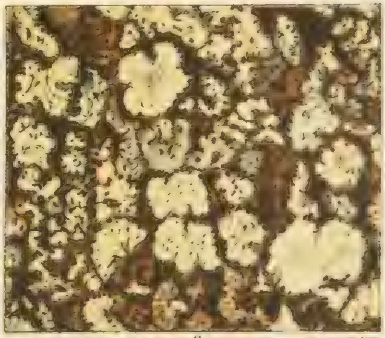

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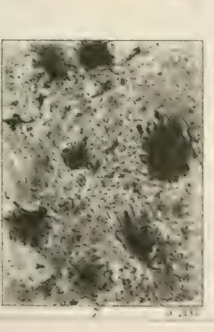

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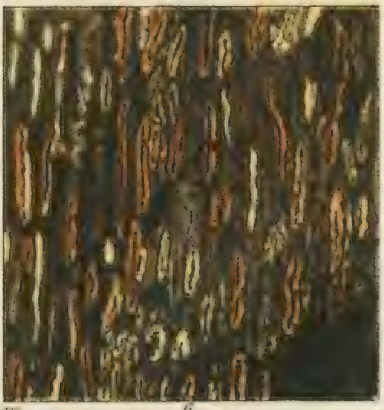

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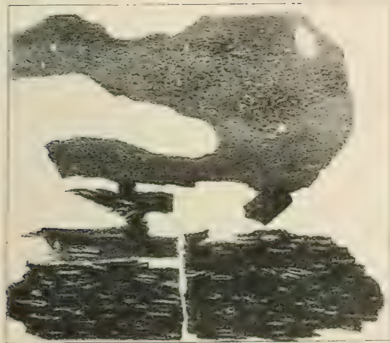

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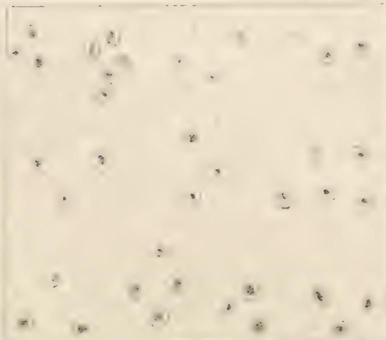

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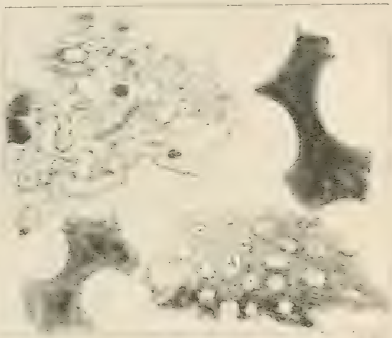

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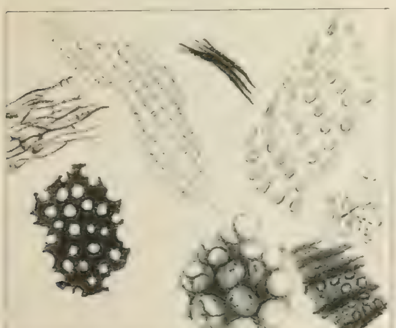

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DESCRIPTION OF PLATE IV.,

Illustrative of Dr. Gregory's Paper on the Diatomaceous Earth of Mull.

Fig.

1.—A variety of *Epithemia Argus.*
2.—A valve of *Epithemia gibberula ?*
3.—Seven varieties of *Eunotia bigibba*, Kütz.
4.—*Eunotia incisa*, n. sp.
4 β.—Variety β, with rounded apices.
  b. Front view.
5.—A *Cymbella*, qu. *?* a variety of *C. Helvetica ?* or a n. sp. ?
6.—Remarkable variety of *Surirella Craticula.*
7.—*Tryblianella augusta.* Three modifications.
8.—*Navicula affinis*
9.—*Pinnularia tennis*, n. sp.
10.—*Pinnularia undulata*, n. sp.
11.—*Pinnularia parva*, n. sp. *?*
12.—*Pinnularia*, uncertain, allied to *P. radiosa* or *P. peregrina.*
13.—*Pinnularia latistriata*, n. sp.
13 β.—Var. β, with front view.
14.—*Pinnularia exigua*, n. sp. *?*
15.—*Pinnularia divergens ?* four varieties.
16.—*Pinnularia stauroneiformis.*
17.—*Stauroneis rectangularis*, n. sp.
18.—*Gomphonema Brebissonii ?* n. sp. ?
19.—*Gomphonema* (?) *Hebridense*, n. sp.
20.—A variety of *Hemantidium Arcus*, or possibly of *H. majus.*
21.—Remarkable sporangial pustules of *Odontidium Tabellaria.*

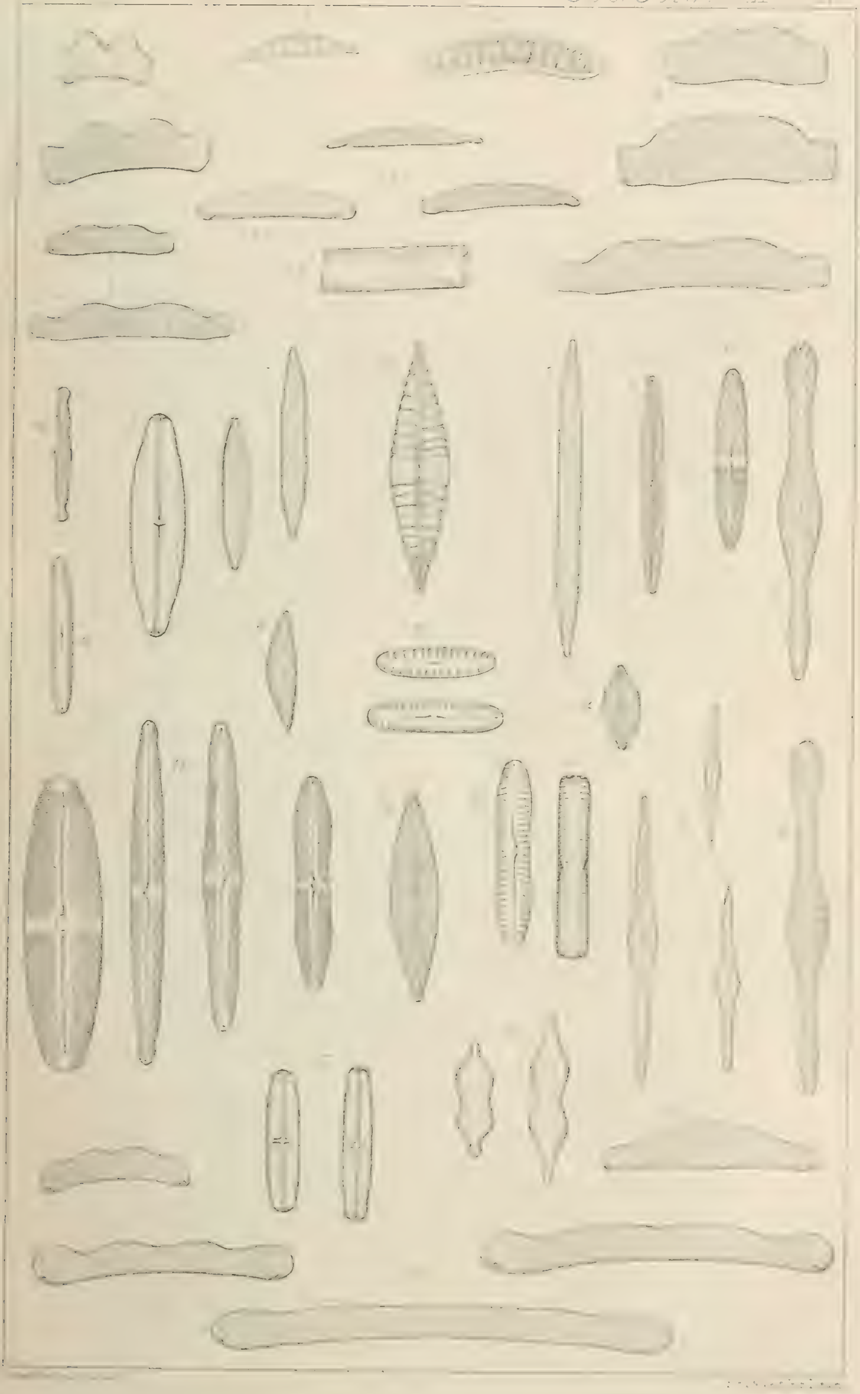
DESCRIPTION OF PLATE III.

Fig.
1, 2, 3, 4, 5, 6.—From the Malpighian body of the Sheep.
7, 8.—From that of Man.
9, 10.—From the Tonsil of Man.

The letters have throughout the same signification.

a. Afferent vessel.
b. Efferent vessel.
c. Traversing vessel.
c'. Capillaries.
d. Line of junction between Red Pulp and Malpighian body.
d'. Fibrous meshwork.
e. Red pulp.
f. Malpighian pulp.
g. Endoplasts.
h. Periplast.
h'. Cell-wall.
i. Epithelium of the tonsil cavity in the section fig. 9; a vascular papilla is seen extending nearly to its surface.

T.H.H. ad nat. delt. Tuffen West sc.



ORIGINAL COMMUNICATIONS.

On the Theory *of the* Illumination *of* Objects *under the* Microscope, *with relation to the* Aperture *of the* Object-glass, *and* Properties *of* Light; *with Practical Methods for special differences of Texture and Colour.* By F. H. Wenham.

Having on a former occasion written a paper published in the 'Transactions of the Microscopic Society for March, 1850,' entitled, "On the Illumination of Transparent Microscopic Objects on a new Principle," I should deem it an intrusion for again appearing on the same ground were it not for the reason that I have since that time effected some improvements, and it has become evident that the theory of the subject was too briefly explained in my first communication to give a general idea of my meaning in the way that I had wished to be understood. As I have no predilection for a theory that does not bear upon a practical result, in that which I bring forward I shall endeavour as much as possible to support it by experimental facts.

I have had the major part of this memoir by me for some time past, and intended to have sent it to the last Journal, but by chance Mr. Rainey made a communication on the same subject, and I have waited to learn the results of his investigations. As I do not agree with this gentleman in the inferences that he has drawn from observed facts, and the hypothesis that he has assumed is contradictory to the theory that experiment has led me to adopt, I find it necessary to make some comment on the contents of his paper. In so doing, I wish it to be understood that I do not question the accuracy of his observation in noting phenomena; but as it is oftentimes the fate of deductions of this character to be overturned by a few practical facts, I trust that he will take my remarks in good part, as they are made without the slightest feeling of personality, and for the mere sake of arriving at the truth.

Mr. Rainey first states, that when the *P. angulatum* is seen with a 1-8th of 150°, and illuminated with either the plain or concave mirror, or an achromatic condenser of small aperture (no matter how intense the light may be), "but little more than the mere outline of the object will be visible." This is true, and arises from the circumstance, that the angle of the illuminating-pencil is not sufficient to develop the full aperture of the object-glass, for I shall show that the latter is dependent on the angular pencil of the illuminator.

The author next proceeds to give a reason why the markings which cannot otherwise be seen on test objects are rendered visible by very oblique light: he attributes this to the structure being composed of parts of different density and refractive power, which cannot be distinguished from each other by central or direct light; but when this is thrown on obliquely at a certain angle, "total reflection" will take place from the denser portions of the objects (meaning the markings), while the lighter or least refractive parts will transmit the rays, consequently the one will appear dark and the other light. I may state with reference to this supposition, that light when incident at any degree of obliquity on diaphanous refracting bodies with parallel sides, cannot be made to suffer total reflection, either externally or internally, and admitting that an accidental or prismatic formation of the object did allow of total internal reflection, and even supposing that the difference of angle for producing this effect could be as great as between water and glass, or as $48° 36''$ to $41° 49''$, on increasing the obliquity of the rays only seven or less degrees, the markings would again become invisible, for *all* the light would then be reflected. I have never found this to occur, nor can I conceive how the generality of difficult tests can possess such a mirror-like consistency as to bring this law into uniform action; and therefore, from these and other causes, I cannot admit that this theory is at all tenable: my own demonstration will rest on more simple grounds.

Attempts have sometimes been made to draw the undulatory theory of light into the subject of microscopic illumination, but without any substantial reason, as it has in reality very little or nothing to do with it. One of the few instances in which its effects are palpably seen is in the case of two lines being blended into one, when the object-glass has not sufficient separating power; but to avoid this it is only necessary to proportion the aperture of the objective to the difficulty of the test. I expect soon to be in a position to exemplify this practically. I conclude these short remarks on this head by stating, that I make a distinction between this and the interference or inflection of light, for the latter sometimes affects the definition of objects, under direct illumination, very materially, and one of the methods that I have to describe affords an effectual remedy.

Mr. Rainey has given a lengthened series of observations on the appearance of a globule of mercury, when illuminated either with my paraboloid or Gillett's condenser. I do not consider that a globule, as being strictly opaque, is at all suited for testing an illumination intended for transparent objects, for however well it may show whether there is any light reflected, either from the brasswork, front of the object-lens, or thin glass cover, I cannot call to mind any ordinary object in which this takes place to such an extent as to create a false appearance. In reference to this Mr. Rainey says, "The glass cover having in this instance served the purpose of a Lieberkühn, has made the object appear in a false light; and as nearly all microscopic examinations are made on covered objects, the paraboloid is in this respect defective, and the error must always be allowed for." My own experiments have led me to a conclusion in direct opposition to this; and what is here pointed out as a defect is the very thing that is required for improving the definition; for, as I shall presently explain, many transparent objects require their upper surface to be illuminated at the same time that the light is transmitted through them. I have for a long time used a special appliance to the parabolic condenser for effecting this desideratum.

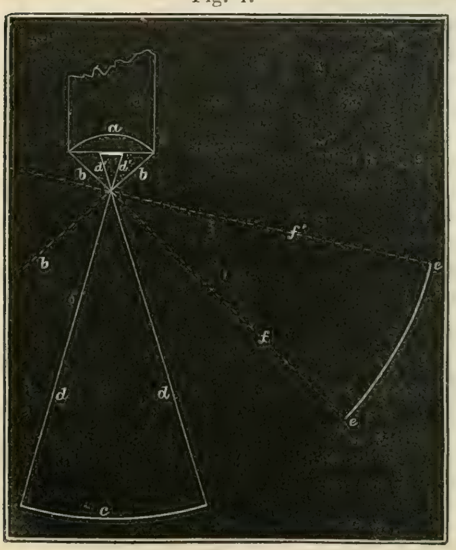
Mr. Rainey also brings forward the appearance of a globule of mercury when illuminated with the parabola as an example to show the fallacy of the "radiated light theory;" but I do not see that it at all affects the question, simply because it is one of those few substances that is incapable of radiating light, and therefore, under such circumstances, can only be seen by the direct reflection of incident rays from the luminous source; or, to make myself better understood, I will observe that the surfaces of mercury, highly-polished speculum metal, silver, or steel, are colourless and invisible, and from the fact of nearly all the light being reflected and none absorbed, are capable of receiving what is technically known as "a black polish," which term is very descriptive. We may also give an exquisite degree of polish to a speculum of gold or brass, but in these part of the incident light will be absorbed and *radiated* in every direction, for on viewing the reflecting surface from any position we may see the metal by its *colour:* and, as a rule, any speculum that shows a symptom of this is not at all suited to its intended purpose, for the surface should be so invisible as to give the appearance of looking into space. I should mention here, that refracting transparent bodies, such as oil-globules, air-bubbles, &c., if colourless, are incapable of radiating light, and therefore must be seen by the refraction or internal reflection of the rays from the source of illumination, and therefore cannot be brought forward as evidence against this theory.

I have thought it necessary to comment at some length upon Mr. Rainey's paper, as it is by far the most elaborate that has appeared on microscopic illumination, and I am desirous of divesting the subject of all unnecessary complexity; for I will suppose the microscope itself, when in perfect adjustment for spherical and chromatic aberrations, as an instrument that in its action upon objects differs so little in principle, from the effect of viewing with the naked eye, that all the combination of lenses may be considered for the time as forming part and parcel of that organ.

If in adopting this simple view of the case we do not infringe upon Nature's laws, why should not the illumination of the object itself be directed by the same rule; and reviewing the endless variety of forms and differences of structure, that we require to investigate by the naked eye, and by considering the position in which habit directs us to hold or place these for the purpose of causing the light to impinge upon them in the most proper direction for ascertaining their configuration and colour, to endeavour, as far as practicable, to apply a similar natural mode of throwing the light upon the microscopic object, in all instances where it is of a like contexture.

There are chiefly two circumstances to be considered which prevent this principle from being directly applied in all cases to the illumination of objects under the microscope. The first is that the effective aperture of the object-glass in a great measure depends upon the angle, size, and form of the illuminating-pencil; and, secondly, certain appearances pertaining to the properties of light are developed or rendered visible by the magnifying power of the instrument itself. The annexed diagram will serve to explain the first of these conditions: *a*, fig. 1, is the object-glass; *b b*, the extreme rays of the aperture; *c*, the mirror; *d d*, rays reflected therefrom.

Fig. 1.



Suppose a test object, such as the *P. angulatum*, to be placed in the focus of the object-glass, and illuminated as thus described, but little else than a mere outline will be seen, and the vision will be rendered rather worse than better by increasing the intensity of the light. The non-appearance of markings in this instance arises from the fact that the cone of light from the mirror has an angle only sufficient to cause a portion of the aperture of the object-glass to operate effectively, or the object is seen with the *central portion* only; for on viewing an object by intercepted light, with the intention of obtaining the maximum effect of aperture, all rays converging from every angle of the objective, and bearing upon the object, should be directly opposed by rays emanating from the luminous source; or, in other words, that the angular aperture of both the object-glass and achromatic condenser should be the same.

The next consideration is, when the object is illuminated under the conditions exemplified by fig. 1, what becomes of the excess of aperture, or such rays from the objective as are not opposed by the luminous source, or, so to term it, "looking into darkness," as those from *b b* to *d' d'*? There can be no doubt that this portion of the aperture gives a comparatively feeble image, arising from the *radiations* from the object; but this does not assist the definition under these circumstances, and cannot blend with the visible image because the central direct light is so intense in comparison as completely to drown the faint image formed by the exterior portion of the aperture of the object-glass.

It now remains to be shown how this radiated image may by itself be rendered visible. Unscrew the object-glass without disturbing the other arrangements, and paint an opaque patch of Indian ink in the centre of the front lens, with a camel's-hair brush, of a diameter corresponding to the space *d' d'*. Replace the objective, and it will be found that the patch has obscured nearly all the rays emanating directly from the mirror, and the object will then be seen entirely by the *exterior* portion of the aperture, and rendered visible by its own *radiated light*.* The markings now become developed by the apparently paradoxical fact of destroying an operative portion of the object-glass; but on considering fig. 1 it will show that instead of seeing the object with a less area of the objective, we are seeing it with a greater; for in the first example the central portion only came into operation, and in

* I have adopted the term "radiated light" in microscopic illumination, merely because it is descriptive and convenient, and as a distinction from light that is simply reflected from polished surfaces, though in other respects not perhaps philosophically correct.

the second the circumferential. I have found Smith and Beck's 4-10th of 90 a very convenient glass for trying this experiment; and by carefully centering the patch, I have obtained good definition of the object with a perfectly-black field; so completely can the stop be made to cut off the direct rays from the source of light

It is decidedly preferable to place the patch on the posterior lens of the combination, as its larger size renders the manipulation more easy. In my experiment it was about 1-20th of an inch in diameter. Those who are apprehensive of causing damage to an object-glass by trying this, may sometimes obtain an approximate effect by holding the point of a needle just over the object when in the centre of the field of view. It is due to Mr. Lister to mention, that in his paper on the "Achromatic Object-glass," published in the 120th volume of the 'Transactions of the Royal Society,' he observes that "some objects are even better seen when the central rays are obscured." I have not brought forward this fact because I think it is at all probable that any improvement can ever be obtained, either in the definition or illumination of objects, by stopping out the central pencil of the object-glass, but because the experiment affords a happy illustration of the relation that the angle of the illuminating-pencil bears to the aperture of the object-glass. In fig. 1, I have shown the concave mirror in use, and therefore the light is, to some extent, convergent; but with the plain mirror, all the foregoing effects are quite as, or even more, palpable.

In the illumination of objects by polarized light, when under view with high powers, for the purpose of obtaining the maximum effect, it is also requisite that the angle of aperture of the polarizer should be the same as the object-glass, each ray of which should be directly opposed by a ray of polarized light. As this circumstance has received but little attention, I will offer a few remarks on the *polarizing condenser*. This instrument is merely an ordinary achromatic condenser of large aperture, close under the bottom lens of which is placed a plate of tourmaline, used in combination with a superposed selenite or not, as required. The effect of this arrangement is in some cases very remarkable, bringing out strongly colours which are almost invisible by the usual mode: it is most useful for small bodies, such as the bitumen granules of the *Torbane-hill mineral*, &c.

To obtain a greater quantity of light, I would recommend that the illuminating lenses should be of the same diameter as a half-inch object-glass of large aperture, for it is oftentimes a fault with the achromatic condenser that these are too small;

for it should be remembered that in all condensing arrangements the intensity of the light as much depends upon the diameter of the lenses as their angle of aperture. The reason why this instrument has not received more universal adoption has been the scarcity and expense of good and large tourmalines; but now that Dr. Herapath has made the valuable discovery of producing them artificially, and the liberal manner in which he has published all the details of their manufacture to the world, will no doubt be the means of causing the polarizing condenser to come more into use.

Before quitting the subject of direct light, I will make one or two remarks on the known methods of obtaining it. The plain and concave mirror, or right-angle prism, are the most generally useful for glasses of low power and small aperture: in principle these are so well known as to need no comment; but for objectives of large aperture there is no instrument better adapted to its intended purpose than Gillett's condenser, for its series of stops allows us to enlarge or diminish the angle of the illuminating pencil, and therefore possesses the valuable property of causing the effective aperture of the object-glass to be reduced or increased, on the principle just explained, to suit the description of object under view, thus in a great measure correcting the false appearance caused by large apertures in objects of bulk.*

I will now proceed to show the effect of oblique illumination, and the dependence of the aperture of the object-glass on its direction.

If the concave mirror be moved slowly sideways from the axial line of the microscope as shown in fig. 1, the intensity of the light will gradually diminish, and after a certain time, the striæ or markings of the object (which is supposed to be the *P. angulatum* as before) will become more and more visible, and increase in distinctness, till the least obliquity of the illuminating pencil is in a direction a few degrees *within* the extreme angle of the aperture of the object-glass; *e e*, fig. 1 represents the mirror in this position. The external rays, *f*, are shown to enter the objective, but the remaining portion of the reflected cone of light to *f'* passes the exterior of the aperture without entering.

Every microscopist is familiar with the effect of this experiment, but not with the cause, which may be explained on precisely the same principle that I have exemplified by the objective with the central patch. The axial position of the mirror shown in fig. 1, as I before mentioned, does not permit

* For explanation of which see my Paper on "Binocular Vision," 'Quart. Journal of Microscopical Science,' Oct. 1853. No. 5.

the markings to be seen, because the intense light, and small angle of illumination, allows only the centre or a small portion of the aperture of the object-glass to act effectively. But on moving the mirror aside from the axis of the microscope, a part of the light illuminating the object at length begins to pass the extreme of the angle of aperture of the objective, and as the field becomes more obscure, the markings on the object itself increase in distinctness, simply because a corresponding portion of the aperture of the object-glass is called into action in collecting the radiations.

In the position of the mirror, *e e*, fig. 1, nearly the whole of the object-glass will be effective, as it will collect the radiations from the object from every angle within its aperture, and no portion of the latter will be annulled by the predominance of the intense rays from the luminous source. Practically it is found that there is a precise but different angle of illumination required for every aperture of the object-glass, in order to give the maximum of distinctness; or that will even at all develop the markings on difficult tests. For if we continue to increase the angle of the mirror, *e*, *e*, the object first acquires a pearly appearance, and is afterwards seen in a dark field known as "Reade's back-ground illumination," and is then rendered visible with a full aperture, entirely by its own radiations; but the markings have again become indistinct or disappear altogether, showing that it is needful to allow a small portion of the light from the source of illumination to pass into the object-glass, and through the object, that the striæ may either be rendered more visible by the rays that they intercept, or that the field shall be partly luminous.

There is one peculiar phenomenon attendant upon oblique illumination at certain angles in one direction, and may be described as a double image, or kind of overlying shadow, having in some instances markings equally distinct with those on the object itself. This appearance has been termed the "diffracting spectrum" among men of science. Taking the name to be descriptive, I sought for an explanation in the known laws of the diffraction of light, but these did not account for it, for on this theory I attempted to find the clue in vain. I have since traced the cause entirely to the mutual dependence of the angles of illumination and aperture, detailed in this paper. One image is caused by the radiations from the object entering one portion of the object-glass, and a different one by the object being directly seen by intercepted light with the other extreme of the aperture, thus giving the appearance of a double image. In proof of this, hold a card over that side of the front lens of the objective which receives the light from the luminous source, and one image will disappear: on reversing the card, so as to cut off the other extreme, the first image will appear again, and the second vanish.

The "diffracting spectrum" may also be produced at pleasure, in an object illuminated by direct light, and seen with a large aperture, by holding a needle or horse-hair before the front lens, so as to split the pencil into two parts. The above misnomer led me somewhat astray, as is oftentimes the case in matters of science, where terms are applied to phenomena that are not understood, but I have no doubt that the explanation that I have here given is the true one; if necessary, I could adduce further proof, but any one desirous of testing it will find other corroborative facts.

It is well known that besides the detriment to the definition of the object, caused by the double image just described, there are other errors created by oblique light from one side only, such as blending a series of dots into lines, &c. I suggested about four years ago, that these defects should be neutralized or "counteracted by a pencil of light of similar form and intensity thrown on the object at the same angle from an opposite direction." To effect this, I proposed that two of Nachet's prisms should be placed oppositely, underneath the stage of the microscope, with a dark well between them: this led me to the discovery of the principle of throwing the light on the object circularly, and preventing "the central direct rays" from the illuminator from entering the object-glass, by means of a dark well or stop, as illustrated by my parabolic condenser. My paper on this subject was published in vol. iii. of the 'Transactions of the Microscopical Society, March 1850,' but I regret that an insufficient detail of the principles involved, and a want of care in the expression, should have given rise to some insidious misconstruction; but as a proof of the utility and correctness of my theory, I have only to mention the many applications of it that have since that time come into general use, in the way of adapting central stops to the achromatic condenser, single and compound lenses, &c.

There are some appearances connected with oblique illumination, on closely-lined glass micrometers and tests, which would occupy too much space to enter upon here; but on a future occasion, I will make this the subject of a separate communication, as I shall soon have in hand an apparatus that will illustrate the changes of prismatic colours, with different angles of illumination and vision.

I have now to treat of such methods of illumination as do not interfere with the aperture of the object-glass, or affect the properties of light, and are, therefore, so simple in their action as to allow us to consider the microscope when in use as forming part of the eye, and to judge of the quality and direction of the light required by a consideration of the simple laws of nature, comprised in the illumination of all surrounding bodies of comparative form and texture.

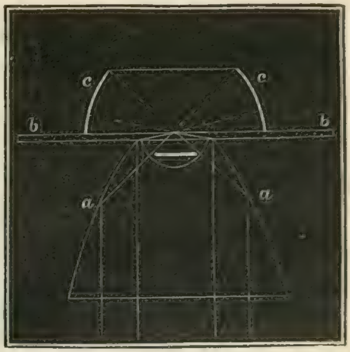
The first relates to ordinary opaque illumination, by merely condensing the light on the object, either by means of a Lieberkühn's lens, or side reflector: this is too simple to need any comment, and is mostly used for objects that are nearly destitute of transparency, and therefore cannot be seen in any other way.

The next is "Reade's background illumination," in which the light is thrown under the object in such a direction as to avoid or pass by the aperture of the object-glass, and give a black field. The structure under view, if large, must possess a sufficient degree of transparency to allow the light to enter into its substance, and be diffused or radiated therefrom in all directions. This method is but little used, on account of the light being feeble and partial, and because shadows are thrown in one direction.

When the stop of my parabolic condenser is raised, so as to obtain a black field, it is nothing more in principle than the "background illumination" carried round the circle, and the object, though transparent, is rendered visible by the operation of the same laws which enable us to see an opaque substance. I will therefore make use of the same illustration as on a former occasion. Lay a black wafer upon a sheet of white paper, and hold the wing of a fly a small distance above it; this evidently appears to be transparent, though no light passes directly through it into the eye. The object also shows an iridescent play of colours, from the decomposition of the light thrown upon its surface by the surrounding hemisphere. The last is a condition that I have found wanting in my paraboloid, for if we illuminate with it such objects as feathers, foot of a diamond beetle, of scales, &c. by artificial light, instead of brilliant colours, the surface will have a dull leaden aspect.

I have endeavoured to imitate the effect of the vault of the universe, by the method shown by fig. 2: *a a*, is the parabolic condenser, *b b*, the glass slide upon which the object is mounted; this also serves to support the *hemispherical reflector*, *c c*: the top of this is cut away so as to leave an opening of sufficient size to admit the nozzles of any one of the object-glasses. It will be seen that when the centre of the hemisphere and focus of the parabola are coincident, the light passing the object being incident at right angles on the spherical surface, will be reflected directly back again, and so illuminate the upper surface of the latter. The reflector that I have used is made of silver; but I propose to construct them of a zone cut from a blown-glass bulb, of about one inch in diameter, silvered by Drayton's process, and cemented into a brass cup with electrical cement. I believe that Lieberkühn's might be made in the same way, as they would then have the advantage of not being liable to tarnish, and would bear to be wiped. I have found the upper reflector just described to answer its peculiar purpose; and before quitting this head, I must express my desire of divesting the principle of all complexity, by again stating, that the parabolic condenser is merely a modification of opaque illumination applied to transparent objects, and that it gives a degree of intensity to the light not obtainable by any other method.

Fig. 2.



When strictly opaque bodies of minute size, such as shells, grains of sand, calcareous particles, &c. are illuminated, even without the upper reflector, they are seen after the usual manner of all opaque objects. On the other hand, when such substances as show colour only when exhibited as transparencies, such as bits of clear and well-polished stained glass, are placed over the paraboloid, they will be quite invisible; but if the surface be greasy, or covered with dust, the colour of the glass may be known by similar coloured radiations from these particles.

When we take a small object into the hand for examination, without any direct appeal to the reasoning faculties, a sort of instinct usually guides us to hold it in one of two positions, either against a dark body, or over a sheet of white paper, as may be most suitable for the development of the structure or colour of the substance. The first of these conditions is already imitated, in its application to the microscope, by the parabolic condenser, by means of which the light is thrown on the object "from every azimuth," as Mr. Shadbolt has aptly termed it.

The second position, which gives such delicate vision of some substances, does not appear to have been truly considered in its relative application to the illumination of microscopic objects. It is true that in the place of the mirror, a plaster-of-Paris, or enamel plate, has been applied, for obtaining the effect of a white cloud, and used either with or without interposed condensing lenses; but this does not quite answer to the conditions above referred to, for the rays diffused from a sheet of paper will pass, from every angle and direction, through the under surface of the object, and if this is held close, not only is the light more intense, but some portion of it must enter at very oblique incidences. The first step towards obtaining a corresponding effect in the microscope is to lay the slide on a sheet of clean white paper, and to throw a strong light upon it through and around the object, by means of the bull's-eye lens. In some instances, this gives a very good result, but if the object is large, it casts a shadow upon the paper, which mars its whiteness; it is therefore better to employ a small enamel or plaster-of-Paris disc in the place of the common dark well: this can be lowered sufficiently to cause the shadow to recede from the field of view. A thick black line should be drawn across the white disc, which may in certain cases be brought directly under some objects, by turning the supporting arm sideways; this will sometimes be found materially to assist the development of their structure: some tests acquire a pearly appearance from the oblique radiations from the enamel surface.

The method just explained gives some promise, but can only be used well with the lowest powers, and even with these, it is scarcely possible to condense the light on the disc sufficiently strongly. The close proximity of the highest powers to the object prevents the disc from being illuminated in this way; it is therefore necessary to use a diffusing film of partial transparency, in order that it may be lighted from underneath. I have had much difficulty in finding a material that will diffuse light well, and yet be microscopically structureless; for example, finely-ground glass diffuses transmitted light, but the *grey* appearance is caused by the contrary prismatic effects of an aggregation of bright fractures, which however minute they may be, are singly still larger than many of the objects to be viewed, so that instead of the illumination being a diffused, it may chance to be an oblique chromatic one. I have tried various kinds of enamel, but the best material that I have yet found is unbleached or yellow bees'-wax; this consists of numberless minute spherules which disperse the light very effectually.

The method of applying this is as follows: select two circles of thin glass about one-quarter of an inch in diameter, and place a shaving of the wax between them, then warm the whole slightly, and compress the wax, till on looking through it the flame of a candle just begins to appear visible. The disc is then to be mounted in a piece of tube, of a size sufficient to cause it to slide easily over the top of the ordinary achromatic condenser, so that the film may be adjusted exactly in its focus. When the apparatus is fitted in place, throw the light through the full aperture of the condenser, by means of the *concave* mirror, and bring the intensely-bright spot of diffused light thus formed, close under the object, by the rack and pinion: thus, for a trifling expense, a most useful and simple illuminator may be procured; but there is still much improvement to be expected, from the discovery of a better diffusing material than bees'-wax, but this I have not up to this time succeeded in finding.

The methods of illumination last described serve to show some objects which cannot be seen with the paraboloid, though others are equally well displayed with both. The effect of diffused light is to give a greater depth or perspective to the object, or a more just appreciation of distance between particles, or superimposed tissues, together with an agreeable softness, and an *entire absence* of the interference caused by the decomposition of direct light, which is known to act so prejudicially to the definition of some objects illuminated in the ordinary way. There is also another fact worthy of notice: it is observed how difficult it is to examine glass tubes, without our being deceived by direct reflection or refractions, but if we illuminate sponge-spicules (which are literally minute glass tubes) with diffused light when under the microscope, we get rid of these refractions, and they appear perfectly smooth, and their tubular structure is quite evident, for the diffusing film answers a similar purpose to the ground-glass shade or cap of the *kaleidoscope* which prevents the direct refraction of light from taking place, through the enclosed broken pieces of coloured glass.

I will suggest, that perhaps in some cases another diffusing disc, perforated and placed over the object, might be of advantage, so as to enclose it in a kind of luminous tent.

In conclusion, I must state, that I have carried the foregoing subject to a greater length than I had previously intended, and yet omitted some curious facts relating to illumination through coloured media, as possessing no great amount of practical utility, for monochromatic light was originally suggested for the purpose of remedying a defect in the objective: but as these are now made so as to be incapable of decomposing compound light, all such methods must consequently be considered obsolete.

In advocating the principle of copying from natural effects, in the application of illumination to microscopic objects, I believe that I have advanced nothing that is preposterous, but flatter myself that I have already made some successful advances in this direction, and I am of opinion that by keeping this point in view in effecting improvements, good will result from it in that department, where the microscope is most usefully employed in the examination of all ordinary objects, and if the binocular microscope should eventually approach to perfection, and thus enable us to combine *natural vision* with natural illumination, it will give to objects an appearance of reality of which we can at present form but little conception.

And, finally, as all the observations here detailed are the result of direct experiment, I advance them with confidence; and if I have brought forward but little that is novel, I venture to hope by the explanation that I have given of the relation that angles of aperture and illumination bear to each other, that I have cleared away some part of the fog that has prevented this portion of the subject from being in many instances more clearly understood.

On the STRUCTURE *of the* FOOT *of the* FLY. By JOHN HEPWORTH, Esq., *Surgeon.*

HAVING paid some attention to the structure of insects, I have carefully examined the fly's foot (*Musca domestica,* &c.), and finding much misapprehension on the subject still existing (in a recent popular publication), I thought a few remarks might not be uninteresting to some of the readers of this Journal, and lead to a further investigation by some one more experienced in these matters than myself. Some months after I had arrived at my conclusions I met with the following remarks, some of which come nearer my own ideas than anything I have yet met with:—

"Another interesting peculiarity observable in the domestic fly arises from the structure of its feet, enabling it to walk with the greatest facility, not only upon upright surfaces, but also upon the ceilings of rooms, back downwards, without its position being disturbed in consequence of being contrary to gravity. Much diversity of opinion has taken place amongst naturalists upon this curious subject; and even in the latest works we find the matter still forming a "questio vexata."

Dr. Derham, in his 'Physico-Theology,' speaking of the means whereby insects maintain their position upon smooth surfaces, states that "divers flies and other insects, besides their sharp-hooked nails, have also skinny palms to their feet, to enable them to stick to glass and other smooth bodies by means of the pressure of the atmosphere, after the manner as I have seen boys carry heavy stones, with only a wet piece of leather clapped on the top of a stone." Gilbert White, of Selborne, adopted Derham's opinion, adding that, although the flies are easily enabled, from their lightness and alertness, to overcome the weight of air in warm weather, yet that in the decline of the year this resistance becomes too mighty for their diminished strength, and we see flies labouring along, and lugging their feet in windows as if they stuck fast to the glass, and it is with the utmost difficulty that they can draw one foot from another, and disengage their hollow caps from the slippery surface.

This opinion, which has been entertained by the majority of entomologists of the present day, has acquired additional weight by the elaborate investigations of Sir Everard Home, undertaken at the suggestion of Sir Joseph Banks, with the assistance of that (then) unrivalled microscopic artist, M. Bauer, and published in the 'Philosophical Transactions' for 1816. The suckers, of which several kinds of flies possess three to each foot, are attached beneath the base of the claws, and are of an oval shape and membranous texture, being convex above, having the sides minutely serrated, and the under concave surface covered with down or hairs. In order to cause the alleged vacuum, these suckers are extended; but when the fly wished to raise its legs, they are brought together, and folded up as it were between the hooks. Messrs. Kirby and Spence have likewise adopted this opinion, considering it as "proved most satisfactorily." Other authors of no mean repute have, however, entertained a different opinion, and have entirely rejected the idea of a vacuum being produced. Thus Dr. Hooke describes the suckers as palms or soles, beset underneath with small bristles or tenters, like the cone teeth of a card for working wool, which he conceives gives them a strong hold upon objects, having irregular or yielding surfaces; and he imagined that there is upon glass a kind of smoky substance, penetrable by the points of these bristles. The same opinion is also given by Shaw in his 'Nature Displayed;' and more recently Mr. Blackwall has considered that the motions of the fly are to be accounted for upon mechanical principles alone; thus, upon inspecting the structure of the parts of the suckers, "it was immediately perceived that the function ascribed to them by Dr. Derham and Sir E. Home is quite incompatible with their organisation. Minute hairs, very closely set and directed downwards, so completely cover the inferior surface of the expanded membranes, *improperly denominated suckers*, with which the terminal joint of the foot of flies is provided, that it cannot possibly be brought into contact with the object on which those insects move by any muscular force they are capable of exerting; the production of a vacuum between each membrane and the plane of position is, therefore, clearly impracticable, *unless* the numerous hairs on the under side of these organs individually perform the office of suckers; and there does not appear to be anything in their mechanism which in the slightest degree countenances such an hypothesis. When highly magnified, their extremities, it is true, are seen to be somewhat enlarged; but when they are viewed in action or in repose, they never assume a figure at all adapted to the formation of a vacuum." Moreover, on enclosing a house-fly in the receiver of an air-pump, "it was demonstrated to the entire satisfaction of several intelligent gentlemen present that the fly, while it retains its vital powers unimpaired, cannot only traverse the upright sides, but even the interior of the dome of an exhausted receiver; and that the cause of its relaxing its hold, and ultimately falling from the station it occupied, was a diminution of muscular force, attributable to impeded respiration." Hence Mr. B. is induced to believe that insects are enabled to take hold of any roughness, or irregularity of surface, by means of the fine hairs composing the brushes, the most carefully-polished glass not being found free from flaws and imperfections when viewed in a favourable light with a powerful lens. A still different opinion has been maintained by other authors upon this subject, who, setting aside all idea of a vacuum, have conjectured that the suckers, as they have been termed, contain a glutinous secretion, capable of adhering to well-cleaned glass; thus, Abbé de la Pluche states, that when the fly marches over any polished body, on which neither her claws nor her points can fasten, she sometimes compresses her sponge, and causes it to evacuate a fluid, which fixes her in such a manner as prevents her falling, without diminishing the facility of her progress; "but it is much more probable," he adds, "that the sponges correspond with the fleshy balls which accompany the claws of dogs and cats, and that they enable the fly to proceed with a softer pace, and contribute to the preservation of its claws, whose pointed extremities would soon be impaired without this prevention. Notwithstanding the ridicule which has been thrown upon this opinion in a recent entomological work, it appears, from still more recent investigations, to be the best founded of any hitherto advanced. Thus, an anonymous writer has published an account of various experiments and examinations upon this subject, which appear satisfactorily to prove that it is not by the application of extremely small points to invisible irregularities on the surface of glass that the *pulvilli* or suckers are attached, but by simple adhesion of the enlarged ends of the hairs, assisted by a fluid that is probably secreted there; and the author is, therefore, reduced to refer the effect to molecular attraction only. It is also stated that when the foot is detached, a distinct fluid trace will often be left by each individual hair, the spotty pattern thus left on the glass appearing to be of an oily character, for if breathed on it remains after the moisture is evaporated. The contrary opinion, although contained in a review of Mr. Blackwall's Memoir, above noticed, was evidently written in ignorance of the subsequent observations of that author contained in the appendix of the volume in which it appeared, and in which several facts are stated, which appear "quite inexplicable, except on the supposition that an adhesive secretion is emitted by the instruments employed in climbing;" and it is subsequently affirmed that careful and repeated examinations made with lenses of moderately high magnifying powers, in a strong light, and at a favourable angle, speedily convinced Mr. Blackwell that his conjecture was well founded, as he never failed to discover "unequivocal evidence of its truth." —*History of Insects*, London, 1835.

In general the foot of the fly is described as being composed of two hooks and two flaps, or hollow cups, which act as suckers. Rymer Jones, in his 'General Outlines of the Animal Kingdom,' 1841, says, "The house-fly is furnished with a pair of membranous flaps, which, under a good microscope, are seen to be covered with innumerable hairs of the utmost delicacy; these *flaps*, or suckers as they might be termed, adhere," &c. In the publication alluded to in the commencement, September Number, 1853, the writer says, "In addition to the two hooks upon the last joint of the foot, we of tenmeet with *flaps* or *suckers*, &c., which," he goes on to say, "are beset with long stiff hairs, to keep them from dust and injury." The flap varies in form in different species from an irregular circle to that of an irregular triangle, and, viewing it from one side, it is somewhat thicker at its base (near its attachment), the under surface being, when isolated. convex, but perfectly flat, as a whole, when applied to a surface of that form. It appeared to be composed of an upper and under layer of areolar tissue, or something similar to it, between which a bundle of tubes, along with the fasciculi of a large muscle, pass; these are placed at its base, and (sometimes protected by a *coat of mail*, formed by long scales overwrapping each other, as a Venetian blind, or in alternate ones, as the scales on a fish, &c., but more frequently wanting) expand in a radiated form; each tube, as it passes along with its fellows on each side, gives off a number of tubules alternately with them; these dip downwards from the under surface, and become expanded into trumpet-shaped extremities, the flap becoming thinner and thinner as it approaches its margin, which sometimes terminates in an irregularly serrated edge, and at others by finely-pointed hairs. The fly has the power of attaching itself to smooth surfaces by these trumpet-shaped extremities, and also of secreting a fluid from them when vigorous, and has occasion to make extra exertions; but in a partially dormant state (the best for making observations), it does not appear to be able to give out this secretion, although it can still attach itself; indeed this fluid is not essential for that purpose: when it is secreted, it is deposited on the glass with great regularity. I have often attempted to preserve these markings, by applying colouring matter whilst they were moist; but have not yet succeeded. The tubules are often seen protruding from under the margin of the flap in a semiarch-like form, giving it a fringed appearance. The foot of the male *Dytiscus* is a type, not only of many of the beetle tribe (not aquatic), but of the whole of that of flies possessed of flaps. The first joints of the tarsus of the anterior legs of this insect are excessively dilated, so as to form a broad circular palette (fig. 1, Plate V.). On examining the inferior surface of this expanded portion, it is seen to be covered with a great number of sucking cups, two or three (three in the specimen, fig. 1, *a*, *b*, *b*) being larger than the rest, but they form collectively a wonderful instrument of adhesion. Fig. 1, *c*, represents the small suckers; fig. 2 and 3, the same enlarged, as they appear when mounted, and viewed with a power giving about 225 diameters. Fig. 4 (50 diameters) is the foot of a rather large fly, which shows the parts so well, that I have chosen it; it is one which appears to subsist on pollen, and is found on umbelliferous plants. The parts are too obvious to require a description. I would remark that the edges of the flaps have got turned in mounting, which shows the suckers more distinctly. Fig. 5 (550 diameters), an enlarged view of the portion of Fig. 4, *a*. Fig. 5, *b*, appearance of the under (through the upper) layer of the flap, showing the points from which the tubules are given off. Fig. 6 back, Fig. 7

front, and Fig. 8, 9, side views, when isolated and at rest. Fig. 10 foot of horse or gad fly, seen from above. Fig. 11, the under surface of a fly's foot, as presented to the eye when in action. The outlines of the sketches have been taken with the camera lucida.

	Linear Inch.
The length of tubules from the under surface of the flap, vary from - - -	$\frac{1}{1200}$ to $\frac{1}{1600}$
Distance of rows - - - -	$\frac{1}{5000}$ to $\frac{1}{12000}$
Expanded ends at rest (being much larger when in action - - - -	$\frac{1}{5000}$ to $\frac{1}{10000}$
Thickness of shaft of tubules - -	$\frac{1}{6000}$ to $\frac{1}{20000}$

REMARKS *on the* INVOLUTION THEORY *of the* STARCH GRANULE, *and on the probable Structure of this Body.* By GEO. JAMES ALLMAN, M.D., M.R.I.A., Professor of Botany in the University of Dublin.*

IT is now about twenty years since Fritzsche published the results of a series of careful and elaborate observations on the structure of the starch granule.† Fritzsche examined starch obtained from the potato and from several other plants, and concluded that the granule was composed of a series of layers of entirely similar composition, surrounding a minute central body or "nucleus," whose behaviour under certain circumstances rendered it probable that it differed chemically from the surrounding layers: he maintained that in the formation of the starch granule these layers are deposited one over the other from within outwards, and that the peculiar concentric striæ visible in the granule indicate the surfaces of contact of the layers.

In this view Fritzsche places himself in direct opposition to Raspail, who had just asserted that the starch granule was a vesicle consisting of an external thin wall, insoluble in water, and enclosing soluble contents, and that the striæ were merely superficial markings.

Since the publication of Fritzsche's memoir, which must be considered as the basis of all the knowledge we at present possess on the structure of the starch granule, numerous observers have applied themselves to the investigation of this body, and have advanced very different and often contradictory views as the result of their inquiries. So great, however, are the difficulties by which the subject is surrounded, that, so far

* Read at a meeting of the Royal Irish Academy, Jan. 9, 1854, and communicated by the Author.

† Poggendorff's Annalen, 1834.

as the structure and genesis of the granule are concerned, our positive knowledge can scarcely be said to have advanced a single step beyond the condition in which it had been left by the memoir of Fritzsche.

Amid the different conflicting statements* respecting the structure of the starch granule, there has recently been advanced a view which at first sight appeared to lead to most important results, as giving us an entirely new and unexpected conception of this difficult question.

M. Martin,† of Vienna, desirous of observing the changes which take place in potato starch during the action of hot water, contrived a simple expedient by which these changes could be watched under the microscope through the whole course of their progress. From observations thus conducted he arrived at the conclusion that the phenomena which occur during the action of hot water on the starch granule consist essentially in an evolution or unrolling of a compressed vesicle; that the starch granule, therefore, in its natural condition, is really a vesicle compressed into a disc-shaped body, and having its edges rolled inwards upon themselves, while the concentric striæ indicate the coils of the sort of volute thus formed.

This theory, presenting as it does an exceedingly elegant and attractive view of the subject, and appearing to be the result of careful observation, naturally excited much interest. In the third Number of the present 'Journal' is a valuable paper on the starch granule by Mr. Busk: in this paper, accompanied by beautiful figures, the author details a series of interesting and accurate observations, which he believes tend to confirm in all essential particulars the theory of Martin. Instead of observing the granules under the action of hot water, in the way done by Martin, Mr. Busk adopts the more easy process of acting on the granule by strong sulphuric acid, and watching the successive stages through which it passes during the continued action of the acid. The effect of the sulphuric acid is very similar to that of hot water, and whatever conclusions are derivable from the one method of experimenting seem equally to follow from the other.

Notwithstanding, however, the accuracy which it is impossible to deny to the observations of the German and English naturalists, I have been led, after carefully repeating their experiments, to arrive at an entirely different conclusion, and

* For an analysis of the more important views concerning the structure of the starch granule, I may refer the reader to Mr. Busk's paper in the third number of this Journal.

† Philosophical Magazine, April 1852.

it is chiefly with the hope of eliminating from the already too-complicated history of the starch granule, a new element of confusion which the theory of Martin appears to me to have introduced into it, that I publish my own researches on the subject.

If potato starch,* exposed for three or four weeks to the action of a solution of iodine, formed by mixing about equal parts of water and the common tincture of iodine, and a few of the deep indigo-blue granules, be then placed on a slip of glass under the microscope, and wetted with a drop of dilute sulphuric acid (about three parts strong acid and one water), the granules will immediately begin to swell, but much more slowly and to a less extent than in the case of un-iodinized granules. When the swelling consequent upon the action of the acid has ceased, many of the granules may be seen with several of their lamellæ beautifully dissected from one another, exhibiting frequently considerable intervals between the layers, and demonstrating, in the most complete manner, the composition of the granule out of a series of concentric shells† (Pl. VI., figs. 1, 2). The condition thus produced seems to be due to the iodinized granule not admitting of a sufficiently-uniform expansion of all its parts at the same time, so that some of the layers tear themselves away from the others. It frequently happens that some of the external layers will become ruptured, and by then peeling off like the coats of a bulb, will render the structure still more evident (fig. 2).

The mode of operating just described is perhaps the most certain in causing the detachment of the layers from one another, but the same phenomenon may be produced by other means. Payen‡ has figured the exfoliation of starch granules, which, after being exposed to a high heat without moisture, were then wetted with water; and Schacht§ describes a similar effect from the action of a solution of iodine in chloride of zinc. If the starch be exposed to heat upon a metallic plate till it has acquired a light-brown colour, the striæ will be observed in many of the granules, when examined under the microscope, to have become even more distinct than before the operation of the heat, while the spot corresponding

* The observations contained in the present paper are to be understood, except when otherwise stated, as having been made upon the starch of the potato. I have examined, however, very many varieties of starch, and in no case has anything been seen to invalidate the views here taken.

† It seldom occurs that more than four or five distinctly separated shells become visible in this experiment; each of them, however, must be supposed to be composed of a greater or smaller number of primitive lamellæ.

‡ Mémoires sur les Développements des Végétaux.

§ Die Pflanzenzelle.

to the nucleus of Fritzsche will have become much enlarged, and is now plainly a cavity filled with an aeriform fluid. If to these granules, first moistened with a weak solution of iodine, in iodide of potassium, so as to tinge them of a light-blue colour without destroying their transparency, the dilute sulphuric acid be applied, a very beautiful detachment of the lamellæ will generally take place (figs. 3, 4).

Some other effects produced by heat upon the starch granule may here be mentioned, as they would seem to throw additional light upon its structure. Let potato starch be placed in a small quantity of water, and exposed to heat until the moment that the whole assumes the condition of a thick paste. If a minute portion of this paste be now removed on the point of a needle *from the surface* of the mass, and placed in a drop of water on the stage of the microscope, it will be found to consist chiefly of granules, on which the action of the hot water has apparently but just commenced. In these (figs. 5, 6) a slight enlargement of the entire granule has taken place, the concentric striæ are still very visible, the central cavity has become much enlarged, and now presents several radiating offsets, which are directed towards the thick end of the granule, while numerous delicate striæ may be seen passing off from the cavity in the same direction, and intersecting the various lamellæ. It is plainly in the course of these striæ that the offsets from the cavity take place: these offsets are simple fissures passing through the lamellæ; and the whole structure, now described, indicates the existence of definite lines of cleavage at right angles with the lamellæ.

There is another mode of examining the starch granule which also tends to throw light upon its structure. By mixing the starch with gum-water, as originally practised by Raspail, and more recently by Schleiden, and allowing the mass to dry, we may, by means of a sharp razor, cut off very thin slices: these slices will contain sections of the granules passing through many different planes. When placed in water under the microscope, the cut surfaces may generally be seen to present, with various degrees of distinctness, a greater or smaller number of concentrically curved lines, each returning into itself. If the divided granules be weakly iodinized, and treated with the dilute sulphuric acid, they will swell up in the same way as the perfect granules, and some of the sections will then be seen to have opened into a large central cavity, rendering the vesicular nature of the swollen granule a matter of absolute certainty (fig. 7).

The observations now recorded leave in my mind no doubt whatever of the formation of the starch granule out of a series of independent lamellæ in the form of hollow shells included one within the other. Another question of importance remains to be determined, namely, whether all these lamellæ are in every respect similar to one another, chemically and physically, from the most external to the most internal.

In no case can there be detected any difference between them in their behaviour towards iodine. All parts of the granule, from the circumference to the centre, appear to be similarly acted on by this reagent. While, however, the starch granule is thus most probably chemically identical, from its surface to its centre, there is, on the other hand, reason to believe that a decided physical difference exists between the outer and the inner lamellæ.

The united action of sulphuric and acetic acid on the granule is very remarkable, and the following experiment seems capable of throwing much light on this question. Let the granules, after being tinged of a pale blue, by a weak solution of iodine in iodide of potassium, be placed upon the glass stage, and treated, in the way described above, with the dilute sulphuric acid. They will immediately present the phenomena characteristic of the action of sulphuric acid, and will swell to many times their original size. After the action of the acid has continued for about a minute, let a drop of strong acetic acid be applied, and the whole covered with a bit of thin glass. On now examining the preparation under the microscope, a remarkable change will be seen to have taken place. The swollen granules will continue to present a perfectly even outline, but in their interior may be seen a wrinkled membrane, which has detached itself, more or less, from the more external parts of the granule. The granule is now, in fact, most distinctly seen to be a vesicle, from whose walls the internal structures have detached themselves. These structures, by the continued action of the acetic acid, become more and more contracted, and will at last be frequently seen as a small irregular mass, lying like a nucleus upon the wall of the vesicle, and giving to the latter almost exactly the appearance of a nucleated cell. Figures 8, 9, 10, 11, exhibit different appearances of the granule under the action of the acetic acid, as seen in the starch of the *colchicum autumnale*.

It is here plain that the corrugated membrane consists of a certain number of the internal lamellæ which have been acted on by the acids in a different way from what takes place in the external ones, and we have thus a proof of a decided difference between the outer and inner lamellæ. This difference is doubtless merely physical, and probably depends on the different ages of the lamellæ. It sometimes happens that a second application of the acetic acid is necessary for the success of the experiment, and if the granules have been too long exposed to the action of the sulphuric acid before the application of the acetic, the characteristic effect cannot be produced. The experiment is most striking in its result on the small and middle-sized granules. Though the acetic acid is the more certain and decided in its action, yet a very marked distinction between the internal and external portions of the granule will be frequently manifested, by the addition of cold water just after the action of the sulphuric acid.

It has been already stated, that when the starch granule is exposed to a high heat, as in the process of roasting, the spot corresponding to the 'nucleus' of Fritzsche, becomes distended into a cavity of considerable size. Now, if the granules with the central cavity thus distended, be placed in water, the water will be rapidly absorbed until it fills the cavity; and if a drop of acetic acid be then added, the contents of the cavity will immediately assume a granular condition, while the rest of the granule remains unaltered. The granules taken from the surface of the paste, already described, frequently exhibit a similar granular condition of the contents of the cavity, even without the application of any reagent. This granular matter is coloured blue by iodine, and its occurrence would seem to indicate that starch in an amorphous, or, at least, non-lamellar state, constitutes the proper contents of the nucleus of Fritzsche, which must itself be viewed as a minute cavity.

It now remains to examine the phenomena presented by potato starch during the action of hot water, and of certain mineral acids, with the view of determining whether these phenomena may not admit of some other explanation than those offered by Martin and Busk; for if the account just given of the structure of the granule be the true one, these observers must have misinterpreted the appearances.

When I first repeated the experiments of Martin and Busk, I must confess that I became an entire believer in the theory of involution, so distinctly did the granules appear to develop themselves under the microscope by a process of unfolding. My adhesion was especially given to the modification of Martin's views advocated by Busk, who maintains that the evolution of the granule consists rather in an unfolding of plates, or rugæ, than in the unwinding of rolls. During some further observations, however, on the starch granule, certain facts presented themselves which did not admit of explanation in accordance with the involution theory, and I am now fully convinced that the phenomena presented, admit of quite a different explanation from that given by the advocates of involution.

I believe that the appearances, referred to a process of unrolling or unfolding, depend upon the different degrees of rapidity with which the various parts of the granule expand under the action of the reagents. Confining ourselves to the action of sulphuric acid, as being the most easily observed, and being in all essential points exactly the same as that of hot water, we shall find that the immediate action of the acid is to cause a swelling up of the external parts of the granule. Now this expansion does not take place uniformly; and during the first moments of the action, a great number of delicate rugæ may be witnessed, which generally surround the granule in regular and very pretty wavy rings. These rugæ now rapidly become deeper, then fewer, larger, and more irregular, and then all at once disappearing, the granule has become greatly increased in size, and lies as a smooth, or slightly corrugated vesicle, on the object-holder of the microscope. All these stages go on so rapidly, that it is exceedingly difficult to follow them. Most of them are represented in Mr. Busk's paper, and they so exactly resemble a process of unfolding of plicæ as very naturally to lead the observer to refer them to such a process. It appears to me, however, quite certain, from repeated and careful observations, that there is no unfolding as a *primary* phenomenon, but that certain parts of the granule being more speedily acted on by the acid than the neighbouring parts, swell out into prominent ridges, leaving, of course, furrows and depressions between them, which, in their turn, respond to the action of the acid, and now unfold themselves from between the ridges. While the process of *unfolding*, therefore, may, to a certain extent, be viewed as real, it depends on a condition which is only a secondary phenomenon, belonging exclusively to the granule *after* it has undergone certain alterations induced by the action of the acid.

The appearances just described are best seen in the larger granules: in the smaller ones, the well-developed and regular rugæ, which, in the larger, show themselves during the first moments of the action of the acid, are not generally produced; but immediately after the application of the acid, the smaller granules will for the most part be seen to present, in one spot, a deep depression, caused by the more rapid distension of the surrounding parts; and this depression itself, receiving immediately afterwards the full action of the acid, will roll outwards, and become more and more shallow as the granule continues to expand, while the latter, after passing through a series of figures, which may be compared to watch-glasses of various depths, will ultimately lie as a smooth spherical vesicle on the stage of the microscope. This is the simplest case of the phenomenon under consideration; and as it is well adapted to illustrate its real nature, I have given a series of figures, semi-diagramatic, of the successive stages of the process (figs. 12—17).

The view now taken is entirely borne out by the fact that when the sulphuric acid is applied so dilute that its action on the granule is very slow, or when the granule, by the prolonged action of iodine, has become less sensitive to the operation of the acid, a gradual swelling of the whole granule, without any appearance of plicæ, may be witnessed. But a fact, which appears to me at once fatal to the involution theory, is the following:—the larger granules may easily be crushed by slight pressure between two slips of glass, and if the pressure has not been too great, it will then be seen that many of the granules present deep fissures, opening upon the surface, and thence radiating towards the centre. Now, when these granules are treated with sulphuric acid, the fissures will be found in the swollen granule to retain exactly their original position and continuity, whereas it is evident that if they had been formed in a membrane rolled up in accordance with the theory of Martin, or even deeply plicated, as maintained by Busk, each would now present itself in the unfolded granule as a series of interrupted slits; unless, indeed, it had passed through the whole thickness of the involutions, a circumstance which, though it may possibly be allowed of some of the fissures, cannot be admitted of all.

A subject of much importance in the physiology of the starch granule, and of especial interest in its bearing upon the very difficult question of the genesis of this body, is the order of deposition of the lamellæ. Whether are the new lamellæ deposited on the inner or on the outer surface of those previously existing?

The data we possess, for coming to a satisfactory conclusion on this point, are at present very imperfect. It appears to me, however, that the structure of the granules, as attempted to be demonstrated in the present paper, is more in accordance with the centripetal than with the centrifugal order of deposition. It will be recollected that while the external lamellæ resist the action of certain reagents, the internal easily yield to them; now, since there appears to be no difference in chemical constitution between any of the layers, we can only attribute this difference in the behaviour of the layers to a less degree of solidity in the internal ones, a condition which is most easily explained by supposing these to be of more recent deposition. When we consider, moreover, the probability that the central cavity contains starch in a fluid or amorphous condition, the weight of evidence appears so far in favour of the gradually-increasing growth of the granule from the circumference towards the centre. But there is another strong argument in favour of this view—the behaviour of the small granules under the united action of sulphuric and acetic acid is in all essential particulars precisely the same as that of the larger ones. Now no one will deny that the small granules are, for the most part, at least, only a young state of the larger ones; and it would therefore follow, if the centrifugal theory were the true one, that the external layers of these young granules, after becoming internal by the deposition of new layers on the outside of them, had lost their original physical characters, and were no longer able to offer the same kind of resistance as formerly to the action of the reagents, a circumstance which seems so improbable, that it cannot be admitted without further proof.

It will be stated in opposition to the theory of centripetal growth, that this would require the constant distension of all the layers which lie external to the new ones. This objection, however, appears to have but little weight; we have already seen the great distensibility of the lamellæ under the action of hot water, mineral acids, and other reagents; and there is no reason to suppose that the very slow distension which the centrifugal deposition would require, may not take place while the granule is contained in the cells of the living plant, and surrounded and permeated by the fluids of the cell. That the lamellæ admit of rapid imbibition of fluid is evident from the phenomena already described as occurring when the granule is plunged into water, after being exposed to an elevated temperature until its central cavity becomes distended, without the exposure being sufficient to effect a chemical change; and even in granules which have been simply dried at a low temperature, a marked increase of size may be observed when they are moistened with water.

An observation originally made by Fritzsche of the occasional occurrence of a compound granule, consisting of two or more simple granules, each with its own lamellæ and so-called nucleus, but the whole surrounded by common lamellæ, would seem at first to decide the point in favour of the external deposition of the new layers. It is obvious, however, that the appearance presented by these abnormal granules may be just as well explained by supposing the granule, originally single, to have gone on up to a certain time forming layers in the ordinary way, and that subsequently in the interior of the granule, by some abnormal influence, two centres of deposition took the place of the original single one.

The growth of the granule may be probably thus explained: the materials for its growth are absorbed from the surrounding cell contents through the previously existing lamellæ, and thus conveyed into the central cavity. The contents of this cavity are fluid or amorphous; and the perfect amylum is subsequently deposited from them in layers on the walls, each layer pushing outwards those which had preceded it. It is true that this only attempts to explain the *growth* of the granule, its *origin* is still unaccounted for, and must remain so till increased light is thrown by new observations on this difficult question.

From the facts mentioned in the present paper the following conclusions would seem to follow:—

1. That the starch granule consists of a series of lamellæ in the form of closed hollow shells, included one within the other, the most internal enclosing a minute cavity filled with amorphous (?) amylum; that the concentric striæ visible in the granule indicate the surfaces of contact of these lamellæ; and that the so-called nucleus of Fritzsche corresponds to the central cavity.

2. That while the lamellæ appear to be all identical in chemical constitution, yet the internal differ from the external in consistency or other conditions of integration.

3. That the order of deposition of the lamellæ is centripetal.

4. That while the starch granule is thus a lamellated vesicle, it cannot be included in the category of the true vegetable cell, from which it differs not only in the absence of a proper nucleus,* but in presenting no chemical differentiation between membrane and contents.

* Unless the obscure indications of such a body as recorded by Busk, in the memoir quoted, p. 68, may be conceived to afford evidence of its presence.

TRANSLATIONS, &c.

On the Development *of* Starch. By H. Crüger. (Abstracted from the 'Botanisch. Zeitung.') January 20th, 1854.

The following observations, the result of lengthened investigations, relate principally to the history of the development of the starch granule, a subject which appears to have been, hitherto, but little studied.

Among the different kinds of *starch* which occur in larger quantity, spherical granules with a central *nucleus* are the most rare, whilst this form is the most abundant in the vegetating parenchyma, in the bark, and in the medulla. In these situations the starch grains are seldom met with of any considerable size; it appears and disappears with equal rapidity, but when it occurs in these parts of a more remarkable form and greater size, it affords very instructive information, as will be seen below.

Although, speaking generally, no systematic characters are afforded by starch, certain peculiarities, nevertheless, pervade most plants in one and the same family. Thus, all the Ferns examined by the author, present a clouded, indistinctly-laminated, irregular form of starch-granule; all the Cyperaceæ, a compressed granule, with large hollow *nucleus*, &c. Exceptions, in this respect, however, exist; the Aroideæ, for instance, exhibiting every variety of form, except the compressed. Moreover, the various forms occasionally pass one into the other, so that cases occur in which it is impossible to say to which category the plant belongs.

The cross of polarization always passes through the *nucleus* of the granule, and is apparent in all positions of the latter; as, for instance, even when a flattened granule is placed on the edge, as in fig. 1, *d* (Plate VII.) The phenomena exhibited under polarized light depend upon the *laminæ;* and the cross remains the same, whether the granule be hollow or apparently solid, except that in the former case the centre is wanting. The author adopts the view that the *laminæ* must be regarded as surrounding the whole granule. Practically it is difficult to prove this, although it is found that, in starch having an excentric *nucleus*, the *laminæ* immediately around the *nucleus* are of uniform thickness throughout, whilst the more exterior *laminæ* become thicker and thicker in one direction. Besides which, it may be remarked that, strictly speaking, the transverse *striæ*, presented in some kinds of starch, never actually reach the border of the granule, but are thence curved towards the *nucleus*. The tenuity of one side of the *laminæ*, in this case, appears to exceed the powers of our present instruments.

Where the *laminæ* are very irregularly deposited, one or more additional branches are superadded to the four rays constituting the cross of polarization.

With respect to the development of starch, the author states that, according to Mohl's Essay on 'The Vegetable Cell,' which contains the most recent résumé he is acquainted with upon the subject, the development of starch is unknown.

It may be regarded as a fact, which, by the concurrent observations of our best writers, has gradually assumed the form of an empirical law, "that all new formation in the plant proceeds under mediation of the protein substances." The author's investigations on the subject of starch have served to confirm this law.

One of the difficulties in the study of the development of starch, resides in the circumstance that it is such a loose secretion of the plant-cell. In the majority of cells containing starch, large and small, separate and aggregated, granules occur, apparently representing all stages of growth, but without its being possible to demonstrate this; as well as others in progress of solution. In order to facilitate his labour, and thus to attain a more certain guarantee for his results, the author felt it necessary to look out for plants in which the granules were not too numerous in the cells, and in which also, in the green cellular tissue, the formation of distinct and thick *laminæ* in the starch might be observed. Plants of this kind are afforded in various species of *Costus*, in *Canna*, *Dieffenbachia seguina*, and *Philodendrum grandifolium* (?). For the study of the compound starch, he found *Batatas edulis* the most convenient, although other plants, for instance, *Carolinea princeps*, would, perhaps, have been more so.

It was first requisite to determine whether Schleiden's opinion, according to which the outer *laminæ* of the starch granule are the young er orthe last formed, was correct; which turned out to be the case. But this answered only half the question which the author had proposed to himself; the other was: Where and how are the outer *laminæ* formed? In Mohl's paper, 'On the Anatomical Relations of Chlorophyll' (Verm. Schrift. p. 349), he found indications upon this point which were followed up. Mohl shows that *chlorophyll* and *amylum* almost always occur associated together. and, moreover, that the *chlorophyll* appears before the *amylum*.

Schleiden assumes the existence of colourless *chlorophyll*, on what grounds the author is ignorant, to whom the fact appears to be this:—the *protoplasma*, that *menstruum universale*, that *tinctura vitalis* of the plant, acquires a green colour, under the influence of light, owing to the formation of a resinous material, which may be extracted by ether, &c., but without losing its other properties, which relate to the life of the plant, and the solution and formation of other substances.

The author regards this *mucus*, the *protoplasma*, as identical with Mohl's primordial sac. He has looked for the latter for several years without being able to find it. Neither by alcohol nor by acids has he ever been able, satisfactorily, to demonstrate the existence of any membrane in the interior of the cell. "When a cell from the *parenchyma* of a ripe or nearly ripe fruit, *e.g.*, of *Mangifera*, *Carica papaya*, after it has been treated with alcohol or acids, is placed under the microscope, the matter at first appears quite clear, and a membrane surrounding the *coagulum*, in the interior of the cell, seems to be distinctly visible. But when an attempt is made, with the needle, to render the supposed *sac* tangible, the thing assumes quite a different aspect. The whole is resolved into mucous filaments, or the like, and the *sac* disappears like a phantom. I have made similar experiments in the cells of the *parenchyma*, in the vegetating parts of *Cactus*, and other plants, recommended by Mohl, for the exhibition of the "primordial sac."

The species or variety of *Canna*, which is cultivated in Trinidad for the seeds, is distinguished from the other variety yielding the "toloman," or "canna-starch," by the circumstance that, in the cells of the rhizome, only a few, but very large, starch granules, with distinct andnumerous *laminæ*, are developed.

Two species of *Costus*, *C. spiralis* and *C. comosus* (?), contain long cylindrical, strongly-laminated granules of starch in the cells surrounding the vascular bundles, and in that situation in small number; whilst in the rhizome all the cells are crammed full of starch.

Philodendrum grandifolium and *Dieffenbachia seguina* present, in all parts of the stem, starch granules with well-marked *laminæ* and of very unusual forms, some being branched.

In all these plants the younger cells contain only spherical starch granules; and it cannot be doubted that the other forms, met with in the older cells, derive their shapes merely from the mode in which the later *laminæ* are deposited around the originally spherical granule. Consequently, also, the minute spherical granules which occur in the older cells, together with the larger irregularly-shaped ones, represent nothing more than lower stages of development.

All starch occurs seated upon the layer of *protoplasma*, lining the inner wall of the cell, so long as the latter is capable of further development, and so long as *protoplasma* continues to exist in the cell. In every kind of starch in which the *laminæ* are distinctly formed, and in which an excentric and distinct nucleus exists, it may be remarked that the *nucleus* is always situated at the point most remote from that by which the granule is attached. This is readily observable in all cases in which there are not too many granules in a cell, and where the cells of the tissue are sufficiently large and transparent to allow of one or a few layers being observed at once. The elongated cells, surrounding the vascular bundles in the species of *Costus* above adverted to, are eminently fitted for this object; but when once the observer is satisfied of the truth of the statement, he will find it sufficiently confirmed in other plants where the observation is not so readily made. Consequently, in uninjured cells, the starch granules are found free only when they are old and the *protoplasma* has (entirely?) disappeared from the cell, whether the latter be more or less filled with the starch. In this case, one granule by its development appears to detach the other from the wall of the cell.

Now, if cells containing starch advanced just beyond the lowest stage of development in one of the above-mentioned plants are examined, it will be found that the granule, at the end by which it is attached to the *protoplasm* or *chlorophyll*, usually presents a layer of substance optically distinct from the latter, and from the bulk of the starch granule itself. If the preparation be treated with iodine, it will be observed that this outermost layer of the starch granule is not coloured blue, but, at the same time, that it does not so rapidly assume a brown or yellow colour, or so deep a tint as the protoplasm and chlorophyll. All granules of young starch do not present this layer of the same thickness—in some it being more, and in others less developed; and in many it is so thin as to be visible only with difficulty, and not distinguishable without trouble from the lines produced by the diffraction of light at the border of the starch granule. This circumstance has chiefly been the cause, perhaps, why it has not been previously remarked in those kinds of starch in which the *laminæ* are deposited with tolerable uniformity on all sides. It should be studied in plants in which thick and excentric *laminæ* exist in the starch grain.

The author regards this layer as a substance on the point of conversion into starch, but which, as yet, does not possess the property of becoming blue on the addition of iodine, and still contains, moreover, nitrogen or a protein substance. He adduces the following reasons for this conclusion:—When young starch is treated with iodine it does not acquire so pure a blue colour, nor nearly so rapidly as it does when older; and minute granules, which, without the reagent are optically indistinguishable from starch grains, are either only coloured yellow or remain uncoloured. It is known that starch is coloured reddish by Millon's reagent.

The above particulars being satisfactorily made out, the varying thickness of the layers in different granules is readily explicable. The layer of substance, which is destined immediately to become starch, is at first gradually formed from the protoplasm, and is not transformed into starch until it has acquired a certain thickness. In this process, also, accidental and individual diversities may occur, inasmuch as the completed starch granules present nothing like perfect uniformity.

Now, what is this transitionary substance? Is it *inulin?* The little correspondence in the results of analyses appears to indicate that *inulin* itself is a complex substance; does it really contain no nitrogen? The transitionary material does not appear to be gum, as it is not seen to dissolve in water, even after prolonged maceration. Does it contain nitrogen in chemical combination, or only a protein substance, as an element? These are interesting questions which offer themselves, and which will probably not be very soon answered.

In *Batatas edulis*, the author has investigated the development of the so-termed "compound starch-granule." The origin of this form of starch has been sought in the breaking-up of the grain into smaller granules; and this opinion has been supported by the observation of granules presenting a fissure in the middle and a *nucleus* at each end, and having external *laminæ* surrounding the whole, an instance of which kind is shown in fig. 3, *b*.

From the examination of the starch in the cells of the very young stem of *Batatas edulis*, the author concludes that the constituent portions of the compound starch granules are all originally distinct, coalescing into the larger compound grain after the disappearance of the transitionary substance above described.

The development of the incised discs or nodular rods which are found in the *Euphorbiæ*, in the proper vessels, as they are termed, so far corresponds with what has been stated above, that the granules in the young condition are also spherical.

The author has been unable to perceive *laminæ*, or transitionary substance in these corpuscles, nor any cross of polarization. Moreover, from its reaction with iodine, he regards this starch as a substance distinct from all other kinds.

It appears to him that starch in general is very far from being a homogeneous substance. Besides the various density of the *laminæ*, the *nucleus* frequently encloses a substance which, undoubtedly, differs in more than in optical properties from the rest of the granule. That the *laminæ*, even the innermost, in passing from the fluid to the solid condition,—a sort of crystallization,—may include foreign materials, is by no means inconceivable, and it is even probable that the included substance may again be removed from the perfect grain.

The author then proceeds to show that what he has observed corresponds, in the main, with Schleiden's statements, and to remark in strong terms upon the views of Kützing, who considers the starch grain in the light of a cell.

The figures here given are selected from the more numerous ones accompanying H. Crüger's paper, but will be found sufficient to illustrate all essential points in this disquisition, which, so far as it extends, is undoubtedly a valuable contribution to our knowledge of starch, and especially of its genesis —a point which, since the discovery of the existence of that principle, or one closely allied to it in various tissues of animals, is more than ever deserving of investigation.

On the Development *of the so-called* "Nuclear Fibres," *of the* "Elastic Fibres," *and of the* "Connective Tissue." By A. Kölliker. From the 'Verhand. d. Physik, Medicin. Gesselsch. in Würzburg.' Vol. iii., P. 1, p. 1. 1852.

1. *Elastic Tissue.*—The view quite recently propounded by Virchow and Donders, that the so-termed nuclear fibres are developed not from *nuclei* but from cells, is perfectly correct; but at the same time I cannot agree with these authors in regarding all the fusiform cells which occur in embryonic connective tissue, and which have been previously variously described, as formative cells of the nuclear fibres, and in their denial of the development of connective tissue from cells. Only the smaller proportion of these fusiform cells have any relation to the nuclear fibres, and they are readily distinguished from the formative cells of connective tissue by their shortness, their dark borders, their fine prolonged extremities, which never

form a bundle of fibres, and their elongated, rodlike nucleus. Many of these cells also present, not merely 2, but 3, 6, or more delicate processes, when, without losing their elongated general form, they exhibit the aspect of nucleiform cells. In every situation where nuclear fibres subsequently occur, these cells are met with, in embryos, at four months or even earlier, and, in the tendons, ligaments, and fasciæ, still readily admitting of being isolated, in the second half of fœtal life; and it is extremely easy to show that they form the so-termed "nuclear fibres" and "nuclear fibre reticulated tissue," by the coalescence of their two or more numerous processes. In very many places, as in the *perimysium*, the external integument, the mucous membranes, fasciæ, fibrous membranes, all traces of the original composition from cells is lost, and the so-called nuclear fibres, which I term *fine elastic fibres*, darstella, the well-known, everywhere uniformly wide, solid fasciæ or fibrous reticulations; whilst in other situations the original enlargements of the cells remain more or less manifest, as occasionally in the fasciæ and ligaments, and especially in the *cornea*. In these cases a certain relation to the nutrition of the organs composed of connective tissue, concerned, may be ascribed to the remains of the cell cavities, though, at the same time, it appears to me that it is going too far when Virchow at once represents the nuclear fibres as a system of cavities in the connective tissue subservient to nutrition. Moreover the nuclei of their formative cells also take part in the formation of the nuclear fibres, and indeed frequently not altogether an unessential one, becoming transformed when the cells coalesce into elongated rod-like corpuscles, in the neighbourhood of which the other parts of the cell are occasionally more retracted. What is true of the nuclear fibres applies also to the common *elastic tissue*. I showed, several years since, that in every situation where elastic tissue exists in the adult, only nuclear fibres are present in the infant at birth; that consequently these two elementary forms are connected, and the elastic fibres are developed from a widening out of the nuclear fibres, which holds good also for the *elastic* and *fenestrated membranes*, which are nothing but metamorphosed elastic networks. This correspondence in itself is necessarily almost sufficient to show, beyond doubt, that the elastic fibres also proceed from cells; but that this is the case is also shown by direct observation, in the *lig-nuchæ*, in the arteries, and the superficial fascia of the abdomen, in which situations the origin of primarily fine elastic fibres, from fusiform cells, is everywhere pretty easy to be seen.

2. The *connective tissue*, in its two principal forms—the

close and the *lax*—is developed in a somewhat different manner. The *lax connective tissue*, as it occurs for instance in the subcutaneous and submucous tissue, and in the large cavities around the viscera, appears first in the embryo, in the form of a *transparent*, *soft*, *gelatinous substance*. This consists essentially of fusiform, or stellate anastomosing cells, and of a semi-fluid, clear pulp, lodged in the interstices of the cellular network, but contains besides, in the latter, a certain number of rounded cells of no definite character. I first observed this *gelatinous connective tissue* in the substance which is found between the *amnios* and the *chorion*, and at first paid attention only to the anastomosing stellate cells, which I denominated as "*reticular connective tissue*."

Subsequently I met with the same gelatinous tissue in the enamel organ, and in this situation ascertained the presence of a considerable quantity of *albumen* and *mucus*, in the gelatinous substance, whilst at the same time Professor Virchow described the Whartonian pulp as of the same nature, and also discovered mucus in it. Upon farther pursuit of the subject, it was soon obvious that *gelatinous tissue* of this kind, which, moreover, Schwann had already briefly described, from the orbit of a fœtus, are very extensively distributed; in a word, as has been already remarked, as a precursor to all large masses of lax connective tissue without exception, and that its further development everywhere proceeds in essentially the same way, which is as follows:—The network of stellate cells becomes gradually more and more *close*, whilst new cells which arise in the gelatinous substance are continually joined to it, and at the same time the reticulations are gradually converted into bundles of fibres, which ultimately differ in no respect from the common lax fasciculi of connective tissue. Whilst this is proceeding, the pulpy substance is constantly consumed, serving as it does as a *cytoblastema* for the formation of the cells, which go through a varied course of development, according to the various situations in which they are placed. Some of them, in the way already indicated, pass into connective tissue, others unite together, and are transformed into fine elastic fibres, blood-vessels, and nerves, the majority lastly produce within themselves and become *fat-cells*.

Thus, from the gelatiniform embryonic connective tissue ultimately arises either common adipose tissue, or rather lax-connective tissue, not containing fat. It is not, however, in saying this, intended to be implied that the gelatiniform connective tissue must necessarily go through these series of development; on the contrary, it is much more disposed to remain in a condition more or less allied to its original one.

Thus, in the Whartonian pulp, even in the mature embryo, we may always notice, still partially remaining, more undeveloped bands of connective tissue with an abundance of pulp between them. Virchow has described this as a special tissue, under the term of "mucous-tissue." I am not, however, able to perceive in it anything more than immature lax, or gelatinous connective tissue, and believe that, notwithstanding the presence of much *mucus*, the impossibility, by boiling, of obtaining *gelatine* from it (Scherer), and the absence of distinct fibrils, there is no reason to distinguish it from connective tissue; for, in the first place, as we have seen, all embryonic, lax-connective tissue contains abundance of mucus; and, secondly, as has been ascertained by Scherer, always at first affords no gelatine; and, thirdly, also at first presents no distinct fibrils, which, moreover, are very manifest in the Whartonian pulp in many situations. Although, therefore, I cannot regard this pulp as anything but an embryonic gelatinous connective tissue, I am not disposed to assert, that every reticulated tissue is at once to be considered as such, much rather is it very conceivable that, besides the cellular networks, which are converted into connective and elastic tissue, other kinds exist. As such, I would just indicate the reticulations formed of pigment cells in the choroid coat and in the batrachia, the former of which also occur without pigment, constituting a pale, fibrous network, which differs in its chemical relations from connective tissue. As these networks occur in the adult, neither they nor the networks which resemble them in general can longer be referred to the lax form of connective tissue, however much they may resemble the embryonic forms of that tissue.

The *solid connective tissue* of the tendons, ligaments, &c., is formed, as was stated by Schwann, solely and exclusively out of cells, without any perceptible connecting substance. If the tendons of very young embryos are examined, nothing is found in them but fusiform cells, some of which have reference to the formation of the elastic fibres of these organs, whilst others constitute connective tissue. The latter are considerably larger and paler than the formative cells of the elastic fibres, contain larger oval nuclei, and present, when they have attained only a slight increase in length, an undulated prolongation of their enlarged extremities, a fibrillar formation, which is constantly more and more distinct. When these cells, which in various animals may be isolated, not without some difficulty, have reached a certain completion, they coalesce by their extremities, and form long cylindrical fibres, which continue for some time to exhibit the original cell-bodies and nuclei, but subsequently are transformed into uniform non-nucleated fibres, which soon exhibit their fibrils with equal distinctness, as the perfect fasciculi of connective tissue. The bundles of connective tissue, which are at first very slender (0·002‴), attain their final completion by a continual growth in thickness and length, until at last the tendons, after their elastic elements are also completed, no longer present any traces of the embryonic conditions. The other forms of solid connective tissue are constituted in precisely the same way as the tendons, and in no portion of them does the intercellular substance play any important part.

From the facts above stated, it is obvious that it is impossible to comprise the connective tissue with that of *cartilage*, as was proposed first by Reichert. Although in many cases the cartilages have a fibrous, collagenous matrix, it does not follow that they should have any nearer relationship, because the matrix of the cartilages differs entirely, *genestically*, from the fibrous substance of connective tissue, *and is not developed from cells*. Besides this, such a matrix is even necessary to the constitution of the cartilaginous tissue, seeing that in the reticular cartilages it is not collagenous, and in many cartilages (*Chorda dorsalis*, gill rays of many fishes, aural cartilage of certain mammalia) may even be wholly wanting. It is true, nevertheless, that a certain parallel may be drawn (as Virchow has done) between the cartilage-cells and the so-called formative cells, because (as I have already stated, before the development of nuclear fibres from cells was known) in many situations (particularly distinct in fibro-cartilages and where fibrous parts adjoin cartilage) cells, which can scarcely be distinguished from cartilage cells, pass into nuclear fibres. This resemblance, however, is imperfect, since the common formative cells of the elastic fibres do not resemble cartilage-cells more than do any other cells of embryos, and in the situations noticed depends more upon an analogous arrangement and grouping of the cells, and on the absence of definite characters in the boundary lines between the tissues in question. If connective tissue be compared with bone, it is quite necessary to distinguish between bones ossified out of cartilage and those which are formed from a soft blastema. The former can be regarded just as little as the cartilages, as analogous to the connective tissue, whilst with respect to the latter the whole question depends upon the histological position which is assigned to the ossific blastema. If the fibrous part of it, as I think I have observed, be really developed out of fusiform, coalesced cells, it may be regarded as a kind of immature connective tissue, in which case the compact bone-substance which arises from a tissue of this last ranks, as far as its matrix is concerned, in the connective tissue, although it does not approximate so closely to it as the spongy bone-substance does to the true cartilage, since its cellular elements, or the bone-cells, proceed from common indifferent formative cells by a thickening of their walls, together with a formation of bone canals, and can in no respect be compared with the so termed nuclear fibres of the connective tissue.

This consideration, when further regarded, leads to the conviction, that in the comparison of the tissues, isolated particulars alone are insufficient for the entire history of their development. However much many of them in their completed condition may resemble each other, so that, with the means we have at our disposal, they cannot be distinguished; still it may not be possible to identify them, if their development has taken place in a different mode. Thus, the common fibrous connective tissue, which is formed from fusiform cells, is not identical with fibrine which has become fibrillated, or a minutely fibrous matrix in cartilage, and although all three appear nevertheless to be chemically so much alike. Just as little is an elastic network arising in a physiological way from fusiform cells, or an elastic membrane, the same as an elastic coat arising from a fibrinous exudation, or an elastic *membrana propria* formed by a secretion from cells, or the elastic basis of a reticular cartilage; and the same applies to osseous tissue, which has proceeded from true cartilage, fibro-cartilage (connective tissue with cartilage cells), a soft blastema (immature connective tissue with ossifying cells), or from organized fibrine with stellate cells—although in all these cases it presents no considerable differences.

For a proper comprehension of the tissues, it is above all things requisite to place together those which agree, in all respects, in their *genesis*, form, chemical composition and function, such as the *transversely striated muscular fibres*, *medullated nerve-tubes*, the *formed connective tissue*, and the *true cartilages*. To these succeed those which, alike at the commencement of their development, afterwards separate, some proceeding further in their metamorphoses than the others. Examples of this kind may be seen in the *non-medullated* and *medullated nerve-tubes*, the transversely *striated* and *non-striated muscular fibres*, and the *muscular fasciculi* of the lower animals, which correspond genetically with the former, the *true* and the *reticular cartilage*, the *true cartilage* and *spongy tissue of bone*, the *elastic networks* and *elastic membranes*. Where the *genesis* differs, but the after form or function, or the chemical condition, is each or all of them alike, the tissues may, perhaps, be placed together, and in certain respects be regarded as coequal, though never as identical. Thus all *collagenous tissues* may be placed together; and also the *contractile fibres containing protein*, the *elastic reticulated membranes*, the *calcified tissues* with canalicular *systems*, all *fibrous connective tissues;* but notwithstanding, by this a perfect histological correspondence is by no means expressed any more than is a morphological identity, when organs, such as lungs and *gills*, the jaw of a mammal with that of a ray, the wing of a bird with that of an insect, are compared from a physiological point of view or from that of the anatomy of the parts when completed. That such associations of the tissues are to a certain extent justifiable, and even demanded by the requirements of physiology, pathological anatomy, and organic chemistry, is, moreover, at once intelligible; and I here only expressly add, in order to make myself plainly understood, that I have regarded the genetic point of view as sufficient for a pure anatomical conception, but not as alone holding good under all relations and respects.

Besides this it also may be remarked, that it is quite possible that, in many situations in which our views and notions at present render distinctions in the *genesis* necessary, perhaps at a future time connecting links may appear and upset all our systems. Thus I believe I am not wrong even now in describing the connective and elastic tissues as belonging to those between which transitions exist, and also the smooth and transversely striped muscular tissue, the latter in as far as it also affords isolated transversely striped muscle-cells. Perhaps even, hereafter, a contractile connective tissue in an improved form will be discovered as a connecting link between muscle and connective tissue. Such and other possibilities cannot seem strange to any one who knows that formative cells of the same kind are the initial points of the most important physiological organizations; that consequently all tissues are, in the beginning, alike. We shall even ultimately be inclined, when we consider the substance which is present before the other cells, and the frequently less distinct manifestation of the cells in embryonic and pathological tissues, not to set up between the parts developed from cells and mucoid substances such an insurmountable limit as may appear to exist in the eyes of many.

Contributions to the Knowledge of CUTANEOUS DISEASES, *caused by Parasitic Growths.* By Dr. B. GUDDEN. From the 'Archiv für Physiolog. Heilkunde v. Vierordt.' 1853. Part III., p. 496. Pl. 2, fig. 1.

(Continued from p. 33.)

II. *Pityriasis versicolor.*

BATEMAN, following Willan, describes four species under the genus *Pityriasis*, two of which, *P. capitis* and *P. rubra*, varying according to their seat, consist merely in a morbidly increased formation of the *epidermis ; P. nigra* is unknown to the author, but *P. versicolor*, as was first noticed by Eichstadt in Froriep's Notizen, 1846, depends upon a parasitic vegetable growth.

Pityriasis versicolor is an affection pretty widely distributed, and is especially common among the poorer classes ; but is also met with among those whose means allow them to enjoy the most luxurious cleanliness. The author has met with it frequently in men, and, relatively speaking, more rarely in women, but never in children. Its more common seat is on the trunk, and especially on the chest. In a young man, whose chest was habitually exposed, the middle of it was tolerably exempt from the affection.

The spots of this eruption are of a dirty-yellow colour, roundish, and usually of small size, or, from the coalescence of several together, more or less irregular, and varying very much in dimension. They are scarcely raised above the level of the skin, the elevation being perceptible only to the touch. Their surface is smooth ; but as the affection progresses, it becomes rough, and scales off in no slight extent in a whitish scurf. When the surface of the spot is peeled off, which is readily done, that which is exposed is more or less moist.

Generally the minute round spots do not scale off: they are smooth, and with very few exceptions, are perforated in the centre by a hair.

To examine the nature of the disease, the cuticle, from a part of the skin presenting a good many of these spots, should be raised by means of a vesicant, and the bladder removed as soon as possible. The portion of cuticle must be spread out in the most suitable manner, upon a glass plate, lying upon a dark ground, and the soft layer on its under surface infiltrated with serum, removed by means of a hair-pencil. With care this is readily effected, and nothing is left but the superficial, thin, transparent, firm layer, together with the hair-sheaths. In it, viewed from below, are seen, quite uninjured, the foreign, whitish-yellow structures with the utmost distinctness, and in the centre of almost each rises the whitish conical radical hair-sheath.

At the same time, upon closer examination, a multitude of extremely minute points are visible, which appear whitish by direct, and opaque by transmitted light. These are the openings of the sweat-ducts. Under the microscope they are seen to be composed of thick, flat, closely-approximated, more or less vertical, much-developed epidermis cells, containing yellow pigmentary matter; and in order not to have to refer to them again in the description of the fungous growth, the author here remarks, that they are very resistant, and remain almost wholly unchanged in the midst of the parasitic growth. Consequently, in the microscopic investigation of the fungous expansion, these orifices are seen enclosed in it, as opaque, usually brownish-yellow, infundibuliform depressions. The spores of the fungus are not unfrequently produced in great number around their border, whilst the cavity itself seems to be less favourable to their development, and is usually found to be unoccupied by them.

In the above-described mode of preparing the *epidermis*, the layers removed may be readily submitted to investigation. Their cells—and this is a point upon which, as in *Porrigo*, the author lays stress—present no abnormal conditions whatever. The same may be said, and it is convenient here to notice the fact, of the undermost layers of the hard, horny *epidermis* which are at all times free from the fungus.

The author now proceeds to the microscopic examination of the spots themselves.

If a spot together with the portion of cuticle immediately surrounding it be cut out of the prepared epidermis, and placed under the microscope, and viewed, first from below and then from above, it is evident, in the first place, that it lies in the uppermost portion of the horny layer, and, secondly, that it is composed of *fungi*. As regards the fungus-layer itself, it is seen, pretty distinctly, to be constituted of two layers, of which, the inferior and larger in circumference, is composed of filaments, and the upper smaller one mostly of spores; moreover, that its vertical thickness is greatest in the neighbourhood of the hair follicle, where the spores grow most vigorously, and least at the periphery, where only isolated looped filaments are seen penetrating the superimposed *epidermis*.

This disposition of the two kinds of fungus-formation may be demonstrated with greater certainty in another way.

The *epidermis* is macerated from 24 to 48 hours in water, at the common temperature. By this means the *fungi* are softened, and the felt-like stratum formed by it is loosened from the subjacent *epidermis*. It is to be carefully raised with a curved cataract-needle, so that its connexion is not broken, and placed under the microscope. It is true, spores alone are never seen, although it is now more evident than before that they lie principally on the surface; and if the epidermis upon which, after the removal of the little fungus-stratum, a slight cloudiness remains, be then brought under the microscope, it will be found that this cloudiness depends solely and entirely upon the presence of fungoid filaments.

The filaments are about 1-600‴ in diameter, roundish, serpentine, occasionally knotted, much branched, and interlaced. They are transparent under a power of 300, either not at all or but faintly yellowish, and having tolerably defined outlines. The older they are, the smaller is their diameter, and the fainter their contours.

The spores are produced at the end of a filament, occasionally also at the side, and form racemes about 1-50‴ in their longer diameter, which are usually so dense that it is only at the border that the individual cells can be distinguished. In a few cases where these racemes consist of only a small number of spores, the author noticed that each spore was placed upon a slender peduncle of the much-divided filament.

The spores are round, with tolerably defined outlines, and have a diameter of about 1-500‴. In most of them, a more highly refractive corpuscle is perceptible, the size of which varies still more than in the spores of the *Porrigo-fungus*, sometimes being scarcely visible, at others nearly filling the whole cell; occasionally it does not exist at all, and, in many cases, is double.

The fungus-stratum is covered on its surface by a thin layer of connected epidermis-cells; and among the fungoid filaments and cells themselves are found fragments of cuticle, together with a molecular detritus.

It was observed above, that almost every patch was perforated in the centre by a hair, and that the spores were accumulated, especially in the hair-sheath. If the hair follicles in question, which may be easily removed from the *epidermis* underneath by means of a needle, be examined, the spores will be found extending deeply into them. Their number is occasionally so considerable that the follicles present a yellow colour, and if the fungus-stratum be detached by maceration in the mode above described, it will exhibit on its surface a conical mass of fungus enclosed in the contiguous epidermic layer. The author was unable to observe any alteration in the hairs themselves, unless occasionally that they were a little thinned.

In the further progress of the growth, the spots enlarge and begin to form a scurf on the surface, owing to the breaking through of the epidermic layer of fungus. The whitish scales under the microscope are seen to consist of epidermis-cells and dried filaments of the fungus. The spots approach each other, coalesce, form large or small islands, and, as has been said before, may extend over the whole trunk. When the *fungi* die, the yellow colour of the spots is removed by their scaling off, and there remains for a longer time a smooth spot containing rather less pigment than the surrounding epidermis.

Pityriasis versicolor is contagious; but for the success of experiments in inoculation with it, the cuticle should *not* be removed.

The fungus enters from without, and invades localities where the necessary conditions for its development are present. It occurs in otherwise perfectly healthy persons, and diseases can only so far have any influence, as they may augment or diminish the condition favourable to its development, whether directly or indirectly. But the fungus has its seat in the uppermost horny layer of the cuticle, and avoids the deeper, soft layer. Whence is explained, on the one hand, the trifling reaction of the cutis, which is limited to a moderate thickening of the epidermis, and the little variety in the external aspect of the disease; and, on the other, the immunity of childhood.

Pityriasis versicolor, contrary to *Porrigo*, in which the fungus finds its nutriment in the soft layers of the *epidermis*, and is consequently especially incident to childhood, is a disease of adults.

Dr. Gudden's experiments in the cure of this affection, based upon the above view of its nature, do not seem to have been wholly satisfactory. The best results appear to have ensued upon the use of vesicants; but relapses seem to have almost invariably taken place after a time, in consequence, as the author believes, of the impossibility of removing the hair follicles to the bottom.

From the figures added by Dr. Gudden, the fungus would appear to be referrible to the genus *Mucor*.

On the Propagation of the Oscillariæ. By Dr. Hermann Itzigsohn. From the 'Botanisch. Zeitung,' Dec. 16, 1853.

The definite form of the *Oscillariæ*, and the processes attending their growth, as regards outward appearances, are well known: their tænia-like, jointed structure; their contents composed of *phycochrom;* their motility; the frequently fringed points of the filaments; the continual subdivision into two of their joints; their rapid growth; their spreading, &c. Premising that all this is known, I shall consider this definite form as the starting-point of their developmental processes, and proceed to indicate the relations of the other forms to it.

I shall select as an example the *Oscillaria tenuis*, Ktz., as being that most abundantly met with, and affording subjects of observation under all sorts of pseudo-forms (*Leptothrix*, *Phormidum*, *Symploca*, &c.).

As they become older, the filaments of *O. tenuis*, at first bluish-green, assume a more yellow-green colour, their contents acquiring pretty nearly the hue of those of the *Ulotrichæ*. The filaments break up into perfectly-distinct joints, which, at first urceolate, soon become spherical. The minute yellowish-green *gonidia* thus arising, gradually increase in size, become motile, and present in all respects the aspect of *Chlamydomonas*. They move by means of delicate *cilia*, and present in the interior, usually in the centre, a clearer green spot, a sort of vacuole, as I believe, which, if I do not err, has been taken by others for an amylaceous granule. The contents of the *gonidia* are still finely granular, and of a yellowish-green colour.

These minute *Chlamydomonades* (as I shall term them, since no one has pointed out any morphological distinction) gradually enlarge; a red *eye-point* becomes visible in them, and, *presenting a thousand intermediate forms, they grow into perfect Euglenæ*. The minute vacuole visible in them from the first, is, in all cases, still recognisable as a larger, clearer space in the mature *Euglena*, when the latter is extended. The finely-granular contents have become coarsely granular, the eye-point [perhaps the first indication of a reflecting spherule, having no further organization, for the purpose of conducting the luminous rays, without the intervention of any other medium than the fluid of the body of the *Euglena* to the perception of the individual; thence, also, the red colour, complementary to green; the eye-point in *Euglena sanguinea*, on the other hand, is green!] has become larger, and of a deeper colour, the filaments much elongated, and the movements more suitable to the now extended shape of the body.

The uniform life of the *Euglena*, prolonged only by endosmosis, terminates, as regards the individual, after repeated division, in the quiescent or "*protococcus*-condition," as it is termed, with which we are already acquainted from the observations of numerous zoologists and botanists (*vide*, among others, Cohn, on *Stephanosphera*). In this condition the

Euglena constitutes a large motionless spore, or *protococcus*-like globule, in which the eye-point gradually disappears; the gelatinous envelope surrounding, at a short distance, a large number of green *gonidia*, which had constituted the granular substance of the mobile *Euglena*.

After this prolonged quiescent condition, the common *Euglena*-envelope ultimately dissolves, and the *gonidia* escape, either singly, or still connected into aggregate masses, in the form of motile corpuscles (the *microgonidia* of authors). If a number of these remain conjoined, and move about with a rowing kind of movement, their locomotion being governed by a common spontaneity, they represent a *volvox*-like colony, which, perhaps, may even have been described as *Volvox* by authors.

The *microgonidia* of the *Euglena*, like those of all the *algæ* hitherto examined by me, are the motile parent-cells of extraordinarily-minute spiral filaments. They are, at first, green, gradually becoming pellucid, exactly like the spermatospheres of *Spirogyra*, presenting a monadiform aspect. A peculiar appearance arises when, in one of these aggregations of *microgonidia*, many remain green, whilst the others have already become clear as water, the mass then presenting, in fact, the aspect of being composed of two kinds of animalcules. Such or similar conditions would represent several species of the supposed genus *Uvella* (*atomus*, *glaucoma*, *bodo*, &c.)

Each ultimately colourless *microgonidium*, then, by the dissolution of its minute gelatinous envelope, discharges a small motile-spiral filament. In these, we have not the large spiral filaments of the *Charæ*, *Equisetaceæ*, and Ferns. Although, in *Selaginella*, these filaments are excessively minute, and visible only to the closest scrutiny (*vide* Hofmeister's fig. in his 'Comparative Researches,' Tab. xxvi., fig. 3), still in *Oscillaria tenuis* they are, perhaps, yet more delicate. This investigation is, of course, one of the most difficult nature, and demands the most acute vision.

These spiral filaments in the *Oscillariæ* do not appear to be destined for the purposes of impregnation, for they gradually increase in length and thickness, soon exhibiting innumerable spiral turns, and between them in the latter condition, and the finest spermatic filaments, a thousand different transitionary forms are met with. From the *spirilla*-like condition, they pass, by an increase in length and a continual spiral movement, into a *spirulina*-like form. Finally, when their motile faculty has become weakened, they affix themselves by one extremity to any near, larger object (for instance, *Conferva*-*filaments*, &c.), whilst the other extremity continues to move about with a creeping motion,—the peculiar Oscillarian movement,—in performing which a young filament frequently returns to the spiral. The last-described condition constitutes the *Leptothrix* of authors. The filaments now gradually become thicker, and though, at first, of the lightest emerald green, they gradually assume a deeper and deeper tint. The first indications of articulation are perceptible in them, until at last a young *Oscillatoria* is again perfected.

The extremities of the young *Oscillariæ*, as is well known, are fringed with hairs: this may be an indication of the existence of *cilia*, previously invisible, on the head-end of the filament, which end has now become the point of the *Oscillaria*. But in young plants it is not merely one or a few apical cells that are so fringed, for I have, in filaments thus furnished, not unfrequently noticed them fringed for the length of, perhaps, 30-50 joints. This circumstance recals to mind the long-ciliated spermatozoa of the Ferns and *Equisetæ*. This investigation will also afford ground for many important conclusions with respect to the motion of the *Oscillariæ*.

I shall return to many peculiarities of the *Euglena* in another place, in speaking of other Nostochineæ, and will here merely state that I first remarked their plant-like connexion in the *Rivulariæ*, in the gelatinous substance of which, when the *Rivularia* was mature, I always met with these bodies.

Much time and continued study, together with a fortunate conjunction of circumstances and ingenious combinations, will be requisite to follow the filaments here described by me through the labyrinth of forms of *algæ*, with their heterogeneous generations, if it be wished to apply this idea throughout that class of plants.

REVIEWS.

A Flora and Fauna within Living Animals. By Joseph Leidy, M.D. Smithsonian Contributions to Knowledge.

That there are animals which inhabit the bodies of other animals as their natural locality has long been known. Many of these are so obvious as to be popularly recognized under the name of "Worms." It is, however, only since the extensive employment of the microscope in aiding vision, that any large addition has been made to those generally known. Not only is it found that each species of animal has its peculiar parasitic animal and plant, but every species of animal appears to have a Flora and a Fauna of its own. Formerly a country was necessary to supply the materials of a Fauna or Flora, but with the microscope in hand the stomach of an insect affords abundance of peculiar species of animals and plants for such a purpose. Dr. Leidy's work is not an account of all the species of plants found in living animals, but an account of certain new genera and species of plants discovered by himself in the stomach and intestines of a few species of insects. In an introduction, Dr. Leidy refers to the plants and animals of the human body. These are treated of at length in the works of Dujardin,* Diesing,† and Robin.‡

The plants described by Dr. Leidy are as follows :—

Genus, *Enterobryus*, Leidy. Thallus attached, consisting of a single very long tubular cell, filled with granules and globules, producing at its free extremity one, usually two, rarely three shorter tubular cells, and growing at the other end from a relatively short, cylindroid, amorphous, coriaceous pedicle, commencing with a discoidal surface of attachment.

E. elegans is found growing from the basement-membrane of the mucous membrane of the small and large intestine of *Julus marginatus*, Say; and primary part of the exterior of *Ascaris infecta*, *Streptostomum agile*, and *Thelastomum attenuatum*, entozoa infesting the cavities of the viscera of the same animal.

E. spiralis is found attached to the mucous membrane of the small intestine of *Julus pusillus*.

* Histoire Naturelle des Helminthes. Paris, 1845.

† Systema Helminthian Vindoboniæ. 1850.

‡ Histoire Naturelle des Végétaux Parasites qui croissent sur l'Homme et sur les Animaux Vivants. Paris, 1853.

E. attenuatus grows from the mucous membrane of the ventriculus of the *Passalus cornutus.*

Dr. Leidy observes that these entophyta are found in the herbivorous *Myriapoda* and *Coleoptera*, and in no instance has he been able to detect them in species which are carnivorous.

Genus *Eccrina*, Leidy. Thallus attached, consisting of a very long tubular cell, filled with granules and globules, producing at its free extremity a succession of numerous globular or oblong cells, and growing at the other end from a relatively short, cylindroid, amorphous, coriaceous pedicle, commencing with a discoidal surface of attachment.

E. longa was found growing in profusion from the mucous membrane of the posterior part of the intestinal canal of *Polydesmus virginiensis.*

E. moniliformis was found growing upon the mucous membrane of the intestinal canal of *Polydesmus granulatus.*

Genus *Arthromitus*, Leidy. Thallus attached, by means of one or more granules, simple, cylindrical, very long, filamentous, articulate without ramuli. Articuli indistinct, with amorphous contents finally converted into solitary oval sporules.

A. cristatus grows from the mucous membrane of the ventriculus and large intestine of *Julus marginatus*, and also upon *Enterobryus elegans*, *Ascaris infecta*, *Streptostomum agile*, and *Thelastomum attenuatum;* from the mucous membrane and its appendages of the ventriculus of *Passalus cornutus*, and *Polydesmus virginiensis,* and upon *Eccrina longa.*

Genus *Cladophytum*, Leidy. Thallus attached by means of one or more granules; filamentous simple, with minute lateral ramuli, or branched inarticulate amorphous in structure.

C. cornutum was found in the same positions as *Arthromitus.* It is very minute. The filaments measured from the 1-700th to the 1-100th of an inch in length, by the 1-30000th to 1-25000th of an inch in diameter.

Genus *Corynocladus*, Leidy. Thallus attached by means of one or more granules; filamentous very compound; branches thicker than the trunk, without ramuli; inarticulate amorphous in structure.

C. radiatus was observed growing from the mucous membrane and its appendages of the ventriculus of *Passulas cornutus.*

In addition to the above genera and species, Dr. Leidy describes some parasitic phytoid bodies, whose structure he could not well make out.

Associated with this Flora, Dr. Leidy found the following Fauna:—

In *Julus marginatus* were found—

Gregarina Juli marginati, *Ascaris infecta*, *Streptostomum agile*, *Thelastomum attenuatum*, *Nyctotherus velox*, *Bodo Julidis*, a species of *Vibrio*.

In *Passalus cornutus* were found—

Gregarina Passali cornuti, *Hystrignathus rigidus.*

In *Blatta orientalis*, the common cock-roach, were found—

A species of *Vibrio*, a species of *Bodo*, *Nyctotherus ovalis*, a species of *Gregarina*, *Streptostoreum gracile*, *Thelastomum appendiculatum.* Of these, *Ascaris infecta*, and the genera *Hystrignathus*, *Streptostomum*, and *Thelastomum*, are new.

Dr. Leidy has also a chapter on *pseudo-entophyta.* He is inclined to regard many of the free-floating vibrio-like, not spontaneously moving filaments, as plants. The following caution may be useful:—

"In the study of the vegetable parasites of animals, particularly those of the intestinal canals, it is necessary to be careful not to confound the tissues of certain well-known cryptogamic plants, which may serve as food, or adhere to the ordinary food of such animals, with true entophyta. Thus fragments of fungi, confervæ, lichens, and the spores of these, used as food, or adhering as foreign matter to food of an ordinary kind, are liable within the intestines to be mistaken for parasites.

"In mid-winter I found beneath an old fence-rail an individual of *Acheta nigra*, or large black cricket, within the proventriculus of which were large quantities of what I supposed at the time to be a free, floating entophyte, resembling in general appearance the ordinary yeast fungus *Torula*, but which I now suspect to be an ergot upon which the animal had fed. The plant consisted of oblong or oval vesicular bodies, apparently thickened at the poles, and filled with a colourless liquid; but this appearance, more probably, arose from the cells being distended with a single large, transparent, colourless, amorphous globule, which pressed a small existing amount of protoplasma to each end of the cavity. The cells were single, or in rows to eighteen in number. Frequently a single cell of comparatively large size had an attached pair of cells, or rows of cells, at one or both ends. Occasionally they are met with containing one or two small round hyaline amorphous nuclei. The isolated cellules measured from the 1-2500th to the 1-1666th of an inch in length, by the 1-8000th to the 1-6000th of an inch in breadth. The rows measured up to the 1-300th of an inch in length."

He adds that it is not improbable that an occasional new species of cryptogamic plant might be discovered in the examination of the contents of the intestine of such animals as the earth-worm, herbivorous *Myriapoda* herbivorous insects, Chelonians and Batrachians. Some such bodies he describes, and regards it as probable that they may produce some of the cryptogamia which appear externally on animals, as *Botrytis*, &c. There can be little doubt of the importance of this field of observation, and in some of these, at present obscure organisms, may yet be discovered the sources or indications of states of disease to which the animal body is subject.

The work is illustrated with ten beautiful plates illustrating the details of the structure of the new plants and animals described by Dr. Leidy.

TRANSACTIONS OF THE PATHOLOGICAL SOCIETY OF LONDON. Vol. IV. (for the Session 1852-53).

THIS volume contains the results of the Society's proceedings during the Session of 1852-53, and presents a great amount of matter interesting to the pathological histologist. Among other papers of considerable value in that point of view, we would indicate more especially an account by Dr. Bristowe, of the appearances exhibited in a case of "Encephaloid cancer of the *dura mater*, and of the *periosteum* of the ribs," &c.; as also a paper by the same observeron the supposed coexistence in the lungs of cancer and miliary tubercle (?). But which latter productions are pronounced by Drs. Jenner and Bristowe to be likewise of a cancerous nature, and not tuberculous.

Dr. Bristowe also records the occurrence of "hematoid crystals in a hydatid cyst," p. 166—a fact of considerable interest. They are thus described: "In every part of the cyst were numerous free vermilion spots, the largest of which were about a line in diameter, and which clearly consisted, to the naked eye, of rhomboidal crystalline plates. Microscopic examination showed that all the vermilion points were really colourless plates of cholesterin, the surfaces of which were thickly studded with ruby-coloured, more or less regularly rhomboidal crystals, having all the characters of the bodies usually described as hematoid crystals. The largest of them were about the thousandth of an inch in the long diameter. The remains of Echinococci were everywhere visible."

We notice also a valuable paper, by Mr. Jabez Hogg, on "Enchondroma of the Testis," and in other situations, illustrated with excellent figures. We do not, however, perceive how the occurrence of true bone in an enchondromatous mass from the mamma of a bitch, or its characters, favours the view entertained by Professors Todd and Bowman, viz.: "that the *lacunæ* are developed from the nuclei of the cartilage cell." The latter statement, moreover, being one of whose truth there is, at any rate, very considerable reason to doubt.

Dr. Handfield Jones describes the intimate nature of the condition of the gastric mucous membrane, termed "mammellation." The small whitish eminences which give the membrane the peculiar aspect thus denominated, were best seen in vertical sections as whitish grains or masses in the submucous tissue. These masses consisted of tortuous tubes crowded and packed together, and filled with an epithelium composed of smaller-sized cell-particles and free nuclei, together with abundant amorphous granular matter and much oil. In the intervals of the masses there were no tubes, and the ordinary parallel arrangement was entirely lost. Dr. H. Jones thinks that the tubes, thus filled, bear some real resemblance to the granulations of a diseased kidney; and like them, result from the decay of the surrounding tissue and the distension of the canals by epithelium. He concludes, therefore, that there is some reason to believe that the glandular secreting structure of the stomach is liable to degenerative disease of the same kind as those which affect the kidney or liver. One of these is a fatty degeneration, the other a change more analogous to granular renal disease.

That there is some truth in these views is highly probable; but at the same time we do not conceive that the condition of the mucous membrane above described is necessarily, in all cases, one of degeneration, or even of disease, consisting probably, in many instances, simply in an infraction of the tubes of the gastric mucous membrane, with epithelium, either temporary, or more or less permanent.

Mr. John Marshall describes the appearances observed on the examination of a case of lobular hepatitis. And Dr. Lionel Beale gives an account, which we regret is so short, of the contents of a cyst in the kidney, which would seem to throw additional light upon the vexed question of these cysts, to which we adverted more at length in our notice of the previous volume of the Pathological Society's Transactions. The large cyst in Dr. Beale's case appears to have been lined with epithelium, and besides this, he states "that the tubes formed dilatations in the meshes of the matrix." Dr. Hare records a similar case, but unfortunately seems to have omitted any microscopic examination of the lining of the cyst. Dr. Handfield Jones notices the occurrence of a "peculiar form of uric acid crystals," presenting the aspect of "spherical or polygonal grains, $\frac{1-3}{500}$ inch in diameter, and showing a concentric and radiated structure. On solution in liquor potassæ they left behind some granulous film, and some transparent capsules of homogeneous membrane." What could be the chemical relations of this membrane?

There is also an interesting communication from Dr. Bristowe on "muscular or fibrous tumours of the uterus," in which he shows that these tumours, at all events in some cases, are composed wholly of muscular fibre-cells, similar to those of the uterus itself, and are not simply fibrous tumours, containing a greater or less quantity of muscular fibre mixed up with them.

Associated with so many valuable contributions to microscopic pathology, we regret to find a paper, which, as regards that branch of science, at any rate, is by no means calculated to advance knowledge, or to add credit to the transactions of the Pathological Society. We refer to a paper by Dr. Black of Chesterfield, descriptive of a case of hydatids expectorated from the left lung, subsequently to the occurrence of typhoid fever, &c. It is a subject for regret to find an observer, so zealous and acute as Dr. Black appears to be, so grievously misled by the procrustean force of preconceived notions, as to describe such objects as those represented in Pl. II. fig. A., as portions of nerve tube and lymphatic vessels contained in the sputa. In no possible condition could either of those tissues present appearances like those figured by Dr. Black. The objects most probably represent portions of vegetable or animal hairs. But a more serious matter in Dr. Black's paper is in what he says respecting the hydatids expectorated from the lungs (p. 52). That these were true hydatids, or *echinococcus*-cysts, is sufficiently obvious from their appearance under the naked eye; but when Dr. Black proceeds to describe their intimate structure as viewed under the microscope, it is difficult to decide, whether we should most admire his ingenuity in building up a theory on the most baseless suppositions, or his apparently complete ignorance as to the true nature of hydatid cysts. All that need be remarked concerning this portion of the paper is, that it is absolute nonsense; and we cannot but wonder and regret that some friend in the Pathological Society, should not have suggested as much to the author before the publication of the paper. If we were called upon to select a typical instance of theorizing run mad, it would be the description here given of the mode in which the walls of hydatid cysts come to be laminated (p. 57). It is to be hoped that Dr. Black will be more careful in drawing conclusions on future occasions.

LECTURES ON SURGICAL PATHOLOGY. By JAMES PAGET, F.R.S. London. Longman.

WE cannot discuss in our pages the general principles of Surgical Pathology, but we are anxious to call attention to Mr. Paget's book, because it recognises the aid which microscopic research is calculated to confer on the principles of Pathology, whether involving the art of the physician or the surgeon. We are sorry to know that there are still some persons practising the medical profession who doubt the benefit to be derived from the use of the microscope in the practice of Medicine and Surgery. Such persons suppose that because the diseases they treat get well without the use of the microscope, that therefore the microscope is of no use in practice. They forget that this argument can be employed by every pretender to medical skill, and that the results of treatment are to be looked for in the improved state of the public health, in the ability to cure disease, and increased intelligence of the members of the medical profession. The discoveries made by the microscope, and their influence on the practice of Medicine, have perhaps been more evident in exposing the absurdity of an empirical practice than in suggesting new methods of rational treatment. But such works as the present are the best answer that can be given to the ignorant assertion that the microscope is of no value in the practice of the healing art. It is all very well for the blind to say that eyes are of no use, but those who see are best able to appreciate their value.

Although at first sight it might be thought that Surgery is more independent of the use of the microscope than other departments of medical practice, a little reflection will show that its application to the study of the nature of inflammation, the healing of wounds, the repair of fractures, the results of inflammation; and the nature and growth of simple and malignant tumours, is of the utmost importance. The mission of the surgeon is not to operate, but to avoid operations; and he who knows most minutely the nature of disease will be most likely to attain this object of his art. That Mr. Paget has been sensible of this we have abundant proof throughout these very able lectures; and we can conscientiously recommend them as examples of the manner in which researches in Pathology should be conducted. There are many subjects treated of in this work that we should liked to have discussed in our pages, but our limits will not permit us on the present occasion; and we are anxious not to allow another Number of our Journal to be published without calling the attention of our surgical readers to its pages.

The work consists of two volumes, illustrated with a large number of woodcuts, which refer more especially to the subjects demanding microscopical research. In the first volume the subjects of hypertrophy, atrophy, repair, inflammation, mortification, and specific diseases, are taken up. The second volume is entirely devoted to the various kinds of tumours. In this volume, of the subjects discussed, the one to which most practical interest is attached is evidently that of the distinctions between malignant and non-malignant tumours. Mr. Paget thinks the distinction can be made out by microscopic characters. With regard to cancer structures, he says they may be generally described as formed of "nucleated cells, or of such corpuscles as are rudimental of or degenerate from the nucleated cell. Herein, and in the fact that the corpuscles are neither imbedded in formed intercellular substance, nor orderly arranged, lies one of the characters by which cancers are distinguished from other tumours and from all natural parts." A connected view, however, of the origin and development of malignant tumours is still a desideratum; and much more remains to be done before a satisfactory histological account can be given of these truly terrible conditions of cell-formation in the animal body.

Gazzetta Medica Italiana, Ser. II., No. 43, for October 25, 1853.

This Journal contains a paper by Professor Pacini, on the 'Structure of the Retina;' communicated chiefly for the purpose of indicating certain points, in which the statements on that subject made by him (*Sulla tessitura della retina*), and published in 1845 in the 'Nuovi Annali di Scienze Naturali di Bologna,' have anticipated the more recent observations of Müller and Kölliker. The points in which the observations of the later authors correspond with those of the Florentine Professor, include the subdivision of the retina into five distinct layers, which Professor Pacini enumerates from within to without, commencing with the limitary membrane (membrana limitante) the discovery of which appears to be due to him, as is also the credit of having been the first to indicate the resemblance between the nucleated corpuscles of the *retina* and the nerve-cells. In fact, the main additional fact of much importance made known by Müller and Kölliker, is the existence of fibres prolonged from the 'rods and cones' of the bacillar layer, and probably communicating with the fibres of the optic nerve. But this discovery is one of so much importance, as alone to mark an era in the histological history of the retina, in which, as regards other particulars, Professor Pacini has played a much more important part than appears hitherto to have been conceded to him.

NOTES AND CORRESPONDENCE.

Composition of the Boghead Coal.—In compliance with the request of the President of the Microscopical Society, I beg to send you a copy of the results of my examination of the Boghead mineral, called Boghead coal (brown variety).

It is of a dingy drab colour, very compact and difficult to pulverize; it has a slaty somewhat conchoidal fracture, and is easily scratched by the nail, a light-brown streak being left. Burnt in a crucible it evolves an abundance of exceedingly luminous gas, and the (coke?) left retains exactly the form of the mass submitted to distillation, and is of whitish colour. This so-called coke will not burn, but when ignited in contact with the air it leaves a white ash. Reduced to very fine powder, and treated with coal-naphtha (the same as is used in the manufacture of marine glue), the mineral yields a brown solution, which evaporated leaves a resinoid mass, and the residue left after treatment with the above-mentioned menstruum may be partially dissolved by treatment with boiling concentrated sulphuric acid.

The specific gravity of the samples examined varies from 1·173 to 1·179.

Its constituents are as follows:—

Volatile matter - - -		67·28
Carbon - - - -		9·43
Ashes - - - -		23·29
Sulphur in coal - -	0·88	0·88
Ditto in coke - -	0·86	
Ditto in volatile matter -	0·02	
		100·88

Composition of the ashes:—

Silica - - - - -	53·6
Peroxide of iron - - -	4·0
Alumina - - - -	39·2
Carbonate of lime - - -	3·2
	100·0

A. Normandy, 67, *Judd Street, Brunswick Square.*

The Boghead Coal.—I have just received the Journal, and I find in it a statement, copied from the 'Commonwealth,' which I must correct. It is there stated, that I said at the Royal Society of Edinburgh that I regarded the Torbane mineral as differing from bituminous shale, *because* I could not extract bitumen from it by any solvent.

Now what I really stated was this, "that bituminous coal and bituminous shale were wrong names, inasmuch as no *true* bitumen existed in either, or could be extracted by those solvents in which true bitumen is soluble. I added that the combustible matter in the mineral was, like that of coal, not true bitumen, and that I could find no chemical difference between it and the combustible part of ordinary coal. Some persons regarded the mineral as earthy matter which had become impregnated with bitumen; but this was not the case, if by bitumen be meant the substance, allied to asphalt, commonly so called. My argument was that since I could find no chemical distinction between the combustible matter in this mineral and that in coal, the mineral was, chemically considered, a coal. As to structure, that is often wanting in portions of coal, and besides, Dr. Traill had just said that no true distinction could be founded on the absence of structure."

By the report in the Journal I am made not only to say nearly the reverse of what I did say, since I maintained the identity of the combustible or bituminous matter in coals, shales, and the mineral, but I am made to talk downright nonsense, as if I maintained that solvents could extract bitumen from shale, which I said they could not.

Allow me also to point out that the Reviewer of Fresenius on Mycology has made a mistake in translating *Unscheinbarkeit* "unsightliness." It means not precisely invisibility, but the want of obvious perceptibility; and F. means to say that it is so difficult to see the structure and organs of fungi that people are thereby deterred from the study. The phenomena are too obscure, too little apparent, to attract the many. This is his true meaning, for which no single English word will suffice.—WILLIAM GREGORY, *Edinburgh.*

A defence of the proposed new genus "Actinophœnia"—Shadbolt.—IN a paper by Mr. Roper on the "Diatomaceæ of the Thames," read before the Microscopical Society in January of the present year, that gentleman alludes to a species previously described by me (under the name of *Actinophænia splendens*, occurring in the Port Natal gathering of Diatomaceæ, and also found abundantly in the Guano from Callao), as being probably somewhat similar to one found by him amongst the Thames deposit, but he refers his species to the genus *Actinocyclus* of Ehrenberg, under the specific name *sedenarius*. As it is evident from the remarks which accompany his description that he has mistaken my reasons for not classing the species in question as he has done, it is probable that others may have not comprehended them; and I think it right, consequently, to endeavour to show, that my object was not a needless multiplication of genera, more especially as I perceive from Mr. Roper's drawing that our attention is directed to one identical plant.

I am aware that it may look like presumption in me to enter the lists against so great an authority as the learned Professor quoted; but, in the first place, I am by no means satisfied that I really do differ from Ehrenberg, and even if I do, it is by no means surprising that a minute flaw should be perceptible to the Lilliputian that was overlooked by the giant.

In all the *Actinocycli* proper, the number of segments formed by the septa is *always even*, and *every alternate* segment occupies a position in a *different plane* from that in immediate contiguity to itself. This is so marked a character, that it cannot fail to be observed when once pointed out. The front view presents an *undulating* outline, and the two valves are so placed that the alternate segments in each fit into one another, the septa also coinciding; but in the genus which I have called "*Actinophœnia*," the case is in every way different even according to Mr. Roper's own description (the correctness of which I quite admit), for here, as in the genus "*Arachnoidiscus*," the segments are *all* in *one* plane, and the septa *all in another*, being placed internally. Moreover, the front view of the two valves presents a sort of double scallop in outline; and to crown the whole, the septa of the inferior valve are not placed opposite to those in the superior one, but intermediately. Surely all these points are sufficient to establish a generic difference; but if not, there is another to which I would direct the attention of microscopists, viz., the structure of the frustule. In the genera *Actinocyclus*, *Coscinodiscus*, *Triceratium*, *Actinophœnia*, and *Arachnoidiscus*, I have most distinctly and unmistakably detected the presence *externally* of a sort of membrane not unlike cellulose, having very minute reticulations, puncta, or cells (according to the fancy of the observer); for they are so minute that it is impossible to pronounce with any degree of certainty even when viewed under the very highest powers of our finest instruments; and this external membrane is not brittle like the siliceous part, but tough and capable of being folded and unfolded without its necessarily breaking; and this in the genus "*Arachnoidiscus*" I have actually accomplished, and recorded the fact in a paper published in the

'Microscopical Society's Transactions.' This external membrane is in all the genera I have quoted, supported by a siliceous framework; but if we examine those of *Actinocycli* and *Actinophœnia*, how different they appear, the one having a strong siliceous network between the septa, and the other being quite destitute of such an appendage. It seems that Mr. Roper has noticed the membrane to which I have alluded, but has not probably been aware of its character; and has evidently fallen into the error of supposing that I had relied upon that as a characteristic difference, because the markings are exceedingly patent in *Actinophœnia* from the absence of the net-like reticulations in the siliceous part, and somewhat obscured by their presence in the *Actinocycli*, and the other genera previously mentioned.

One other objection I have to make is to the specific designation *sedenarius*, founded upon one of the most inconstant characters that can be employed, viz., number—and it unfortunately happens in the present case, that the variation in the number of septa is so frequent, that seventeen to twenty might quite as well have been selected as the number employed. I am aware that Mr. Roper merely took up what he conceived to be Ehrenberg's view, and that the specific designation was not of his selecting; but if he will examine a slide of Callao Guano, he will, I am sure, be satisfied as to the inconstancy in the number of septa.

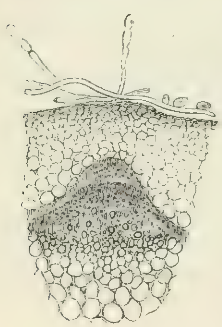
Under all these circumstances, I submit that I have adduced sufficient evidence to establish the claim of the "*Actinophœnia*" to a separate generic distinction, whether I have chosen a proper designation or not, I must leave to others to decide.—GEORGE SHADBOLT.

On a Developing Solution for Microphotographs made by artificial Light.—The developing solution, of which the formula is here given, appears to possess a considerable advantage over pyrogallic acid, used in the common way, in the production of microphotographic collodion negatives. The black is much more intense than that which I have been able to procure by the use of pyrogallic acid, and the lights fully as clear, if not more so. That it will keep, is also an advantage, though a trifling one, as it can be made extemporaneously in a few moments at any time. Whether it will answer as well for microphotographs made by daylight, and for the usual camera-pictures, my experience will not allow me to state: for the latter I have fancied it not well adapted—but why, I do not know. The solution is made by dissolving

30 grains protosulphate iron,
10 grains tartaric acid,

in 1 fluid ounce of water, acidulated with two drops of nitric acid. It is, perhaps, needless to remark that the iron-solution without the tartaric acid, answers very well for positives on the glass. The light I have used for microphotography has latterly been common gas, with a good Argand burner, which I find quite sufficient for the 4-10ths and under. The time required is from 5 minutes to 15 minutes. Under longer exposure the collodion begins to dry.—G. B.

Mode of Growth of Parasitic Fungi.—As all the scientific men of this country agree in opinion as to the mode of attack and growth of fungoid diseases on plants, it may seem presumptuous, if not superfluous, to add anything to what has already been said upon the subject. May it not, however, be possible that many of these men whose names stand so high as to cause their opinion to be received as authority, without further investigation—may it not be possible that in the great



variety and multiplicity of their pursuits they may have passed over so comparatively unimportant a subject? For, if they had brought the full powers of their investigations to bear upon it, they could not fail to have been convinced of what I am now about to advance: that fungi do not enter and ramify in the tissues and send up stems through the stomata of living healthy plants,* but that they only grow upon the surface, as may easily be seen, if proper care be observed in preparing sections for the microscope. I have bestowed a great deal of time and attention on the subject, and feel fully convinced that the mode of growth of this class of *fungi* is as represented in the annexed illustration, which, should you think it, with the accompanying remarks, worthy your notice, you will oblige by inserting in your excellent Journal for April.—Edward Tucker, *Margate*.

* I wish to be understood to refer especially to the class of fungi, *Hyphomycetes*, or *Mucedines*, which includes *Botrytis* and *Oidium*.

PROCEEDINGS OF SOCIETIES.

MICROSCOPICAL SOCIETY. *January 25th*, 1854.
George Jackson, Esq., President, in the Chair.

Geo. Coles, Esq.; Montague Leverson, Esq.; J. H. Roberts, Esq.; T. W. Burr, Esq.; Wm. Stuart, Esq.,—were balloted for, and duly elected Members of the Society.

A paper was read by F. C. S. Roper, Esq., on the Diatomaceæ of the Thames (Transactions, vol. ii., p. 67).

A paper was read by R. S. Boswell, Esq., entitled Remarks upon the Bird's-head process found upon *Cellularia plumosa*, and other zoophytes.

February 15.—Anniversary Meeting (Transactions, vol. ii. p. 83).

ROYAL INSTITUTION. *March 17th*, 1854.

On the Construction of the Compound Achromatic Microscope. By CHARLES BROOKE, M.A., F.R.S., Surgeon to the Westminster Hospital.

AFTER briefly adverting to the ordinary phenomena of reflection, the lecturer illustrated those of refraction by a moveable diagram, which readily explained the total reflection of a ray of light incident on the common surface of two media, at an angle greater than the critical angle, corresponding to which the angle of refraction is 90°.

The aberration of reflected or refracted light at a spherical surface was then alluded to; and although the reflectors employed in microscopes may be rendered free from spherical aberration by giving them an elliptic, and those of telescopes a parabolic form, there is no practicable method at present known of constructing lenses otherwise than with spherical or plane surfaces; and from the difficulty of obtaining sufficiently perfect reflecting surfaces, and of preserving them when obtained, refracting microscopes are now universally employed.

Chromatic dispersion was then mentioned, and the usual mode of producing achromatism by the combination of various kinds of glass, which differ in their dispersive power, which was illustrated by a combination of three prisms. The construction of achromatic object-glasses was next explained, as well as the nature of the aberration produced by the presence or absence of a plate of thin glass, covering the object, and the mode of correcting it in object-glasses of high power, by varying the distance of the anterior from the posterior combinations, as first applied in practice by Mr. A. Ross, and fully detailed in his article on the Microscope in the 'Penny Cyclopædia.'

The angle of aperture of object-glasses was then explained, and the power of those of large angular aperture in developing the structure of certain test objects, such as the siliceous shells of Diatomaceæ, was explained to be totally distinct from the mere increase of light transmitted.

Mr. Brooke offered an hypothesis as to the structure of these objects, from which it would necessarily follow that the structure would be rendered visible by oblique rays alone, and the necessary degree of obliquity would depend upon the smallness of the elevation on the undulating surface of the shell. This view was then shown to be highly probable. A specimen of the *Pleurosigma formosum* (first found by Mr. Brooke, at Walton-on-the-Naze) was viewed under a $\frac{1}{2}$-inch object-glass by Ross, and an achromatic eye-piece of high power (which was stated to be unquestionably superior to a deep Huyghenian eye-piece); when an opaque disc was interposed between the object and the centre of the object-glass, which cut off a large portion of the central rays, the diagonal rows of dots were still distinctly visible; but when the marginal rays were stopped out by a diaphragm, although a much larger quantity of light was admitted than in the former case, the markings were entirely lost.

In order to render visible the more difficult objects of this class, glasses of large angle of aperture have been constructed; but their employment is much limited, owing to the greatly-increased difficulty of correcting the aberrations of the transmitted pencil of light, and consequently the small amount of correction, that is, of adaptation to altered circumstances, that they admit of. From investigations which he knew to be in progress, the lecturer expressed a hope that by due adjustments of the illuminating pencil, the most difficult objects would be rendered equally visible under object-glasses of moderate aperture, which are much more generally useful.

Mr. Brooke then alluded to the preposterous angle of aperture of certain foreign object-glasses, viz., 172°, and explained the fallacy of the ordinary method of determining that angle, which consists in viewing through a microscope the light of a lamp placed at a few feet distance, and moving either the light or the microscope, so as to traverse the centre angular distance through which the light is visible. In this method, the course of the rays is contrary to what is usual, and oblique pencils may be brought to an imperfect focus at the back of the object-glass, and produce a glare of light, but which meet at a greater angle than the extreme rays that can enter the object-glass from the field of view, and which, consequently, are the extreme available rays.

A very perfect instrument for measuring the angle of aperture, designed by Mr. Gillett, was then explained. This consists of two microscopes, the optical axes of which may be adjusted to coincidence. One of these is attached horizontally to the traversing arm of a horizontal graduated circle, and is adjusted so that the point of a needle, made to coincide with the axis of motion of the moveable arm, may be in focus and in the centre of the field of view. The other microscope, to which the object-glass to be examined is attached, is fixed and so adjusted, that the point of the same needle may be in focus in the centre of its field. The eye-piece of the latter is then removed, and a cap with a very small aperture is then substituted, close to which a lamp is placed. It is evident that the rays transmitted by the aperture will pursue the same course in reaching the point of the needle, as the visual rays from that point to the eye; but in a contrary direction, and being transmitted through the moveable microscope, the eye will perceive an image of the bright spot of light, throughout that angular space, that represents the true aperture of the object-glass examined. The applications of this instrument in the construction of object-glasses are too numerous to be here detailed.

The important subject of illumination was then so far considered as the short space of time allotted to the discourse would permit. It may be taken as an axiom, that in the illumination of transparent objects, the amount of definition will depend upon the accuracy with which the illuminating rays converge upon the several points of an object; consequently, the source of light and the field of view must be the *conjugate foci* of the illuminator; of which an achromatic combination, similar to an object-glass, is the best form, and the concave mirror commonly employed is probably the worst, inasmuch as in a pencil of rays obliquely reflected at a spherical surface, no focal point exists.

The first compound microscopes on record, as those of P. Bonnoni, about 1697, which were placed horizontally, and that of J. Marshall, in the beginning of the eighteenth century, which was vertical, were furnished with central condensers; but in later years the perfection of the illuminating apparatus has by no means kept pace with that of the ocular portion of the microscope, though scarcely of less importance, in attaining the utmost practicable perfection in the vision of microscopic objects.

The advantages of employing an achromatic condenser were first pointed out by Dujardin; since which time an object-glass has been frequently but inconveniently employed, and more recently achromatic illuminators have been constructed by most of our instrument-makers. Some years since Mr. Gillett was led by observation to appreciate the importance of controlling, not merely the quantity of light, which may be effected by a diaphragm placed anywhere between the source of light and the object, but the angle of aperture of the illuminating pencil, which can be effected only by a diaphragm placed immediately behind the achromatic illuminating combination. An elastic diaphragm, or artificial pupil, as it might be called, was first proposed by Mr. Brooke, which was shown to answer very well in a large model, and produced a remarkable semblance of vital contractility; but mechanical difficulties interfered with its application, and the revolving diaphragm in the instrument now well known as Gillett's condenser was substituted.*

When the rays of light converging on the field of view meet at a greater angle than that of the extreme rays that can enter the object-glass, the dark-ground illumination is produced, in which the objects are seen in strong lines of light on a dark ground: this is best suited to objects having a well-marked outline, such as spicula of sponges,

* A description of this very useful apparatus has been recently published in the 'Elements of Natural Philosophy,' by Golding Bird and Charles Brooke.

or the shells of the Polygastrica. This may be effected either by Wenham's truncated parabolic reflector, or by a central opaque stop in Gillett's condenser.

The value of this kind of illumination in certain cases was shown by its effect in rendering visible the persistent cell-walls in a specimen of hard vegetable tissue, a section of a plum-stone; which could hardly be distinguished by the ordinary or bright-ground illumination. A white cloud brightly illuminated by the sun has long been recognized as the best source of illumination; but as this is not often obtainable, the light of a lamp thrown upon a flat surface of plaster of Paris, or powdered carbonate of soda, has been used as a substitute. A flat surface of white enamel, finely ground, but not polished, has been used with advantage by Mr. Gillett, as the surface can always be rendered perfectly clean by a little soap and water. By either of these means the glare resulting from throwing the unmodified light of a lamp on the object is completely obviated.

The effect of glare, or diffused light, in interfering with the vision of an object, was illustrated by reference to an experiment of Professor Faraday's, in which a screen of gauze, partially blackened, is held in front of a printed placard or diagram: the diffused light reflected from the white gauze considerably obscures the object, which is scarcely interfered with by the blackened portion.

The influence of illumination upon definition was rendered very evident by placing the two halves of a fly's tongue, similarly mounted, under two microscopes, having precisely similar object-glasses and eye-pieces: the one was carefully illuminated by an achromatic condenser and artificial white cloud; the other by the light of a similar lamp reflected from a concave mirror. The difference was so conspicuous, that some were inclined to doubt the identity of the objects.

The whole subject of the illumination of opaque objects, as well as that of oblique illumination, by Kingsley's condenser, and by the prisms of Nachet and Amici, of which diagrams were exhibited, and by other means, was unavoidably omitted. Microscopes by the three leading makers were placed on the table, between the optical parts of which Mr. Brooke declined the task of drawing any invidious distinctions; he, however, expressed a preference for the stand of Mr. Ross, on account of its having a secondary stage, with rectangular adjustments, and a rotary movement, by which any illuminating apparatus may be made to revolve after its axis has been brought to coincide with that of the microscope. A stand of Mr. Ladd was also exhibited, in which the various movements are effected with great smoothness, and without "loss of time," by means of wrapping chains; also, the ingenious apparatus of Mr. Highley for obtaining photographs of microscopic objects, of which time did not admit of any explanation being offered. In a curious and complicated microscope, the property of Professor Quekett, constructed about the middle of the last century by Benjamin Martin, might be noticed several points of construction that have been introduced as recent improvements.

DESCRIPTION OF PLATE VI.

1.—Cocconeis placentula.
2.—Actinocyclus sedenarius ?
3.—Triceratium striolatum.
4.—Campylodiscus bi-costatus.
5.—Zygoceros Rhombus.
6 *a.*—Odontidium Harrisonii ?
6 *b.*— ,, ,, front view.
7.—Rhaphoneis gemmifera.
8.— ,, fasciolata.
9.— ,, pretiosa.
10.— ,, rhombus.
11.—Zygoceros Surirella ; front view.
12.— ,, ,, side view.
13.—Front view of Actinocyclus sedenarius, showing the undulations.
14.—Side view of the same, showing the cellular markings.

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DESCRIPTION OF PLATE V.

To Illustrate Mr. Hepworth's Paper.

Fig.

1.—The three last joints of the tarsus of the foot of the *Dytiscus*, which has attached to it—
- *a*. One large sucker.
- *b*, *b*. Two of smaller dimensions.
- *c*. Hairs, to the end of each of which a small sucker is attached.

2.—One of the above-mentioned hairs, the disc of which is flattened singly into the same plane as the shaft; in their natural position they stand at right angles.

3.—As No. 2, with the sucking-disc doubled upon itself.

4.—Foot of large fly looking from above, showing hooks and flaps, the last being turned up a little at the edge, on which the tubules are seen with their trumpet-shaped extremities.

5.—Enlarged view of a portion of No. 4 (a <), showing the points.

6.—Back view of fly's foot, isolated.

7.—Front ditto.

8, and 9.—Side ditto.

10.—Foot of Horse or Gad-fly, having three flaps, furnished with tubules, very distinctly seen.

11.—Foot of large fly, as presented to the eye from the under surface, *in action*, where the tubules are seen extending considerably beyond the margin of the flap, and the hooks are seen through the transparent flaps.

DESCRIPTION OF PLATE VI.

Illustrating Dr. Allman's Paper.

Fig.

1, 2.—Starch from the Potato, treated with dilute sulphuric acid, after the prolonged action of weak tincture of iodine.

3, 4.—Potato starch roasted slightly, iodinized, and treated with dilute sulphuric acid.

5, 6.—Granules after the incipient action of hot water, showing the radiating lines and fissures.

7.—A granule of potato starch, which, after the removal of a slice, had been slightly iodinized, and then treatad with sulphuric acid. The granule is swollen, and the section is seen to have opened into an internal cavity.

8—11.—Granules of starch from *Colchicum autumnale*, exhibiting dlfferent appearances under the united action of sulphuric and acetic acid.

12—17.—Semi-diagramatic views of the successive stages of expansion presented by a small starch granule under the action of sulphuric acid.

DESCRIPTION OF PLATE VII.

1.—Starch of *Musa paradisiaca* (fruit):—*a*, from the interior of the fruit; *b*, the same viewed on the side; *e*, from the bark of the fruit; *c*, *d*, *f*, seen in polarized light.

2.—*Euphorbia neriifolia*, from the "proper vessels:"—*a* and *b*, viewed on the flat surface; *c* and *d*, on the edge.

3.—*Carolinea princeps*:—*b*, granule in which two distinct *nuclei* are enclosed in a common laminated envelop.

4.—*Philodendrum grandifolium* (?).

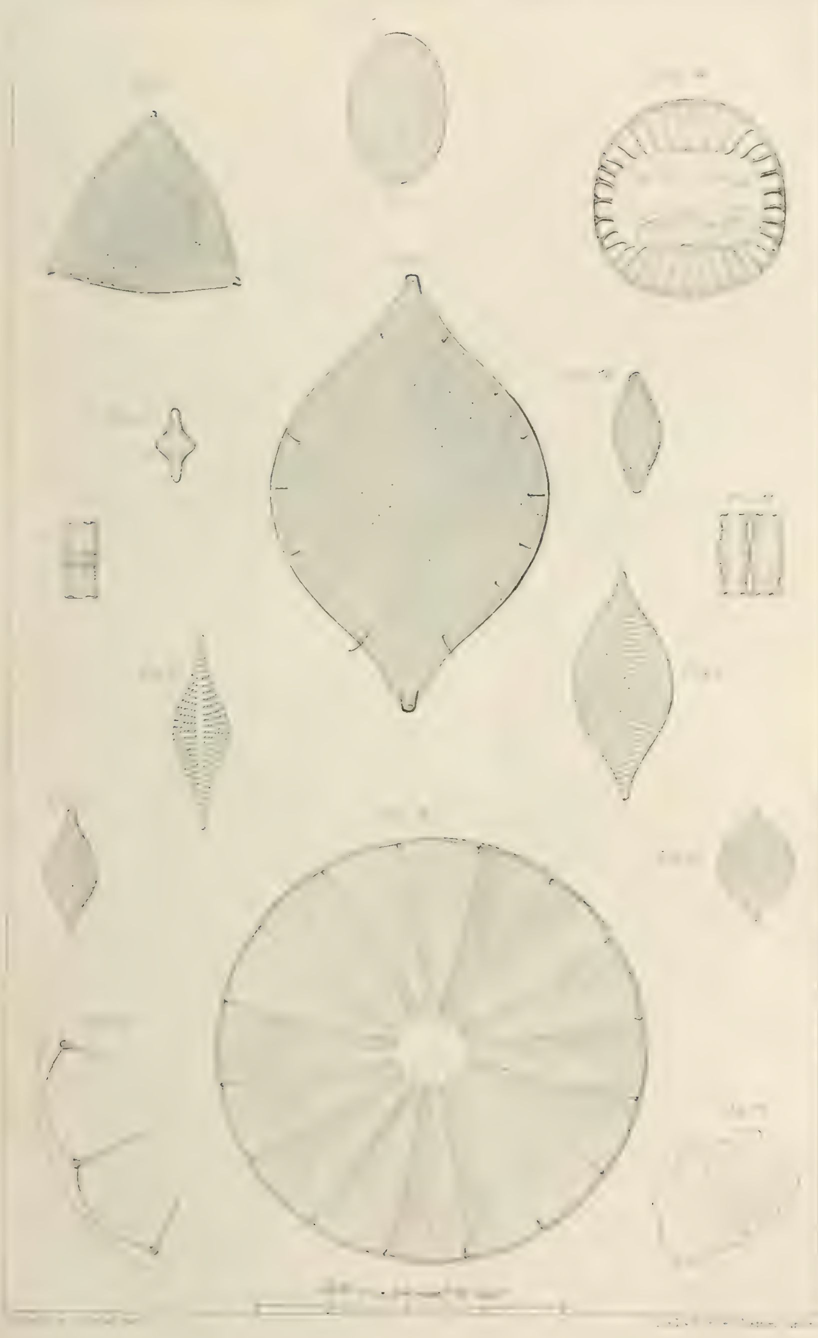
5, 6, 7.—*Id.* Beneath each granule in Fig. 6, may be remarked a layer of transitionary substance, which is not coloured blue by iodine.

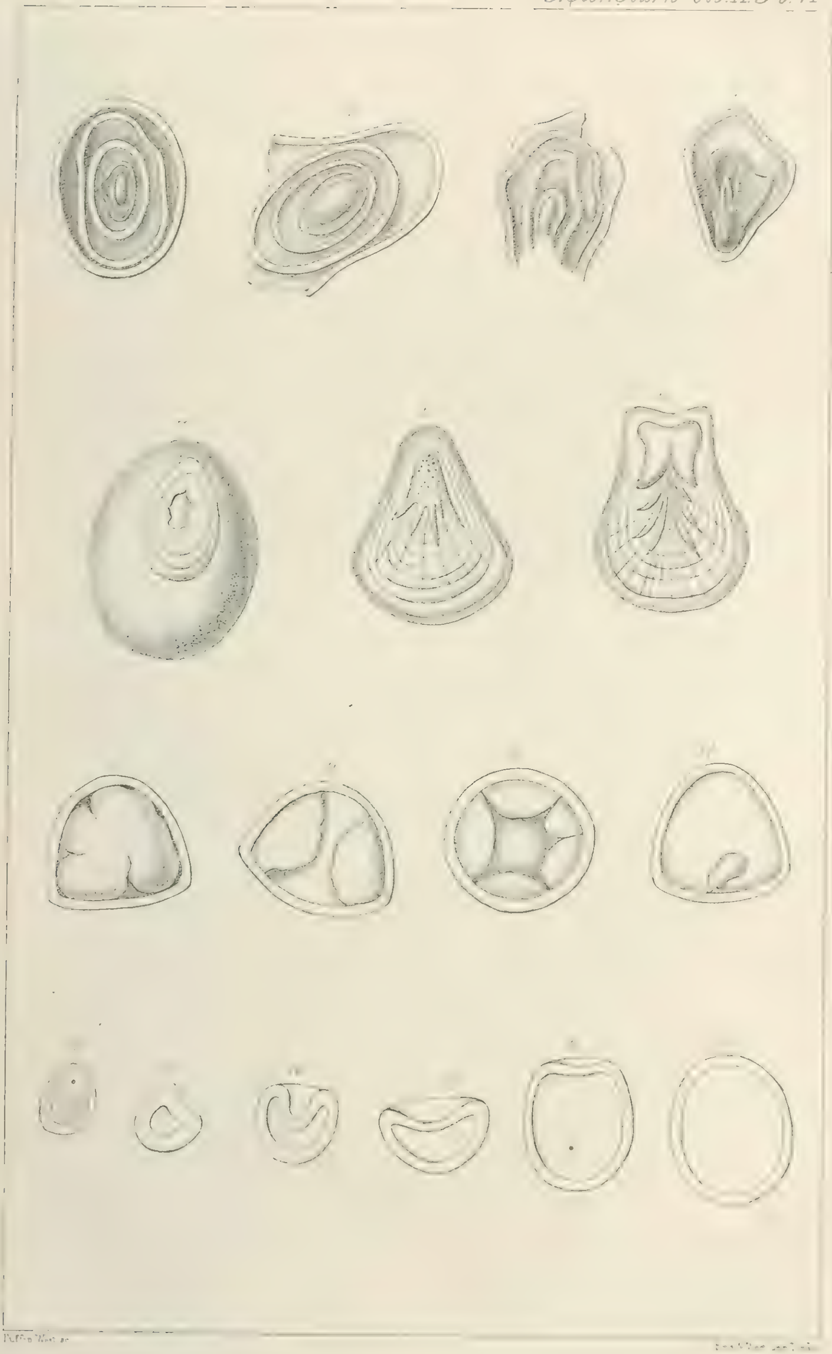
8, 9.—*Dieffenbachia seguina.*

10.—*Batatas edulis*:—*a*, two still separate granules, with the transitionary substance between them.

11.—*Costus spiralis.*

12, 13, 14.—*Costus comosus* (?)—12, very young cells; in 14, may again be remarked the layer of transitionary substance. (Object viewed in polarized light).





ORIGINAL COMMUNICATIONS.

On the Aperture *of* Object-Glasses. By F. H. Wenham.

In the 'Quarterly Journal of Microscopical Science,' for Jan. 1854, I described a method of measuring the angular aperture of object-glasses (which had previously been made use of by Professor Amici). Since that time there has been some discussion in relation to this subject, and as the plan that I proposed has been somewhat misunderstood, I will offer some further explanation.

The principle consisted in placing a lens of short focal length over the top of the lowest eye-piece of the ordinary microscope, so that the focus of the emergent pencil, and that of the examining-lens should be coincident; as thus adjusted, the microscope is converted into a kind of telescope, and a view of objects, at an infinite distance, may be obtained. On rotating the microscope in a horizontal plane, taking the focus of the object-glass as the centre of motion, a distant object will be seen throughout the arc, that includes the aperture of the objective. When the object is bisected, or becomes very indistinct, this point will be the limits of useful aperture.

The foregoing method has been objected to, on the ground that it is as much a measurement of field of view as aperture, but this is by no means the case, as the three eye-lenses form an optical combination that takes up the rays from the objective at its posterior, or conjugate focal point; and but a very minute portion of the field lens of the eye-piece comes into action, for by substituting a stop of only 1-20th of an inch in diameter in place of the ordinary one, there is no difference in the resulting measurement,—in fact, the definition and distinctness is rather improved by it than otherwise. It is, perhaps, advisable to have a small stop at a short distance above the upper lens, as it will serve to keep the eye central.

On first considering the *modus operandi* of this instrument, it would appear that as it causes the focus of the object-glass to be an infinite one, that there is no similarity between focal distance and the relation of aperture, and its effect on objects seen with a microscope under ordinary circumstances; or, in short, that the aperture of the object-glass, in the form that it is usually understood, is apparently quite destroyed; but it is a property existing in the object-glass, thus circumstanced, that it is still capable of receiving rays from distant objects, at every incidence within its aperture, and forming them into an image in the axis of the microscope, and it is a somewhat important feature in the principle, that we can ascertain the relative distinctness of the image at the same time that the aperture is being measured. I did not recommend this for the purpose of superseding existing methods of measurement, as Mr. Lister's is perfectly accurate up to a certain number of degrees; but it is more especially useful as a means of corroboration, and for detecting errors that Mr. Lister's plan will not always show; for example, if an object-glass be selected with a large aperture that is surrounded by coloured zones, or a false light, and measured with the examining-lens attached, on reaching the extreme, if this lens be removed, it is then simply Mr. Lister's arrangement, and the clear light from the candle will be seen exactly to bisect the field of view; but the coloured rings, and all other light that does not tend to form an image, will be outside, and not included within the aperture.

I have found the method very serviceable, and am the more inclined to advocate it from the circumstance of its having been employed by Professor Amici, whose practical experience in the construction of object-glasses gives his opinion much weight.

There is another plan of measuring apertures, contrived by Mr. Gillett, lately described before the Royal Society, and which has been announced, in the last Journal, as being, "a very perfect instrument for measuring the angle of aperture." Supposing this assumes all preceding methods to be imperfect, I will venture to give some reason for believing that it does not advance to such a standard. In describing the instrument, I infer that proper arrangements have been made for ensuring accuracy of motion and centering, and which fully answer the required end.

The principle of action is as follows:—The object-glass, whose aperture is to be measured, is attached to an ordinary microscope body, fixed in an horizontal position; a candle or lamp is placed close in front of the eye-piece, which is removed, and a cap, with a very small aperture, inserted in its stead. Under these circumstances the rays will pass through the lenses of the objective, and on finally emerging therefrom, will form a cone of light corresponding to its aperture. It is merely the simple measurement of the angle of this cone that Mr. Gillett has endeavoured to arrive at. To effect this, he has thought it necessary to employ another microscope, with eye-piece and object-glass complete;

the focal point of which is made to rotate horizontally from a centre coincident with the axis and focus of the first microscope. The traverse of what may be termed the examining-microscope is indicated by an arc divided into degrees. This arrangement is not new to me, as I have used the same for some years, for examining the oblique correction of object-glasses; but I always considered it to be too incorrect for a measurer of angular aperture, for the indication will be nearly as the sum of the two object-glasses; or a large portion of the aperture of the examining-glass is added to the angle of the objective to be measured, and for this there is no direct ratio. I have two object-glasses, one having a clear and definite aperture of 95°, and the other 90°, which, when applied together in this way, the indicated angle is very near 180°.

Mr. Gillett appears to have discovered this source of error, as he has since attempted to remedy it by making an alteration in his instrument, which now somewhat differs from that originally described. To explain the improvement, I will consider the two microscopes and objectives placed with their axes in the same right line with foci coincident. A patch or stop is then placed over the front of the examining object-glass, which cuts off exactly one half of its area in a vertical position. Supposing this stop to occupy the left-hand side, the body of the examining-microscope is then moved to the right; and when the light disappears, the number of degrees are noted. The microscope is next moved back to its linear position, and the stop shifted round, so as to cut off the right hand half, and the degrees taken from this direction, added to the first indication, will give the angular aperture. This has thus to be obtained by means of a double traverse and shifting of the stop, which must be, to some extent, detrimental to accuracy. The last modification certainly reduces the aperture of the examining object-glass, but the same may also be done by using a stop with a very narrow vertical slit; the whole angle can then be taken at one operation, without shifting the stop. I have tried both these arrangements with different assortments of object-glasses, but still find that there are some curious discrepancies.

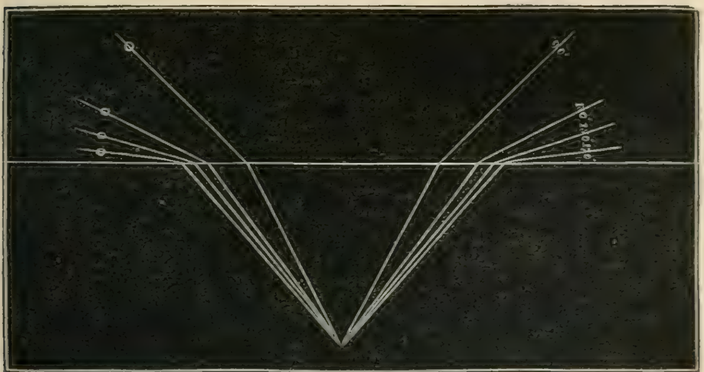
If every precaution be taken for obtaining an accurate result according to Mr. Gillett's method, and then the instrument be reversed, so as to rotate or traverse the object-glass *with the aperture to be measured*, the same indication will be obtained. This, then, merely resolves itself into Mr. Lister's method of measurement, and which will certainly perform better, and be more accurate, if the optical arrangement which intervenes, and only serves as an impediment to the free passage of the light, is removed; in short, I am of opinion that Mr. Gillett's complex contrivance will not prove serviceable to the practical optician. If it is only the measurement of the angle of the cone of light emergent from the object-glass that he desires to accomplish, it may be accurately effected by means of a very simple and portable instrument, without any optical appliances whatever, as these only tend to falsify the result; but, for many reasons, I do not approve of measuring apertures in this manner.

There is a paper in the Proceedings of the Royal Irish Academy for January 23rd, 1854, 'On a New Method of Measuring the Angular Aperture of the Objectives of Microscopes,' by the Rev. T. R. Robinson. This is perfectly accurate in principle, and, as the paper contains some original information that leads the way to matters of much practical utility, I shall refer to it at some length.

The mode of measurement cannot be better described than by using the author's own words. "As a lucid point in the focus of the objective sends out from the eye-piece rays nearly parallel, so light sent in the opposite direction through the microscope will converge at that focus, and then diverge in a cone, whose angle equals the aperture of the objective. If this cone be intercepted at right angles to its axis, by a screen, and the diameter of its section, together with the distance of the screen from the surface (focus) of the objective, be carefully measured, they give the aperture."

The luminous source may be either a camphine lamp or sunlight; the latter gives "a beautiful map of the objective's light territory," and shows with remarkable distinctness the most minute errors of workmanship in any of the lenses, such as scratches, defective centering, dirt, &c. Another important application of Professor Robinson's principle, is the measurement of the diminution of effective aperture that the object-glass sustains, when used upon an object immersed in balsam, or other medium; I do not think that this measurement can be so well effected by any other method than that here spoken of. The author mentions the fact that objects in balsam will be *less illuminated* than in the other way; this alludes to the diminished angle of the illuminating pencil, and the same reasoning also applies to the aperture of the object-glass. The annexed diagram will show to what extent various angles of aperture are reduced, when viewing a structure immersed in Canada balsam. The exterior aperture is 170°, which is assumed to be the largest effective pencil that can be got through an object-glass; this is at once reduced to 82° or less, and the inner angle of 90° is brought down to about 55°.

I may here observe that a parallel plate of glass over an object, *mounted dry*, has no effect in reducing the aperture, for



the rays, after being deflected by the first surface, emerge again from the second one parallel to their original direction, and all converge to a point at the same angle as at first, consequently the object is seen through the glass with an angle of aperture the same as if it was not interposed. This is not the case when the object is immersed in a refracting medium with a plane surface, for the first, or single deflection, is not compensated for a second time, and hence the angle of aperture will be considerably reduced, according to the refractive power of the medium. Without resorting to theory to demonstrate what the reduction of aperture ought to be, I will show, practically, what it really is. In order to ascertain this, I employed a piece of polished plate-glass with parallel sides, 0·508 of an inch thick, which possessed very nearly the same refractive power as Canada balsam; I ascertained this by filling a plano-concave lens that I had by me, made of the same glass, with that material; when the concave side was placed on the plate of glass, on looking through them both, no optical effect could be discovered.

The method of using the glass plate was as follows:—I first covered one side of it with a thin film of bees'-wax, to serve as a screen, and laid this downwards on the stage of the microscope. I then focussed the object-glass to be measured, exactly upon the upper transparent surface of the plate. Without shifting the microscope from its horizontal position, I next placed a candle before the eye-piece; a bright circle of light appeared on the bees'-wax screen, the diameter of which I carefully measured; an angle was taken from the circumference of the circle to the focal point of the objective (the distance being equal to the thickness of the glass plate). The angle thus obtained will represent the effective aperture of the object-glass for an object mounted in *Canada balsam.* The glass plate was now removed without disturbing the other adjustments, and a paper, or card screen placed in exactly the same plane as the bees'-wax film formerly occupied. The diameter of the circle of light was again measured: an angle taken from the circumference to the same point as before represents the aperture for an object mounted *dry.*

This last is in strict accordance with Professor Robinson's method. The following were the results :—A 1-12th, having an aperture of 146° on a object mounted *dry*, was reduced to 75° on an object in *balsam;* an 1-8th of 125° to 71°; a 1-5th of 105° to 68°; and a 4-10ths of 90° to 56°.

I have not had an opportunity of trying to what extent the aperture is reduced by the various other known media used in mounting objects. This may be very easily done by filling a parallel glass cell with the fluid, and it will exactly represent the conditions under which such objects are mounted.

These experiments will readily account for the difficulty of discovering the markings or structure of a severe test when mounted in balsam; for, as thus seen, it may be inferred that no aperture exceeding 85° can be made to bear upon it, and this is even supposing that the largest aperture object-glass that has ever been constructed is used. Such being the case, I am somewhat puzzled at an announcement that appears to contradict this fact, coming from one that must be considered an authority in these matters. I refer to Professor Bailey, who, in a letter addressed to Matthew Marshall, Esq., dated January 20th, 1852, first speaks of an American object-glass of very large aperture (172½°), and its performance on the *most difficult tests known*, and then proceeds to say: "In all these cases (and, in fact, whenever I allude to a test object), I mean the *balsam-mounted* specimens. The dry shells I *never* use as tests." This assertion seems to me to be extraordinary, and very like saying that an aperture of 85° or 90° will do everything that is required. I have invariably found that when very difficult tests are mounted in balsam, I cannot discover the markings, and certainly, the reasons herein given will account for it. It is to be hoped that the American opticians have discovered some new and peculiar principle in object-glasses, that will render a smaller amount of aperture serviceable; but however this may be, I think that Professor Bailey's statement requires some further explanation.

As the nature of the markings on test objects is now exciting some degree of attention I will offer some remarks on the subject. The prevailing opinion with some theorists is, that the striæ are rendered visible by the contrast induced by an inherent refraction of the siliceous prominences, throwing a portion of the rays from the source of illumination without the limits of the aperture of the object-glass, and thus causing the markings to appear opaque. This is, in effect, comparing the object to a piece of fluted glass. Now, if this were correct, if even the most easy of this class of objects were to be mounted in Canada balsam, the refractive index of this and silex being so nearly the same, every appearance of structure would be entirely obliterated; but it is found not to be so, for the markings have the same appearance when in balsam as out of it; what want of distinctness there may be is partly accounted for by the effect of diminished aperture, which is, of necessity, reduced under this condition, and therefore, less of the radiations from the object are collected.

I cannot persuade myself that any vital organism can be so devoid of structure, and so perfectly homogeneous as this theory would imply. I believe that all test objects are seen in the same way as any other transparent ones, by means of the different degrees of opacity of the parts. This opacity may arise from varying thicknesses, or from an imperviousness to light arising from colour, or the aggregated structure of the markings. In any of these cases refraction is not called into operation; and, further, I can show the markings on the most difficult tests when illuminated as *opaque* objects under such circumstances, that no light can be *refracted* from the striæ into the object-glass.

Those who advance such speculations as these appear to forget that the definition of tests depends entirely upon aperture, and that this must be increased in proportion to the closeness of the lines or dots upon the object, and if the aperture of the objective is insufficient, no method of illumination will call them into view; there is no occasion even to employ a microscope to ascertain this fact. In my paper on illumination, contained in the last Journal, I have made comparison between the optical properties of the eye and a microscope; this has been rather doubtfully received, for some cannot see the analogy; but I must again refer to the same organ for a demonstration of the properties of aperture.

If we place some small print against a wall, and retire to such a distance that the words are barely legible, and then apply to the eye an optical combination similar to an opera-glass, which will give it greater aperture, *without increase of magnifying power*; it is most remarkable how this assists vision, and appears to illuminate the object, enabling the print to be easily read.

The eye by itself is also a natural lens, possessing some amount of *linear aperture*, and if further comparison were wanting, the marginal rays show evident symptoms of imperfect achromatism;—but to return to the point in question. If we hold the blade of a penknife diametrically across the pupil, and examine the flame of a candle, it will appear double; as the images formed at the opposite extremes of the pupil, or aperture, do not unite on the retina, this is quite analogous to the *diffracting spectrum* seen in the microscope.* By using a piece of paper in the form of a cross, in place of the penknife, four images may even be obtained. The apparent mobility of distant objects, when a body is brought in a line with them at a short distance from the eye, may be attributed to the same causes.

To illustrate the effects of the aperture of the eye in separating lines, suspend in a good light a piece of textile fabric, printed either in stripes or dots. Stand at such a distance off that the lines and interspaces are just clearly defined. Now examine them through a small perforation, made with a pin in a black card. The lines or dots will become invisible; approach nearer and they will reappear; by substituting a smaller stop they will vanish as before, and again become distinct at a shorter distance. I cannot go further than merely to mention this fact, which has been thoroughly investigated by Mr. Lister, who from the three data, of size of stops, number of lines in a given space, and distance of the eye from the object, has obtained very practical results, and ascertained the degree of aperture necessary for separating lines or spaces a certain distance asunder. In making comparison between experiments with the eye and microscopic object-glass, it is assuming angular and linear apertures to be the same in effect, of which fact there can be no question.

For a demonstration of aperture I will again quote Professor Robinson's paper:—"The effect of *angular* aperture is merely an increase of illuminating power analogous to that of *linear* aperture in a telescope. Let O be a point of an object seen by an objective whose anterior surface is A B; this point, in case of a test object, may be considered as self-luminous, and equally so in every direction."

This exactly confirms what I have endeavoured to explain, that aperture is just effective in proportion to the quantity of *radiations* collected from the object.

* See 'Quarterly Journal of Microscopical Science,' for April, 1854, page 152.

All these facts must tend to prove that the separation of distances and definition of tests is entirely dependant upon aperture, and not upon illumination, as the latter will be quite ineffectual without the former. In concluding these remarks I may mention, that of two object-glasses of equal performance, the best is that which does its work with the *least* amount of aperture. Microscopists are but too apt to judge of the value of objectives, and select them entirely by the latter element.

Some years ago I announced my opinion that 150° might be considered as the limits of useful aperture. This was asserted from practical data, and theory has led Professor Robinson to the same conclusion. There is little to be gained beyond this; and now that 160° and even 170° are not uncommon, I consider it quite absurd to suppose any wonderful effects will be produced from an extra 2½°. Besides the small assistance and little light to be obtained by means of the most oblique rays, they have another bad effect in giving a distorted image of the object. This latter circumstance alone has made me desirous of trying any method that would give the probable result of causing an object-glass to perform effectively with a less degree of aperture. Apparently this can only be accomplished through the reduction of the obliquity of the exterior rays, incident upon the first surface, by making the front of the anterior lens concave. For many years foreign glasses of this form have been sold, but their performance has not been such as to tempt an imitation of any peculiarity in their construction. Some time ago I gave this a trial, but not with that degree of care necessary to ensure a certain result.

The concave form has been investigated mathematically by Professor Robinson with such good promise, that I have been once more induced to take it in hand, though he to some extent over-estimates the advantages to be gained from it; for he assumes the first surface to be dense flint, which would reflect a greater quantity of light, whereas this loss is lessened, as all our best object-glasses have of late years been made with triple fronts, with the first lens of crown glass.

With excessive difficulty I have succeeded in making 1-8th of 138° with two separate anterior combinations, each giving the same degree of aperture and magnifying power. The first has a plane incident surface. The second front is worked to a concave radius of 0·625 of an inch. On comparing them together I could not discover any appreciable advantage, in point of *quantity of light*, in favour of the one with the concave surface. I have tried the experiment with every degree of care, and consider that it sets this point finally at rest, and that it is a theory that does not tell in practice; I also understand that Ross has long ago arrived at the same result. A front combination of this form considerably increases the difficulty of correcting the oblique pencils

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On the MAGNIFYING POWER *of* SHORT SPACES, *illustrated by the transmission of Light through minute Apertures.* By JOHN GORHAM, M.R.C.S.L., Memb. Soc. Arts, &c.

IN the present communication it is my intention to furnish a few phenomena, illustrative of *visual angle*, of the *magnifying power of short spaces intervening between the eye and the object*, and of the *inversion* and *multiplication of images by the simple transmission of light, without refraction, through small apertures in plane surfaces.* As these illustrations are somewhat novel, I hope they may prove interesting to the readers of the 'Microscopic Journal.'

The apparent enlargement of minute objects held and examined at very short distances from the eye, has received less attention at the hands of the writers on optics, than has the minifying power of longer spaces with respect to greater objects. We are more conversant, for instance, with the appearance of a church, or a tree, at one mile, than of a pin's head at one inch; and, at these respective distances, we are more surprised to find the image of the latter enormously enlarged, than that of the former proportionally diminished, forgetting that the very same law is in operation in both instances.

This comparative indifference with respect to the examination of minute objects has arisen, doubtless, partly from their very insignificance, as we are accustomed, though not always justly, especially when examining those minute portions of the handiwork of God which bear upon them, without and within, the impress of the "hiding of his power,"* to consider little things contemptible, and beneath our notice; partly, again, from the fact of our being able to avail ourselves of the assistance which a lens made of glass affords, whenever we wish to examine any small body; and especially from the recollection that at any distance less than six inches, which is usually considered the shortest limit for distinct vision, a feeling of distress has always accompanied the attempt to distinguish the object, whilst at longer distances, the exertion is not only easy but pleasurable. In the former

* Habakkuk iii. 4.

case, too, as we shall have occasion to show, certain artificial contrivances are necessary to ensure vision at all, whilst, in the latter, the adjustment of the eye is without effort, and requires no adventitious aid.

Whilst the transmission of light through small apertures, in the way hereafter to be explained, serves to show, in a conclusive and striking manner, that apparent enlargement of objects which accompanies their diminished distance from the eye; the variation in the size of the images at different ages, and in different eyes at the same age, and which is found to be very considerable, may, possibly, at some future time, assist in determining the relative refractive powers of the transparent media of the eye; and hence may come to prove of utility in the diagnosis of disease, although it is not my intention to insist upon the application of the phenomena elicited by these experiments, either to the one or to the other of the above important uses, in this present communication.

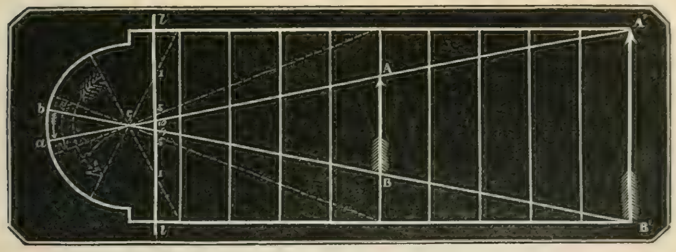
For the few preliminary remarks about to be offered, some apology might be necessary, were it not that a difficulty seemed inevitably to connect itself with any attempt at arranging the subsequent portions without them. These remarks will be found, indeed, to consist of a mere repetition of those principles in optics, which, to a certain extent, and in particular aspects, have received abundant illustration from experiments, to prove their correctness, and make them intelligible; and which, nevertheless, in other aspects, are calculated to afford new and important results,—for the laws of nature, so far from becoming deteriorated by a scrutinizing examination, always derive additional confirmation from the process. The *visual angle*, for instance, when applied to the investigation of objects at distances beyond ten inches from the eye, defines and measures the sizes of their images as depicted on the retina; and, when applied to the imitation of such images by delineating them on a vertical plane, it explains the art of perspective; and these are such familiar examples of the application of a general law as scarcely to require specifying. But, on the other hand, if the magnitude of the visual angle is inversely as the distance, the size of the object remaining the same; if bodies really do appear larger when brought very near to the eye, as we know they appear smaller when more remote; if, in short, the law of visual angle holds good for all distances, for the small as well as the great, it may be worth inquiring as to the kinds of pictures which are formed on the retina, when exceedingly small objects are held very near to the eye, so near, indeed, as to exceed the limits of distinct vision. That such objects present wondrously magnified images, if discerned at all, is evident. This, therefore, is just that kind of inquiry which, from having been instituted in comparatively a few cases only, serves to elicit new phenomena, of which it will be one of the objects of this paper to furnish a few illustrations.

That the *apparent* size of objects depends on their distance from the eye may be shown, with respect to short distances, by the following simple experiments:—Having closed an eye, place the forefinger of one hand across the palm of the other, and both at arm's length. Now gradually bring the finger towards the open eye, and its image will be gradually and yet sensibly magnified; and when the finger is about midway between the hand and the eye, its apparent size will be doubled. If a watch be used instead of the finger, the enlargement of its image will be very evident, and the palm of the hand which remains stationary will soon be completely hid by the apparent expansion of the watch. Whilst experiments of this, and of a like nature are, in themselves, of easy performance, and require no costly apparatus, yet they are not to be supposed the less useful on this account, although, there is reason to fear, this is too often the case; as if the mind were incapable of receiving any idea of the science of optics, excepting through the medium of finely-ground lenses set in burnished brass and polished mahogany. We are delighted, on the contrary, to discover the elements and exponents of general laws, themselves of the greatest importance, in combinations of such homeliness and simplicity.

Take a couple of cards, the one four times as large as the other, that is, twice as long and twice as broad; a card from a pack and the cover of a book will answer the purpose. Let the experiment be conducted as the preceding one, the small card being substituted for the finger, and the book-cover for the hand. Then, not forgetting to keep one eye closed, when the former is advanced to a station midway between the latter and the eye, both will *appear* exactly of the same size; in other words, the images of both will, as regards magnitude, be identical. Hence it appears that the linear magnitude varies inversely as the *distance*, and the superficial extent, or area, varies inversely as the square of the distance, for the finger was *twice* the length, and the small card *four* times the area, at half the distance.

The explanation of this law is founded on the properties of the *visual angle.* Let AC, BC, fig. 1, be rays from the extreme point of the arrow AB, which cross within the eye somewhere about its centre at C; then the angle ACB is termed the visual angle. A glance at the figure shows that the greater the divergence of the lines composing that angle,

Fig 1.



the greater will be the arc on the retina occupied by the image *a b;* and, also, the greater that image, the greater will be the angle included by the lines AC, BC, the lines of visible direction, for the angle *a* C *b* is always equal to the angle ACB. The visual angle varies exactly as the arc of the image, and as that angle may be formed with sufficient accuracy by drawing lines from the centre of the eye to the extremities or boundaries of an object, it affords a convenient expression of the length or size of the image. If a second arrow, A'B', twice as long as AB, be placed parallel to AB, and at double its distance from the centre of the eye, then, by the properties of similar triangles, their visual angle will be equal, and their apparent magnitude identical. Conversely, if the two arrows be parallel, have the same visual angle, or apparent magnitude, and one be twice as distant as the other, the more remote one must be twice as long as the other. The apparent magnitude of the *same* object, at different distances, may be inferred on the same principles. Thus, if AB approach the eye, remaining upright all the time, the visual angle will enlarge, and, at half its length, will appear double; or, if AB recede from the eye, it will be seen under a smaller angle, and appear proportionally smaller,—in fact, the apparent length of an object increases in the same ratio as its distance from the point C, within the eye, decreases. Hence, to bring an object near the eye is to magnify its image.

The degrees of divergence, or convergence, of the visual angle for smaller and larger distances of the object may be well shown by substituting black threads for the lines composing the angle. These threads may be attached to the extremities of the arrow, which should be made of wood, as well as the plane on which it is intended to slide to and fro. Ten vertical lines may be drawn on the plane, dividing any given interval between the arrow and the eye into ten equal parts, which we will suppose to be inches. When the threads emanating from the arrow A'B, fig. 1, are drawn through a hole bored at C, the arrow can be moved to and fro, at pleasure, by traction at the knot from behind, and thus the gathering and spreading of the rays of the visual angle are well imitated. When the arrow is brought near to the eye the threads are seen to diverge rapidly from C, and when it is removed to a greater distance they are brought nearer together. During the passage of the rays toward C, they may be supposed to be transmitted through a plane of glass, *l l*, held vertically before the eye, and on which the varying lengths of the image of the arrow, at different distances, may be measured. In this way it will be seen that, at ten inches, the space included by the visual rays (10 to 10), when they cut the plane of glass is very small, and this may be regarded as unit. At five inches (5 to 5), the space is exactly doubled, indicating that the image is twice as large at half the distance. At one inch the image is ten times as large, for the interval from 1 to 1 is ten times as great as the interval from 10 to 10. In all these cases the relative magnitude of the image, compared with its size when the object is ten inches distant, is found by dividing the distance for distinct vision of any object, and which is assumed to be about ten inches, by the new and shorter interval, produced at pleasure, by holding the object very near to the eye. Hence:—

At 10 inches	the image	will be	$= \frac{10}{10} = 1$
,, 9	,,	,,	$= \frac{10}{9} = 1\frac{1}{9}$
,, 8	,,	,,	$= \frac{10}{8} = 1\frac{1}{4}$
,, 7	,,	,,	$= \frac{10}{7} = 1\frac{3}{7}$
,, 6	,,	,,	$= \frac{10}{6} = 1\frac{2}{3}$
,, 5	,,	,,	$= \frac{10}{5} = 2$
,, 4	,,	,,	$= \frac{10}{4} = 2\frac{1}{2}$
,, 3	,,	,,	$= \frac{10}{3} = 3\frac{1}{3}$
,, 2	,,	,,	$= \frac{10}{2} = 5$
,, 1	,,	,,	$= \frac{10}{1} = 10$
,, $\frac{1}{2}$	,,	,,	$= \frac{10}{\frac{1}{2}} = 20$

At $\frac{1}{4}$ inch the image will be $= \frac{10}{\frac{1}{4}} = 40$

" $\frac{1}{8}$ " " $= \frac{10}{\frac{1}{8}} = 80$, &c.

The expression "to magnify an object," common in optics, is an unhappy, because incorrect, mode of stating the fact, that the *images* formed on the retina of the eye are always magnified, as the distances of the objects by which they are produced are diminished. This should be constantly borne in mind. To say that an object appears larger is merely to refer it, by a mental process, to some imaginary and greater distance at which it looked smaller; and it is said to be *magnified* when it appears greater than it is usually found at those distances, ordinarily considered necessary for distinct vision.

In using a single lens, the magnifying power, resulting from proximity of the object to the eye, and which is independent of the peculiar function of the lens itself, is commonly much underrated. When, for instance, we place an object in the focus of a lens held close to the eye, and having a small focal distance, and find its image magnified by the process, we are apt to merge the whole of these effects in the lens, and in its so-called magnifying power, forgetting the new position of the object, its short distance from the eye, its enlarged visual angle, and the resulting increased magnitude of its image. The magnifying power of a lens is equal to ten inches,* divided by the focal length of the lens. But it is worthy of remark that this is just that power which is possessed by diminished distance, independently of a lens, and is found precisely in the same way; that is to say, by dividing the distance at which we see any small objects most distinctly, by the interval of nearer approximation to the eye. Now a lens of half-an-inch focus is seen to magnify twenty diameters, for $\frac{10}{\frac{1}{2}} = 20$; but it has been shown, by the properties of the visual angle, that the apparent dimensions of any small object placed at the same distance are identical. Hence the difference between the magnifying power of a single lens, used as such, and the magnifying power of a short interval between the eye and the object, is equal to 0; in other words, a lens would appear to accomplish nothing more, magnitude alone considered, than proximity, aided by the refraction of the humours of the eye, can effect.

* This is stated differently by authors; some supposing the shortest limit for distinct vision equal to ten inches, others to five.

In giving such prominence to the powers of unaided vision, it must, however, be conceded that, in using a lens, the divergent rays are made parallel, and are thus prepared to enter the eye, and produce distinct vision. The outermost rays, too, those especially which are spread beyond an angle of 48°, would be reflected from the surface of the cornea, and altogether lost, and with them, a large quantity of light, otherwise appropriated to the illumination of the object, would disappear, were a lens dispensed with. Hence a lens of glass is useful in rendering parallel the rays which emerge from it after refraction; and it is more especially necessary, in gathering the most divergent rays, and thus bringing out the magnified image more distinctly by the larger quantity of light received from the object.

These remarks are not at all likely to have any effect in deteriorating the value of such an useful optical instrument as the double convex lens, the utility of which, in some form or other, everybody acknowledges. They are made the rather to bring out by contrast the remarkable fact of the increase, which may be observed in the size of those images which are formed, by examining objects at very short intervals *without* a lens. A subject which is inviting, comparatively unexplored, and doubtless worthy the attention of those engaged in optical science.

It is supposed that those rays only which fall within an angle of 48°, or thereabouts, measured on the surface of the cornea, pass through it, and are refracted in their passage. Those which are not included within this angle are refracted by the verge of the cornea and the sclerotic coat. Rays in rapid divergence, however, may enter the eye, provided the point from which they emanate be very near or close to it. There is, probably, no easier method of proving this than by tracing the course of the rays of light which are transmitted through small apertures of known dimensions, through the humours of the eye to their final destination on the retina. For when the size of any object and its distance from the eye are known, the visual angle is also known, and hence the magnitude of the image formed on the retina is easily discovered. But these small apertures may be held in any position, either near to, or remote from, the eye; and thus the magnifying power of short spaces is seen to be capable of easy demonstration.

This is shown in the following curious, and, as far as I am aware, previously unnoticed phenomena, which are based upon the transmission of light through small apertures.

In order to exemplify this subject, let a number of common pill-boxes be procured, about half an inch in depth and one inch in diameter;* and let small holes about the 1-40th of an inch in diameter, and at regular intervals of the 1-10th of an inch, be punctured through the bottom with a needle;† as in Plate VIII., fig. 1.

These apertures are best made by carrying the needle from without to within, to prevent their size and shape being afterwards altered by friction. Then let small pieces of tracing paper be stuck over them with gum water, to prevent too much light entering the eye. For this, by contracting the pupil, would diminish the size of the image. Their translucency may be still further slightly diminished by communicating a tint with water colours, and for this purpose the three primary colours, *red*, *yellow*, and *blue*, may be used, for reasons hereafter to be specified.

Combinations of these perforations related to the square, the pentagon, or the hexagon, may be left to the ingenuity of the reader. A few, partaking of the latter form, are subjoined; wherein the letters *r*, *y*, *b*, and *w*, refer respectively to the colours red, yellow, blue, and white. It may be premised that the tints should be laid on, as in ordinary tinting, coat after coat, until they become brilliant, without being at the same time opaque.

(1.)

y
b r
r y b

(2.)

b r
y y
r b

(3.)

r
b b
y y
b b
r

(4.)

b w r
w y b w
y r w r y
w b y w
r w b

(5.)

b
b r y b
y r
b r y b
b

(6.)

r y r
b w b w
r y r y r
w b w b
r y r

To these combinations we shall have occasion hereafter to refer.

* The size marked two drachms used by chemists and apothecaries.

† Sewing needles are generally sold in papers, which are numbered from one to twelve, according to their thickness. With a micrometer under a microscope, I examined the diameter of apertures made with needles from the papers marked Nos. 6, 7, 8, 9, and 10, and found them respectively equal to the 1-36th, the 1-38th, the 1-44th, the 1-50th, and the 1-70th of an inch.

Now, if distance produced no change in the apparent magnitude of objects, the apertures should remain as needle holes; and the colours should appear red, yellow, and blue, at all distances. But this is not found to be the case. On the contrary, when examined by looking through the bottom of the box from the inside, they are found to present the following phenomena:—

Fig. 2.

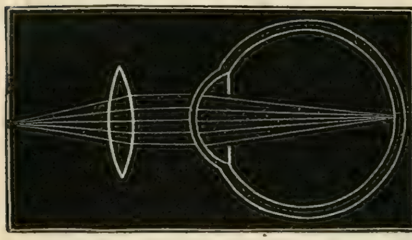
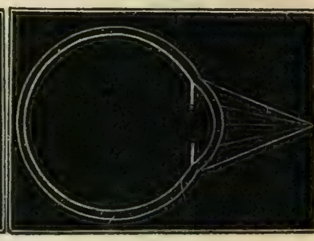


Fig. 3.



1. Bringing the box within an inch or two from the eye these small inlets appear to expand into circular discs, which touch one another at their margins (see Plate VIII. fig. 2). And when held still nearer to the eye they become so much enlarged as to overlap at their edges (Pl. VIII. fig. 3). But if the discs overlap, the colours must blend; and it is worthy of especial notice that this is actually found to take place. For when the three primary colours, red, yellow, and blue, are thus united in pairs, the secondary tints, *orange*, *green*, and *violet* are produced; and when the three primary tints all combine there is formed *white* light (Pl. VIII. fig. 3).

These effects are demonstrable, and the colours are sufficiently distinct by the light of a taper held a few inches from the eye; but they are more brilliant and beautiful when seen in a room screened from the direct rays of the noonday sun by an ordinary white blind.

2. Assuming each of these apertures to be an object, the circular disc is its enlarged image painted on the retina of the eye. It is difficult to obtain a correct idea of the size of these images without actually measuring them; inasmuch as this is estimated very differently by different individuals. To some, for instance, they appear to exceed an inch in diameter; to others they are as small as a fourpenny piece or a split-pea. My own imagination presents them as exactly resembling small coloured wafers. But it is probable that a cognizance of the actual size of the apertures themselves being associated with the perception of the image leads to an erroneous conclusion as to their real dimensions. In every instance, how-

ever, the magnitude assigned to them has been less, and never greater, than their true size.

In order to measure them it is merely necessary to compare them, that is, the images, when the apertures are held at half an inch from the eye, with circles, or diameters of circles, of known dimensions, placed at ten inches. This is effected in a rough way by holding a measure horizontally ten inches from the eye, and noticing, when this is examined by looking through the aperture, how many inches or parts of an inch are included within the area of the circular image. When used for this purpose the apertures should not be covered with tracing paper, and the kind of light used should be specified.

Now, by the properties of the visual angle it may be shown that when a small round opening, the 1-40th of an inch in diameter, or thereabouts, is held at ten inches from the eye, it presents an image equal to about the 400th of an inch across, and which appears as a mere speck. But when this same opening, or aperture, is examined at half an inch only, its image will be found so much enlarged as to cover a circular area two inches and a half in diameter, placed, in order to institute the comparison, at ten inches from the eye; and this may be easily proved by direct experiment. The apparent size of the disc has therefore undergone an immense increase by this simple process of bringing the aperture nearer to the eye. It is, in fact, magnified a thousand diameters. For—

$$\tfrac{1}{400} \text{ inch} : 2\tfrac{1}{2} \text{ inches} :: 1 : 1000.$$

But this gives the rate of increase in one dimension only. Hence, if our calculations are carried a little further, we shall find the entire area of the disc magnified one million times. For:—

Area of circular disc (α) $2\frac{1}{2}$ inches in diameter $= 4{\cdot}90873$

" " (β) $\frac{1}{400}$ " " $= {\cdot}000004908$

Then area of (α) : area of (β) : : (diameter)^2 : (diameter)^2

$$\therefore \frac{\text{area } \alpha}{\text{area } \beta} = \frac{(\frac{5}{2})^2}{(\frac{1}{400})^2} = \frac{25}{4} \times \frac{40000}{1} = 1000000.$$

Thus it appears that the image which is produced by examining a small hole made with a needle is magnified one million times by simply diminishing its distance from ten inches to half an inch from the eye.

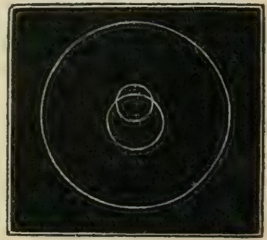
3. These discs are invariably diminished in size when, from any cause, the intensity of the light is increased. While, therefore, on viewing them by the direct rays of the sun, their margins scarcely touch, by diffused daylight, or the light of a taper, they directly overlap. This result is clearly owing to

the alternations of size of the pupillary aperture: for this circular opening expands when the light which enters the eye is diminished, and contracts when the light is increased.* For the same reason the discs always become smaller when the other eye is opened, and again resume their size when it is closed. This effect is instantaneous, and may be repeated again and again, as often as we choose to make the experiment. These rapid alternations of size in the circles, resulting from the alternate contraction and dilatation of the pupil, show, in a striking manner, how the quantity of light which enters the one eye regulates and controls the pupillary aperture of the other, and thus points to the necessity for shading both eyes in those diseases where it is important to exclude the light from either.

4. That the pencils of light emanating from these apertures cross within the eye, come to a focus, and form an inverted image at the bottom, may be inferred from the fact of their visual angle being less than 48° when they are examined at half an inch. And that the image itself is really inverted may be proved by making a second very small aperture close to the edge of the larger one (fig. 4). The image of this double aperture is an ovate disc seen in its true or erect position (fig. 5), in which position it would not be seen were it not inverted on the retina.

Fig. 4.

Fig. 5.



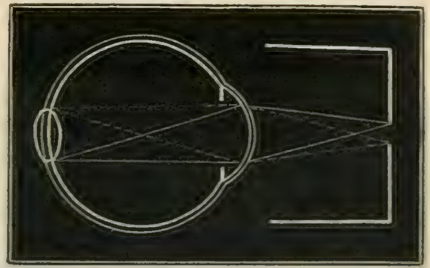
The visual cones emanating from these apertures, therefore are thus disposed (fig. 6) and transmitted through the humours of the eye, by which they are rendered convergent, and so come to a focus at the bottom.

5. It is worthy of notice that the innermost rays of these cones of light, which we have just examined, cross each other almost directly after proceeding from their radiant points towards the eye: and, as they do not again intersect during

* The ordinary dimensions of the opening of the pupil, seen through the cornea, are from the 0·27th to the 0·13th of an inch; and its mean size is the 0·20th of an inch.

their passage through its humours, the image of any small object held within the angle *a c b* should be formed erect on the retina, and should be seen, consequently, in an inverted position.

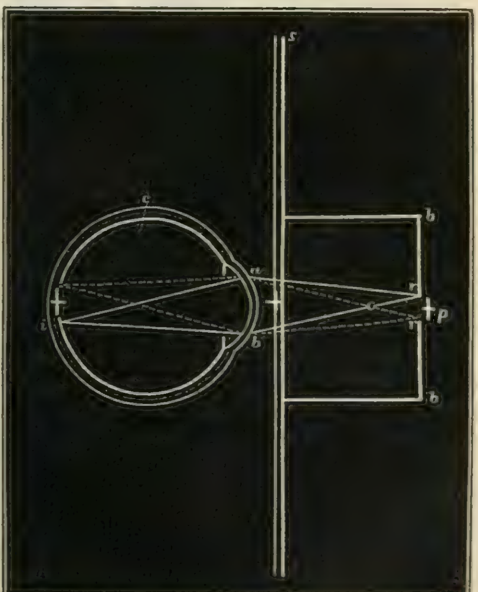
Fig. 6.



That this is the case may be proved by direct experiment. For this purpose I mounted a small cross about the tenth of an inch in length, and the twentieth of an inch broad, cut from a sheet of coloured gelatine, on the centre of a glass slide, with gum water, so that it might be used at pleasure. On holding this small cross between the eye and the aperture a small inverted image of it is seen, as if drawn in the centre of the disc. Moreover I noticed generally, that the images of all objects which were so placed as to subtend the angle *a c b* were seen in an inverted position.

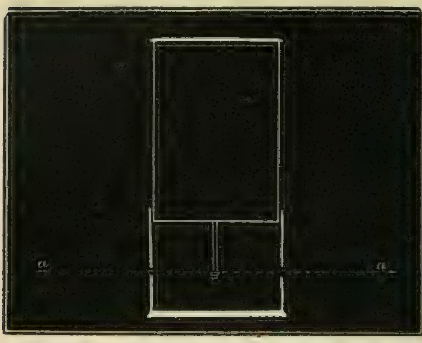
To illustrate this, let *r r* (fig. 7) be the radiant points proceeding from the aperture *p*, in the bottom of the box, *b b*, and let *r a b* *r' a b* be the cones of light passing through the slide of glass *s* and the eye *e*, to form a circular image on the retina *i*. Let the cross which occupies the centre of the slide be sufficiently small to subtend the angle *a c b*; then the rays of light *c a* and *c b*, which illuminate its extremities, are transmitted to the retina without intersection, forming there an erect image *i*, which is seen in an inverted position at *p*.

Fig. 7.



A rude modification of this experiment may be made with a pin, a cork, and a small pillbox. This latter should have a small hole, about the fortieth of an inch in diameter, made in one of its sides with a needle; and a circular piece, about the eighth of an inch in diameter, should be excised at a point exactly opposite the first from the other side. The pin is now stuck into the cork and the box inverted over it, the centre of each opening and the head of the pin being all in one straight line (fig. 8, *a a*). On looking through the small hole the pin is seen much magnified and in an erect position; but when examined through the larger opening it appears inverted.

Fig. 8.



6. In pursuing these investigations it occurred to me that, whilst in the *polyscope*, or *ordinary multiplying glass*, the multiplication of the images is effected by causing the rays which proceed from the object to travel towards the eye in as many different directions as there are images, *by refraction:* the same result might be obtained without a glass, *by transmission.* For it was clear, that if it be possible to view the same object by rays of light, all emanating from different points, and concentrating themselves in the centre of the eye, such an object must appear multiplied. Hence, on examining a small cross, similar to that which was used in the last experiment, under the light admitted through six apertures, by holding it between them and the eye, I was gratified to find my anticipations realized; for six inverted images of the cross immediately appeared: one being painted on the centre of each disc, as if by magic.

This phenomenon was scarcely less interesting than either of the former, for it seemed to present a new and anomalous position of the object, with respect to transparent media, in the formation of multiplying images; in short, a new kind of *polyscope* was discovered, having properties distinct from those belonging to an ordinary multiplying-glass. In order to point out the difference between these two optical instruments, it must be borne in mind that, in a common *multiplying-glass*, the object to be multiplied is placed on one side of the glass, and the eye on the other, while the images are conveyed to the eye by the aid of the refracting power of the glass. Thus, in the multiplying-glass with three faces, *a b c*, fig. 9, let the object be placed at *o*, and the eye at *e;* then the new images, *e i*, are seen in the direction of the emergent rays, *a e*, *b e*, after refraction; that is to say, the rays *a o*, *b o*, emanating from the object at *o*, converge on the opposite side of the glass, and enter the eye at *e*, which thus perceives three images instead of one: the first being that of the object in its real position, *o*, and the other two in the directions *e i* and *e i*. But the same result may be obtained, as we have said, without refraction, by holding a small object nearly close in front of the eye, and inspecting it by the light transmitted through small apertures, when the number of the images will be found to coincide with that of the apertures, and will be seen in the direction of the pencils of light which travel through them. In order to illustrate this, let *a b c*, fig. 10, be three small apertures, about the 1-40th of an

Fig. 9.

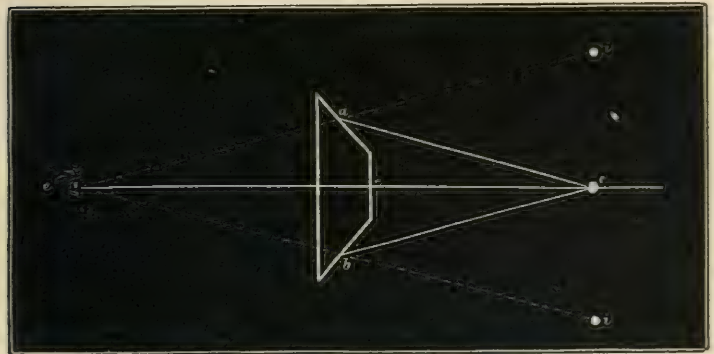
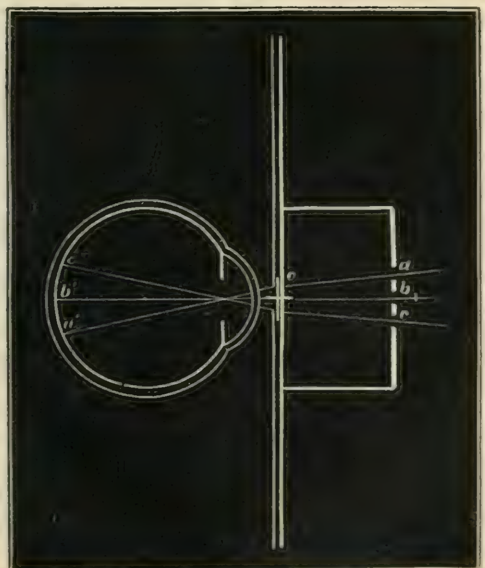


Fig. 10.



inch in diameter, and the 1-8th of an inch apart, made in one straight line in the bottom of a pillbox, and let *o* be a small cross of gelatine, as before described, or any other small, transparent, well-defined object mounted on a slip of glass. Let *e* be a section of the eye, and $a' \, b' \, c'$, the images painted on the retina: these images will be circular discs, having, in the centre of each, an inverted image of the object *o*, as shown in the opposite figure (11).

Hence the bottom of the box, which contains these openings *a*, *b*, and *c*, is similar to a multiplying-glass in producing as many images of the object as there are transparent facets in the glass, but unlike in these important particulars, that those facets are all in the same plane; that the light is not refracted by them, but merely transmitted; and that the eye and the object are both on the same side, that is, in front, of the multiplying medium.

In this experiment it may be instructive to notice: 1. The rectilineal direction of the visual cones in their passage from the apertures to the eye, each of which, although intersecting the others, and being crossed by them again and again, travels in one undeviating course, until it falls upon the cornea, and is finally brought to a focus on the retina, thus defining the *shape of the images*. 2. The last direction of the pencils of light which fall upon the cornea, determining the *position* of the images in space, for an object always appears in the direction in which the *last* ray of light comes to the eye. If the light which comes from a star were bent into fifty directions before it reached the eye, the star would, nevertheless, appear in the line described by the ray nearest the eye. 3. The influence of proximity of the object to the eye in increasing the *magnitude* of the image. 4. The perception of many images, of only one object, that object being rendered visible not by one pencil of light only, but by many pencils in different directions, explaining the cause of the *multiplication* of images; and 5. The peculiar arrangement and disposal of the rays, during their passage from the apertures to the eye, in producing an *erect retinal image*, and an *inverted mental one*:—for these are so many phenomena, each and all of which serve to show how the laws of light may be illustrated by means the most simple.

7. The multiplication of images, referred to in the last paragraph, has been shown to result from the simple transmission of light through small apertures. But there is a second case to be noticed, wherein double images are produced by refraction, that is, refraction during the passage of the rays through the eye, and without the intervention of a lens of any kind.

These double images are seen on holding a small object *behind*, instead of in front of, the apertures; and when seen, it is to

Fig. 11.

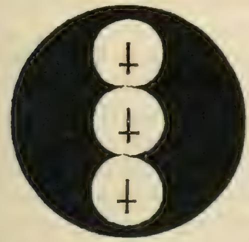
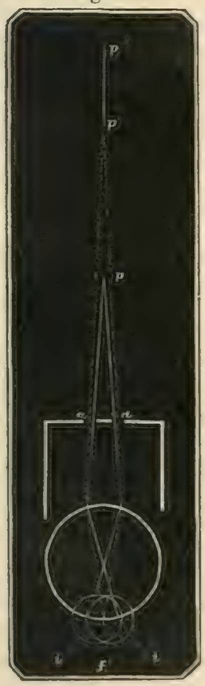


Fig. 12.

be noticed that they always occupy that oval space formed by the mutual intersection of the circular discs, fig. 12, *s*.

In order to explain this, let two small holes (*a a*, fig. 13) the 1-70th of an inch in diameter,* and the 1-20th of an inch apart, be made in the bottom of a box, and let a pin, *p*, be held behind the box, at the distance of an inch or two. On looking at this through the apertures, a double image of it will be seen at *i i*. When the pin is withdrawn to a considerable yet certain distance, say *p′*, a single image only will be noticed, the rays coming to a focus at *f*; but on bringing it nearer, the two again become visible. It is evident that these phenomena are to be referred to the refraction of light, during its transmission through the humours of the eye.

Fig. 13.



The refracting power of the eye varies at different ages, and in different individuals of the same age; and it would appear that its intensity may be measured by ascertaining the distance at which any small object, such as a pin, produces a single image, when viewed through two apertures, in the manner just described; for when the distance is known, the direction of the rays being determined by the interval of the apertures, the visual angle is also known; and when one image only is seen, the rays by which it is formed are brought to a focus exactly at the bottom of the eye, thus measuring the refractive power of its humours.

* Such apertures can be made with a needle marked No. 10.

But if the same object, at precisely the same distance, were examined by another eye, and if two images were seen instead of a single image, there is good reason to infer that, in the latter case, the transparent media of the eye would be endowed with powers of refraction greater than in the former; hence the relative refracting power of two eyes may be found by measuring the intermediate space between two points, say p and p', at which the same object appears as one, that is, forms a single image in two different individuals.

I have, in this paper, laid before my readers an account of several new phenomena which have occurred to me whilst investigating some of the laws of optics; in doing which it accorded with my design and professional avocations to be brief and perspicuous, rather than to write an elaborate essay. On a careful perusal, especially of the sixth paragraph, it will, doubtless, not have escaped notice, that certain of the results involve principles bearing a direct application to the construction of one or two amusing and instructive optical instruments, not heretofore invented. But I shall beg to reserve a more particular description of these for a second paper.

(*To be continued.*)

On Closterium Lunula. By the Hon. and Rev. S. G. Osborne. Communicated by Jabez Hogg, Esq.

"I have now examined with great care more than one hundred specimens of *C. Lunula.* I will give you the result as impressed upon my own mind.

"I believe this plant (if plant it be) consists of an outer case, of a hard and almost insoluble material, having at each end a minute aperture, opening to receive the water in which it lives. This case is divisible in the centre; about the middle of the concave side there is some difference in texture or construction, for the slightest pressure will rupture it, allowing the endochrome to escape.

"The endochrome itself is contained in a very thin membranous sac, free of the outer case, except, perhaps, at one or two spots about the centre, where the streak of light is perceptible. This sac is highly elastic, and often by its contraction or expansion alters the appearance of the plant. At its extremities it has apertures to receive the fluid brought through those in the outer case.

"The endochrome, consisting of green matter, changes its appearance with the growth of the plant; sometimes it nearly fills its investing membranous sac, at other times it leaves a good deal of it, as well at the ends as at the margin, empty. At the extremities of the green matter there are certain bodies acting with a ciliary movement within what has been called a chamber, being towards the point of the membranous sac: certain bodies, apparently of the same kind, occasionally separate from the endochrome in a small mass, appearing at the extreme end of this so-called chamber, or at the side close to the end; these also impart a ciliary movement to the water within the sac, around them.

"Over the whole surface of the endochrome I can not only trace a distinct circulator, but the action of cilia. In one specimen, which I had the pleasure of showing for some hours to Mr. Mansel, of Spetisbury, a neighbour of mine, devoted to the microscopic observation of the Desmidieæ and Algæ, the bunch of ciliary matter had got to the extreme end of the internal sac, which was so expanded as to fill up the whole point of the outer case; they thus abutted on the outer aperture opening upon the water; the result was an evident action on the water, in which we could see the points of cilia working externally to the point of the plant; the water was thrown in jets quite as far as in the annexed sketch. I use

the word jets, for it was not like any other action on water I have ever seen produced by cilia. The water was spirted in globules, similar to what one would expect to see in a microscopic fountain. We had the pleasure, with the members of my own family, of watching it for many hours. I used a $\frac{1}{4}$-inch of Ross's, a $\frac{1}{2}$-inch of the same maker as illuminator, with a prism, and the usual bull's-eye condenser; the light was taken from a strong moderator lamp; the eye-piece, a very powerful and clear one, made for me by Mr. Ladd.

"As to the circulation (no new discovery) I never find any difficulty in tracing its course. My theory is this:—there are cilia, more or less in number, over the whole endochrome; peculiar clusters of them at its extremities; these keep up an action, attracting and repelling fluid drawn to them, through the apertures in the internal and external cases. The fluid, when received within the membranous sac, is impelled over the whole surface of the endochrome by the cilia. Between the outer case and the inner one currents are kept flowing, receiving, as I believe, their impulse from the same action.

"When we saw the ciliary action external to the plant, as described above, we saw that the marginal currents had ceased to flow. I infer from this, that by the accidental pressure of the glass, the active mass of ciliary bodies had got so close to the end of the sac as to press out to the edge of both apertures, that of the sac, and that of the external case. The machinery thus displaced gave us that hydraulic action *outside* which was proper to the *interior* of the plant; I assume that the normal action of these cilia would jet what water they did not send over the endochrome back with some force against the interior of the outer case, and thus force it into the currents we see.

"If I put a specimen on the stage, cover the stage so as to exclude the light, use the parabolic illuminator, with the direct light of the sun; in certain focal positions I see what appears to be cilia working evenly and continuously along the whole external margin of the plant. I am inclined to believe it is not so, that this is some ocular deception, and that these cilia, so seen, are within the outer case; it may be that there are cilia on the external surface of the membranous sac, as well as over the endochrome. More practised observers, with higher *powers*, may yet determine that; of the existence of the cilia throughout the plant there can be no doubt, and no object I have ever seen will bear comparison with this, when beheld under a sun light; it is, indeed, a Godlike work, as wonderful as beautiful.

"It is very seldom that I can trace a current up one margin and round the point down the other; these currents seem to me *as the rule*, to pass from the point, when they reach it, down to the centre of the spot, where the cilia are seen terminating the endochrome.

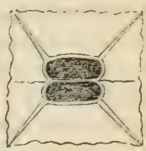
"I have just seen a specimen,* but not of *C. Lunula*, of this shape; the shell of the plant had formed itself into two halves, one overlapped the other, its outline quite clearly defined;

in these two divisions we could see the circulation going on, as I have dotted it, round each segment separately, at the circular extremities. The loose bodies seen in the *chamber* of *C. Lunula* have very generally cilia, and are, I believe, zoo-spores; loose pieces of endochrome are sometimes brought round in the current, but these are easily distinguished; I

* *C. Leibleinii.*

have never seen anything like true cyclosis, *i. e.*, molecules, in circular movement within the so-called chamber. Although I have purposely burst many specimens when under view, I have never seen the green matter, in passing out, get between its own sac and the outer case. I leave the above at your disposal, hoping it may prove interesting, only claiming for it the attention due to the working of one who is but a tyro compared to many of you, though a hard-worker, and devoted to the studies the microscope so abundantly affords, of his Maker's works.

"Are you aware that the *Arthrodesmus Incus* drawn in the books should be drawn as represented in the annexed figure? It has a very beautiful hyaline membrane stretching from point to point, cut at the edges, something like the *Micrasteria.* A moment's good manipulation under a high power will prove it, especially with the aid of colouring matter in the water.



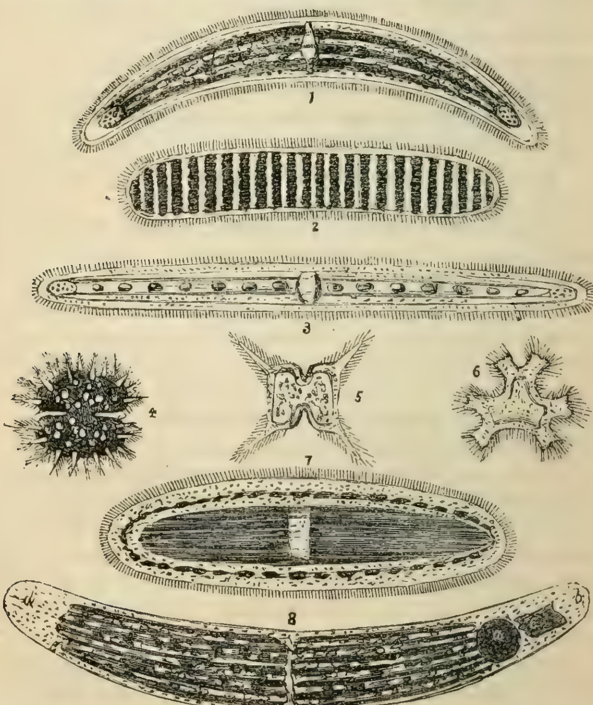
"Why is *Xanthidium armatum* drawn without the setæ, clearly to be seen between its processes?

"I can discover cilia in the *Penium*, *Docidium*, and *Xanthidium;* but not to the same degree of clearness as in the *Closteria;* circulation can also be seen in some other *Desmidieæ.* Make any use you please of this, I fear, lame account of what has been more interesting to work than it is easy to describe, or, I fear, likely to interest those who read it."

Having received a liberal supply of specimens from the Rev. Mr. Osborne, I have great pleasure in confirming his observations with respect to the ciliary motion. I made use of Shadbolt's glass parabolic reflector, with an $\frac{1}{8}$-inch object-glass and a deep eye-piece. The sun was shining at the time, and I threw a ray upon the *Closterium*, when, to my delight, I saw the whole frond brilliantly glittering with the moving and active cilia, as represented in the drawing, fig. 1; whilst in the cyclosis numerous zoo-spores were most actively moving about by the same agency. It is impossible to imagine a more beautiful object and spectacle than was here presented. When the sunlight, falling on these little bodies, warmed them into life and motion, the rapid undulations produced by the action of the cilia, illuminated the whole frond with a series of most charming and delicately-coloured prismatic fringes or Newton's rings.

The motion and distribution of the cilia must be seen by the aid of the direct sun-rays and parabola; for, although I tried every other mode of illumination, and, with Mr. Brooke, used Gillett's condenser, yet neither of us noted satisfactorily their situation and distribution until we resorted to the parabola.

At the same time the circulation may be most accurately observed to take place over the entire surface of the pond. The stream is best seen to be running up the external margin, just internal to a row of cilia with another taking a contrary direction next to the serrated ciliary edge of the endochrome, the whole being restricted to the space between the mass of endochrome and hyaline integument, passing above and around the cyclosis, but not entering into it. Mr. Bowerbank appears to have observed this a few years since, and Mr. Ralfs in describing it, says:—"I at first supposed that the circulation was confined to the margins, nor did I perceive it elsewhere until Mr. Bowerbank adjusted the microscope, and showed me the motion extended over the whole surface of the endochrome." The Rev. Mr. Osborne has just sent me a drawing, with description and other observations, which may prove interesting to microscopists.



Intended to show the cilia surrounding the frond; by mistake of the engraver the outer *hyaline case* has been cut away.

The Rev. Mr. Osborne, in a letter to me dated May 31, says :—

"I have scarcely failed in one attempt to see the circulation and ciliary motion in the *Closterium Lunula;* I tried to-day heating a little water, by putting a small bottle in a cup of warm water; the effect seemed to retard the circulation, but to make the globules larger. I have traced it over the whole extent of the endochrome, but it is best seen at the convex side, a short way from the edge. I am more than ever convinced the cyclosis is the waving of *attached tongues of cilia.* The specimens are capricious in the results they afford; they show best when the sun has been on the jar for a time. I have watched the movements of the globules in *Vallisneria*, *Nitella*, &c., and they are to me altogether of a different nature to that in the *Closterium*, &c. To my eye there is no real analogy between this circulation and that in the above plants; but there is much more with the branchial action in the mussel.

"When the endochrome is of a rich dark green, I find the chamber at the extremity very plain and defined, with its cilia very active, margin of hyaline case very clear and broad, and the circulation most evident. As the endochrome gets of a lighter colour, it pours a greenish matter towards the edges, excluding a sight of the former circulation, but yet itself showing a more sluggish movement. The chamber gets smaller; the cilia are barely seen. The next stage is without the sporangia, or as represented in the drawing fig. 8. The sporangium at *a* was of a brownish colour, with the endochrome broken on each side of it. The circulation was very plainly seen in two or three currents all round the edge. At *b* I could easily trace it in several rapid streams coming from each side and then turning down, as if it was attracted to, or had some channel under the detached piece of endochrome, *c*. There were several clear round globules, whose course was easily traced; they seemed carried about by the regular streams to the end, from whence they returned and joined one or other of the streams passing back along the edges. The cell in which the cyclosis, or cilia, are usually seen, was not to be made out, nor could we see anything like the moving bodies I believe to be cilia. At *d* the same streams were visible, and had the same appearance as at *b*, coming up to the sides where the arrow is placed, then returning towards the centre of the endochrome, seeming to pass under it, the back stream beginning at *e*. The hyaline edge was very narrow, the endochrome having disappeared from the ends, there remained large spaces without chamber or cilia, but with a distinct circulation going to the centre of the apex from each side in many streams. I used a $\frac{1}{4}$-inch Ross; having no good eye-piece to my travelling instrument, I applied the upper half of an excellent telescope of the same maker's, which answered very well."

I may also state that I found the ciliary motion going on in all the specimens I examined; and the water supplied was exceedingly rich in this class of objects, containing *Closterium Lunula*, *C. acerosum*, *C. didymotocum*, *C. costatum*, *C. rostratum*, and *C. setaceum*.

Docidium nodulosum and *clavatum*, *Penium digitum*, and *closterioides*, all well showing the circulation and ciliary movement. *Anthrodesmus incus*, some good specimens showing an active motion of cilia around the spines. This was better and very beautifully seen in specimens of the *Xanthipium fasciculatum* and *armatum*, *Cosmarium undulatum*, *C. pyramidatum*, and *C. margaritiferum*, a very active internal motion of granular masses and ciliary motion surrounding the external margin.

Euastrum oblongum, *Micrasterias denticulata*, rather more flattened in its longest axis than those represented by Mr. Ralfs, circulation of granular particles around the internal cells evidently of a ciliary character.

Didymoprium Borreri and *Desmidium quadrangulatum*, with many other specimens, all showing ciliary movements of a more or less active character imperfectly represented in the drawing, figures No. 1 to 7.

On two new FUNGI. By FREDERICK CURREY, M.A.

I HAVE lately met with two minute *Fungi*, which do not appear to have been hitherto described, and as their microscropic structure is curious and interesting, the following account of them may be acceptable to some of the readers of the 'Microscopical Journal.'

The plants are figured in Plate IX. Figs. 1 and 2 represent specimens of one of these *Fungi* drawn to the natural size; figs. 3 and 4 represent the same specimens drawn with the camera lucida, under a two-inch object-glass; and fig. 5 represents the *capillitium* and *spores*, drawn in the same way, with a $\frac{1}{4}$-inch object-glass. This *Fungus* grew very sparingly on the inner bark of a dead tree. The *peridium* is irregular in shape, varying much in different specimens, but in almost all it is more or less serpentine, following the sinuous cavities between the wood and the bark. In colour the *peridium* resembles that of many of the *Trichiæ*, and one of the most perfect of the specimens in my possession bears considerable resemblance *externally* to *Trichia reticulata*, or, as it is now called, *Trichia serpula*, figured by Dr. Greville in the Scottish 'Cryptogamic Flora.' An examination of the *capillitium*, however, proves that the plant does not belong to that genus, but would rather lead to the supposition of its being allied to *Arcyria*, the *capillitium* being somewhat reticulated and minutely denticulate. But it will be observed, that the dehiscence of the *peridia* is longitudinal, and not circumscissile, as is the case with *Arcyria;* nor does the *capillitium* appear to be persistent.

Another characteristic of the present *Fungus* is, that the *capillitium* (fig. 5) is of two kinds, the spores being attached to the more delicate one, which may be seen in fig. 5 as a narrow yellow line connecting the spores, but of which it is almost impossible to give a correct idea in a drawing, on account of its excessive fineness and transparency. The coarser *capillitium* is densely interwoven and somewhat reticulated, but much of the apparent reticulation arises from the passage of the threads over and under one another, and does not result from actual conjunction. The existence of a double *capillitium* has hitherto, so far as I am aware, been observed only in *Leocarpus vernicosus* (*Diderma vernicosum* of Fries), from which the present *Fungus* is distinguished by the absence of the double *peridium.* The coarser *capillitium* is swollen here and there, and the swellings are of a darker colour than the rest of the threads. These swellings occur usually in the course of the threads, but they seem to be sometimes terminal; and one of those represented in fig. 5 has the appearance of being in the act of discharging small granules from a hole at its apex.

From the above description it will be seen that this *Fungus* unites some of the characteristics of three different genera not far removed from one another, viz. *Trichia*, *Arcyria*, and *Leocarpus* (or *Diderma*); whilst, at the same time, there is not sufficient similarity to admit of its being classified under any one of them. If it has not been hitherto observed I should propose the name *Ophiotheca*, in allusion to its flexuous *peridium.* The colour of the spores would lead to the adoption of '*chrysosperma*,' or some analogous epithet, as its specific appellation.

Figs. 6 and 7 represent the other *Fungus* to which this paper relates, drawn to the natural size; and fig. 8 shows a single specimen, laid open with needles and magnified 220 diameters. From the different opinions which I have received respecting it there seems to be considerable doubt as to its affinities. It appears to me to be almost identical with *Trichoderma*, consisting, as it does, of a central mass of spores surrounded by a dense covering of hairs. It differs from *Trichoderma* in the jointed nature of the hairs, and in the colour of the spores, which, in fresh specimens, is a brilliant yellow. This plant was found upon fragments of dead leaves and sticks lying on the ground in a very moist, almost muddy spot, in a wood. The specimens grow sometimes singly, and sometimes in small patches. If a new genus be constituted for it, I think that, from the jointed nature of the hairy covering, '*Arthroderma*' would be an appropriate name.

DESCRIPTION OF PLATE IX.

Fig.

1 and 2.—Natural size of one of the Fungi.

3 and 4.—The same magnified by a 2-inch object-glass.

5.—The capillitium and spores under a $\frac{1}{4}$-inch object-glass.

6 and 7.—Natural size of second Fungus.

8.—Spores and hairs in its interior under a power of 220 diameters.

TRANSLATIONS, &c.

On Contractile Tissues *in the* Hymenomycetes. By Prof. Hermann Hoffmann. Botanisch. Zeit. (Dec. 9th, 1853.)

The *annulus* on the stipe of *Agaricus muscarius* is free at the lower margin, hanging down in folds; above it blends more and more intimately with the outer surface of the stipe, and ultimately can no longer be definitely distinguished from it. The upper side of this frill-like investment of the stipe, or that which is directed towards the angles of the gills, is covered with a delicate loosely-felted tissue, which is, at first, white, but, as the *fungus* grows, assumes a yellowish tint. This covering is especially well developed, superiorly in the obtuse angle, formed between the projecting *annulus* and the stipe. If a small portion of the surface of this felted tissue be raised with a fine elastic needle, and placed in a drop of water, care being taken to avoid any strong pressure, whilst the point of the needle is moved about in the water until the particle is detached, it will be observed even under a moderate magnifying power, but very distinctly under one of 363 diam., that the delicate filamentous cells of the structure are furnished with a vast number of gelatinous nodosities, or verrucosities, from which, occasionally, and frequently in numbers together, actively *vibrating* filaments project. They have the apparent length of ½ to 1 inch, and the thickness of a needle; but more close investigation soon shows that they vary in length and thickness. The latter reaches, on the mean, 1-450th of a Paris line, varying from 1-800th to to 1-200th. The length is usually about 3-100ths, varying from 1-200th to 7-100ths. A gradual transition may be observed from this filament to immotile, discoid, minute elevations of the gelatinous matrix, and lastly, to simple, verrucose, nodular corpuscles. The movement of these vibratile filaments is very remarkable, and is especially well adapted for microscopical demonstration. It has the closest resemblance to that of the horns of the Snail, but is far more lively; it resembles also, in some respects, the rowing motion of certain *cilia*, and the sweeping undulations of the *Oscillatoriæ*, or even an infundibuliform movement will be observed to predominate. Irritation of the filaments is succeeded by a violent convulsive movement, followed by a depression of the extremities; sometimes by a spiral involution of the upper portion towards the lower; lastly, and most frequently, a rapid retraction of the filament into itself may be noticed, exactly as in the horns of the Snail. Spontaneous and perfect retraction, without any external cause, I have never observed. It is extremely rare that this movement alternates with a remission, or actual brief repose; it is independent of the time of day, or the age of the *fungus;* and it continues for a long time after the separation of the shred from the parent stem. If the preparation be placed in a little glass cell, closed so as to prevent evaporation, and brought under the microscope, distinct, though very faint, movements may be perceived, even at the end of 48 hours; ultimately, the substance is affected by the water in which it is macerated, the filaments shorten, and, at last, their remains, together with the gelatinous nodosities, appear to become distended into vesicles with large vacuities in the interior.

The *form* of these vibratile filaments is usually perfectly cylindrical, and they frequently present, at the upper end, a minute head formed of the same substance as the filaments, that is to say, of a greenish, brilliant, highly-refractive material, without a trace of its being jointed, even under the highest magnifying power (680 diam.). In the more rare, thicker vibratile filaments, there may be perceived in the filamenf, a tubular cavity occupied by fluid, and presenting a faint rose colour; this cavity is continued into the head, where it assumes the appearance of a sharply-defined *vacuole,* like a cell *nucleus*; on rare occasions, this head is separated by constriction, when it remains motionless, close to the filament. Although the head is usually formed by a manifest enlargement of the extremity of the filament, I observed, on one occasion, the following peculiar process. The end of the filaments became suddenly thickened in the upper fourth of its length, in such a way, that it presented the appearance of a thicker cylinder supported by a more slender inferior one; the thicker portion quickly became divided in the interior, almost throughout the entire length; in the middle of this cleft, a minute quickly-growing globule suddenly appeared, whereupon the whole upper portion increased in width, becoming shorter at the same time, ultimately assumed a globular form, and presented a well-defined articulation with the slender stem, whilst the usual vacuole was developed in the interior. The head very often forms its vacuole quite independently, the stem remaining solid.

These vibratile filaments, when the felted tissue is in the dry state, do not exist ready formed; they require, for their development, to be moistened either by rain, or purposely, and it not unfrequently happens that they may be directly seen, under the microscope, to spring from a gelatinous nodule, scarcely a minute being requisite for the process. In the same space of time, one of these filaments will attain the length of 3-100 to 6-100ths; it occasionally, also, happens that a lateral shoot is formed, which then continues to grow instead of the original principal axis of the disc or filament; in this way is constituted an unequally bifurcate vibratile filament.

The *matrix*, out of which the filaments are protruded, and into which they retract, invests the cells of the felted tissue, in the form of a gelatinous, nodular, faint-yellowish substance, sometimes presenting nearly isolated nodules, and closely adherent, but sometimes forming larger fragments, united, so as to constitute loosely-applied lobes, in the interior of which may be perceived larger and smaller vacuoles of variable forms, in great numbers. In making the preparation, it very rarely happens that a vibratile filament is wholly detached, but when isolated, it still exhibits movement.

The most important circumstance characterizing the *relation* of these structures towards external influences, is the following; from which, also, the very necessary caution to be observed in the exposition of them is shown.

Iodine colours them yellow, afterwards brownish; sulphuric acid does not affect this colour, nor is any change observed when the reagents are employed in the reverse order; and particularly is no *blue* tinge observed, as is, by no means, unfrequently the case, otherwise with regard to the tissues of *fungi*: the application of syrup and sulphuric acid does not alter the colour. In both cases the application of the reagent is followed by the rapid death and collapse of the filaments, which are fused into a gelatinous mass, which gradually becomes very transparent, and somewhat vesicularly distended; some vacuoles, at the same time, appearing in the interior.

Caustic potass produces a rapid contraction, and afterwards dissolves the filaments so that they become unrecognizable; nothing remaining but a fluid abounding in granules. This is effected in a few minutes and without the aid of heat.

Carbonate of potass induces lively extension, curvature, and contraction, succeeded by the formation of numerous nodules and vacuoles, the substance becoming perfectly clear as if solution had commenced; at the same time several gelatinous nodules, in the form of drops or vesicles in which vacuoles may be distinguished, float on the fluid.

Ammonia causes rapid contraction, without any immediate solution.

Common salt induces gradual contraction. When afterwards immersed in pure water for an hour, not a single filament could be seen protruded; and even at the end of twenty-four hours only indistinct traces of them could be perceived.

The *electro-magnetic current*, sufficiently powerful to decompose water, kept up for ten minutes, exhibited no definite and visible effect upon the filaments.

Alcohol caused rapid contraction and hardening.

Oxalic acid produced the same effect.

Nitric acid caused rapid contraction, succeeded by a vesicular protrusion of the gelatinous nodules. When warmed for a short time the substance assumed a yellowish colour; it is consequently perhaps of an albuminous nature.

Chloride of calcium: rapid contraction into nodules; this agent consequently is not fitted for the preservation of such preparations; for which purpose I do not know any general vehicle.

In *sulphuric ether* the motion continues some time, but is ultimately succeeded by subsidence of the filaments and the subsequent formation of vacuoles. Nor does prolonged maceration in water again render the filaments apparent.

Cherry-laurel water exerts little influence; whilst some of the filaments contract into little masses, the majority vibrate (undisturbedly) even after immersion for ten minutes.

Syrup, added gradually, hinders the movement and causes contraction. If the fragment be placed at once in concentrated syrup, not a single vibratile filament is apparent; but if the preparation be afterwards placed in water, the filaments make their appearance in about ten or fifteen minutes; a fresh application of syrup induces renewed contraction.

The filtered or unfiltered juice, expressed from the stipe of the *fungus* itself, destroys the motion and causes contraction; but immersion of the preparation in water for half an hour restored the extension of the motile filaments. The reapplication of the juice of the *fungus* acted in the same way as before. *Monades* and other *infusoria* were not affected by it.

The quality of the water, except as regards the accidental admixture of saline or pungent substances, is of no great consequence. Rain or spring-water act in the same way. The influence of *temperature*, however, is much more important. In a drop of water at the usual temperature (13° R.) the motion goes on actively, nor is it altered by the continued addition of luke-warm water (20° R.); but when water warmed to 28° is added by drops it is immediately stopped, the filaments rapidly contracting on themselves. The addition of very cold water produces the same effect (+ 5°); but in the latter case the contraction takes place but slowly, and an excellent opportunity is thus afforded for the study of the mode and nature of the contraction. If warm water (29°) be added every half minute, for about a quarter of an hour, and the preparation be then let alone, a few filaments make their appearance in about an hour, whilst the main bulk of the gelatinous nodules have become vesicularly distended, not unlike detached soap bubbles. If a preparation presenting this nodular condition be heated over the flame of a spirit-lamp till it boil, no change is apparent in it, and especially is there no protrusion of discs, filaments, or any other kind of elongations.

Out of water and lying free in the air between plates of glass, no filaments are developed, nor can any indication of movement be perceived in the minute gelatinous nodosities.

A moist atmosphere keeps the *fungus* for some time,—several days,—in the condition fitted for this investigation, on which account it is best preserved under a bell-glass; when exposed to the dry, warm air of a room, the felted substance is so closely matted that it is impossible to detach it from the point of the needle in the drop of water, without tearing it so much as to destroy it.

Much pressure is especially prejudicial in this inquiry. The delicate filaments are thus reduced to a consistent mucus or fat, lose all form, and never regain any definite figure or vital activity; whilst the same degree of pressure has no injurious effect whatever upon any spores that may happen to be contiguous.

The *occurrence* of these filaments is not limited to the situation above indicated. With patience they may also be found occasionally at the upper end of the stipe, as well as towards the border of the *annulus* (upper side); on one occasion I met with them on the free borders of the *lamellæ* in the felt-like substance, which in the partially open *fungus* had previously separated the *lamellæ* contiguous to the stipe from its surface. I have noticed them in no other part of the *fungus*.

I have repeatedly met with similar bodies in the corresponding situations, in *Agaricus eburneus*, Bull., and also in the widely-remote group of the *Hygrophori*, whilst I have in vain looked for them in the nearly allied *A. phalloides*, *campestris*, *fascicularis*, *lateritius*, *melleus*, *procerus*, and *Boletus granulatus*.

The consideration, especially of the situation of these structures, would naturally lead to the supposition that they may be connected with the process of fertilization. In fact, at first, I thought I had at last discovered in them '*sterigmata*' and '*spermatia*.' The careful pondering upon the above-described relations, however, compelled me to abandon this notion.

Another view had regard to the relation of these bodies to infusorial organisms. But the constancy of the occurrence, and that at all ages, even in *fungi* still completely closed, and in a perfectly recent condition, excluded the notion of their being parasitic, animal formations, to which their conditions of organization were also opposed. But, although from what appears, we are not justified at once in referring these structures to the animal kingdom, this much is obvious, that we have to do with an organism, having much of the animal nature in it.

This appears above all in the remarkable movement; but is also indicated in their physical and chemical properties. It seems to me indubitable, that we have here presented to us a contractile substance, identical with, or very closely allied to that, whose very extensive occurrence in the animal kingdom has been pointed out by Dujardin (Hist. Nat. des Zooph. 'Infusoires,' 1841, p. 35), and termed by him 'sarcode,' and which is also described by Ecker (Zeits. f. wiss. Zod. I., 218, 1849). This substance constitutes the bodies of the Infusoria, especially of those belonging to the polygastric class; contractile drops, nodosities, filaments, and even root-like ramifications are frequently produced with great rapidity under the influence of water, especially if slight pressure be employed; this may be seen in the *Rhizopoda*, *Hydraida*, *Polypes*, &c.

Some time ago I left a number of fresh specimens of *Polyporus versicolor*, covered, in a porcelain dish, and at the end of several days found at the bottom of the vessel a few drops of a white fluid, which I submitted to the microscope. Together with a great multitude of spores I noticed in this fluid a number of that remarkable, amorphous animalcule, termed by Müller *Proteus*, and by Ehrenberg *Amœba* (*Amiba Gleichenii*, Dujardin, Tab. 4, fig. 6), and which consists wholly of 'sarcode.' I am therefore in a condition, from my own observations, to establish the similarity of the structures now in question, with respect to the physical conditions of their substance.

The little masses of sarcode constituting the living tissue of the common *Spongilla* also presents scarcely any difference (Dujardin, l. c. Tab. 3, fig. 19).

This remarkable substance constitutes the entire body of the *Euglenæ* and of *Hydra*; in the *Rotifera* and *Tardigrada*, it constitutes distinct organs corresponding to muscles; it forms the contractile, yolk-substance of the eggs of *Limax;* it occurs in the *Trematoda*, *Cestoidea*, the cystic worms, and in the *Annelida;* of it are composed the muscles of the young, just liberated insect-larvæ; and lastly, according to Dujardin (l. c. p. 40), it probably constitutes the substance between the skin and muscles of the fish, and, further modified, the fibrous muscles of such peculiar morphological characters in the higher animals. In these lower structures, therefore, of the vegetable kingdom we may perceive the first distinct indication of an organ, which presenting a continually increasing development extends even up to man; a new link in the chain connecting the animal and vegetable kingdoms.

It does not appear to me inadmissible to assume that the *cilia* of the spores of *Algæ* present the closest analogy to our vibratile filaments; *cilia* are also known to be very widely distributed throughout the animal kingdom; and everywhere presenting lively movement without any articulation, an active molecular motion, as the innate property of the substance. Moreover, I conceive there is no ground for supposing that motile phenomena of this kind, occurring so extensively, are to be explained by '*endosmosis*,' which would, in fact, be an attempt to elucidate one fundamental phenomenon by another, and to get lost in a maze of hypotheses.

The similarity of these filaments with certain *algæ* is more apparent than real, and disappears when they are more attentively considered. The most obvious comparison is with *Oscillatoria*, the refractive property of which is almost the same, and even whose colour is frequently identical. [The author then proceeds to describe the effect of certain chemical reagents upon *Oscillaria limosa* and *Phormidium vulgare*, from which he concludes that neither of those plants contains *sarcode*, or at all events presents a substance only distantly related to that principle.]

But the sarcode appears to me to be very closely allied, if not identical, with the 'primordial sac,' a conclusion to which Cohn (*Hematococcus*) was also led, although in a different way. I am led to make this assertion by the relations of the primordial sac in the conjugated cells of *Spirogyra*, as well as during the development of the spore-sac, as I have observed in *Peziza vesiculosa*, *Bulgaria inquinans*, in the process of growth of several Agaric spores, and in the germination of *Uredo* and *Fusarium*.

I cannot here refrain from broaching a supposition, relative to the motile phenomena of higher plants. My observations (Unters. üb. den Pflanzenschlaf; Giessen, 1851) show in the most indisputable manner that in these remarkable phenomena there may be witnessed fatigue, torpor, and recovery by rest, precisely as we are accustomed to observe under similar circumstances in animals. Fée (*Mimosa pudica*, in Mém. de la Soc. d'Hist. Nat. de Strasbourg, t. iv., 1849,) from similar observations has been induced at once to affirm, that with our present knowledge of this process, it is impossible not to assume the existence of a contractile substance in *Mimosa pudica*. I believe that this opinion is in some degree supported by the observations communicated above, but I must also add that I have not succeeded in actually finding in the *struma* of the leaves in the sensitive plant, a substance similar to that which occurs in the *fungi* above enumerated, even under the application of the best reagents. It is true, the specimen upon which I experimented, it being late in the year, was nearly dead, and presented even upon violent agitation only very faint movements. It is possible the result might be different in the warm summer season.

[The author's argument with respect to the identity, at any rate in some respects of the motile substance in the animal and vegetable kingdom, is strongly supported by the effects of the vapour of chloroform, which appear to be exactly similar in both cases. This effect may be readily witnessed if a bunch of blossoms of the common Barberry be exposed for a very short time to the vapour of chloroform. The irritability of the filaments is annihilated, and if the exposure have not been too prolonged returns after a certain interval. We have been informed that a similar influence is exerted upon the motions of the 'sensitive plant.' Experiments to the same effect upon the cyclosis in *Valisneria*, &c., would be of interest. The effects of chloroform upon the motile element throughout the animal kingdom, from the highest to the lowest members, are precisely alike.]

On some new or little-known Diseases *of* Plants, *caused by* Fungi. By Dr. Alex. Braun. With Appendices, by Dr. Caspary and Dr. Anton de Bary. Berlin, 1854, pp. 33, and 2 Plates.

In the introductory part of this brochure, Dr. A. Braun says that, "As the history of the civilization of the human race has its dark side in the development of many obstinate social evils which accompany the march of advancing civilization, so also has horticulture and agriculture, together with the bright, their dark side. There are certain diseases to which plants, in the state of cultivation, show themselves to be more obnoxious than when they are in their natural localities, and subject to the original conditions requisite for their flourishing. That this dark side fully deserves the attention of the gardener and of the farmer, is obvious, from the incalculable evils that ensue upon the mysterious and unexpected appearance of one of these vegetable plagues. The more difficult it appears to investigate the causes of these diseases, and to discover means by which they may be arrested or prevented, so much the more are they worthy of profound study."

With these observations every one must agree, and we feel it needless to insist upon the utility of attention to this subject by those whose opportunities and scientific acquirements may render them competent to employ the microscope in researches demanding no less a clear and scrutinizing vision than an unprejudiced and clear-seeing mind. Many diseases of plants, so important in their injurious effects upon some of the most valuable of Nature's offerings to the use or enjoyment of mankind, such as the potato and the vine, appear to be, especially, subjects of microscopical research, "since, in most cases, the presence of a parasitic organism, a fungoid growth will be found to be the cause of the disease." And these organisms are all of microscopic size.

In cases where the diseases of plants have occurred in connexion with fungoid growths, the question whether the disease has produced the *fungus* or the *fungus* the disease, has been answered in various ways. The view which was earliest entertained, viz., that the *fungus* was developed simply as a product of the morbid process by spontaneous generation from the diseased parts of the plant, has, at present, but few supporters; it is opposed to the positive observations that have been made respecting the germinating power of the spores of these parasites, as well as with respect to their persistent vitality, inasmuch as it has been proved by experiment (in the spores of *Botrytis bassiana*), that they will retain their vitality, in the dry state, for years together. But since the development of the *fungus* cannot be regarded as the direct product of the disease, the next thing is to inquire whether it has any dependent relation to the disease in another way. It has been assumed that the appearance of the fungus is always preceded by a morbid affection of the plant or of its parts, by which a fitting soil was provided for the growth of the *fungus*; and it cannot be denied that there are a vast number of *fungi* growing either upon plants entirely dead, or upon parts of them in a state of disease, as well as upon animal substances, and which, consequently, are not to be regarded as the *cause* of the disease, or of the death, but as accidental concomitants or followers of it. Thus, for instance, in potatoes rotten in consequence of the potato-disease, numerous forms of *fungi* grow, of which more than a dozen are known, but to none of which can the potato-disease be attributed.

It is equally certain, however, that even the sound organisms of plants and animals may be attacked by parasites from both kingdoms, whose presence is followed sometimes only by local morbid processes and manifold malformations, but sometimes, also, by general disease and death. To parasites from the animal kingdom it is needless here to advert; and with respect to parasitic plants, it may be remarked that some belong to the higher orders of the vegetable kingdom, as, for instance, the various species of *Orobanche*, the misletoe, *Cuscuta, &c.;* but the majority of the parasites belonging to this kingdom, exist in the multitudinous class of *fungi*. Of plants of this kind, infesting living animals, Robin (Hist. Nat. de Végétaux parasites qui croissent sur l'homme, et les animaux vivants) enumerates not less than 86 species. To the fungi which vegetate upon living plants, belong, in the first place, the whole host of 'rust' and 'smut' fungi (*Uredo*, *Puccinia*, *Phragmidium*, *Æcidium*, and the allied genera) which, as '*entophytes*,' are developed in the interior of the tissues; to these, moreover, belongs the well-known "ergot," whose previously enigmatical nature has lately been cleared up by M. Tulasne; who, by a lengthened series of the most profound observations, has shown that the body of the ergot, which is, externally, of a blackish colour, and internally white, and which has been described as *Sclerotium clavus*, is only the vegetative rudiment of a claviform *fungus*, which is not developed until it has fallen to the earth. This fungus is very closely allied to the *Sphæriæ* growing upon caterpillars, and is described by M. Tulasne under the name of *Claviceps purpurea*. The destructive fungus of the vine-disease, the so-termed *Oidium tuckeri*, is certainly nothing else than a kind of mildew; and in the instance of this *fungus*, it has been placed beyond all doubt by numerous researches, and particularly by those of Mohl, that it originates in perfectly *sound* parts of the vine, the disease and death of the cuticle, with their injurious consequences upon the ripening of the fruit, not commencing till after the fungus has made its appearance. The infection of the potato-plant by the *Botrytis* (*Peronostoma*) *infestans*, has the greatest analogy with the vine-disease; the brown spots are the invariable sequel of the appearance of this *fungus*. So soon as these spots are perceived, the delicate fungoid growth must be sought for around their margins, as it rapidly disappears from the centre of them. The circumstance that the brown spots remain, although the vegetation of the *Botrytis* subsequently disappears, or is, occasionally, so interrupted that its presence is not readily to be detected, has given rise to the erroneous views with respect to the primary origin of the brown spots.

The morbid phenomena produced by *fungi* are either *local* or, as it may be termed, *general.* The former is the case with most of the *fungi* known under the name of 'rust,' when they do not exist in too great quantity, such as *Trichobasis* (formerly *Uredo*), *Rubigo vera*, and *lineæris*, *Puccinia*, *Cystopus*, *&c.* These *genera*, however, and others, which, when existing in small quantities, have merely a local effect, when more extensively developed may exert a pernicious influence upon the entire organism of the plant or tree, by causing the falling off of the leaves, &c. This local influence, however, when it affects the blossom or its essential parts, will, of course, be followed by sterility; and in this situation the attacks of the parasite are often indicated by monstrosities in the flower or its parts; sometimes, again, sterility is caused by the effect of the fungus being shown in the prevention of the formation of the blossom, as in the case of *Euphorbia cyparissias* when attacked by *Æcidium Euphorbiæ*, or even by *Puccinia Euphorbiæ*, whilst *Uredo Euphorbiæ*, on the other hand, induces neither sterility nor any other striking change in the plant.

When the array of diseases produced by fungi, which, formerly unnoticed, have, in later times, attracted universal attention by their destructive consequences, is surveyed, it might readily be supposed that, as in the diseases to which the human frame is subject, every period has its prevailing character, so, also, in the vegetable kingdom, certain variable and secular influences prevail, to which is to be ascribed the circumstance of the present activity of diseases produced by fungi. At any rate, it appears that the meteorological conditions in the last few years have not been so unusual that a sufficient explanation of the destructive outbreak of these diseases is to be sought for in them. Besides the formidable potato-disease, numerous other diseases belonging to the same category have, of late years, invaded the vegetable kingdom, among which is to be enumerated, as of the greatest importance, the vine-disease, whose increasing extent may be traced from year to year. In 1852, the mulberry trees in Italy were attacked by a disease previously unobserved, the cause of which was discovered by Montagne in a *fungus* which invades the leaves, and is described by him under the name of *Fusisporium cingulatum.* In 1851, a disease very destructive to the orange trees, was described by Rendu, as caused by the attack of a fungus upon the roots, a species of *Rhizoctonium*. In the summer of 1851, the different species of *Ribes*, in the neighbourhood of Bercelli, in Piedmont, were invaded by a fungus named *Nematogonium byssinum*, and whose effects were very fatal to the plants. Diseases in a similar way, produced by *fungi*, and invading the cultivated Parsnip, and also observed upon *Pimpinella saxifraga*, and upon *Anthriscus sylvestris* and *Angelica sylvestris*, have been described. The author concludes by describing and figuring several new species of *fungus*, one of which attacks the species of *Robinia*, —*Septosporium curvatum;* another, *Acrosporium cerasi*, invading the Cherry; *Hemphylium ericoctonum*, a disease of the species of *Erica;* and *Heirochœte malvarum*, one affecting the mallow tribe.

On CHROMATOPHORES *in the* FROG. By R. VIRCHOW. Abstracted from the 'Archiv. f. path. Anat. u. Physiol.,' &c. Vol. vi., part 2, pp. 266.

AXMANN ('Beiträgen z. mikrosk. Anat. u. Physiol. des Ganglien-Nerven Systems.' Berlin. 1853.) has communicated the results of a series of transverse sections of the nerves, which would appear to establish the important fact, that the sympathetic nerves regulate the circulation, whilst nutrition is under the influence of the ganglio-spinal nerves. With respect to the latter point especially, he has adduced the remarkable fact, that when the ganglio-spinal nerves in the Frog are cut across, the well-known stelliform pigment-cells are said to lose their rays, and to become atrophied.

The importance of these representations seemed to make it desirable that they should be subjected to farther inquiry; and the more so, since Valentin had already adduced many considerations opposed to some previous statements to the same effect, brought forward by Axmann.

H. Meyer (of Oldenburg) undertook this inquiry, and will at a future time himself report the results at which he may arrive. But it appears at present desirable to make a few observations with respect to the pigment-cells.

It is a fact, that after division of the ganglio-spinal nerves going to the foot, in a frog, the pigment was rendered pale, a change which was manifest even to the naked eye; and, as we were examining the web under the microscope, the radiating processes, as Axmann describes, actually disappeared. But the comparison of both extremities very soon showed that, in proportion as the processes became invisible, the centre of the pigment-cells, their proper body, increased in size, so that it was evident that the pigment was collected from the processes or prolongations into the body of the cell.

Consequently the same change takes place in the pigment-cells of the Frog, as occurs in the *Chromatophores* of the Chameleon and Cephalopoda, with which we have been made acquainted by the researches of Brücke. The change of colour depends upon the alteration in form of the pigment-cells, and the change in place of the pigment itself, so that, in fact, the colour of the Frog appears to be the darker, the more the pigment is poured into the processes, and the brighter, the more it is collected into separate aggregations in the interior of the cell-body. The phenomenon, therefore, presented in this case, is manifestly not one simply of nutrition, but of contraction.

Harless (Münch. gelerhte Anzeigen, 1853; No. 35, p. 286) would appear to have observed the same phenomenon in the nictitating membrane of the common Frog, and throughout the integument of the Tree-Frog. He describes a varying distribution, in consequence of a change in the dimensions of certain pigment-cells, of a stratum of apparently viscid fluid between the clear, slightly brown-coloured granules of stelliform or irregularly-formed cells with elastic walls, the colour, as in the soap-bubble, depending upon the thickness of the fluid film. The passage is the less clear to me, because Harless speaks, besides this, of an unchangeable, gold-yellow colouring matter contained in other cells, and of the colouring matter of the black pigment-cells. In the natatory membrane of the Frog, however, it is precisely the latter upon which the change of colour depends.

On the Transformation of CYSTICERCUS PISIFORMIS *into* TÆNIA SERRATA. By C. TH. v. SIEBOLD. From the Zeitsh f. Wiss. Zoologie, B. IV., pp. 400.

IN the year 1844 I first drew attention to the similarity between the head-end of *Cysticercus fasciolaris* of the rat and mouse and *Tænia crassicollis* of the cat, and to the mutual relations of these two parasitic forms, on which occasion I broached the assertion that the *Cysticercus fasciolaris* is an aberrant and degenerated *Tænia*, but one still capable of attaining the normal form of a tapeworm, when it is transplanted into the intestinal canal of a suitable animal. In this case the short joints of the *C. fasciolaris* are completely formed, and the generative organs which are wanting in this *Cysti-*

cercus as in all the cystic *Entozoa* arrive at their due development. At the same time I pointed out the way in which this transformation of the asexual *C. fasciolaris* into a fertile *Tænia crassicollis* might be traced, showing that when mice and rats, whose livers are infested with this cyst-worm, are devoured by cats, the latter would digest in their stomachs the livers of the rodents swallowed by them, but not the *C. fasciolaris* contained in the livers. This entozoon, on the contrary, feeling itself transplanted into a proper soil, throws off the dropsical degenerated joints in the digestive canal of the cat, and acquires the form of *T. crassicollis*, and arrives at sexual maturity. Allan Thompson, of Glasgow, without, as it seems, his being acquainted with my researches and published observations on this subject, had recognized the correspondence of *C. fasciolaris* with *T. crassicollis.* Pursuing the subject farther I finally became convinced that all cystic *Entozoa* are nothing else than undeveloped or larval tapeworms, which, arrested in their wanderings, have become aberrant and dropsically degenerated. I presented it to the helminthologists as a problem, to determine what completely developed and sexual species of cestoid worms belonged to each degenerated and asexual cystic form, warning them, however, against over hasty conclusions and deceptive appearances, which so readily arise in inquiries of such a difficult nature.*

Dr. Küchenmeister, of Zittau, has devoted himself, during the past year, with the most untiring zeal to these difficult researches. But owing, perhaps, to this extreme zeal Küchenmeister was induced to publish his researches and experiments before they could be regarded as completed. He first stated that from 40 individuals of *Cysticercus pisiformis* of the rabbit he had succeeded in producing 35 specimens of *Tænia crassiceps* of the fox, and indeed *Tæniæ*, in 22, 15, 8 days, and 30 hours.† Some weeks afterwards he corrected‡ this preliminary communication, stating that he had been wrong in regarding the tapeworm produced in his experiments from the *Cysticercus pisiformis* as *Tænia crassiceps*, having learned from Dr. Creplin that it was the *T. serrata* of the dog. Dr. Küchenmeister also sent to me for determination various *Tæniæ*, produced by him from *Cysticercus pisiformis;* but as these specimens were not sexually developed, and the *ova*, which afford such excellent specific characters in the *Tæniæ*, were wanting in them, I did not venture to give a definite judgment

* Zeitsh. f. w. Zool. 1850, p. 201.

† V. Günsburg's Zeits. f. Klinische Vorträge, 1850, p. 240.

‡ Ib. p. 295.

as to the species of these *Tæniæ*, and was inclined, until I should receive from Dr. Küchenmeister some sexually mature specimens of the species, to regard those transmitted by him as a distinct species. Dr. Küchenmeister consequently was induced to abandon Dr. Creplin's determination of the tapeworm produced from *Cysticercus pisiformis*,* and to regard it as a new species, under the name of *T. pisiformis*. This rapid succession of contradictory statements would naturally have the effect of disinclining the medical public to believe in the possibility of the transformation of the *Cysticercus* into a *Tæniæ*. But even with helminthologists themselves Küchenmeister's statements could not meet with any real acceptance, as it was but too apparent from the whole exposition of his researches, that he was still, as he himself confesses, deficient in helminthological information.

A prime error, into which Dr. Küchenmeister fell, consisted in this, that when, in a *Cysticercus* which had been swallowed, and examined in the intestines of a dog, he found the usually retracted *head* and *neck* protruded, he at once explains this condition as the already commenced transformation of the *Cysticercus* into a *Tænia*. It is in this way that in his experiments he must have arrived at this very remarkable and, to helminthologists, incredible result, which he describes when he states that the cystic worms swallowed by one dog were transformed into *Tæniæ* in *five*, and in another after *three* hours. If Dr. Küchenmeister imagines that a *Cysticercus*, which, after it has passed into the intestinal canal of a dog, has lost its *caudal vesicle* and protruded its *neck* and *head*, has already undergone a transformation into a *Tænia*, it may be stated that a similar transformation may be very simply effected spontaneously when a *C. pisiformis* is put into lukewarm water and time allowed for it to protrude its *head* and *neck*, when a snip with a pair of scissors will at once complete the metamorphosis into a *Tænia*. All the *Tæniæ* figured by Küchenmeister, and which he states himself to have obtained by his "feeding" experiments, are also nothing more than a caudal and extended *Cysticercus*. Statements of this sort appearing to me likely to throw discredit upon the account I had given of the metamorphosis of the cystic worms, I determined to undertake myself the researches and experiments demanded in the investigation of this perplexed inquiry. I commenced in the March of the present year with *Cysticercus pisiformis*, with which Küchenmeister had already instituted six "feeding" experiments. Dr. Lewald, one of

* 'Ueber Finnen und Bandwürmer;' in the Vierteljahrs f. prakt. Heilk. Prag. 1852. Bd. i., p. 150.

my most zealous pupils, aided me in this, with the intention also of making these researches the theme of his Inaugural Dissertation. This has just made its appearance,* accompanied with a plate of figures, exhibiting the gradual transformation of *Cysticercus pisiformis* into *Tænia serrata*, as presented in the intestinal canal of ten dogs fed with these cystic worms, and which were killed at the most various periods, viz., from ¾ of an hour up to 65 days after the feeding had taken place.

In the first place, three rabbits and two guinea-pigs were fed with the cyst-worms. These experiments afforded no result whatever; for several days afterwards, upon opening these rodents, the cyst-worms were never to be found in the digestive canal. The happiest results were arrived at in "feeding" experiments made with young dogs. For a full detail of these experiments and their special results I must refer to Dr. Lewald's Dissertation, here merely noticing, in general, the destiny and vital condition which the cyst-worms underwent in the digestive canal of the dogs. Remarking at the same time that the cyst-worms used by us were always retained in the peritoneal cysts, in which they had been found in the omentum of the rabbits.

The first effect produced upon the *Entozoa* thus enclosed in their cysts, after they had been swallowed, was the solution of the cysts by the gastric juice in the dog's stomach, whereupon the caudal vesicle was attacked and destroyed by the same digestive agent; so that, of the whole *Cysticercus pisiformis* nothing was left but the whitish and rounded body which had been concealed in the caudal vesicle, consisting of the head and neck of the creature invaginated in the body. The caudal vesicle, before its digestion, was frequently shrivelled and collapsed, probably because the thin fluid contents were excreted by exosmosis into the more dense pultaceous contents of the stomach. At the same time the remains of the cyst-worms, that is to say, the tailless bodies with the invaginated head and neck, pass through the *pylorus* into the *duodenum*. Arrived there, the *head* and *neck* are protruded from the body of the entozoon, for the purpose of seeking a spot of adhesion between the *villi*, in which situation it has to await its subsequent growth and the further completion of its body. In the first hours of their abode in the small intestine, these extended tailless *Entozoa* often continue to present a bloated, œdematous aspect; but gradually the body

* De Cysticercorum in Tænias Metamorphosi pascendi Experimentis, in Instituto Physiologico Vratislaviensi administratis illustratâ. Auctor G. Lewald. Berolini, 1852.

becomes more slender, probably from its giving off the superfluous fluid by *exosmosis*, and thus the *Entozoa* acquire pretty nearly the same specific gravity as the more or less dense chyle. At the posterior end of all these extended tailless cyst-worms, the point at which the caudal vesicle was previously attached is distinctly indicated by a sort of cicatrix, in the form of a notch or excavation, from which, at first, very delicate membranous *flocculi*, the remains of the destroyed caudal vesicle, are seen to depend. In a few days the growth of the *Entozoa* commences, in which the body alone participates, the head and neck having attained their full development and perfection, whilst the creature was still resident in the *peritoneum* of the rabbit. As the body, which is still altogether unjointed and furnished only with closely-approximated transverse wrinkles, continues to increase in length, its transverse wrinkles are multiplied; and the growth of the body being continued uninterruptedly, are, in the course of a few days, formed into distinct *joints* or *segments*. The joints, which are at first very short, elongate, and soon present, sometimes upon one sometimes upon the other lateral margin, a papillary elevation, which is afterwards developed into the aperture of the generative organs. In this condition the cyst-worms completely resemble a *Tænia*, and betray their origin only in the cicatrix, which still exists on the last joint of the body. After a residence of 25 days of these worms in the intestine of a dog, they have attained a length of from 10 to 12 inches. The growth of these *Tæniæ* continues uninterruptedly, in the course of which their posterior joints increase in breadth, and the generative organs in their interior approach more and more nearly to their full development, whilst behind the neck the formation of continually new joints proceeds out of the transversely wrinkled anterior portion of the body. In three months these *Tæniæ* have attained a length of from 20 to 30 inches or more, and in them the posterior joints appear to be sexually quite matured. In some of them the last joints are now also detached—an additional proof of their sexual maturity. The ova contained in the ripe joints are seen to be completely developed, and containing in their interior the mobile embryo armed, in the way well known, with six hooklets.

This stage of development of the tapeworm, which is produced from *Cysticercus pisiformis*, enabled me to determine its species with certainty, and I satisfied myself that it belonged to *Tænia serrata*. The form of the *head*, the number, shape, and arrangement of the hooklets on the head; the conformation of the joints, and of the sexual organs contained in them;

the figure of the mature ova—all proved to me that I had before me *Tænia serrata*.

It should be mentioned, that on the dissection and examination of the intestines of the dogs thus fed with *Cysticerci* some individuals of *Ascaris marginata*, and several sometimes longer, sometimes shorter, individuals of *Tænia cucumerina* were always met with. Although, from the above-mentioned experiments and the results obtained from them, I am now myself almost convinced that the *Cysticercus pisiformis* is transformed in the digestive canal of the dog into *Tænia serrata*, I am not sure that even these experiments will carry the same conviction to other zoologists and helminthologists. Shall I not have opposed to me this question: how, from my experiments, can I be certain that there were no *Tænia serrata* in the intestine of the dogs before they were fed with the *Cysticerci?* For there would be no more difficulty in those entozoa finding their way, in another mode, into the dog than in *Ascaris marginata* and *Tænia cucumerina* doing so. With respect to which I must remark, that in my experiments I made use only of parlour or house dogs, and *Tænia serrata*, according to my experience, very rarely occurs in domestic dogs of that kind, whilst it is much more abundant in hounds. I have examined the intestines of many domestic dogs which had not been fed with *Cysticerci*, and scarcely ever met with a *Tænia serrata* in them, but, on the other hand, *Tænia cucumerina* almost always occurred. I would, moreover, remark, that after a "feeding" with *Cysticercus pisiformis* the number of tapeworms found in the dog's intestine, and more or less developed into *Tænia serrata*, always corresponded with the number of the *Cysticerci* which had been administered in each experiment. Another very noticeable circumstance is this, that the size and stage of development of the individuals of *T. serrata* found in the intestine of the dog fed with the cyst-worm always precisely accorded with the time which had elapsed since the "feeding."

Important as this demonstration of the transformation of *Cysticercus pisiformis* into *T. serrata* is with respect to the natural history of the *Cestoidea*, care must be taken not to apply too much of what we learn from the history of this one tapeworm to all others of the same class. Küchenmeister seems to have concluded that all the other *Tæniæ* are also derived from cyst-worms, which must be altogether denied; for were all *Tæniæ* obliged to pass from the condition of an embryo furnished with six hooklets, first into a sexual *Cysticercus* armed with a coronet of hooks, before it could be developed into a perfect, jointed, and sexual individual, we should certainly be acquainted with a much greater number of cystic forms than have hitherto been made known. According to the most recent enumerations the number of known *Tæniæ* includes about 188 different specific forms, whilst of the genus *Cysticercus* scarcely 16 defined species can be adduced, and our knowledge of all the genera of cystic worms in general does not embrace 25 species altogether. As cyst-worms, as is well known, occur only in animals, and thus can only be introduced by the feeding upon flesh, it can hardly be explained, on the supposition that all *Tæniæ* proceed from them, in what way the *Tæniæ* of the herbivorous mammalia can have been introduced as cyst-worms into the intestine of the animals infested by them. That all *Tæniæ* have not previously been cyst-worms is shown in the development of a tapeworm observed by Stein.* His observations distinctly indicate that the embryo with six hooklets which escapes from the tapeworm egg is not immediately transformed into a *Tænia* or cyst-worm, but that, in the first place, a young tapeworm is developed in the interior of this embryo in the form of the head-end of a *Tænia* (*Scolex*-form). Such a *Tænia*, were the posterior end of its body expanded into a vesicular form and filled with a serous fluid, would completely resemble a *Cysticercus*. Under what circumstances such a degeneration in *Tæniæ*, as yet asexual, comes to pass, is, it must be confessed, as yet unknown.

Very recently objections have been raised to my statement, that cyst-worms are *morbidly degenerated* tapeworms. Küchenmeister has propounded, in opposition to me, the notion that the caudal vesicle of the *Cysticercus* is a necessary organ in the cystic state of the entozoa, assigning it the function of a nutritive reservoir. How far this assertion is correct, or the reverse, must be left to special researches in the subject to decide. At the same time I am quite ready partially to modify my definition of the cystic condition, and, though willing to abandon the term "morbid," must, on the other hand, the more firmly retain the designation "degenerated" (*entartet*), as my latest researches have more and more tended to convince me that the cystic worms are really degenerated tapeworms, and that the form and size of the caudal vesicle do not depend upon the specific form of the *Cysticercus*, but upon external, adventitious, accessory influences. I must confess that I cannot rightly perceive upon what grounds endeavours are made to show the possibility of varieties in form and figure in the worms, since, in the higher animals, the deviations from the normal type induced by climate, condi-

* Zeitschrift f. w. Zool. iv. bd. 1852, p. 205.

tions, and altered nutriment are admitted without dispute. That these varieties occur according to certain laws, and always return to a definite form, is shown in the different "races" of domesticated animals. When, in many of these races, an excessive secretion of horn-substance takes place in the growth of hair, in others an unusual deposition of fat, why, in certain lower animals, should not an accumulation of serous fluid, or dropsy, take place, when they have deviated from their usual mode of life?

It must now be an important task for helminthologists to trace the further development of the embryos which proceed from the ova of *Tænia serrata*, in order that they may determine in what way the *Cysticercus pisiformis* is produced from them.

To those who wish to repeat the "feeding" experiments with *Cysticercus pisiformis*, in order to obtain from it the *Tænia serrata*, and who, for the more sure determination of the *Tæniæ* thus obtained, are desirous of consulting figures, I would remark, that numerous errors have crept into the various helminthological papers under the head of *Tænia serrata*, which have up to the present day remained unnoticed, and which have been caused by the circumstance that formerly *Tænia serrata* and *Tcrassicollis* could not be suitably distinguished. Both species of tapeworm, although in the conformation of the joints closely allied, are easily recognizable by the *head*. *T. crassicollis* has a very strong and broad rostrum, almost as wide as the head. Its short neck is continued without any contraction uniformly into the jointed body. In *T. serrata* the rostrum with its coronet of hooks is much narrower than the head, its somewhat longer neck is always contracted behind the head. This distinction is very apparent in every figure which Goeze has given of *T. crassicollis* and *serrata;* but, notwithstanding this, Goeze appears to have originated confusion in the matter, because various specimens which he has figured as *T. serrata* according to him are said to have been procured from the intestine of the cat. Whether Goeze in this statement has made a mistake, or whether *T. serrata* may not also occur in the intestinal canal of the cat, I am not at this moment in a condition to decide. In any case the following figures in Goeze refer to *T. serrata:* Tab. XXV. A, figs. 1—5; Tab. XXV. B, figs. A—D; and Tab. XXVI., figs. 1—4. The last-mentioned plate is incorrectly cited by Rudolphi * as referring to *T. crassicollis;* and subsequently this extremely good figure of *T. serrata* has been altogether lost sight of, and never cited by later helmintholo-

* Entozoor. Hist. Naturalis. Vol. ii. p. 2, 1810, p. 174.

gists; whilst Goeze's figure of *Tænia serrata*, in Tab. XX. A, figs. 1—5, is erroneously assigned by Diesing* to *T. crassicollis.* Besides Goeze's figures of *T. serrata* I would notice Gurlt's † representation of this cestoid-entozoon.

On a "BLACK FUR *on the* TONGUE." By Dr. EULENBERG, of Coblenz. Abstracted from the 'Arch. f. Physiol. Heilk.,' August, 1853.

THE author relates that in the preceding year a child, two years old, was brought to him whose tongue was covered with a perfectly black coating. The organ, from the tip to the back, appeared as if it were smeared with ink; and at first sight the supposition necessarily entertained was, that the child had licked some black object, or had swallowed a coloured liquid. Except a slight diarrhœa, the boy presented no other morbid symptoms. For his age, he was well developed, and had never had any important illness. The author's immediate treatment was confined to washing the tongue with vinegar and water.

Fourteen days afterwards, the child was again brought to him, when the mother stated that the washing of the tongue had removed the black colour only for a short time, at most for not more than a day, when it returned with the same intensity as at first. Dr. Eulenberg prescribed some indifferent medicines, in order to keep the child under observation; directing the continued use of vinegar and water as an external application. But, notwithstanding the diarrhœa had long ceased, the tongue remained the same for three months. When the organ was cleansed, the black colour reappeared, first in the middle and anterior half, afterwards gradually covering the entire dorsum of the tongue, and extending as far as could be seen. The *lingual papillæ* were, at the same time, much developed. The *papillæ filiformes* were very distinct, and were especially dark-coloured. The *papillæ vallatæ*, projecting in a conical form, presented, particularly at their apices, a deep-black covering. Even after the tongue was washed these *papillæ* retained the colour, and were merely surrounded by a pale border, owing to which the black hue of the apex was rendered the more striking. If the coloured tongue was scraped, a viscid brownish mucus was obtained, which, under the microscope, exhibited a large quantity of thickened epithelial cells and granular pigment.

* Diesing. Systema Helminthum. Vol. i. 1850, p. 519.

† Gurlt. Lehrb. d. Patholog. Anatomie der Haussaugethiere. Th. i. 1851. Tab. ix. fig. 9-10.

If the mucus thus scraped off were dried upon paper, there remained extremely delicate black or dark-brown filaments, about as thick as a fine hair, and from $\frac{1}{4}$ to $\frac{1}{2}'''$ in length, or minute irregular plates of the same length and breadth. If the latter were divided, they frequently afforded minute, crisped particles, like fine down. Particles of the same kind, however, were often met with independently. Their elasticity was evidenced in this, that they often sprung away when an attempt was made further to subdivide them with needles.

Under the microscope they represented distinct, very much thickened, and brownish-coloured epithelium-scales, among which, in the less dark but somewhat transparent places, the pigment granules could be remarked. The latter, however, presented themselves with especial distinctness at the edges of the epithelium-scales, and appeared as irregular, rounded, flat granules, the border of which was dark, and the centre always clearer, but no nucleus was ever remarked in them. In the centre of the epithelium-scales they occasionally constituted a beautiful mosaic area of rounded, closely approximated, elongated, or sub-angular granules. Punctiform granules were more rarely met with. The moniliform arrangement of the granules was remarked more especially at the border of the epithelium-scales. The more transparent the epithelium, the more transparent, also, were the separate granules which then occurred isolated. The author seldom noticed a single isolated granule, for however few might be connected together, they were usually supported on a small particle of an epithelium-cell. When free, they were rounded or punctiform, and appeared connected in the form of a small rod or coronal. This description of the granular pigment does not agree, in all respects, with those given by other authors, as J. Vogel and Höfle. According to Vogel ('Path. Anat.' p. 159), it consists of fine granular molecules of a brown or black colour, which are most usually contained in cells of various form and size. Occasionally, it would appear, these pigment-molecules occur free, particularly in the parenchyma of melanotic lungs. According to Höfle (Chemie *v.* Mikroskopie, p. 274), the pigment corpuscles are characterized by the intense black colour and almost immeasurable smallness of the constituent granules. According to him, they would seem never to be surrounded by a membrane, but frequently encompassed by a homogenous cortex, not differing from the substance connecting the granules together. Henle (Allg. Anat., p. 282) is more inclined to the assumption of the cellular nature of the pigment-corpuscles, as Schwann states that he has noticed a molecular motion of the pigment-corpuscles within the cell, which Höfle, on the other hand, declares to be impossible, since molecular corpuscles can never perform any movements within a gelatinous substance.

In the case now in question, the author never observed a cell or membrane, since the pigment-corpuscles rarely occurred in the free state, but were almost always deposited on or among the epithelium. They most resembled the pigment-corpuscles figured by Henle (l. c., Tab. I. fig. 12 D), in which also the border is dark and the centre somewhat clearer. According to him they are 0·0005-0·0007‴ in the longest diameter, and about 1-4th as thick as long. Under a stronger power, Henle also noticed some as transparent as water. On some occasions, the author observed, on the borders of the epithelial-scales and connected with them, elastic fibres, distinctly characterized by their dichotomous mode of division. A few times he noticed filaments lying quite isolated, which very closely resembled the thallus-filaments figured by Henle (l. c., note, p. 29). They were never connected with the epithelial-cells, and exhibited perfectly cylindrical canals, without transverse septa, and beset externally with black points. The latter were in no case pigment-corpuscles, as they were globular, which was particularly evident in the granules situated on the external borders, whilst the pigment-granules were always flattened, however small they might be. Even in the almost punctiform pigment-granules, the darker border could be distinguished under favourable conditions of illumination. It is well-known that the granular pigment, besides its normal deposition in the *corpus ciliare*, in the pulmonary tissue, in the integuments, &c., is, for the most part, presented only in the most various pathological structures. In the tongue it has not yet been met with, especially to the extent in which it occurred in the present case. Höfle (l. c. p. 59) observed in five cases, in the fur of the tongue occurring in the healthy condition, dark-brownish bodies, partly of a cylindrical, partly of an irregular form, and with three or four times the circumference of the largest epithelial-scales, thickly beset with granules, and internally containing a sort of medullary body. He regarded this as an epithelial investment of the lingual papillæ. These cases would seem to present no similarity with the instance observed by the author. Höfle could never effect a division of these bodies into separate epithelial-scales, which Dr. Eulenberg could always succeed in doing; nor could the latter ever observe the so-called medullary body; and, as regards the granules, Höfle describes them as black, scattered points, whilst in the author's case, they were aggregated in many ways, and represented roundish or angular and flattened granules or plates, with dark borders, and, much more rarely, simple points. The dark epithelial-scales upon which the pigment-corpuscles were chiefly deposited, the author also regards as an epithelial covering of the *papillæ.*

After he had observed the progress of the case for a sufficient length of time, and found that the phenomena remained unchanged, the author directed the internal use of an aqueous solution of chlorine, to be administered every three hours. After about two ounces of this medicament had been thus taken, not a vestige of the black coloration remained. The tongue had resumed its normal appearance, and the *papillæ* more of their natural size. The colour had not recurred even at the end of a year, so that the cure seemed to be complete.

REVIEWS.

The Microscope, and its application to Clinical Medicine. By Lionel Beale, M.B. London, Highley.

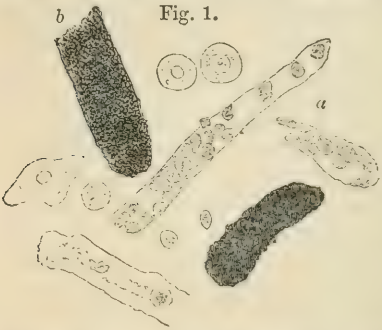
We suppose at the present day that there are few properly-educated medical men who would deny the value of the stethoscope as a means of ascertaining the presence of disease. The man who would be hardy enough to forego its use would run the hazard of even popular neglect for his temerity. This is not, perhaps, the case at present with the microscope, but we feel sure that whatever argument could be advanced in favour of the stethoscope, as a means of diagnosis, might be urged with tenfold force in favour of the microscope. The stethoscope, in fact, only facilitates the use of the organ of hearing; whilst the microscope widens and extends the power of vision, and creates a new world of observation. By it that which the unaided mind could only imagine, or vaguely indicate, as a probable existence, is demonstrated, and the structure, which was a puzzle and a mystery, becomes perfectly understood. This instrument has now become a necessity for the anatomist and physiologist; no structure can be truly investigated, no function perfectly understood, without its aid. If, therefore, a knowledge of disease can only be obtained by a knowledge of the structure and functions of the body in health, it is necessary that those who undertake to treat disease should be conversant with the results of microscopic research. The microscope, in fact, should be put into the hands of every medical student, and he should be expected to be as well acquainted with the results of its use as he is of the scalpel, the test-tube, the stethoscope, or any other means of investigation. We fear this is not the case, and that many a young man passes his medical curriculum without even the inquiry being made as to whether he is acquainted with the powers of this mighty instrument of research. We are glad, however, to find that the means of microscopic instruction are multiplying. In many of our medical schools demonstrations with the microscope are given, our medical journals devote some portion of their space to micrological discussions, and here we have a professor in one of our metropolitan medical colleges, writing a book on the microscope especially adapted for medical

students. We give Dr. Beale's remarks on the value of the microscope as a means of diagnosis, entire.

"It may be well here briefly to refer to a few of those instances in which the microscope is known to have afforded valuable aid to the practitioner in the diagnosis of disease.

"*Diseases of the Kidney.*—There is no class of diseases in which its powers have been more advantageously brought to bear by the practical physician, than in those of the kidney. By a microscopical examination of the urine, we are frequently enabled to ascertain the nature of certain morbid changes which are going on in the kidney, and even to distinguish, during life, the existence of certain well-defined pathological conditions of that organ. The laborious researches of Dr. Johnson have shown us how, by the peculiar character of the casts (fig. 1) of the uriniferous tubes, which are found in the urine, we can ascertain whether the epithelium be desquamating, or, on the other hand, whether it presents no such tendency, but remains firmly attached to the basement membrane of the tube. If the epithelium be undergoing that peculiar change termed fatty degeneration, we shall often be able to ascertain the fact by examining a specimen of the deposit from the urine by the microscope. So again, by the presence of certain other deposits, and a knowledge of the symptoms usually associated with them, the physician is enabled to direct his attention, as the case may be, to the existence of local changes, affecting some part of the genito-urinary mucous membrane, or to more general disturbance, in the changes which take place in primary and secondary assimilation.

Fig. 1.



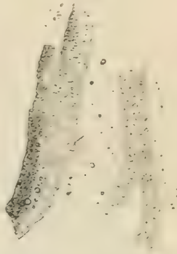
"*Fatty Degeneration.*—Of late years, the remarkable changes which take place, and which have been described under the name of fatty degeneration, in some of the highly complex textures of the body, in consequence of which their properties become changed, and their functions impaired, or altogether destroyed, have been undergoing careful investigation by a vast number of highly-talented investigators.

Fig. 2.

"The recent discovery of a state of fatty degeneration affecting the arteries of the brain (fig. 2), in the majority of cases of apoplexy, by which the strength of their coats becomes deteriorated, and their elasticity entirely destroyed, would tend to lead us to infer, that this disease is dependent rather upon complicated changes affecting nutrition, than upon the presence of a condition of plethora or hyperæmia, as was formerly supposed, and acted upon.

"The connexion between fatty degeneration of the margin of the cornea (arcus senilis), and similar changes taking place in the muscular tissue (fig. 3), of the heart (a subject which has been carefully investigated by Mr. Canton), or in the cerebral vessels, must be regarded with great interest by every practitioner.

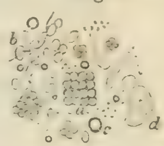
Fig. 3.



"The microscopical examination of the matters vomited in certain cases, has proved to us that the presence of minute fungi, originally discovered by Professor Goodsir, and named by him Sarcinæ Ventriculi (fig. 4), occurs in connexion with certain morbid conditions of the stomach. These remarkable cases are much more frequently met with than was formerly supposed, and form an exceedingly interesting class of diseases.

"*Tumours and Morbid Growths.*—The microscope has many times afforded important aid in the diagnosis of tumours, although it has certainly failed in many instances; which circumstance has been brought forward by some as an argument against its employment altogether. After careful microscopical examination, the best observers have failed in deciding as to the nature of a particular tumour submitted to examination; and they have been unable to pronounce as to its malignant or non-malignant character. On the other hand, not unfrequently this question has been positively and correctly answered in the affirmative or negative, and therefore it would surely not be right altogether to discard the use of an instrument which, although eminently useful in many instances, is not infallible; for it would appear to be the opinion of some, that the use of the microscope ought to be altogether abandoned, in the diagnosis of tumours. We shall have to return to this important question at a future time.

Fig. 4.



"*For the discovery of Imposition*, the microscope is invaluable, as it almost necessarily follows that, in consequence of the frequency with which urine is subjected to minute investigation, patients often resort to various expedients to deceive the practitioner. Perhaps flour, starch (fig. 5), sand, and milk, are more frequently employed for this purpose than any other substances. The microscope will obviously enable any one to detect the first three. If milk be added to urine, the mixture may very readily be mistaken for a specimen of the so-called chylous urine. Although a considerable quantity of fatty matter is present, in either case,

Fig. 5.

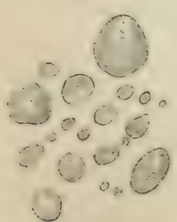
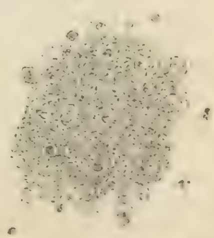


Fig. 6.

Fig. 7.

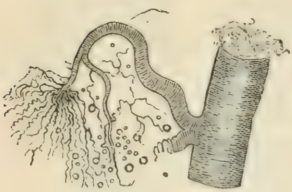


this fatty matter exists in a very different state. In milk, we find the oil-globules (fig. 6), so characteristic of this fluid, while, in true chylous urine, not a single oil-*globule* can be found, although the specimen may contain a large quantity of fatty matter in a molecular state (fig. 7).

"*Larvæ of the Blow-fly in Urine.*—A specimen of urine containing several bodies of about half an inch in length, and of a rounded form, was once sent to Dr. Todd for examination. The bodies in question looked not unlike the larvæ of some large fly, but, as it was confidently affirmed that they were passed by the urethra of a gentleman, the accuracy of this view of their nature was doubtful.

"Upon placing a portion of one of them under the microscope, tracheæ (fig. 8)—(the air-vessels characteristic of the class of insects) were observed in considerable numbers; and this circumstance alone enabled me to say positively that they were not entozoa, and that they could not have been passed in the manner stated. They were afterwards proved to be the larvæ of a fly.

Fig. 8.



"The claws of echinococci and portions of hydatid cysts have on several occasions been discovered in the urine, sputa, &c., upon submitting portions of these fluids to microscopical examination, proving beyond a doubt the existence of hydatids.

"*Substances passed by the Bowels.*—If the practitioner have a good knowledge of the use of the microscope, he can often ascertain the nature of substances passed from the alimentary canal; and, by the aid of this instrument, he can often at once decide as to the nature and origin of substances, which, to the unaided eye, only present most doubtful characters. Considerable perplexity has arisen from the presence of bodies in the stools of patients, which afterwards proved to be portions of almonds, gooseberry skins, portions of potato, the testa of the tamarind, husks of wheat, &c.: not many years ago the uredo of wheat was mistaken for, and described as, a peculiar fungus, to which it was supposed the phenomena observed in cases of cholera were due.

"Portions of vessels which, unlike the other constituents of the food, have resisted the process of digestion, have been met with in the fæces, and mistaken for small intestinal worms, which they much resemble when examined by the unaided eye. Upon being subjected to microscopical examination their true nature was readily discovered.

"*In Medico-legal Inquiries* the microscope has often afforded valuable aid. The distinction between blood spots and red stains produced by fluids resembling blood in colour,—between human hair and that of animals,—and the detection of spermatozoa in cases of rape, need only be adduced as examples of the importance of the microscope in such investigations.

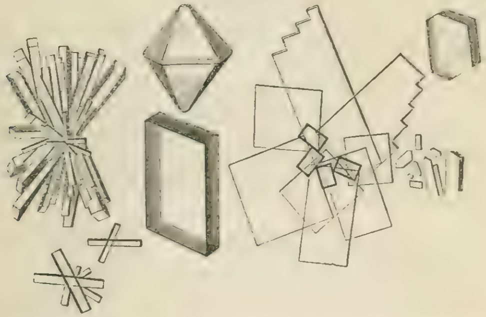
"*For detecting Impurities in Food and Drugs* the microscope has afforded important aid, and there are several other purposes to which it may be applied, some of which will come under consideration in a subsequent chapter."

These are some of the positive uses of the microscope in the investigation of disease, uses which ought at once to arrest the attention of the mere "practical man," but its importance is even greater in those instances where it corrects our theories of life and disease, and thus becomes the means of modifying a whole system of practice. It is thus that it lays the axe at the root of spurious systems of medicine, and is certain to bring about the destruction of all absurd generalizations, whether they go by the name of homœopathy, hydropathy, or more legitimate terms.

We give one other extract as a specimen of Dr. Beale's book. It relates to a class of substances, of which there is, at present, much to learn both in health and disease:—

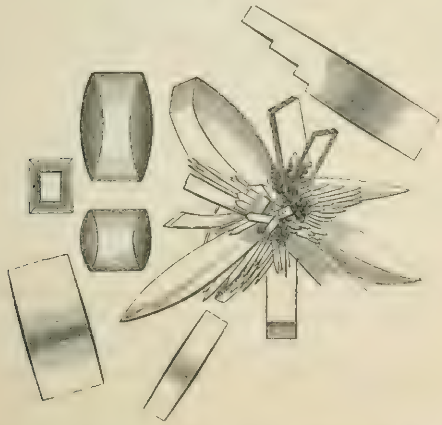
"**Creatine—Creatinine.**—Creatine exists only in very small quantity in muscular fibre. According to Dr. Gregory, it is most readily pre-

Fig. 216.



pared from the flesh of the cod-fish; from 25 lbs. of which, in one experiment, he obtained 164 grains of creatine. The flesh is to be chopped in

Fig. 217.

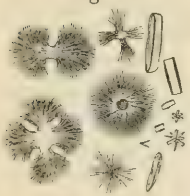


small pieces, and well kneaded with water. After all the fluid has been expressed by powerful pressure, it is very carefully raised to the boiling-point, and the coagulated matter removed by filtration. The phosphatic salts are precipitated by caustic baryta. The solution must be again filtered, and evaporated at a gentle heat (130°-140°) to about one-twentieth of its volume, or to the consistence of syrup; any scum which forms being, from time to time, removed from the surface. This concentrated solution may then be set aside. On cooling, it forms a thin jelly, and, after standing for some time, crystals of creatine are deposited.

"Crystals of creatine are represented in fig. 216, and those of creatinine in fig. 217, which have been copied from M. Robin's Atlas (see 272).

"Liebig obtains creatine and creatinine from urine, by evaporating the fluid, after the precipitate produced by the addition of lime-water and chloride of calcium has been separated by filtration. During evaporation, the greater part of the salts are deposited, and the mother-liquor is treated with 1-24th of its weight of chloride of zinc: after some days round granules separate; these are composed of a compound of chloride of zinc and creatinine, with which the creatine is mixed (fig. 218). The granules are dissolved in boiling water, and treated with hydrated oxide of lead, until the reaction becomes alkaline. The fluid is next filtered, decolorized with animal charcoal, and evaporated to dryness. The residue consists of creatine and creatinine, the latter of which may be removed by boiling alcohol, in which creatine is almost insoluble."

Fig. 218.



We cannot speak more highly of Dr. Beale's book than to say that we should be glad to see a copy in the hands of every medical student and every medical practitioner in the kingdom. Those who have employed the microscope will find it of great assistance, whilst those who have not yet commenced the use of this instrument cannot fail, we should think, by the perusal of a volume like the present, to be convinced of its importance in the investigation of disease. The work abounds with beautifully executed wood-cuts, and is got up in the best possible style.

Die Infusionsthiere, auf ihre Entwickelungs-geschichte untersucht. (The Infusoria investigated with respect to their development.) By Dr. F. Stein. Leipsic. 1854; pp. 265. 4to. Plates.

To no one have we been more indebted of late years for our advanced knowledge with respect to the Infusoria than to Dr. Stein. His previous writings in Wiegmann's Archiv for 1849, on the development of the Infusoria, and on the same subject, together with observations on their more intimate structure in Siebold's and Kölliker's Zeits. f. Wiss. Zoologie in 1851, as well as his first Essay on the nature of the *Gregarinæ* in Müller's Archiv for 1848, have stamped him as an original observer and thinker, and have not only added very materially to our previous knowledge of those subjects, but have also in so doing aided very efficiently in the dissipation of numerous erroneous views, propounded by Ehrenberg and other early microscopists. It may be as well therefore, briefly to state what points connected with the Infusoria have been made out mainly by the author of the present work, and in what state he leaves the question to which chiefly he has addressed himself, viz., the development and metamorphoses of certain classes of Infusory animalcules.

Led by his observations on the structure and habits of the *Gregarinæ* to conclude that they certainly belonged to the animal kingdom, and represented, in fact, the 'animal' in its simplest form, he was obliged to consider what place they should hold in the zoological system. In order to decide this it was requisite to ascertain distinctly the organization and structure of the Infusoria, and to decide whether Ehrenberg's view, according to which all the Infusoria were furnished with a complex digestive apparatus, with male and female sexual organs, and with muscles and nerves, were correct, or whether the more rational views propounded by Focke, and especially by Dujardin, as to their much more simple constitution should be adopted. According to which the structure and organization of the Infusoria is not more complex than that of the *Gregarinæ;* and most of their vital functions are performed without any special organs.

One result of Siebold's observations, which upset Ehrenberg's erroneous notions of the nature of certain organs which he supposed to be connected with the generative function, was to prove that a certain body described by Ehrenberg as the *testis*, though it had not the significance of such an organ, was nevertheless essentially related to the function of reproduction. This body was termed by Siebold the 'nucleus,' and it appears to form a pretty constant element of the body of most Infusoria, at any rate in one phase of their existence. "This nucleus," says Siebold, "whose presence gives the Infusoria a resemblance to a cell, demands very special attention, inasmuch as it does not at once perish after the death of the animalcule in which it was contained. Probably," he goes on to say, "this nucleus is subsequently developed into a peculiar animal, and, in fact, many Infusoria are only the *larvæ* of other animals, whose complete cycle of metamorphoses has not yet been made known." In adducing the *Euglena viridis*, as an instance in which he had observed the development of the so-termed 'nucleus,' Siebold notices the fact of its surrounding itself with a sort of capsule or cyst, a circumstance already remarked by Ehrenberg; and it was this notice of the occurrence of an encysting process that first attracted Dr. Stein's attention, who had been struck by the remarkable correspondence in many respects between the motions and structure of the *Euglenæ* with those of the *Gregarinæ*. The addition in the former creature of an 'eye-spot' did not appear to him justly to entitle the *Euglenæ* to a higher rank in the zoological scale than the *Gregarinæ*, inasmuch as that spot has none of the attributes of an organ of vision, appearing to be nothing more than a particle of pigment. The *Euglenæ* moreover, and this was the point which principally struck Stein, became encysted like the *Gregarinæ*. He felt it necessary to inquire whether the encysting process was of the same import in the one case as in the other. Nothing appeared to support the notion, propounded by Ehrenberg, that the *Euglenæ* became encysted only at the moment of death; according to all analogy he was compelled to suppose that the formation of the cyst of the *Euglenæ* was the commencement of a process of propagation.

Here was a new starting point in the inquiry respecting the mode of development of the Infusoria, and from this point Stein started, and where he has arrived travelling in this path is shown in the works above cited, but chiefly in that which forms the subject of the present notice.

His attention was naturally led in the first place to the *Euglenæ*.

In a glass, in which were contained a great variety of ciliated Infusoria, and among them also numerous individuals of *Euglena viridis*, *Eacus*, and *Edeses*, he remarked after the lapse of some days the formation of a thin film on the surface of the water, composed of an interlacement of confervoid filaments and *Oscillatoriæ*. This film swarmed with *Euglenæ*, many of which had lost their beaks and crawled about with a worm-like movement among the *Confervæ* and *Oscillatoriæ*-filaments. Besides these he discovered to his great joy a great many transparent, gelatinous, or quite soft cysts, which sometimes contained only a single *Euglena* contracted into a globular form, sometimes two of a hemispherical form appressed together. The encysted *Euglenæ* proved to be still living, inasmuch as they moved about in the cysts, and if the cysts were ruptured the previously globular individuals reassumed their pristine elongated figure, and crawled about in the same manner as the other beakless individuals among the confervæ.

For what purpose was this encysting? The cyst was evidently intended for something more than a coffin. Further observations soon showed that the encysting process of the

Euglenæ had really reference to their multiplication. The process, however, appeared to be different in *Euglenæ* from that in the *Gregarinæ*, inasmuch as in the latter case two individuals are conjoined before the cyst is developed, whilst in the *Euglenæ* the case is formed usually around but one. For where two individuals were found enclosed in a cyst, it was at once apparent that they had proceeded from the division of an originally single individual.

Whilst thus investigating the *Euglenæ*, his notice was also directed to other forms of Infusoria contained in the same water, such as *Paramecium aurelia*, *Prorodon niveus*, and *Holophrya discolor*, the latter two of which species he frequently observed enclosed in well-defined gelatinous cysts; and as these Infusoria belonged to quite another principal division of the class, he began to hope that the process of becoming encysted would probably turn out to be of general occurrence in the Infusory world.

This proved to be the case, and the work then proceeds to describe the way in which Dr. Stein was led to detect the connexion between *Epistylis plicatilis* with a species of Ehrenberg's genus *Acineta*; an observation which pointed the way in his future researches. One of his earliest additional observations was that of the heterogeneous generation of *Epistylis digitalis.* In this species he traced first the metamorphosis of the *Epistylis* into an *Acineta*, and secondly, observed in the latter the extraordinary fact of the development and evolution of a *Trichodina*, a discovery which Ehrenberg has attempted to explain by the supposition that the *Trichodina* had been previously swallowed by the *Acineta*. Dr. Stein's important researches are continued through the family of the *Vorticellinæ*, and his observations given upon *Actinophrys*, *Podophrya*, the genus *Trichodina*, and on the nature of the *Opalinæ*, the propagation of *Chlorogonium euchlorum* and *Vorticella microstoma*, and particularly upon the quiescent condition of the latter Infusoria; upon *Spirochona gemmipara* and *S. Schentenii*, and upon the *Acineta* state of *Dendrocometes paradoxus*, *Zoothamnium affine*, &c. &c.

The concluding chapter is concerning *Paramecium bursaria*, *Ophridium versatile*, *Nassula ambigua*, and *Glaucoma scintillans.*

The bulk of this mass of original and valuable observations precludes our giving more than the above meagre outline of their purport, nor will our space allow of our making any copious extracts.

We are a little surprised to find that Dr. Stein is still inclined to retain some suspicion as to the correctness of the more modern views, according to which the family of the *Volvocinæ* should be referred to the vegetable kingdom; but, as he justly observes, however this may be, his observations will still be valuable as showing how extraordinarily near the development of the lowest plants is related to that of the lowest animals.

With respect to the development of *Volvox globator*, Dr. Stein's observations agree very closely with those of Mr. Williamson and Mr. Busk, recorded in the 'Transactions of the Microscopical Society,' and contained in our last volume, but they add nothing to the results arrived at by those writers. And with respect to the existence of more than one mode of propagation with *Volvox globator*, Dr. Stein's observations have led him to precisely the same conclusions as those at which Mr. Busk had arrived, and with which Dr. Stein appears to be unacquainted. After describing, for instance, the usual mode of multiplication by segmentation of certain of the zoospores, Dr. Stein proceeds to observe that this mode of propagation does not explain the appearance of *Volvox globator* in localities which had been completely dried up and afterwards refilled with water, or which had for a long time been dry land. There must, as he says, be another mode of propagation, in which germs are produced which do not suffer injury from the drying up of the water, and are capable of being dispersed through the air.

These 'winter spores,' as they may be termed, constitute the forms termed *Volvox stellatus*, which, together with *V. aureus*, Mr. Busk had already stated should be regarded as forms of *V. globator*, and as representing the 'winter,' or quiescent spores of other *Algæ*. Dr. Stein's view of *V. aureus* differs somewhat from that of Mr. Busk; he regards it as the quiescent form of a distinct species of *Volvox*, to which he assigns the name of *V. minor*. But the distinction he draws between the two does not appear sufficient to justify their separation. The chief difference, according to him, between *V. globator* and *V. minor* consists in the circumstance that in *V. globator* *eight* young colonies are produced, whilst in *V. minor* the number is very inconstant, varying between one and nine, most usually four. The formation of a second coat around the quiescent or gold-coloured spore is described by him, as it is figured and described in Mr. Busk's paper.

We must now conclude our notice of this highly-valuable work, which is indispensable to those who may make the nature and development of the Infusoria the subject of study.

CATALOGUE OF MARINE POLYZOA IN THE COLLECTION OF THE BRITISH MUSEUM. Part II. (Cheilostomata, Port.)

THIS second part of Mr. Busk's illustrated catalogue of Marine Polyzoa concludes their first subdivision, including the Cheilostomatous subclass, or the Celleporina of authors. The families contained in it are as follows :—

		Species.
Fam. 10.	Membraniporidæ.	
	Membranipora .	16
	Lepralia . . .	46
„ 11.	Celleporidæ.	
	Cellepora . . .	8
„ 12.	Escharidæ.	
	Eschara . . .	11
	Retepora . . .	3
Fam. 13.	Vinculariadæ.	
	Vincularia . .	1
„ 14.	Selenariadæ.	
	Capularia . .	5
	Lunulites . . .	4
	Selenaria . . .	1

As in the former part, magnified figures are given of every species, and a short appendix is added, containing the substance of Mr. Busk's observations upon the nature, structure, and uses of the avicularian and vibracular organs of the Polyzoa, which were communicated in a paper read before the Microscopical Society, and published in their 'Transactions.' The importance of these organs in a systematic point of view, in the classification of the Polyzoa, is exemplified in the Catalogue, and may be judged of from the fact, that of thirty-six genera therein described, twenty include species armed with either vibracula or avicularia, or with both; and that of 191 species, no less than 126 are furnished with avicularia or vibracula.

THE MICROSCOPE, ITS HISTORY, CONSTRUCTION, AND APPLICATIONS. By JABEZ HOGG, M.R.C.S. London, Orr and Co.

IT is a pleasing task to us to have to notice so many works as guides to the use of the microscope. Although these are not all of equal merit, they clearly indicate that there is a demand for instruction, and that the value of this instrument, as a means of observation, is making sure and certain progress. We are glad also to find, in the literature of microscopy, a tendency to seek a wide field of demand, by placing a low price on the works issued. We do not see why our intelligent artizans should not have the field of their vision increased and the pleasures of their life multiplied by the use of this instrument, as well as those who possess more of this world's goods than themselves. On this account we can strongly recommend Mr. Hogg's volume. It is not only well got up with a large number of plates, and woodcuts, but it is very cheap. This volume might be called "the microscope for the people." We hope our better microscope-makers will take the hint, and see if they cannot manufacture a microscope that a working man can afford to buy, and yet so good that he may be able to recognize with it the more interesting forms of microscopic structure and life. Such a microscope is wanted for schools, and the maker who will construct one of this kind for twenty or thirty shillings, will not only probably make his fortune, but be a benefactor to his race.

It is impossible for us to give, in detail, an account of Mr. Hogg's book. It is what its name implies—a cyclopædia of information on all subjects relating to microscopy. The author seems to have collected information from every source, and we are glad to find he has recognized in our pages so large a quantity of the interesting and useful contents of his volume. We do not like to find fault, but there are some things in the printing the book—mere matters of taste—which we hope to see altered in a second edition. In the meantime, they do not detract from the value of the information.

The Micrographic Dictionary, Parts I. and II. By J. A. Griffiths, M.D., and A. Henfrey, F.R.S. London, Van Voorst.

All possible microscopical subjects are to be treated of in this volume in an alphabetical way. Both the authors are known as good microscopic observers, and the two parts of the work before us promise well. The plates are accurately executed, and the Micrographic Dictionary, when complete, will, we make no doubt, be a standard work on the subjects of which it treats. We shall notice it more at length as it proceeds.

The Aquarium an unveiling of the Wonders of the Deep Sea. By Philip Henry Gosse. London, Van Voorst.

We are beginning to connect Mr. Gosse's name with the sea-side. For the future, when he announces a book, we shall conclude that it is to detail more of his pleasant experiences on the sea-shore, and to shed further light on our knowledge of the habits and manners of the tenants of the deep. Who that has seen the aquavivarium (we prefer this word to aquarium) in Regent's Park, has not longed to have a tank in their study or drawing-room? Who has not felt that beautiful and wonderful as plants are, animals are more wonderful still. One of the objects of Mr. Gosse's book is to give directions for keeping and domesticating marine animals. Our wood anemonies in Ward's cases are to be supplanted by sea-anemonies in Mr. Gosse's vases. Instead of dogs and cats we are to have dog-fish and cat-fish. Our gold carp is to be supplanted by the ancient Wrasse, and the corals on the fire-place are to give way to living corals working in a little ocean of their own. But what has this to do with the microscope? Very much. All the animals that can be kept in the aquarium, afford interesting employment for the microscope. Much yet remains to be known of the structure of many of the creatures which are most easily kept in this artificial manner. To those who would wish to examine these creatures we recommend Mr. Gosse's volume. It has several illustrations done in chromolithography, and also wood engravings of scenery and natural objects, and is, throughout, written in Mr. Gosse's usually felicitous style.

BOTANICAL AND PHYSIOLOGICAL MEMOIRS. RAY SOCIETY.

THIS volume consists of Professor A. Braun's Treatise on Rejuvenescence in Nature, Menighini on the Animal Nature of Diatomeæ, and Kohn on the structure of *Protococcus pluvialis.* Three more valuable or important works to the microscopist, at the present time, we could hardly mention. In Braun's Essay will be found one of the most complete accounts of cell-development extant. His general views will be looked upon by English readers as speculative, but they are full of sound thought based upon extensive and accurate original observation. The remarks of Menighini will be read with greater interest in this country, just now, than at any other time, as at no past period have the structure and functions of the Diatomaceæ attracted so much attention as at the present. The paper of Kohn, on the *Protococcus*, has also, especial interest, when the question of the nature of animal and vegetable functions is being so keenly discussed. We must give the Council of the Ray Society credit for descrying the signs of the times, in publishing a volume so entirely devoted to microscopic research as this. We are glad also to find that this Society has undertaken to publish other works interesting to the microscopical observer, such as Professor Allman on the British Fresh-water Zoophytes, Bowerbank on the British Sponges, and Williamson and Carpenter on the Foramnifera.

NOTES AND CORRESPONDENCE.

Description of a Magnetic Stage to the Microscope.—Some time ago, at one of the meetings of the Microscopical Society, the model or perhaps the incomplete stage itself of a microscope was exhibited, in which Mr. King, of Bristol, if I remember rightly, had applied a magnet for the purpose of retaining a soft iron object-bearer, and at the same time of allowing it to be moved about in all directions with great smoothness and facility. I am not aware whether the inventor ever carried the design further into effect, but as the idea struck me as highly ingenious, and capable of very useful application, I have endeavoured to carry it out in the following simple manner. The contrivance, it should be remarked, can only be applied to a simple stage, that is to say, to a stage consisting of a single plate. The magnetic forçe is contained in two semicircular magnets of hard steel (fig. 1), which are fixed on the under side of the stage, one on either side of the opening, with whose size the curve of the magnets should of course be made to correspond. The force is conveyed to the upper side of the stage by means of four soft iron pegs passing through four holes drilled in the stage, at points as near as may be corresponding with the four poles of the magnets, as at *o, o, o, o* (fig. 1). The iron pegs should slightly exceed the thickness of the stage in length, and should have a thin flattened head (fig. 2).

Fig. 1.

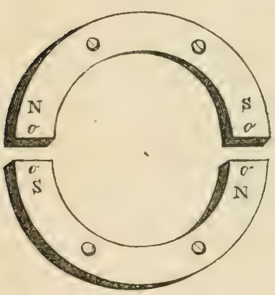
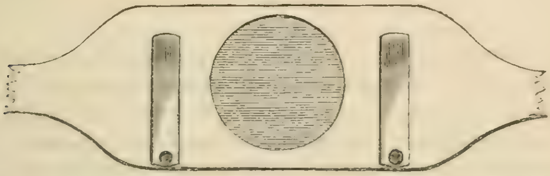


Fig. 2.

When inserted in the holes, into which they should fit closely, the magnets are screwed down upon the heads so as to be in close contact with them. All that is now requisite to be done is to see that the ends of the pegs, which project through the stage, are precisely in the same plane, and raised about a hair's breadth above the surface of the stage, so as not to interfere with its level when used in other ways, and yet, at the same time, to prevent the object-bearer from coming in contact with the stage itself, by which its free movement would be interfered with. The object-bearer, which is made of thin soft iron, may be of any form or dimensions; that which I have found useful, is of the shape shown in fig. 3.

Fig. 3.



The ends of the bearer being made to project beyond the sides of the stage, and being tapered off, serve as handles by which it may be most conveniently moved in any direction, with one or both hands, and as readily, if not more so, as by the usual screw-heads or lever of a compound stage.

In the case of a live-box all that is necessary is to have the plate, upon which the upright part is fixed, made of soft iron instead of brass, to do away with the need of any other bearer. It is very important that the under surface of the iron object-bearers should be ground true and made very smooth, which may be done easily enough with a water of Ayr stone and fine emery-paper.

The advantages of this stage are the following:—

1. That the stage may be made very thin.

2. The universality of the movements, and the ease and simplicity of their execution by one or both hands.

3. Its trifling cost.

In conclusion, I would merely remark, that whatever credit is due for the original idea of a magnetic stage to the microscope belongs, as I believe, to Mr. King, of Bristol, and that it is only in following out the suggestion thus derived from him that I was led to the simple contrivance above described, in the execution of which I was assisted by Mr. Hudson, optician in Greenwich.—G. Busk.

Note.—Since writing the above I have been kindly favoured by Mr. King, No. 1, Denmark-street, Bristol, with an account and sketch of his contrivance. It is more complicated, rather, than the one above described; but is very ingenious, and, perhaps, if carried out practically by him, which has not yet been done, might, in some cases, be superior. It has the disadvantage, however, of not allowing the stage to be so thin as my contrivance does.—G. B.

Meaning of the word "Unscheinbarkeit."—It cannot be denied that *one* meaning of the word "Unscheinbarkeit" *is* "unsightliness." This is beyond all doubt; and it may be admitted that in certain cases, the word might have the meaning attributed to it by Dr. Gregory (Q. J. Mic. Sc vol. II. p. 201), although, perhaps, such meaning would be better expressed by "Unsichtbarkeit," or "Unbemerkbarkeit." But in the place where Fresenius uses the word, he is speaking not of the *organs* of Fungi, but of the tribe generally, to which the attribute of "want of obvious perceptibility" certainly does not belong. Moreover, the difficulty of studying Fungi, owing to their minuteness and the delicacy of their organs of fructification, is specially referred to by Fresenius, in the very next line, and it is not, therefore, probable, that he intended to use the word "Unscheinbarkeit" in a sense which would make the next sentence but one of mere tautology. Again, a little further on, the author says, that the causes he has mentioned may account for the neglect of the study of Fungi by those persons whose main object is, that their collections of plants should have a *striking*, *neat*, or *elegant* appearance. I think I have said sufficient to show that Fresenius did not use the word to express "want of obvious perceptibility." "Unsightliness" may be rather a strong expression, but we have no other single English word which would at all convey the author's meaning. It might be paraphrased by "insignificance of appearance," "meanness of appearance," or "want of power to captivate the eye."—The Reviewer of Fresenius' Mycology.

On colouring Animalcules.—In feeding infusorial animalcules with carmine, one very great disadvantage presents itself, viz., the field of view becomes crowded with the dark particles of carmine, by which the object is hidden from correct observation, and the eye embarrassed; another is, the length of time often required to render the gastric organs visible, owing to the slow imbibition of the carmine.

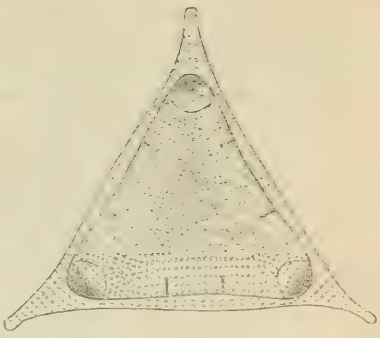
These difficulties, I think, may be obviated by the use of the red pigment which lines the cornea of the common house-fly. I have tried it for two years, and, without an exception, in every case, found it devoured with avidity by the carnivorous animalcules, and from its being capable of such minute division, the field was left almost as clear as before the addition of it. The ciliary vibrations were perfectly distinct, and a beautiful bright red speedily made its appearance in the internal organs of the smallest animalcule present, while, in some cases, the larger crustacean *Daphniæ*, &c., appeared as if their blood had become coloured with it.

If you consider these simple suggestions of any service to your readers, they are perfectly at your disposal.—Thos. C. White, 65 *Warwick Street*, *Pimlico*.

Three new British Species of Diatomaceæ.—In the course of a recent examination of the deposits of some of our tidal harbours, the following species of Diatomaceæ have come under my notice; and as they have not been figured, as far as I am aware, in any former publication, I annex camera drawings, with a short description of each, the insertion of which may prove interesting to some of the readers of your valuable Journal.

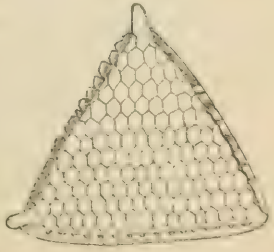
Triceratium armatum, n. s. (Fig. 1.)—Frustules large, with straight or slightly convex sides; angles produced into long horn-like processes, with rounded extremities; cellular structure minute, partially radiating towards the sides and angles; six or more spinous processes projecting from the surface of the valve.

Fig. 1.



I have seen three specimens of this fine *Triceratium*, sent me by my friend, F. Okeden, Esq., from Neyland and other localities, near Haverfordwest. It approaches nearly to the *T. tridactylum*, Ehr., of Mr. Brightwell, from Petersburg, Virginia,* but differs in the size and form of the angles, and in wanting the siliceous plate that extends beyond the sides of that species. Professor Bailey has described a form from the same locality, with four lateral spines, which he named *T. spinosum*; but, from his description and figure in Silliman's Journal,† I cannot satisfactorily identify it with the Neyland specimens, I therefore venture to apply the specific name of *armatum*. The spinous processes are very similar to those which occur on some species of *Eupodiscus*.

Fig. 2.



Triceratium comtum? Ehr. (Fig. 2.)—Sides straight or slightly convex, with a row of cells projecting above the margin of the valve; the horn-like processes at the angles short and obtuse; cellular structure large.

* Journal of Microscopical Science, vol. i. t. 4, f. 3.
† Vol. xlvi. t. 3, fig. 12.

Specimens of this species were sent me, along with the preceding, from the same deposit, and appear to agree very closely with the figure given by Mr. Brightwell in the first volume of the Microscopical Journal.* The cellular markings are as large as in *T. favus*, and I am rather doubtful whether it may not be a young form of that species; but the length of the angular processes and fringe-like row of cells at the margin appear to give it a distinctive character. It has not hitherto, I believe, been figured as British.

Doryphora? elegans, n. s. (Fig. 3.)—Valves obovate, divided by a median line, with transverse striæ, disposed in curves, concentric with the extremities; the interspaces occupied by minute cells.

Fig. 3.

This pretty little species I met with in some mud from Pembroke harbour; in form and structure it approaches the Natal specimens described as *Euphyllodium spathulatum* by Mr. Shadbolt;† but I have lately heard from Mr. Brightwell that similar forms were previously referred by Mr. Smith to the genus *Doryphora*, and as his absence from England precludes me from submitting the specimens for his determination, I adopt his generic appellation. Until living specimens are obtained it must be considered a doubtful form; but if stipitate, it would be included in the second sub-tribe of Mr. Smith's Synopsis; and in structure would be nearer *Doryphora*, or *Rhaphoneis* of Kutzing, than any other genus. The structure of the valve is exactly similar to that of the Natal specimens, but differs in being smaller and less ovate. All the figures are drawn to a scale of 400 diameters.—F. C. S. Roper.

Since writing the preceding note I have received the following description of the habitat of the two species of *Triceratium*, and I have reason to believe that the same deposits will yield several other curious forms. "The first specimen of this species (*T. armatum*), I found in a sample of the alluvial mud deposit at Neyland, which is a Creek of Milford Haven. The mud was obtained from a depth of twenty feet, with a boring tool constructed for the purpose; I have since found it in a surface gathering from the same locality, and it also occurs in the upper deposits of the River Cleddon, within a mile of Haverfordwest. The other species of *Triceratium* occurs rather abundantly in the gathering from Neyland at twenty feet depth."—Fitzmaurice Okeden, C.E.

* Journal of Microscopical Science, t. 4, f. 4.
† Ibid., vol. ii. p. 41.

On the Illumination of Transparent Objects.—I shall feel greatly obliged if you will insert in the next number of the 'Quarterly Journal of Microscopic Science' a few observations on Mr. Wenham's last paper on Microscopic Illumination, in reply to some strictures made by him on a communication of mine upon the same subject.

I have no wish to impute to Mr. Wenham any other than the best motives, and I trust in the remarks which I am about to make he will give me credit for the same.

My principal object in the communication which you were so kind as to publish on the subject of microscopic illumination was to detail certain facts, the accuracy of which Mr. W. admits, designed to show the advantages and disadvantages of the present methods of illuminating transparent objects. The explanations which accompanied them I considered only to be of secondary importance, and though they appeared to me the most probable, yet I was ready to renounce them if they should be shown to be incorrect, or if reasons more probable should be advanced. Now in both these points Mr. W. has in my opinion failed, for on a most careful perusal of his paper I am unable to discover that he has either proved my explanations to be in any respect untrue, or advanced more feasible ones in their place.

As the explanation I have given of the cause of certain appearances presented by the *Pleurosigma angulatum*, under a particular kind of illumination, is merely the application of an optical fact, I see no reason for changing this opinion until it is shown either that I have misapplied this fact, or that what I have taken to be a universal law is not so. If I understand Mr. Wenham aright, he seems to me to deny the universal application of this law, "stating that light when incident at any degree of obliquity on diaphanous refracting bodies, with parallel sides, cannot be made to suffer total reflection either externally or internally." Now, as in this assertion there is so much ambiguity and complexity, and as it differs so much from the simple manner in which I have always seen the law enunciated, I cannot give up that which is universally admitted as a fact for what I cannot clearly comprehend, and therefore I must beg to retain my opinion until Mr. Wenham has made good this assertion. In the application I have made of the law in question parallelism or non-parallelism has nothing to do with the subject, as the reflection referred to in my paper is supposed to be from one surface only, namely, from that on which the rays are incident. I am well aware that all refracting bodies are too thick to have only one surface, but this forms no reason for complicating the enunciation of the law, the refraction or reflection taking place at each surface, requiring only a separate and independent observation. However, if Mr. Wenham can prove by experiment that this fact is not universal I will give up my point, and in the mean time I am curious to know what the relation of the angle of incidence will be found to be to the angle of refraction, in the case of light passing from a denser into a rarer medium, after the complement of the angle of refraction has vanished.

Mr. Wenham next observes that "attempts have sometimes been made to draw the undulatory theory of light into the subject of microscopic illumination, but without any substantial reason, as it has in reality *very little or nothing* to do with it." By this assertion, which is sufficiently dogmatical, I understand that the author means that the undulatory theory may have a little to do with microscopic illumination, but that, in fact, that little is so very inconsiderable as to deserve to be looked upon as nothing. Now this assertion cannot be correct, for as at present there are only two ways known of accounting for the phenomena of light, and these so dissimilar that if one be right the other is wrong. Hence, if the undulatory hypothesis be the correct one, it must apply as well to the facts connected with illumination as to the general facts of optics, and therefore if it apply in one instance to microscopic illumination, though in the least possible degree which Mr. Wenham seems to admit, it must be equally applicable to all.

With respect to Mr. Wenham's comments upon the globules of mercury, where he states that "he does not consider that a globule of this substance, as being strictly opaque is at all suited for testing an illumination intended for transparent objects," I may observe that this remark is unnecessary, as Mr. Wenham must have seen that these globules were never intended by me as a test of an illumination suited for transparent objects, but were merely employed to show some of the disadvantages of condensers, which purpose they answer extremely well by enabling us to illustrate a fact, which to my knowlege was not before demonstrated, namely, that in lenses of a short focus a great portion of the rays emanating from the source of light is reflected upon the object by the lenses composing the object-glass. Now these bodies being in all respects well adapted for illustrating and establishing this fact, and that in the most simple way, were I think very properly employed. But I did not stop here, the same effect was shown in my paper to be produced by transparent objects, though less in degree, that depending upon their form and density, and thus in the latter objects the light transmitted through them was shown to blend with that which they reflect, and so to produce a degree of confusion which, I maintained, ought to be taken into account, and allowed for, as far as possible, in the examination of all transparent objects.

This blending of transmitted and reflected rays, proceeding from the source of light, is greater when condensers are used than when only a plane mirror is employed, and therefore will be considered, I have no doubt, by all excepting Mr. Wenham, as an objection to these instruments, though, this being known and duly allowed for, the objection will be but trifling. Mr. Wenham says, "he cannot call to mind any ordinary object in which the reflection alluded to above takes place to such an extent as to create false appearances." If Mr. W. will take the trouble to look at globules of air in glycerine, illuminated by means of his own instrument, he will see it for the first time. These globules thus circumstanced have certainly a most unnatural appearance, and would never be taken to be globules of atmospheric air. All other bodies partaking of the spherical form when examined in a fluid of a different refractive power, will exhibit the same fact. Oil globules in glycerine do not show it, but in water they do. See my paper. Lastly, Mr. Wenham objects to the employment of globules of mercury as a means of disproving the radiated light theory, stating "that he does not see that it all affects the question, simply because it is one of those few substances that is incapable of radiating light." In reference to this part, I may observe that these objects were not employed with the positive intention suggested in this remark, but were simply referred to as a satisfactory means of showing that all the appearances attributed by Mr. Wenham to radiated light are explicable on the common principles of reflection, and thus I consider that they have an important bearing upon the question; for it must be admitted that if these facts allow of an easy and obvious explanation upon long-established principles, there can be no necessity to invent new theories to explain them.

When I first became acquainted with Mr. Wenham's condenser, which I acknowledge to be a very beautiful instrument, whose utility will rather be enhanced than abridged by any observations which I have made upon its mode of action, and when I first read his paper upon its use and construction, I was in favour of his hypothesis, and of the term "radiated light," chiefly because the facts are so represented in his paper as to leave no other way of accounting for them, but on finding that all these facts admitted of an easy explanation upon well-known principles, I renounced his theory of radiated light, and if I can judge anything from a note in Mr. Wenham's last paper, in which he evinces great dissatisfaction with the term "radiated light," as "not being perhaps philosophically correct," I am strongly of opinion that he either has or soon will follow my example.—GEORGE RAINEY, *St. Thomas's Hospital.*

The Spiral Vessel of Rhubarb, as seen under the influence of Polarized Light.—I have, in my possession, a specimen of the spiral vessel of the rhubarb, given to me by a friend, but although I have viewed it with a low power of great excellence (Smith and Beck's 2-3rd), yet I have never been able to see it satisfactorily with clearness and precision. This induced me to look at it with the polarizing apparatus, and when the Nichol's prisms are turned so as entirely to darken the field, I was much pleased to see the spiral vessel beautifully illuminated and the spiral perfectly distinct. I was induced to try a "selenite stage," in which the tints are violet and yellow in the alternate quarter revolutions of the polarizer, and the effect was not only exceedingly beautiful, but also very instructive, as the spiral exhibits, when the field is deep blue or violet-coloured, a most beautiful crimson, and is very clearly distinguishable from the investing membrane, thus affording an example of the utility of polarized light in certain circumstances.—G. HUNT, *Birmingham.*

List of Diatomaceæ.—The following Diatomaceæ were discovered in the recent United States Exploring Expedition, under Capt. Wilkes. The list here given is in the order of their geographical distribution.

Puget's Sound, Oregon.

DIATOMACEÆ.

Actinoptychus senarius, Ehr.
*Aulacodiscus Oreganus, H. et B.
*Amphitetras Wilkesii, H. et B.
Arachnodiscus Ehrenbergii, Bailey.
*Cocconeis rhombifera, H. et B.
* ,, sulcata, H. et B.
Coscinodiscus oculus-iridis, Ehr.
,, radiatus, ,,
,, excentricus, ,,
Epithemia gibberula, Kütz.
Grammatophora angulosa, Ehr.
,, stricta, ,,
,, undulosa, ,,
Isthmia obliquata.
Rhabdonema arcuatum, Kütz.
Surirella fastuosa, Ehr.
*Triceratium Wilkesii, H. et B.

Spongiolites, &c.

Spongiolites Agaricus, Ehr.
Dictyocha splendens, Ehr. This is now known to be a calcareous plate from a species of Synapta. It dissolves in acids, and polarizes light.

San Francisco, California.

Arachnoidiscus Ehrenbergii, Bail.
Cocconeis scutellum, Ehr.
Gomphonema minutissimum, Ehr.

Terra del Fuego.

Entopyla australis, Ehr.
Grammatophora serpentina, Ehr.
Grammatophora stricta, Ehr.

Rio Janeiro.

Climacosphænia australis, Kütz.
Grammatophora oceanica, Ehr.
*Isthmia minima, H. et B.
Dictyocha splendens, Ehr.
Spongiolites anchora, Ehr.
The last two come from the calcareous particles of an Echinoderm (Synapta).

Valparaiso.

Stauroptera aspera, Ehr.
Cocconeis scutellum, „
Actinoptychus senarius, Ehr.
Gallionella sulcata, Ehr.
Grammatophora hamata, Ehr.
Dictyocha speculum, „

Philippine Islands.

*Amphitetras favosa, H. et B.
Amphora libyca, Ehr.
*Campylodiscus Kützingii, H. et B.
Coscinodiscus linearis, Ehr.
Denticella Biddulphia, „
Gallionella sulcata, „
Grammatophora oceanica, Ehr.
Navicula elongata, „
Navicula Lyra, Ehr.
Pinnularia didyma, Ehr.
Surirella fastuosa, Ehr.
Tetragramma asiatica, Ehr.
*Triceratium orientale, H. et B.
Dictyocha splendens.
Spongiolites Agaricus.

Sooloo Sea.

Coscinodiscus excentricus, Ehr.
„ marginatus, „
Gallionella sulcata, „
Grammatophora oceanica, „
*Isthmia minima, H. et B.
Surirella fastuosa, Ehr.
Triceratium Favus, β. acuminatus, Bail.
Spongiolites Agaricus, Ehr. In situ, forming bunches in the tissue of a sponge.

Wilson's Island.

POUMOUTA GROUP.

Climacosphænia australis, Kütz.
Podocystis adriatica, „
Stauroptera aspera, Ehr.
Pinnularia didyma, „

Tahiti.

Climacosphænia australis, Kütz.
*Cocconeis Parmula, H. et B.
Denticella Biddulphia? Ehr.
Grammatophora oceanica, Ehr.
Gallionella sulcata, „
*Hyalosira punctata, H. et B.
Navicula Sigma, Ehr.
Podocystis adriatica, Kütz.
Stauroptera aspera, Ehr.
Triceratium concavum, H. et B.
Epithemia musculus, Kütz.

Tongataboo.

Denticella Biddulphia? Ehr.
Epithemia musculus, Kütz.
Grammatophora oceanica, Ehr.
Synedra superba, Kütz.

New Zealand.

A large collection of marine Algæ from New Zealand was examined, but no Diatomaceæ could be detected adhering to them.

Those marked thus (*) are believed to be new, and have been described. —Professor J. W. Bailey, *in Proceedings of the Academy of Natural Sciences of Philadelphia*, Oct., 1853.

Match Photographs, or Camera Lucida drawings of Microscopic Objects for the Stereoscope, made by means of the ordinary Monocular Microscope.—Professor Wheatstone, the eminent physicist, in connexion with his remarks upon the value of the binocular microscope, in the July number of the 'London Microscopical Journal,' suggests that the monocular microscope may be made to give match stereoscopic pictures, by successively changing the inclination of the axis of the objective and ocular to the stage holding the object. This plan, though not easily made applicable to microscopes of the present construction, must, I think, give excellent results with the low powers, say with the two inch and inch objectives, and possibly with the half inch. But with the higher powers of large angle of aperture, the close proximity of the front surface of the objective to the thin glass cover of the objects totally precludes its being put in practice.

The method described below may be readily adapted to any microscope, at an expense comparatively trifling; it is applicable to every grade of objective; and upon fair trial I find it to give satisfactory results.

Behind, and close to the objective, insert an isosceles glass prism, say a half or a quarter inch equilateral or rectangular prism, adjustable for position, and capable of being inclined at pleasure any required number of degrees, on a central axis transverse to the axis of the ocular and objective, said axis being parallel to the polished faces of the prism. When the hypothenuse or reflecting surface of the prism is made coincident in direction with the axis of the microscope, the position of the prism being appropriate, the light travelling from the objective to the ocular will suffer reflection in its transit through the prism; but the appearance and position of the field, except its reversal in one direction, will be essentially the same as if no prism were there. By inclining the prism a little, other objects are brought into view, as though the slide containing them were moved. If now, the slide be re-adjusted, so as to restore the field as at first, the objects will be seen from a different point of view, and will therefore wear a modified appearance.

The mode of proceeding is as follows: two good successive views of the same object are to be obtained, between which there must be a difference of inclination of the prism, say from four to eight or nine degrees, according to the depth of stereoscopy desired. In each instance, the principal object is brought to the centre of the field, by adjusting the position of the slide. In each instance, a careful camera lucida drawing is to be made, or a photographic impression taken; which, when properly viewed, each by an eye, will be found to coalesce into a single image, manifesting the fine stereoscopic effect, which characterizes the image seen through the binocular microscope.—Professor RIDDELL, *New Orleans Medical and Surgical Journal.*

On the use of the Camera lucida as a Micrometer.—Several communications on micrometers have appeared in the 'Microscopical Journal,' and I am induced to address you on the subject from having particularly noticed the two following extracts of a letter from Mr. Jackson in your last Number:—

"The inquiries for a cheap form of microscope which I constantly hear, make me think that the difference between 4*l.* and 1*l.* for an adjunct to the instruments, would, in many instances, be a serious obstacle to the use of any means of minute measurement; and it is with the view of placing these means within the reach of all observers that I have advocated ruled glass.

"To induce observers to make accurate measurements, which is the aim both of H. C. K. and myself, it is not sufficient to place an instrument in their hands; they must be taught to use it with little trouble."

I think that the following method of using the camera lucida with a stage micrometer answers the requisites of cheapness, facility, and accuracy:—

Place a stage micrometer in the focus of a microscope; adapt a camera lucida, and then accurately trace on a piece of card-board, one, two, three, or more of the divisions. Subdivide each division by tens or (if need be) by hundreds; then place the object to be measured in the focus of the microscope, and observe, through the camera lucida, the number of divisions it extends over on the card-board. For instance: I have an object-glass and eye-piece which, with the length of tube in my microscope, magnify 500 diameters; and on looking with these at a stage micrometer, with 200 divisions to the inch, I find that each division occupies $2\frac{1}{2}$ inches on a card-board placed underneath the camera lucida on the table. I mark one of these spaces on the card-board, and divide it into 25 parts; that is, into tenths of an inch, and, consequently, each tenth of an inch on the card-board corresponds to the 5000th part of an inch of an object in the focus of the microscope. Obviously, also, if instead of card-board, I use a slip of ruled glass, with a hundred divisions to the inch, each division will then correspond to the 50,000th part of an inch.

For convenience of calculation, it is desirable that each division of the micrometer should coincide with the lines of inches, or large fractional parts of an inch, on the card-board; and this is easily effected when the microscope is furnished with a draw-tube; but when the latter is wanting, the same point may be gained by elevating the card-board on a book or some kind of stage; of course, always taking care that the distance of the camera lucida from the card-board should be precisely the same, when an object is to be measured, as it was when the divisions were marked on the card-board. The ease of this method, also, in accurately determining the magnifying power of any combination of lenses and eye-pieces he may happen to possess, will be evident to any one attempting to practise it. The cost of a camera lucida is very trifling, and there would be no need to purchase a stage micrometer, if one could be borrowed for a short time, since a piece of card-board, once accurately marked* in the above manner, would supersede its further use.—HENRY COLES, *Hammersmith.*

* For each power.—[ED.]

PROCEEDINGS OF SOCIETIES.

MICROSCOPICAL SOCIETY. *May 17th*, 1854.

Dr. Carpenter, President, in the Chair.

J. F. Streatfield, Esq.; J. Lubbock, Esq.; — Shelly, Esq.; Dr. T. J. Sturt; A. Mongredieu, Esq.; and J. G. Noble, Esq.,—were elected Members of the Society.

A paper on the Circulation in *Closterium Lunula*, by the Rev. S. G. Osborne (see Journal, p. 234) was read.

March 29th, 1854.

Dr. Carpenter, in the Chair.

James M'Mahon, Esq.; Andrew Yeates, Esq.; Antonio Brady Esq.; C. O. Dayman, Esq; and George Hanby, Esq.,—were, balloted for, and duly elected.

A paper by Mr. Jabez Hogg, on the development and growth of the Water-snail, was read (Transactions, p. 91).

April 19th, 1854.

Geo. Shadbolt, Esq., in the Chair.

R. W. S. Lutwidge, Esq., and James Townley, Esq.,—were balloted for, and duly elected.

A paper by Dr. W. Gregory, of Edinburgh, on some deposits of Fossil Diatomaceæ, was read (Transactions, p. 104).

ROYAL SOCIETY. *March 9th*, 1854.

On a new and more correct Method of determining the Angle of Aperture of Microscopic Object-Glasses. By W. S. GILLETT, M.A.

THE author began:—"With the consideration that the central pencil was alone to be regarded, and that the marginal rays of this were the true limits of the angle of aperture, and that consequently the rays of all oblique pencils were to be excluded, as these might cross at a point not coincident with the principal focus, and being measured separately, might form an angle (apparently of aperture) not coinciding of course with the true one, although perhaps not differing from it in amount." Mr. Gillett's mode of measuring the aperture is as follows:—"The microscope is placed horizontally, and centred by an object placed in the focus. A hollow cone is substituted in place of the eye-piece, having an aperture at its summit. Light passing through this aperture is made to form an image of it in the principal focus of the object-glass, in the place of the original object. On this image a horizontally-placed examining microscope is then directed, which traverses as the radius of a graduated circle, having its centre corresponding with the plan of the original object, and therefore with the image to be received; and the angle of aperture is measured by the arc passed through between two extreme positions in the usual manner."

May 11*th*, 1854.

On the relation of the Angular Aperture of the Object-Glasses of Compound Microscopes to their penetrating power and to oblique light. By J. W. Griffith, M.D.

Dr. Griffith's remarks had reference only to transparent objects.

The ordinary cause of the outlines of objects becoming visible consists in the refraction of the light out of the field of the microscope, or beyond the angle of aperture of the object-glass; and another condition affecting distinctness consists in the relation which the luminousness or darkness of an object bears to that of the field or background upon which it is apparently situated. An increase of the angular aperture of the object-glass in certain cases will therefore impair the distinctness of their images, because this increased aperture will allow of the admission of those rays which would otherwise have been refracted from the field, and the margins will become more luminous and less contrasted with the luminous field.

If the parts of an object which refract light are large in proportion to the power of the object-glass and of irregular form, they will refract a certain number of rays, so that these cannot enter the object-glass; hence certain parts will become dark, and will map out in the image the structural peculiarities of the object. But if the parts are minute, of a curved form and nearly symmetrical, they will act upon the light transmitted through them in the manner of lenses, and their luminous or dark appearance will vary according to the relation of the foci of these *quasi* lenses to that of the object-glass.

In certain objects, however, of extreme minuteness, such as the valve of a *Gyrosigma* (*Pleurosigma*), the irregularities of structure are so very inconsiderable, or the difference of the refractive power of the various portions of the structure is so slight, that the course of the rays is but little altered by refraction on passing through them, and under ordinary illumination all the rays will enter the object-glass; neither are the rays collected into little cones or parcels of sufficient intensity to map out the light or dark spots in the field of the microscope, according to the relation of their *foci* with that of the object-glass.

This is the case with light transmitted directly through the object as in the ordinary mode of illumination; but when oblique light is transmitted, one of the two sets of rays passing through the depressed and the undepressed portions of the object will be so refracted as not to enter the object-glass, whilst the other set will gain admission, and thus the two parts will be rendered distinct. If the markings are more delicate, or if the difference between the refractive power of the two portions of the object is less, both sets will enter the object-glass. But when the light is rendered still more oblique, one set would be again excluded being refracted out of the field. Hence it is evident that the angular aperture must be larger as the markings are finer, or the difference between the refractive power of the portions of the tissue is less; because the obliquity of the light will be required to be very great to cause the exclusion of one set of the rays, and the other set would necessarily be too oblique to enter the object-glass unless it be of correspondingly large aperture.

The author then proceeds to explain the reason why a central stop will in certain cases render delicate markings more distinct, by stopping off the more direct rays, and allowing only the oblique ones admissible into an object-glass of large aperture to enter it. The direct rays, in fact, not conducing in any way to the more distinct perception of the object, the image of which is formed, as above explained, by what may be termed the difference between the more and the less refracted rays. And the difficulty in explaining how an object-glass of large angular aperture will render markings evident which were not visible under an object-glass of smaller aperture, vanishes when it is considered that the *additional* rays admitted by the object-glass of larger aperture are more oblique; hence one set of rays, as in the above case of oblique light, will be refracted from the field of the microscope, whilst the other set will enter it. Now as it is these *additional* rays alone which render the delicate markings evident, it is obvious that the more direct central rays (being at any rate useless) can only serve to impair the distinctness of the image, and that advantage will arise from cutting them off by a central stop.

The paper then proceeds to discuss the relation of the *penetrating* power of an objective to its *defining* power. The author's definition of what he terms penetrating power; would appear to make it depend upon the amount of angular aperture, but it is not very clear what is meant by *defining* power as contradistinguished from the above, or from the power of giving a clear definition of objects—a quality obviosuly dependent simply upon the accuracy with which the chromatic and spherical aberration of the objective are corrected, and wholly independent of magnifying or separating power

Royal Irish Academy. *January 23rd*, 1854.

On a New Method of measuring the Angular Aperture of the Objectives of Microscopes. By the Rev. T. R. Robinson, D.D., President of the Royal Irish Academy.

The effect of angular aperture is stated to be merely an increase of illuminating power analagous to that of linear aperture in a telescope, and from mathematical considerations given in the paper, it appears clear that, especially for covered objects, nothing is gained above 150° at all commensurate to the difficulty of constructing such objectives. "But in addition to this," the author goes on to say, "that the whole of these great apertures is not in every case thoroughly effective."

This made him seek some mode of measurement which would not only give the angle of aperture, but also show how the light was distributed; and the following seemed to fulfil both these requirements:—

"As a lucid point in the focus of the objective sends out from [to] the eye-piece rays nearly parallel, so light, sent in the opposite direction through the microscope, will converge at that focus and diverge in a cone whose angle equals the aperture of the objective. If this cone be intercepted at right angles to its axis by a screen, and the diameter of its section, together with the distance of the screen from the surface of the objective be carefully measured, they give the aperture. If S be the diameter of the section, D the distance, O the diameter of the objective, and I that of the image of the luminary used which is formed in its focus:—

$$2 \tan. \frac{A}{2} = \frac{S+0}{D} \times \frac{0}{0 \pm I},$$

the upper sign being used if the section is measured within the penumbra and *vice versâ*. In most cases, I will be so small that the second factor is = unity; for the author directed the light of the sun into the instrument by means of the reflecting part of a solar microscope, and not only got measures with extreme facility, but had at once a beautiful map of the objectives light-territory.

The results of measurements of available aperture taken in this way as compared with those by the usual method were briefly as follows, and from which the importance of this mode of examining stated apertures of object-glasses, will be at once apparent:—

	Focal length of Objective.	Aperture. By New Method.	By Old Method.
No. 1	$\frac{1}{4}$	80°.8	80°.75
,, 2	$\frac{1}{16}$	110°.8	160°.0
,, 3	$\frac{1}{12}$	109°.3	129°.0
,, 4	$\frac{1}{12}$	102°.0	126°.4
,, 5	$\frac{1}{16}$	114°.6	156°.0
,, 6	$\frac{1}{12}$	122°.8	170°.0

The facts adduced are sufficient, as the author believes, to show the necessity of attending not merely to the amount of aperture but also to its quality. What is the cause in the deficiencies in the latter respect can be determined only by one familiar with the construction of the objectives, but it probably arises from some of the lenses being so small that their edges meet the luminous pencil and reflect false light. The disturbance, however, may also be owing to the brass of the cells, but if so the remedy is the same, viz., the increasing a little the diameter of the posterior lenses. The author would also suggest another alteration, in case it be thought desirable still to make objectives of these extreme apertures—that the anterior surface be concave instead of plane.

TRANSACTIONS OF MICROSCOPICAL SOCIETY.

DESCRIPTION OF PLATE VII.

To illustrate Mr. Hogg's Paper on the Development and Growth of the Water-snail.

Fig.

1.—A magnified representation of the increase and change of situation occurring to the yolk of egg of *Limneus* on the fourth day.

2.—The change observed on the sixth day, showing the transverse fissure or divisional line in the mass.

3.—The formation of the shell proceeding more rapidly, it appears on the sixteenth day as the larger portion of the embryonic mass.

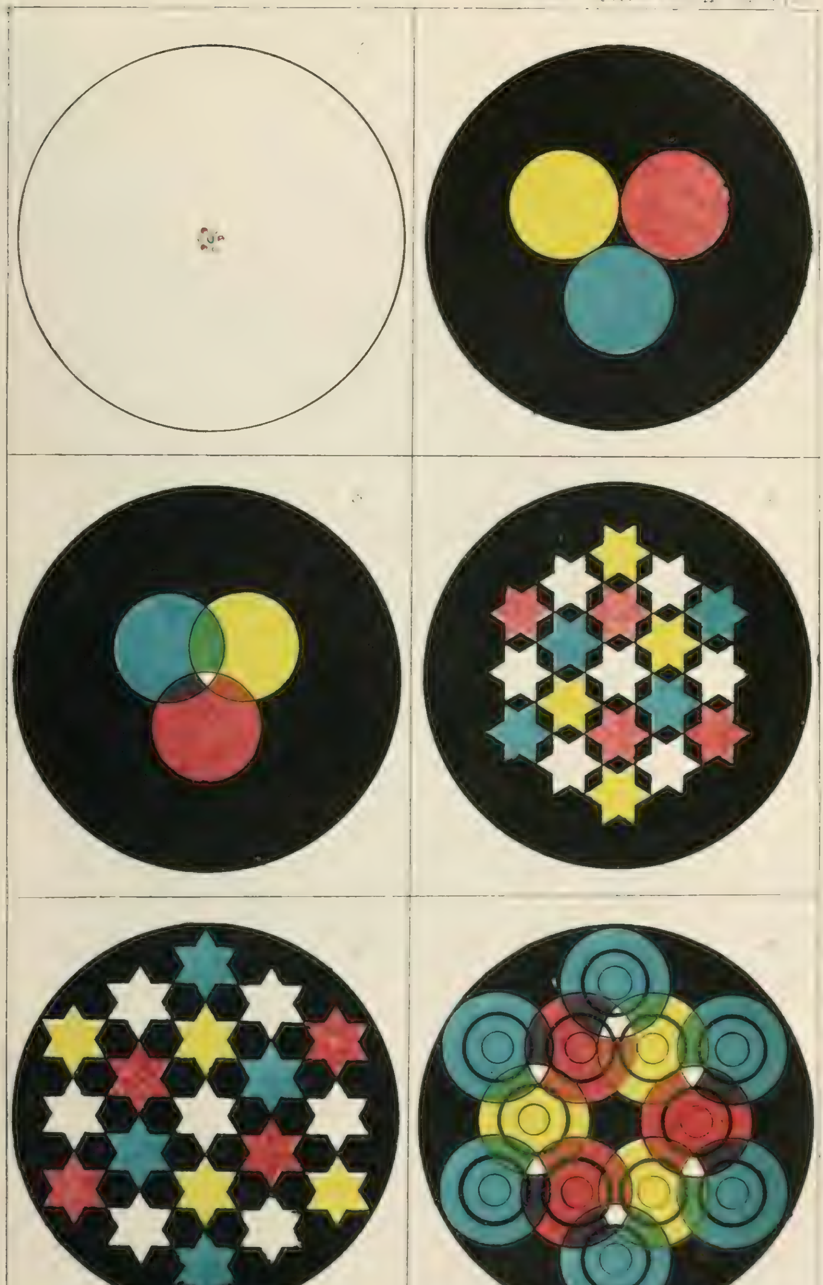
4.—The embryo performing its heliacal windings around the shell.

5.—The embryo, or young animal, seen soon after it has issued from the shell.

6.—The tentacles, with cilia, seen under a $\frac{1}{2}$-inch object-glass; the arrows indicating the course of the current produced by the cilia.

7.—The natural size and form of the shell of a full-grown *Limneus*.

8.—Parasitic animal found on the body of *Limneus*, magnified 100 diameters.



DESCRIPTION OF PLATE IX.

In illustration of Mr. Currey's Paper on two new Fungi.

Fig.

1 and 2.—Natural size of one of the Fungi.

3 and 4.—The same magnified by a 2-inch object-glass.

5.—The capillitium and spores under a $\frac{1}{4}$-inch object-glass.

6 and 7.—Natural size of second Fungus.

8.—Spores and hairs in its interior under a power of 220 diameters.

Tuffen West, chromo lith

TRANSACTIONS

OF THE

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OF

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VOLUME II.

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VOLUME II.

TRANSACTIONS

OF THE

MICROSCOPICAL SOCIETY

OF

LONDON.

On the Application of Binocular Vision *to the Microscope.* By F. H. Wenham. (Read May 25, 1853.)

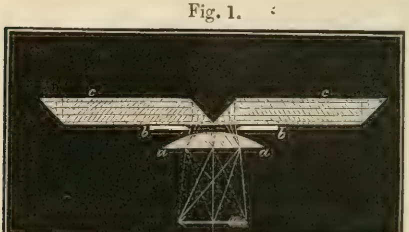
On viewing objects by the unassisted eyesight there are two conditions which enable us to appreciate or judge their various distances. Firstly, the object is observed by each eye from a separate point of view, and the consequent difference of outline, light, and shade between the images formed on each retina allows us to form an accurate idea of their various sizes and positions. The angle of *stereoscopic vision* has been stated somewhat definitely to be about 18 degrees, but this must be subject to considerable variations, as whether the observer is long or short-sighted, the difference of distance between the eyes, and also the *bulk, form, and position* of the object. I may state that I have obtained a very good perspective of minute objects when the angle of vision has exceeded 50 degrees.

If we perforate a card with a pin, and examine the articles in a room illuminated by candle-light, with one eye looking through this aperture, we shall be able to judge of distance only from the relative intensity with which the objects are illuminated, the nearest receiving and giving off the greatest quantity of light, and the farthest being in comparative darkness.

I make these preliminary observations because, in viewing the greatest portion of objects under the microscope, the conditions here referred to, which give us the faculty of judging of bulk and distance, do not exist in the same degree, if at all. In the first place, in viewing an object, as a transparency, with a single lens of short focus, we see it under such circumstances as seldom happen to such surrounding objects as come under our daily observation, and in the illumination of transparent objects by direct transmitted light, the effect is the reverse of that which is necessary for us to judge of distance by the relative intensity of the light, for that portion of the object farthest from the lens will receive the greatest share. This objection may probably be removed by a different system of illumination, but of this I shall treat hereafter. If the single lens is provided with a proper stop, what is known as the angle of aperture will be so exceedingly small that a series of uniform opaque particles lying behind each other in an object will be only seen by the direct light that they intercept, and the underlying ones will be invisible. These are the reasons why most microscopic objects, which we know must have a visible thickness, appear so perfectly thin, that we might almost imagine that they were painted on the glass slide upon which they are mounted. This illusion may be attributed to the natural effects of monocular vision, and in this case the only remedy is to view the object from different points at the same time with each eye, under equal magnifying power. I shall now enter into various methods of effecting this. One of the most simple and obvious is to employ two lenses, one to each eye, only differing from an ordinary pair of spectacles in the foci being shorter and the optic axes converging till the points where the foci intersect become coincident.

Binocular vision may also be obtained through a single lens, if the diameter is sufficiently large to allow both eyes to see through it at the same time as in the common reading-glass. In these instances we cannot well use glasses of shorter focus than four or five inches; and in cases where higher magnifying powers are required it becomes necessary to adopt some method which shall produce the effect of bringing the two eyes proportionately closer together, to suit the diminished diameter and shorter focus of the lens. This may be accomplished by means of a system of four plane reflectors, inclined at an angle of 45 degrees, and fixed behind the lens in a line at right angles to its axis, or else by four rectangular prisms in the same position; both these can be made to adjust to suit the diameters of various lenses and difference of distance between the eyes.

Fig. 1.



The arrangement that I have tried for lenses of short focus is represented by fig. 1: *a a* is a plano-convex lens, behind which is placed the usual stop *b b*. *c c* are two rhomboidal prisms of glass, with the reflecting ends inclined at an angle of 45 degrees. All the four surfaces of both prisms should be well polished, and their combined length when placed together should be such that the distance between the centres of the external diagonal reflecting planes should be the same as that between the eyes. I prefer the two solid prisms to a combination of four rectangular ones, as there is less loss of light, and error arising from external reflection. This combination makes a remarkably fine hand magnifier, giving such a depth and substance to objects as cannot be obtained with a single eye; the field of view is also large, as we are able to see the object obliquely through the lens. For lenses of low power, as from one to two inches focus, the prisms would require to be separated to some extent, or we should not obtain a sufficient angle for stereoscopic vision; in fact, we must consider this merely as a method of bringing the eyes closer together, that we may be enabled to see through a lens of small diameter with both of them at the same time, in a similar way as with the ordinary reading-glass before referred to.

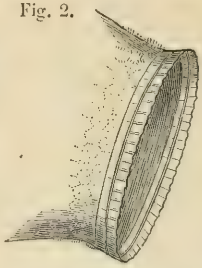
On looking through the prisms, fig. 1, without the magnifier, a singular illusion is produced, for the vision with the two eyes is brought so nearly to a state of parallelism that they are in effect blended into one, and we so far lose the power of appreciating distance, that we appear able to grasp objects several feet away from us, as the deceptions arising from monocular vision are increased by seeing with the two eyes from the same position as with one.

In obtaining binocular vision with the compound achromatic microscope, in its complete acting state, there are far greater practical difficulties to contend against, and which it is highly important to overcome, in order to correct some of the false appearances, arising from what is considered the very perfection of the instrument. All the object-glasses from the one inch upwards are possessed of considerable angular aperture, consequently images of the object are obtained from a different point of view, with the two opposite extremes of the margin of the cone of rays; and the resulting effect is, that there are a number of dissimilar perspectives of the object, all blended together upon the single retina at once. For this reason, if the object has any considerable bulk, we shall have a more accurate notion of its form by reducing the aperture of the object-glass.

Select any object lying in an inclined position, and place it in the centre of the field of view of the microscope, then, with a card held close to the object-glass, stop off alternately the right or left hand portion of the front lens, it will be seen that, during each alternate change, certain parts of the object will alter in their relative position. To illustrate this, figs. 2 and 3 are enlarged drawings of a portion of the egg of the common bed-bug (*Cimex lecticularis*), the operculum which covers the orifice having been forced off at the time the young was hatched. The figures exactly represent the two positions that the inclined orifice will occupy when the right and left hand portions of the object-glass are stopped off. It was illuminated as an opaque object, and drawn under a two-thirds object-glass of about 28° of aperture.

Fig. 1.

Fig. 2.



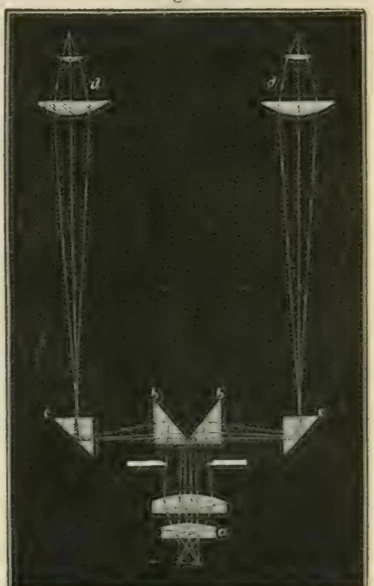
If this experiment is repeated, by holding the card over the eye-piece, and stopping off alternately the right and left half of the ultimate emergent pencil, exactly the same changes and appearances will be observed in the object under view. The two different images thus produced are just such as are required for obtaining stereoscopic vision. It is therefore evident, that if, instead of bringing them confusedly together into one eye, we can separate them, so as to bring figs. 2 and 3 into the left and right eye, in the combined effect of the two projections we shall obtain all that is necessary to enable us to form a correct judgment of the solidity and distances of the various parts of the object.

I shall now explain some plans for effecting this. The most obvious method is to have two microscopes placed side by side, and converging towards the object, each tube to be furnished with a similar objective and eye-piece. For very low powers this would, no doubt, be the most perfect form of binocular microscope, but it is liable to objection, firstly, on account of its expense, and, secondly, from the difficulty, if not impossibility, of using the higher powers. I do not think that it would be practicable to use anything beyond the half-inch object-glass; but I believe where vision is assisted by the use of both eyes together, it would be of advantage to employ objectives of smaller angular aperture, in this case, the focus would then fall at a greater distance from the front lens. I should also mention that a microscope of this description would require the two tubes to be placed at a different angle of convergence for every pair of object-glasses employed, of either longer or shorter focus.

It has also been proposed to bisect the whole combination, of which our best objectives are composed, and separate the semi-lenses a sufficient distance asunder to obtain the effect of stereoscopic vision, each half being made to serve the purpose of a distinct combination; but this, I believe, would not answer at all, for if we escaped the total destruction of the object-glass during the operation of sawing it through, we should render it useless for all the ordinary purposes of investigation, and also because any separation of the semi-lenses is quite unnecessary; for the angle of aperture of all the object-glasses by our best makers now exceeds that which is requisite for obtaining stereoscopic vision; and the methods that I have now to explain refer to the principle of obtaining two images of the object through the same object-glass, which is in all cases of the usual construction.

In the last 'Quarterly Journal of Microscopical Science' there appeared a notice of a binocular microscope by J. L. Riddell, from Silliman's Journal. According to his description, fig. 4 will represent the arrangement; *a* is the objective provided at the back with the usual stops. The pencil of rays emergent from the object-glass is bisected and reflected in opposite directions, by means of the internal surfaces of the rectangular prisms *b b*, which surfaces are inclined at an angle of 45°. The rays are again reflected in a vertical direction, by means of two similar prisms, *c c*, the distance between which must be regulated by the position of the eyes. The last prisms must be placed upon a lower level, as from the direction in which the rays are incident upon the first reflecting surfaces of the prisms *b b*, they have a downward tendency. The rays, after crossing each other, are received by two Huygenian eye-pieces, *d d*. In the diagram I have shown the prisms no larger than necessary for collecting all the rays from any of the object-glasses to be used; but it must be evident that Mr. Riddell makes use of prisms of

Fig. 4.



a larger size, as he states that "The outer prisms can be cemented to the inner by Canada balsam." This amounts to the same thing as using a pair of prisms of solid glass, such as is represented by *c c*, fig. 1. I have carefully tried both of these methods, and find that the prisms alter the chromatic correction of the object-glass, and also materially injure the definition; for in making arrangements of prisms of this description we must always bear in mind that they produce a similar kind of aberration as a piece of glass of the same thickness as the distance which the ray passes through, both before and after its reflection. There is also great difficulty in getting a perfectly flat surface to the small reflecting planes. All these defects will be greatly magnified by the eye-piece.

Fig. 5.

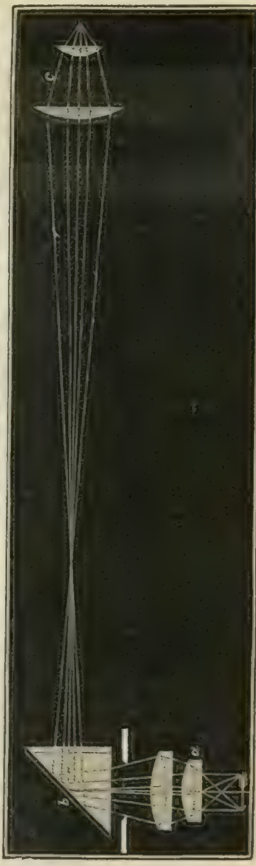
I have also tried what effect could be produced by means of plane reflectors, as Mr. Riddell says, "I use, for lightness and economy, four pieces of common looking-glass instead of prisms." My experiment was not tried with common looking-glass, but with thin microscopic covering-glass, silvered at the back. The definition with the lower powers was tolerably good, but the loss of light very great.

In order to remove the illusion of elevations appearing as depressions, Mr. Riddell proposes the "additional use of erecting eye-pieces;" but I am afraid that when the microscope is taxed with this addition, the loss of light and defining power will become very great, and that even easy test-objects will appear so obscure as to preclude all hope of our making any additional discovery relative to their structure.

I must remark, that I have made these last observations and experiments merely for the sake of arriving at the truth, and not with the view of detracting in the slightest degree from the merits of Mr. Riddell's invention; for very great credit is no doubt due to him for leading the way to the practical application of a principle, in the absence of which the microscope still remains an imperfect instrument; and, for my own part, I may, in all probability, shortly see the day when my own designs for effecting the same end may be rendered obsolete by the march of improvement.

In the plan just referred to, the error arising from the length or thickness of the prisms may be diminished by making the two microscope bodies converge towards *b b*, fig. 3, and adopting two rectangular prisms with the reflecting surfaces at the proper inclination for directing the rays of light from the object-glass, up the centre of each tube: by this means we can much reduce the substance of glass that the rays will have to pass through.

Fig. 6.



Figs. 5 and 6 represent another method that I have contrived for using only two prisms, which can be made of the smallest possible size, and also at the same time do away with one reflecting surface, which is one of the principal sources of error. Fig. 5 is the plan, and fig. 6 the elevation. *a*, fig. 6, is the object-glass, over which are two right-angled prisms *b b*, placed side by side with the reflecting surfaces, at an angle of 45°. *c c*, fig. 5, are two Huygenian eye-pieces, placed a sufficient distance asunder to suit the eyes, and converging towards the vertical axis of the object-glass. The contact sides of the prisms must be equally ground away, till their two emergent surfaces are in a plane at right angles to the axes of their respective eye-pieces. This method involves the necessity of having the object-glass at right angles to the two bodies of the microscope, and is therefore just suited to some of the foreign form of stands, but is of course inapplicable to the English ones, unless, indeed, we mount the bodies after our usual fashion, and allow the object-glass to point upwards in an inclined direction, and pass through the bottom of a stage, on the upper surface of which the objects can be placed, and which surface should be parallel to the axes of the bodies. This would give great facility for direct illumination; for whether we used a superposed achromatic condenser or not, we should not require a mirror either by day or candle-light. I have not yet tried this arrangement of prisms, but intend to do so, as I have a favourable opinion of the method, although the one next to be described is probably better.

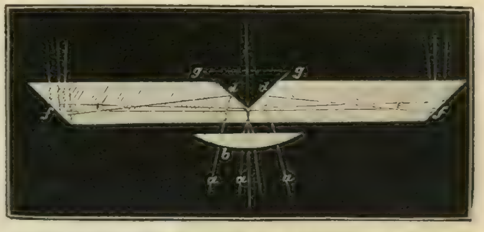
If we consider the relative position of the two reflecting surfaces of the prisms *b b*, figs. 5 and 6, they will form the same angle represented by the top line of fig. 7. It is therefore evident, that if a rectangular plate of speculum metal is ground and polished, so as to form two reflecting facets inclined to each other at the required angle, as represented by fig. 7; and this being placed at an angle of 45°, with the division of the facets intersecting the axis of the object-glass, we shall divide the rays, and reflect them horizontally, just in the same way as represented in figs. 5 and 6, merely by means of one single reflection. Any other direction than a right angle, with respect to the axis of the object-glass, may of course be given to the rays, by inclining the reflector more or less. From the simplicity of this contrivance, and the facility with which it may be constructed, I shall take an early opportunity of giving it a trial. The only question I have is, whether a material may not be found that will reflect more light than even speculum metal: I have heard an alloy of cast-steel and platinum well spoken of, but have never seen any of it.

In considering the aberrations which the thickness of glass contained in the reflecting prisms must inevitably produce when placed immediately behind the object-glass, it occurred to me, that if the same prisms were placed close to the top lens of the eye-piece, these errors, not being magnified, would be less sensibly felt.

I have before mentioned, that the final image of an object, when it leaves the eye-piece, is compounded of several different images or perspectives of the object, all blended together, and which are as equally capable of separation there as behind the object-glass itself, as exemplified by figs. 2 and 3, which bear exactly the same appearance when under view, with the alternate sides of either the object-glass or eye-piece stopped off.

Fig. 8 represents the methods that I have contrived for obtaining the effect of bringing the two eyes sufficiently close to each other to enable them both to see through the same eye-piece together. *a a a* are rays converging from the field lens of the eye-piece. After passing the eye lens *b*, if not intercepted, they would come to a focus at *c*, but they are arrested by the inclined surfaces *d d*, of two solid glass prisms. From the refraction of the under incident surface of the prisms the focus of the eye-piece becomes elongated, and falls within the substance of the glass at *e*. The rays then diverge, and, after being reflected by the second inclined surface *f*, emerge from the upper side of the prism, when their course is rendered still more divergent, as shown by the figure. The reflecting angle that I have given to the prisms was $47\frac{1}{2}°$. I also find it is requisite to grind away the contact edges of the prisms as represented, as it prevents the extreme margins of the reflecting surfaces from coming into operation, which can seldom be made very perfect.

Fig. 8.



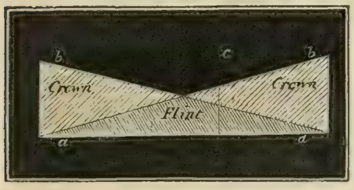
The definition with these prisms is good, but they are liable to objection, on account of the extremely small portion of the field of view that they take in, and which arises from the distance that the eyes are of necessity placed beyond the focus of the eye-piece, where the rays being divergent, the pupil of the eye is incapable of taking them all in; also there is great nicety required in the length of the prisms, which must differ for nearly every different observer.

I have constructed an adjusting binocular eye-piece, not differing in principle from the last. The first reflection is performed by means of a triangular steel prism, with the two inclined facets very highly polished; this is represented by the dotted outline *g g*, fig. 8. The rays, after having been reflected at right angles, are taken up by two rectangular glass prisms, shown by the dotted lines at *f f*.

The loss of light in this is much greater than in the former instance, and the field of view more contracted; for the rays from the eye-piece, after being reflected from the surface of the steel prism, fall to their natural focal distance, instead of being elongated, as in the solid prism, consequently the eye is still further removed from the focus. I had chosen hard steel for the reflector, on account of the property this material possesses, of allowing the figure of a small flat surface to be retained, or even perfected, during the operation of polishing. I have also tried a combination of prisms over the field-glass, using two eye lenses, but with no good result.

The best effect that I have yet produced in the way of binocular vision applied to the microscope, is that next to be described, in which I have altogether dispensed with reflecting surfaces, merely using three refracting prisms, which, when placed together, are perfectly achromatic. *a a*, fig. 9, is a single prism of dense flint glass, with the three surfaces well polished. *b b* are two prisms of crown glass, of half the length of the under flint prism, to the upper inclines of which they are cemented with Canada balsam. The angle of inclination to be given to the prisms must depend upon the dispersive power of the flint and crown glass employed. In the combination that I have worked out, I have used, for the sake of simplicity, some flint and crown that Mr. Smith kindly furnished me with, in which the dispersive powers are exactly as two to one, consequently I have had to make the angle of the crown just double that of the flint, in order to obtain perfect achromatism. The refractive power of each must also be known, that we may determine the angles of the prisms suitable for refracting the rays from the object-glass into the two eyes, at a distance of nine inches. *c c*, fig. 9, represents a ray of light incident at right angles upon the under surface of the flint prism. On leaving the second surface and entering the crown prism it is slightly bent inwards, and on finally emerging it is refracted outwards, in the direction required.

Fig. 9.



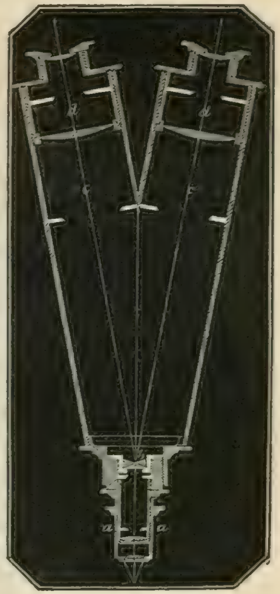
On looking through this prism I could not discover the slightest colour or distortion ; it is almost like looking through a piece of plain glass, and the loss of light is so inappreciable, that it is difficult to distinguish any difference between an object and its refracted image.

The base of the compound prism should not be larger than is sufficient to cover the stop of the lowest object-glass, in order that they may be made very thin.

The method of applying the prism to the binocular microscope is shown by fig. 10. *a a* is the object-glass, *b* the prism, placed as close behind it as the fittings will admit. The prism is set in an aperture in a flat disc of brass, which has an horizontal play in every direction, in order that it may be adjusted and fixed in such a position that the junction of the prisms may bisect the rays from the object-glass, and at the same time be at right angles to the transverse centres of the eye-pieces.

c c are the two bodies of the microscope, provided with draw tubes and the usual eye-pieces, *d d.* The distance between them should be rather less than the average distance asunder of the eyes, and in cases where these are very wide apart we can pull out the draw tubes, which will increase the distance between the eye-pieces.

Fig. 10.



With this apparatus I obtain *the whole of the field of view in each eye*, which circumstance I was not prepared to expect, as this must in some measure depend upon the correction of the oblique pencils of the object-glass, for we cannot expect to look obliquely through the objective of a compound achromatic microscope in the same way as in the single lens arrangement, fig. 1, but can only avail ourselves of such oblique pencils of rays as are corrected for passing through the axis of the microscope. The arrangement represented by fig. 10 certainly gives a larger and better field than any other that I have yet tried; and on examining a globule of mercury I could not discover any aberration or inward or outward coma when viewed by the eyes, either separately or together. I should here mention that the same illusion is occasionally produced in the appearance of some objects with the instrument last described, as mentioned by Mr. Riddell, the vision being to some eyes *pseudoscopic*, or projections appearing as depressions, *et vice versâ*. Probably habit would enable us to judge of their true form without our being under the necessity of resorting to a special expedient for the removal of the deception.

I have not yet tried a *binocular polariscope* applied to this instrument, but I have reason to expect some curious effects from it.

I have thus far announced the progress of my experiments towards the attainment of complete binocular vision with the microscope, and I cannot too strongly insist on the importance of striving to arrive at a perfect result, particularly with the highest powers, for I feel convinced that it will be the means of settling many disputed points of structure. Whether it will require objectives of a peculiar construction I am not at present able to determine, but I may observe that the high power object-glasses, as now constructed, are best suited for viewing very thin objects. We obtain far more pleasant vision of bulk and depth with a smaller aperture. I have no doubt that the defects of the larger aperture arise from the confused medley of stereoscopic images blended together in one eye, and which confusion must increase with increase of aperture; but if, on the other hand, we can divide these images between both eyes, then I admit that the aperture cannot be too great, as the largest portion of microscopic objects, from the way in which they are mounted, would be all the better shown under an exaggerated perspective, if I may so express it.

The binocular microscope has already explained to me some of the false appearances arising from oblique illumination. I refer particularly to what is known as the *diffracting spectrum;* for example, if we illuminate the *Podura* by very oblique light, we see a kind of overlying shadow, upon which the markings of the scale are also visible. As I cannot reconcile this appearance to the known laws of the diffraction of light, I think that it is miscalled, and apprehend that the phenomenon merely arises from the oblique light illuminating one of the perspective images partly as an opaque, and the other as a transparent object, and that they are, consequently, so far separated as to give the appearance of a double image.

In illuminating objects under the binocular microscope with the ordinary concave mirror some management is required in order to get both images equally intense, for we can readily get one brilliantly illuminated, while the other is in comparative darkness, appearing on a black ground almost as an opaque object, exactly resembling in their combined effect on the eyes what is known as the diffracting spectrum. It also occasionally happens that the angle of light from the mirror is not sufficient to illuminate both images at the same time. These appearances lead me to conjecture that the instrument will require a particular kind of illumination, but I am hardly yet in a position to express a decided opinion on the subject, but will investigate the matter shortly.

This is the sum and substance of my present experience with respect to binocular vision applied to the microscope, and I do not think that mere enthusiasm has led me to overrate the importance of the subject, but hope that what has already been done is only the commencement of a new era in the advancement of this useful and important instrument. I believe that there is yet much to be looked for in the way of improvement by the investigation of unexplored optical combinations and principles.

In conclusion I must express my thanks to Messrs. Smith and Beck for the prompt assistance that they have afforded me in the construction of the instrument, and also for the free use of such apparatus, selected from their stock, as might be useful to me in conducting my experiments.

A Short Description of some New Forms of DIATOMACEÆ *from Port Natal.* By GEO. SHADBOLT. (Read May 25, 1853.)

THE constantly increasing interest evinced in the examination of the elegant forms of the Diatomaceæ has recently received an additional impetus from Messrs. Smith and Beck's publication of the first volume of the long expected 'Synopsis of the British Species,' by the Rev. W. Smith, F.L.S.

A work of the kind alluded to will supply a want that has been much felt by microscopists, both as a record of what has already been accomplished in this branch of study, and also as a foundation for a general system of classification and nomenclature, for not only is the latter in the most deplorable state of confusion, but by far the greater number of the foreign species are only capable of being referred to by their "local habitation," being destitute of the other appendage generally considered so necessary.

Under these circumstances I propose conferring a provisional name on such new species as I am about to describe, trusting to the indulgence of any prior claimant to this right, whom I may unintentionally supplant, and promising to withdraw such name on cause being shown.

About twelve or fifteen months back I was supplied, by the kindness of Mr. Geo. Busk, with a gathering of Diatomaceæ from "Port Natal" so rich that I shall not attempt to give a detailed account of the forms already known, but, merely noticing a few of the most prominent of these, describe more particularly those species that are, so far as I am aware, entirely new, endeavouring by the respective designations to recall them to the mind, by fixing upon some prominent peculiarity of appearance in each as the foundation for such distinction.

From the prevalence of certain forms (although I am not at all acquainted with the facts of the case) I should be inclined to pronounce the locality whence they are derived as subject to *marine influence*, and at the same time probably not far from the mouth of some river, and it is also evident that the specimens are undoubtedly recent.

Mixed with the *Diatomeæ* are some other bodies, which are scarcely capable of being classed with them, although, like the spicula of many of the sponges (of which there is a goodly proportion), they are of a siliceous character, such as the *Dictyocha*, and also a form to which my attention was directed by Mr. Busk, and which I purpose identifying by the name of *Bacteriastrum*, from Βακτηρια, a stick, and Αστρον, a star.

By the kind assistance of another of our members, Mr. Capron, I am enabled to lay before you drawings of these most interesting bodies (of which may be distinguished three species), as also of most of the other novelties.

From the tenuity of their structure the various *Bacteriastra* are better observed without being mounted in balsam; they consist of a central irregular annular portion (not unlike the connecting membrane which may be observed in the *Diatomeæ* during self-division), surrounded by from eight to twelve rays, each many times longer than the diameter of the central portion, and the construction of these rays affords a good specific distinction, viz. 1st, *B. furcatum* (fig. 1), the marginal rays forked; 2nd, *B. curvatum* (fig. 2), marginal rays entire and curved in one direction; 3rd, *B. nodulosum*, marginal rays entire, straight, and covered with small protuberances like a knotted stick. This last species is by far the most rare.*

A form tolerably abundant and quite distinct from anything I have ever met with from any other locality I propose to call *Euphyllodium*, from ευ, and φυλλον, having somewhat the outline of a spathulate leaf (fig. 3). It is characterised as follows: viz. valve symmetrical, convex, divided by a median rectilinear rib, reticulations of an irregular oblong form, disposed in regular and elegant curves around centres formed by the terminations of the median rib. I have noticed but one species, which I have called *C. spathulatum*. There was at first some doubt in my mind whether this might not belong to the genus *Cocconeis*, but the very distinct appearance of the median line, and the absence of anything that could possibly represent the inferior valve, which in the latter genus is generally (I believe always) somewhat different from the superior one, and likewise the marked character of the outline, satisfied me that this supposition was incorrect. It is not unlike the aspect of the genus *Podosphænia*, but here again there are differences so distinct as to satisfy me that it cannot be referred to it with propriety; for instance, there is no reflexure of the valve, and the markings are not moniliform striæ, but rather tessellate in character.

* Since writing the above, I have made out most unquestionably that this pseudo-annular portion is a distinct cell, and not a mere annulus.

Of the genus *Triceratium* there are no less than five new species, one old, and one doubtful, making seven in all.

The first I shall notice is of moderate size, and is distinguished by peculiarly delicate markings somewhat obscurely disposed about *three equidistant pseudo-nuclei*—*Tr. sculptum* (fig. 4).

Next we have one in which the markings appear like minute dots, closely crowded together, but disposed in a very regular manner from the axis of the valve; this is about the same size as the preceding, but the outline differs materially, each margin being arcuate with the concave surface outwards —*Tr. arcuatum* (fig. 5).

Another species of medium size is in form nearly the converse of the preceding, the margin being so inflated as to cause the triangular outline to approach that of the circle; hence *Tr. orbiculatum* as its specific designation (fig. 6). This species exhibits a structure similar to that of *Coscinodiscus radiatus:* the reticulations, however, are not so regularly hexagonal, but they are largest at the centre, and diminish in size gradually towards the margin of the valves.

I have noticed also a single specimen of what appears to be *Tr. alternans*, but, as it presents only its front view, it is difficult to determine.

The next I shall allude to is, however, the most important, being very remarkable and especially interesting from the front view exhibiting the disposition of the horn-like appendages (figs. 7 *a* and 7 *b*) and the mode of dividing. On the lateral view of the valves the most striking peculiarity is a sort of twistedness in the angles (fig. 7 *a*), which is very marked: the specific name proposed is to note this fact, viz. *Tr. contortum.*

The surface of each valve is adorned with short spines arranged in a tri-radiate double row, and at the termination of each double row is one *very long one*, being about one-third of the length of a side of the valve. These long spines are independent of and placed nearer to the axis than the horn-like processes from which the genus derives its name. Fig. 7 *b* shows a specimen undergoing self-division.

Another new species, much smaller than the last, is characterized by the reticulations being coarse and irregular in form, and the horns very large as compared with the size of the valve—*Tr. crassum.*

Another species is, I believe, a variety only of the *T. favus*, which has been called gibbosus.

The genus *Pleurosigma* has no less than *five* species, two being quite new, and both having the markings arranged diagonally, that is, with (what the Rev. W. Smith considers) cells placed alternately in contiguous rows. The outline of the largest (fig. 8) is very clumsy and the ends obtuse, and the median line but slightly flexed—this I call "*validum;*" the other, *P. inflatum*, on the contrary, is of a graceful outline, the apices acute, the flexure of the median line considerable, and is broad in proportion to its length (fig. 9). A third species is, I believe, also new; it was observed by Mr. Capron when making the drawings; but as I have not had an opportunity of examining it, I have not further noticed it. Specimens of *P. formosum* and *P. Hippocampus* are also found.

There are two new species of *Amphitetras*, viz. *A. ornata* and *A. tessellata;* the former (fig. 10) is of small size, the margins of the valves being considerably hollowed or emarginate and folded over so that each valve is not unlike in form to a collegian's cap. The surface is elegantly but somewhat irregularly ornamented with delicate markings. *A. tessellata* (fig. 11) is of larger size, and the markings coarse and resembling a tessellated pavement.

There is a very striking and beautiful discoid valve, tolerably abundant, of the same genus as one commonly found in the guano from Callao, but which, I conceive, has never yet had a generic name. It differs in essential characters both from the *Coscinodiscus* and *Actinocyclus*, and its position would probably be midway between them.

It is possessed of a pseudo nucleus, is minutely embellished with delicate markings similar to those seen in *Pleurosigma angulatum*, &c., but in segments radiating from the centre, so that, in all probability, the front view would exhibit slight undulations. The absence of any distinct division between the segments, however, separates it from *Actinocyclus*. I propose for this form the generic name *Actinophænia*, from ακτιν, a ray, and φαεινος, glittering, with the specific designation *splendens*.

Fig. 12 is a new species of *Eupodiscus*, having four processes arranged regularly, and with the markings of a somewhat similar character to those in the last described species, forming an elegant cross; I have, therefore, named it *E. crucifer*.

Fig. 13 represents a *Campylodiscus latus*, also new, the canaliculi being wide apart and few in number.

Moderately abundant in this gathering are specimens of a highly interesting nature, on which the generic name of *Asterolampra* has been conferred by Professor Baily, of New York: one species, *A. marilandica*, has been figured in 'the

American Journal of Science and Art,' vol. xlviii.; a copy of the paper is in the library of this Society, it having been presented by the late Mr. Edwin Quekett. The Port Natal species differs in many respects from *A. marilandica*, as the following description will show:—

Frustules disciform, slightly convex, cellular (?), elegantly marked around the margin with 7 or 11 segments of an elliptical or parabolic outline, radii proceeding from the centre to the apex of each segmental curve, and strengthened with bracket-like projections. The aspect is not unlike an ornamental wheel, the radii forming the spokes. The segments are regularly and minutely divided into dots or cells (?), but it is necessary to use a high power and careful manipulation to display them: when properly shown, however, nothing can well be more exquisitely beautiful. There is a very noticeable peculiarity in the number of the segments; in every specimen I have seen, being either 7 or 11 (a few only of the latter number), and in the normal state the two valves, as far as my observation extends, are, without exception, disposed alternately, that is, that a segment of the superior valve always corresponds to an interspace of the inferior one, and *vice versâ*. I have named this species *A. impar*, from the odd number of segments. Fig. 14 is a representation of this beautiful frustule.

A species of another genus, established by Professor Baily, *Climacosphænia*, is shown at figs. 15 *a* and 15 *b*, *b* being the lateral view, and *a* the front view, in which the ladder-like divisions more resemble the links of a chain than in the only other species I have seen: I have consequently called it *Cl. catena*.

Two very interesting forms, by no means rare in this very rich gathering, belong to a genus that has been described under the names of *Zygoceros* and *Denticella*, the latter by Professor Baily, and as they clearly differ from the former, as may be seen by the most casual observer on inspecting fig. 16, which is a single frustule of a true *Zygoceros*, moderately common in this collection, I shall adopt the latter designation. I have only seen them either single or in pairs, having just completed the process of self-division, having a somewhat persistent connecting membrane. Front views are shown of the two new species in figs. 16 and 17. The side view is more or less elliptical in outline at the junction of the two valves, but sections in almost every other plane parallel to this would present a different figure, owing to the protuberances shown in the front view. In both species there arise from the central inflations two slightly curved spines from each valve, which, as in *Triceratium*, &c., in the process of self-division, are arranged across each other. The smaller of the two species (fig. 16), *D. simplex*, has but one central inflation, and the lateral expansions are symmetrically placed so as to give each valve the appearance of a sort of mitre or head-dress. It is marked with numerous well-defined dots or cells (?). The second species, *D. margaritifera* (fig. 17), has, besides the two lateral expansions, three intermediate inflations, the central one being considerably the largest, and the whole is covered by numerous pearl-like eminences similar in aspect to those so common in *Cosmarium* and other *Desmidieæ*. In both species the union of the two valves is marked by a sort of projecting band, which completely encircles the frustule.

In addition to those I have described there are very many other forms already familiar to observers of the *Diatomeæ*, and doubtless some new forms which I have overlooked. I am well aware that the present is a most imperfect sketch of a highly interesting gathering, containing, as will be shown by the following summary, no less than 55 species, of which 20 are certainly new, and in addition 4 forms of Sponge spicula.

	Species.			Species.
Bacteriastrium	3	of which are certainly new		3
Calophyllum	1	,,	,,	1
Triceratium	7	,,	,,	5
Pleurosigma	5	,,	,,	2
Amphitetras	2	,,	,,	2
Actinophænia	1	,,	,,	1
Eupodiscus	3	,,	,,	1
Asterolampra	1	,,	,,	1
Denticella	2	,,	,,	2
Campylodiscus	1	,,	,,	1
Climacosphænia	1	,,	,,	1
Navicula	3			—
Stauroneis	1			20
Pinnularia	2			
Nitzschia	1	And 4 forms of Sponge Spicules.		
Grammatophora	3			
Tabellaria	2			
Striatella	1			
Zygoceros	1			
Acnanthes	1			
Cocconeis	2			
Doryphora	1			
Podosphænia	1			
Synedra	1			
Coscinodiscus	3			
Tryblionella	1			
Meloseira	1			
Biddulphia	1			
Epithemium	1			
Dictyochas	1			
	—			
	55			

Observations on the Examination of SPONGE SAND, *with Remarks on Collecting, Mounting, and Viewing* FORAMINIFERA *as* MICROSCOPIC OBJECTS. By M. S. LEGG, A.I.A. (Read June 22, 1853.)

VARIOUS papers on the structure of the *Foraminifera* have been brought before the Society by Mr. Williamson and others, but the impression conveyed by those communications has been that the shells in question are not very easily obtained, and consequently they are likely to be passed over by microscopists from the want of specimens by which to study them.

Although the matter of this paper has no pretension to originality, I am induced to offer these remarks in compliance with a suggestion thrown out by a Member of our Council, that if Members would occasionally communicate facts, apparently unimportant in themselves, or new modes of manipulation under the general title of 'Microscopic Memoranda,' such remarks would contribute to diffuse a taste for the pursuit by facilitating the labours of those whose time and inclination admit of such researches.

Under this impression I now lay before the Society the result of my experiments on sponge sand, and the methods employed to bring the specimens more immediately within my reach without the labour of picking them out from an indiscriminate sample of the sand itself.

Having observed that there was some degree of uniformity in the magnitude of certain species of the *Foraminifera*, it occurred to me that by sifting the mass of sand through wires of different gauges important results would follow, and I therefore obtained some specimens of wire-gauze of 10, 20, 40, 70, and 100 wires to the inch, and, having also procured from a sponge merchant about a peck of the rubbish arising in sorting the sponges, I proceeded to separate the sand into parcels of different degrees of fineness.

In the first process (employing a gauze with 10 wires to the inch) I cleared the mass of clippings of sponge, small pebbles, &c., without obtaining any specimens of shells worth retaining.

In the second (20 wires to the inch) I obtained some very nice specimens of the *Orbiculina adunca* and *complanata*, but scarcely anything else; these specimens, of which the Members may recollect that Dr. Carpenter exhibited a series of very beautiful drawings at one of our *soirées*, were thus brought together, instead of being, as before, scattered through the mass, at intervals few and far between.

By means of the third gauge of wire-gauze, specimens of *Peneroplis*, and smaller specimens of *Orbiculina*, were brought together, with other species of *Foraminifera* of considerable variety of beauty and form, the result here obtained being very decided and characteristic of particular species; for, although the quantity retained after this process was comparatively small in relation to the original mass, yet the specimens were such as to afford an ample reward for the time and trouble incurred in obtaining them.

But the most surprising result was obtained by using the next quality of wire-gauze (that of 70 wires to the inch), the amount retained being much larger in quantity, and the proportion of shells to sand and other *débris* being such that sliders mounted indiscriminately from it yielded several very good objects in every instance.

From the above samples I was enabled to select, without difficulty, shells for microscopic observation; in the first two by the naked eye, and in the latter by using a hand magnifier, and removing them with the moistened point of a camel's-hair pencil.

The remaining portion of sand, forming probably 19-20ths of the original mass, will contain, as may easily be imagined, a very small comparative quantity of shells; but, nevertheless, it must not be thrown away. I again passed some of it through a gauze of 100 wires to the inch: the sample then retained yielded a fair quantity of shells by washing it in water, and thus other species characterized by their size were brought out by adopting the following process: having selected a dish of sufficient size and depth, and spread at the bottom of it a quantity of the sand, as much water was poured on it as would cover the whole to the depth of half an inch; after allowing the floating particles to settle, the dish was slightly raised at one end and gently agitated, so as to produce little eddies in the water. In a short time it was observed that small channels were formed in the sand of a whiter aspect than the other portions; allowing the water to settle gradually, the dish was slowly tilted at one end until the surface of the sand was exposed: the whiter particles being then carefully removed by means of a camel's-hair pencil, they were found to consist almost entirely of very minute shells; and the process being repeated a few times a large amount (microscopically speaking) was obtained for future examination.

In connexion with this subject I may venture a few remarks upon collecting, mounting, and viewing specimens of the *Foraminifera* as far as my experience has enabled me to speak.

When about four years ago I was staying at Weymouth with my friend Mr. Woodward, in walking over the Smallmouth Sand, which is situate on the north side of Portland Bay, we observed the surface of the sand to be distinctly marked with white ridges, extending many yards in length, and parallel with the edge of the water. Upon examining portions of these we found that they consisted of *Foraminifera* in considerable abundance, and, upon scraping up a quantity of it carefully with a card, we obtained in a short time a bottleful of material which contained thousands and probably millions of these minute shells.

My friend Mr. Cocken, during a recent residence at Brighton, was very successful in obtaining a considerable quantity of the *Foraminifera* from the surface of the mud exposed by the receding tide in Shoreham Harbour; and here also the surface only should be taken, in order to have a large proportion of shells.

It is very well known to many of our Members that the ouze from the oyster beds yields a very fair proportion of *Foraminifera* and other materials for microscopic examination, and I am inclined to think from these evidences that the surface, and the surface alone, of sand or mud banks will yield satisfactory results. And I should recommend to all those who contemplate collecting for themselves, or employing others to do so for them, to take *only the surface*, being convinced that a few spoonfuls obtained in this way will yield more than a spadeful taken indiscriminately. My view of this is also confirmed by the large amount of shells in the sponge sand, for being taken from the sea-shores, that which is gathered up with them is such as occurs only on the surface. I think it also very probable that a locality sheltered from the direct action of the sea would be more favourable for finding these organisms than a bold shore exposed to all the violence of the wind and waves.

If these conjectures should be borne out by experience, it is to be hoped that the increased facilities of finding their habitats will lead to extended observations on their living economy, a subject rendered extremely interesting by the papers of Mr. Williamson and the writings of Dr. Carpenter and others, where the subjects of their structure and zoological position are very ably discussed.

The species of *Foraminifera* are so numerous that the mere mention of 575 species described by D'Orbigny as peculiar to the torrid zone, 350 species to the temperate zone, and 75 species to the frigid zone, sufficiently attests their abundance, and the samples of sand which have come under my own observation from the Caramatta Strait in the China Sea, from

Australia, and other localities, afford ample proof of numerous and beautiful forms.

After collecting these minute shells, two very important points are, mounting them for future examination, and viewing them so as to obtain their true structure. Much depends on the different genera, as to the most eligible mode; the simplest and most natural is that adopted by Mr. Marshall of placing them in cells made of perforated card, putting a piece of thin glass over and sealing it down, so that the objects roll about loosely, and are viewed as opaque objects by a side light; another mode, also adopted by the same gentleman, is placing the shells on a glass slip with a little very dilute gum water, which causes the shells to adhere sufficiently to the glass as to admit of the air being exhausted from them when mounted in Canada balsam. They may then be viewed either as opaque or transparent objects, but it will be observed that the texture of the shell which invests the segments of the animal is (as observed by M. d'Orbigny) very variable, but it almost always follows the different mode of growth upon which the orders of that author are founded. When the segments are closely packed together, the shell is opaque, of a close texture like porcelain, and without any indications of external porosity; when the segments are alternate without a spire, and when the spire is oblique, the shell is porous, and pierced over the last cells with a great number of little mouths, through which proceed the filaments, but which become obliterated when the animal no longer needs them; when the segments are in a straight line, when they are coiled upon the same spiral plane, or when they are alternate, and the shell inequilateral, their texture is almost as transparent as glass.

From the above description it will be evident that one single mode of illumination will not suffice for duly developing the structure of these shells, and I should therefore recommend that some be mounted loosely in a cell, so that all parts may be viewed as they roll over; and others be mounted in Canada balsam, and viewed by means either of the annular condenser of Mr. Shadbolt or the parabolic reflector of Mr. Wenham. By these means the difference in structure between the upper and under surfaces of the same species of shell is brought out, and that confusion avoided which occurs when direct light is transmitted.

A Method of employing ARTIFICIAL LIGHT *for the* ILLUMINATION *of* TRANSPARENT OBJECTS, *by which it is so deprived of Glare and Colour as to be equal in its Illuminating Power to the best Daylight.* By GEO. RAINEY, M.R.C.S., Demonstrator of Anatomy at St. Thomas's Hospital. (Read June 22, 1853.)

THE principal disadvantages attending the use of gas and lamp light, as they are ordinarily employed for microscopic illumination, are the disagreeable and somewhat painful glare and the unnatural colour which is given to all objects thus illuminated.

These inconveniences are not felt so much where only a plane or concave mirror is used, as when the light is concentrated upon the object by an achromatic condenser; and, in the former case, they are partially remedied by transmitting the light through a piece of ground glass, either common or coloured; or by dulling the surface of the mirror; but in the latter one, these means, by cutting off too much light, are productive of more harm than benefit, especially where the markings upon an object are very delicate and require a particular kind of illumination to render them distinctly visible, as, for instance, the dots on the *Pleurosigma angulatum*. Hence, unless some other plan be adopted for moderating the intensity of all artificial light and correcting its colour, the employment of the achromatic condenser must either be limited to the hours of a good daylight, or the observer be in danger of materially injuring his eyesight.

Mr. Gillett's apparatus for producing the effect of a white cloud does, I am informed, remedy all the defects of lamplight, but, from some difficulty or other in constructing or in applying it, its employment has not become general.

The plan to which I have to call your attention is especially applicable to Mr. Gillett's condenser, and may at a comparatively small expense be made a part of that most useful instrument.

Before proceeding further I may observe that the principle upon which my apparatus is constructed is one which has been of general adoption for the preservation of the eyes of those who use glasses, and therefore so far has no claim to originality; but its precise construction and application to the microscope, and its effect in rendering artificial light equal if not superior to the very best daylight, are admitted by those who have seen it, and, to the best of my knowledge, are also new. But my motive has not been novelty but utility, in bringing this subject under the notice of this Society.

Now that which gives the peculiar burnish or glow to all objects when highly illuminated, whether by the direct rays of the sun, or by light proceeding from ignited matter, is due to the heating portion of the spectrum and certain coloured rays. In the former case we make use of light for microscopic illumination which has been deprived of this burnish by its having passed through the clouds; and in the latter this can be equally well effected by passing the light emanating from gas or a lamp through such transparent coloured media as will stop the calorific rays, and at the same time furnish the kind and amount of colour necessary to form, with the coloured rays of the flame, white light.

The combination which I find to answer best is the following:—

One piece of dark blue glass, free from any tint of red, one of a very pale blue with a slight shade of green, and two of thick white plate glass, all cemented together with Canada balsam.

This combination so completely stops the calorific rays, that when the direct rays of the sun are concentrated by a bull's eye of the ordinary size upon a lucifer-match with this medium intervening, it does not become ignited; and when this medium is used with Gillett's condenser, objects illuminated by the light of a camphine lamp appear as if they were seen by a bright daylight.

Remarks on ACTINOPHRYS SOL. By R. S. BOSWELL, *Spring Hill Cottage, Charmouth, Dorset.* (Read Oct. 26, 1853.)

IN an interesting paper in the 'Journal,' of a description of *Actinophrys Sol*, by A. Kölliker, &c., (Vol. I., pp. 25 and 98,) the author, after entering into a very minute description of this curious animalcule, says:—"The creature also seems to be capable of altering its entire form to a certain extent, and to be able to expand and again contract itself *in toto.* More extensive and more energetic movements do not occur at all, and I am consequently altogether ignorant as to how locomotion of the animal is effected." My object in sending this short notice is to give him that information which he seems to want, though perhaps by this time he may have made the same discovery as myself, if not, it cannot but be interesting, not only to him, but also to others, who take an interest in these minute wonders of creation.

The "Sun animalcule" is very common in this part of Dorsetshire; it abounds in pools where *Desmidieæ* are found: they are ravenous feeders, not only upon the *Desmidieæ*, but also upon all kinds of minute spores and animalcules. It was on examining some beautiful *Desmidieæ*, a few evenings back, that my attention was arrested by the curious appearance of two or three very small *Actinophrys* floating very lightly upon the surface of the water in the form of a ball, with their delicate tentacular filaments perfectly erect all over their bodies; in fact, they seemed to be floating upon these delicate filaments. This beautiful and curious appearance, so different from what I had generally observed, induced me to request Mrs. Boswell to look at it, but, while she was rising from her seat, I exclaimed, "You are too late, the little creature has given a leap, and I have lost it!" but upon moving the slide in the direction of the leap, I found the creature composedly resting in the usual manner upon the surface of the water—that is, in a flat position. This was not a solitary instance, for about five minutes after another gave a similar leap: the distance must have been very great considering the size of the animal, as it was in the centre of the disc under Messrs. Smith and Beck's 2-3rd object-glass with the second eye-piece, and I had to travel full an inch beyond the radius. These are the only two instances I have met with at present, not having made the little creature my peculiar study.

I have frequently mounted the *Actinophrys Sol* with *Desmidieæ*, but they generally burst at the edge of the sphere owing to the pressure of the glass cover, although they are to all appearance considerably thinner than *Micrastorias denticulata.*

Remarks on the Structure and Function of the Avicularian *and* Vibracular Organs *of the* Polyzoa; *and on their value as diagnostic characters in the classification of those creatures.* (Read Nov. 23, 1853.)

The Polyzoa, or, more properly speaking, one class of the Polyzoa characterized by the possession of a movable semi-crescentic lip, furnished with a corneous rim, at the mouth of the cell,—the *cheilostomata* as I have elsewhere termed them, or *Celleporina* of Ehrenberg,—are, many of them, distinguished by the presence of appendicular organs affixed to one part or another of the cells of which the polyzoarium is composed. These organs are of two kinds, the one a sort of pincers, and the other consisting of a long, slender, movable seta. To the former set of organs, of whatever form, the term *avicularium* is here applied, and the latter are designated as *vibracula.* With respect to the structure of these organs of either class it is sufficient to remark that, however diverse their appearance may be, they are all constructed upon the same general type, that is to say, the organ consists of a hollow cup or receptacle containing two sets of muscles for the movements of its motile portion, the *mandible*, as I term it in the one case, and the *seta* in the other. Beyond this general conformity in type, however, my knowledge of the more intimate structure and contents of the cup in the vibracular organs, does not allow me to approximate them to the *avicularia.*

The *avicularia*, besides the movable mandible, it may be observed, always have a corresponding fixed *beak*, the opponent as it were of the mandible, and necessary to constitute the organ, what I presume it to be, an instrument of prehension. This beak is needless, and is therefore wanting in the *vibracula*, and its absence in cases where the movable part is detached, would serve to distinguish one kind of organ from the other.

I. The *avicularia.*—

The first notice we have of the existence of these organs or rather of one form of them, is contained in Ellis's account of what he terms the "Bird's-head coralline," (Nat. Hist., Zooph., p. 36, pl. 20,) where he says, "On the outside of each cell we discover, by the microscope, the appearance of a bird's head, with a crooked beak opening very wide."

Mr. Darwin, in the 'Voyage of the 'Adventure' and 'Beagle,'' adverts at some length to these organs, and describes their actions in the living state very graphically. They have also been described with great care by Dr. Van Beneden and the late Professor John Reid, and also by Nordmann and Krohn, the latter (as I extract from Dr. Johnston's 'Hist. Brit. Zooph.') classifying them under three different forms: 1, those which have the figure of the crab's arms; 2, those which resemble pincers; and 3, those which are formed like bristles or hairs; the last corresponding to what are here termed the *vibracula.*

In a paper read before this Society, October 27, 1847, and published in the 'Transactions,' I have described more particularly the structure of the curious and unique form presented by this organ in *Notamia bursaria*, pointing out, I believe for the first time, that the muscles were divisible into two distinct sets, one for the closure and the other for the opening of the mandible, with other minute particulars of their mechanical arrangement previously unnoticed; I also stated that the muscles not only in this organ but throughout, in this and other Polyzoa, are of the striped kind, or resembled those of the Brachiopoda more than of any other class in the Mollusca. I also indicated that the mandible and the beak of the cup were differently constituted to the rest of the organ, being composed of a horny instead of calcareous substance; and that besides the two sets of muscles above noticed, the cup contained a "peculiar body of unknown nature."

I believe that up to the present time our knowledge of these organs is pretty nearly limited to the above particulars. With respect to their homologies and functions, nothing but conjectures have been offered; in fact, as regards the homologies of these organs with any existing in animals belonging to the same or any other class, no conjectures beyond the most vague have been offered. They may, perhaps, be regarded as analogous in *function* with the *Pedicellariæ* of the Echinoderms, as well as with the little accessory cups filled with prehensile filaments, or thread cells, which are found in the Plumulariæ and Campanulariadæ; but, I conceive that any *homology* with these organs is quite out of the question. They are as nearly related to the claws of a lobster or the feet of a Pygnogonum.*

* Mr. Huxley has pointed out to me several points of resemblance between the avicularia, especially as to their mechanical and muscular arrangements, and the shells of the Brachiopoda. An ingenious idea, and calculated to lead to the more serious consideration of the relationship between the Polyzoa and Brachiopoda, as suggested by Mr. Hancock

Their structure so obviously indicates their aptitude for prehension, that the supposition of such being their function has been long entertained: and I have myself no doubt whatever as to its being so; for, as Dr. Johnston observes, " although they are too short to hand the prey to the mouth, yet, retained in a certain position, and enfeebled or killed by the grasp, the currents set in motion by the ciliated tentacula may then carry it within reach." (' Brit. Zooph.' p. 334.) Some time in the last year, a specimen of *Scrupocellaria scruposa*, if I remember right, was exhibited at one of our meetings, with a minute vermicule, retained in the grasp of its *avicularia;* and the same thing seems to have been repeatedly noticed. An instance of the kind occurred to me when at the sea-side this autumn, and I have made a figure to represent the occurrence (Plate II. fig. 12). It is of a portion of *Scrupocellaria scruposa*, two of the avicularia on which have, apparently simultaneously, caught a minute vermicule which they retained with a most tenacious grasp. I kept the zoophyte under observation for several days, in the living state, and during that time, in fact, till the whole died, the grasp of these organs was not relaxed; and, although the movements of the captive were very active and apparently energetic, it was unable to liberate itself from the grim hold of its tiny but persevering antagonist. Another instance of the grasping propensity of these organs is exhibited in fig. 10, where two of them appear to be engaged in deadly combat. This figure is also intended to show the disposition of the muscles when thus employed.

Considering, therefore, the conformation of the *avicularia*, and the instances in which objects of prey of different kinds have been noticed engaged by them, I think it is impossible to avoid the conclusion that they are for the prehension of objects, either for the purpose of using them for food when dead and powerless, as suggested by Dr. Johnston; or it may be for purposes of defence.

With respect to the structure of the *avicularium*, I have already stated what is known; and have, in addition, only to remark that it has occurred to me to notice a circumstance hitherto overlooked, and which may eventually serve to throw some light upon the " peculiar body " contained in the cell to which I adverted in my observations on *Notamia.* It was in

(' Ann. Nat. Hist.,' 2nd Ser., vol. v. p. 198), than has hitherto been given to it. And with regard to this, it should be borne in mind, though perhaps the character is of no great importance, that all the Brachiopoda have striped muscular fibres, whilst for the most part the other classes of Mollusca, with some exceptions—*Pecten*, for instance—all have muscles of the unstriped kind.

that species, also, that I first noticed (in 1852) the fact that when the mandible is thrown back, or, in other words, when the *avicularium* is open, a slight prominence comes into view, covered with delicate setose hairs, which do not seem to be of the nature of cilia, because they exhibit no motion. These minute *setæ* appear to be seated on the "peculiar body" above adverted to, and which again seems to be so connected with the muscles by which the mandible is closed, or rather, perhaps, to a membrane by which they are covered, or by which the opening of the cell is closed, when the mandible is thrown back, as to be protruded, simply by the throwing back of that process; so that the *setæ* are then made to project beyond the level of the cup, and are withdrawn as the mandible closes. Fig. 2 represents this apparatus in *Notamia*. Fig. 7, the same thing in *Bugula plumosa*, and fig. 9, in *Bugula avicularia*. These are the only three species in which, up to the present time, I have been able to perceive this arrangement; but not having had an opportunity of examining the *avicularia* of any other, except *Scrupocellaria scruposa*, for the purpose, and in which I was unable to detect it (fig. 15), I am not prepared to say that it obtains universally. It is probable, however, that in a modified form it may do so. I am inclined to the opinion that it is a tactile organ, the object of which is to apprize the occlusor muscles of the contact of any minute floating object, upon which the muscles immediately contract, and either close the avicularium against the invasion of a foe or capture the appropriate prey.

A second point that I have also observed in these organs, and which, I believe, has not been before noticed, is that the portion of the cup in which the muscles and the greater part of the peculiar organ (which might probably be regarded as a nervous ganglion) are placed, is closed in by a delicate membranous tympanum, which has a central perforation, through which the conjoined tendon of the occlusor muscles passes, and also a second smaller opening (at all events in *B. avicularia*), the object of which I do not know. This tympanic membrane is shown in fig. 8.

So much for the structure and conjectural function of the *avicularia*; and to proceed to consider the *vibracula* in the same particulars.

1. As to the structure of these organs, I have nothing new to offer. They consist, as I have said, of a cup containing the muscular apparatus, and of a movable *seta*, articulated to the cup, and which appears to be moved in the same way as the mandible of the *avicularia*. This *seta* is in most cases simple and terete; in others, as, for instance, generally in the genus

Caberea, it is toothed on one side; and in others, as in the species forming the family *Selenariadæ*, of which we have no British representative, the *seta* is very variously and curiously formed, in some being trifid or bifid at the extremity, and in one, *Selenaria maculata*, it is spirally contorted and minutely annulated, so as very closely to resemble the proboscis of a butterfly.

As to the function of the vibracula, it would appear, in most cases, to be simply defensive. The *seta* may be observed in almost constant motion, sweeping slowly and carefully over the surface of the polyzoary, and removing what might be noxious to the delicate inhabitants of the cells when their tentacula are protruded.

Another circumstance often to be observed with respect to these organs, is this, that each presents inferiorly a rounded perforation, as in *Scrupocellaria* and *Canda*, sometimes channelled as in *Caberea*, which indicates the point of attachment of a radical tube or fibre. That this connexion with a radical tube, however, is not an essential attribute of the vibracular organ is sufficiently obvious from the circumstance that those tubes are frequent where no such organs exist; but where there are vibracula, the tubes invariably enter them, and not the cell itself. This is especially evident in the genus *Canda*, of which the British species, *Canda (Cellularia) reptans*, affords an instance.

In the case of the *Selenariadæ* or *Lunulites*, I think it not improbable that the *vibracula* may be subservient to locomotion.

These organs, both avicularian and vibracular, appear to me to be of very considerable importance in a systematic point of view; and, although from our imperfect knowledge of them, and in fact of the Polyzoa in general, the supposition can only be regarded as problematical, it seems not improbable that the presence or absence, especially of the *avicularium*, may be connected more directly with the intrinsic nature of the species upon which they are found, than has hitherto been supposed. It may, for instance, be the case, that those furnished with these offensive weapons live upon a kind of food different from that of the others who do not require such an aid in the capture or weakening of their prey. The Polyzoa may, perhaps, thus be divided into vegetable and animal feeders, or into feeders upon dead, and those which feed upon living organisms. One thing, however, is certain, that these organs afford, in many cases, excellent and available systematic characters; and this part of the subject I will now proceed briefly to discuss.

I have already stated that the accessory organs we are now considering are divided into two kinds, apparently with distinct functions—*avicularia* and *vibracula;* the one, probably prehensile, the other defensive. Of these, the *avicularia* are found by far the most extensively; in fact, they are wanting in but few of the genera constituting the *cheilostomatous* class of Polyzoa. In employing them for the purpose of classification, it is necessary to subdivide them into three classes. 1, the pedunculate. 2, the sessile; and 3, the immersed. The two latter classes, however, run insensibly into each other, whilst the pedunculate form is obviously quite distinct, inasmuch as it presents an additional member, in the form of a basal joint. It is this form of *avicularium* to which the term "birds," or "vultures' heads," is more properly applied. It is well known in this form, as it occurs in *Bugula avicularia*, *B. plumosa*, and *B. flabellata:* it is also found in *Bugula tridentata* (Krauss), a South African species; and in *Bicellaria ciliata;* whilst it is wanting altogether in *Bugula neritina*, *Bicellaria grandis*, and *Bicellaria gracilis*. A second modification of pedunculate *avicularium*, where it assumes the form of a long trumpet-shaped or infundibuliform tube, exists in *Bicellaria tuba*. So far as I know then, at present, the pedunculate form of avicularium is restricted to the genera *Bugula* and *Bicellaria*, though it does not exist in every species of either genus, and in one, assumes a form quite different from the ordinary. All that can be said about it, therefore, in those two genera, is that where the *avicularia* exist, they are of the pedunculate variety. The true "bird's-head" *avicularia* are always placed on the anterior aspect of the cell, on one side below the level of the aperture, whilst the tubiform variety arises on the back of the cell.

The sessile form of *avicularium*, as I have observed, may be subdivided into the projecting and the immersed. Of these, the latter is much the more extensively distributed: it is placed either at the angles or margin of the cells, or on some other part, usually of their anterior aspect, but sometimes on the posterior: instances of the latter are presented in *Caberea nuda*, where the vibracular organs, so characteristic of the other species in the same genus, are replaced by what may be termed *avicularia*, though, in fact, they should more properly be referred to the vibracular type, inasmuch as the radical tubes enter their bases. The genus *Retepora*, also offers an instance of the posterior position of true *avicularia;* but, with these exceptions, I am not acquainted with any other in which the *avicularia* are not on the sides or front of the body, as indeed, if our surmises with respect to their function be well founded, might be expected.

Of angular imbedded *avicularia*, the numerous species of the genus *Catenicella* afford examples. This organ, in fact, in that genus, often furnishing very satisfactory specific characters. In some, as *C. plagiostoma*, it is of gigantic size, in others very minute, and in one it seems to be aborted, being replaced by channelled processes, *C. carinata*. In several other species it is in some cells replaced by long ascending cornua or hollow spines, as in *C. cornuta* and *C. taurina*.

The genera *Menipea*, *Canda*, *Scrupocellaria*, and *Cellularia*, are respectively distinguished by the presence or absence of avicularian and vibracular processes. The former are always of the sessile kind, either *immersed*, and at the superior and outer angle of the cell, or *projecting* and placed on the front of the cell, below the level of the aperture. In the genus *Menipea*, there is an angular, superior, imbedded avicularium in many of the cells, and a projecting sessile organ on the front of the cell below the aperture, and no vibracula.

The British species, *Menipea ternata*, affords an instance; the other species similarly characterized, are—*M. cirrata*, *M. fuegensis*, *M. triseriata*, *M. ornata*, *M. patagonica*, and *M. multiseriata*. The genus *Canda*, differs from *Scrupocellaria*, mainly in the want of any avicularian process at the superior and outer angle; but the cells sometimes have a sessile avicularium in front, below the aperture. This is particularly the case with *Canda arachnoidea*, in which the avicularia appear to occupy an unusual position: they do not seem to be seated on the fronts of the cells themselves, but to form a series affixed to the median septum between the two rows of cells; and, what is very curious with regard to them in this species, they are apparently developed long after the completion of the cells, seeing that they are totally wanting in the upper and younger portions of the branches of the polyzoary, and gradually increase in size towards the inferior portion. In *Scrupocellaria*, we arrive at the full development of these accessory organs; the species in this genus being all distinguished by their being furnished with both *avicularia* and *vibracula*. Of the former, one is always imbedded in the superior and outer angle, as in *Scrupocellaria scrupea*, *S. scruposa*, and *S. Macandrei;* whilst in others, such as *S. ferox*, and *S. cervicornis*, there are superadded to these, sessile avicularia on the front of the cell below the aperture, in the former species, of colossal dimensions. The genus *Cellularia*, again, is distinguished by the entire absence of both *avicularia* and *vibracula*. The British form *Cellularia Peachii*, affords an instance.

The genus *Caberea*, except in the single species above referred to, and which might, perhaps, on that account, almost be regarded as the type of a separate genus, is distinguished, and very remarkably so, by the extraordinary size and curious arrangement of the vibracula on the back of the branches, which thence derive a very close resemblance to an ear of barley. It is in this genus that the vibracular organ acquires its extreme development. A British example is afforded in *Caberea Boryi*, a not uncommon denizen of the Channel coasts. In the genus *Emma*, we have afforded an instance, in which the *position* of the organ may be used in the generic character. In the species belonging to this genus, the sessile, lateral avicularium is situated on a level *below* the aperture. *E. g.*, *Emma crystallina* and *E. tricellata*.

The peculiar disposition and form of the avicularia, in *Notamia*, have been sufficiently adverted to in this and my previous communication to the Society.

It would require too much of your time and too much space to enter very particularly into all the instances in which I have found the form, position, and existence of avicularia and vibracular processes useful in the classification of species. The above remarks, hasty as they are, will serve to give an idea of the extent to which they may be so employed; and I would only observe, in addition, that a similar attention to these organs will be found indispensably requisite for the due appreciation of specific and even generic distinctions, in the difficult and hitherto much confused families of the *Flustradæ*, *Membraniporidæ*, and especially of the *Celleporidæ*, *Escharadæ*, and *Selenariadæ*. In *Lepralia*, particularly, in which genus I have placed nearly 60 species, I have found the use of these organs of the utmost importance, and easily available. In fact without them it would have been a most difficult task to marshal into due order such an irregular and mutinous host. For the mode in which I have so employed this character, I must refer you to my 'Catalogue of Marine Polyzoa,' just published by the British Museum, and will conclude by saying, that the names of the Polyzoa here employed are those by which they are distinguished in that work, in which the appropriate synonyms will be found.

On the Minute Structure *of a peculiar* Combustible Mineral, *from the Coal Measures of* Torbane-hill, *near* Bathgate, Linlithgowshire, *known in Commerce as* Boghead Cannel Coal. By John Quekett, Professor of Histology to the Royal College of Surgeons of England. (Read Nov. 23 and Dec. 21, 1853.)

The substance in question has lately excited the greatest interest in the scientific world; and a trial, second to few in importance, has recently taken place in Edinburgh, having for its object the determination whether the Torbane-hill mineral should be called a coal or not, and whether it should be included in the missive of agreement for a lease, and let as coal. Those of my hearers who may wish for a particular account of the matter in dispute, and a statement of the facts brought forward, both by the pursuer and defender, or, with us in England, plaintiff and defendant, I would beg to refer to Mr. A. W. Lyell's Report of the trial, published by Messrs. Bell and Bradfute of Edinburgh.

Upon this trial no less than seventy-eight witnesses were examined—thirty-three for the plaintiff, and forty-five for the defendant. They might be classified as geologists, mineralogists, chemists, microscopists, and practical engineers, such as gas managers, miners, &c.

With four of these classes of scientific witnesses I have no immediate concern, and will, therefore, leave them to settle their own differences; but not so with the microscopists, with many of whom my opinions are entirely at variance.

In order that you may all fairly understand the nature of this question, as far as the microscopical observers are concerned, I will, in the first place, give you a detailed account of the minute structure of the Mineral itself. Secondly, I will give a brief description of the minute structure of Coal. Thirdly, I will lay before you the whole of the evidence given by the microscopists on the part of the pursuer as well as the defender; and lastly, make a few remarks upon the conflicting testimony of some of the witnesses.

I wish that the matter had fallen into abler hands than mine; but having been intimately acquainted with the mineral in dispute for some time past, and as two of the oldest members of this society, Mr. Bowerbank and myself, have had their competency called in question, and have been represented by the Judge as no botanists, and, therefore, "are not, as I understand, conversant or skilful in fossil plants," and the society itself not having escaped his ridicule, the jury being informed that the Microscopical Society of London is "a learned body, who make it their object to pry into all things," I cannot be silent; but I would have you keep in mind that my sole motive in now appearing before you, is, the cause of truth, and in this cause I come forward fearlessly, but honestly, to state *that the Torbane-hill mineral is not, microscopically speaking, a Coal; that it is not like any of the combustible substances used in this country as Coal; and that, although possessing some of the properties of Coal, it is, notwithstanding, a mineral* sui generis, *having a basis of clay which is strongly impregnated with a peculiar combustible principle, and that when plants are found in it, they are accidental, and have no more been concerned in the formation of the mineral than has a fossil bone in that of the rock in which it may be imbedded.*

1. *External characters of the Mineral.*—Of these you will have a good general idea from the specimens on the table before you. It frequently occurs in seams of some considerable thickness, and always in the neighbourhood of coal, sometimes in immediate contiguity with it, but at other times, according to Mr. Ansted, separated from it by a layer of fire-clay. The colour is generally a dark brown or black, without lustre, but varies according to its position in the seam; its specific gravity is $1\frac{2}{10}$ or $1\frac{3}{10}$, water being as 1. When scratched with a knife it exhibits a brown streak, in which particular it is said to differ from all the known coals with one or two exceptions. It is tough and not so brittle, but that very thin sections may be made of it, and when struck with a hammer, it emits a dull sound; the remains of plants, especially *Stigmariæ*, are of constant occurrence, and can be distinguished by the naked eye without difficulty.

2. *Characters exhibited under the Microscope.*—When a small chipping or fragment, about half an inch square, is examined as an opaque object under a power of 40 or 50 diameters, it will be found to consist of masses of a yellow material, some being of irregular figure, others more or less rounded, imbedded in a granular matrix, varying in colour from a yellowish-brown, almost to black. The whole of the mineral appears to be composed of granules of various sizes, and although the part which has been termed the matrix is black, this also will become brown if the surface be scraped. The scraping can readily be done under the microscope whilst the fragment is being inspected; and, curiously enough, both the surface of the mineral, and the minute particles scraped off, assume a light-brown colour. Portions of plants imbedded in the mineral can, by the process of scraping, be readily distinguished from the impressions of plants: the former are

always black and do not alter in colour, whereas the latter become brown, the same as other parts of the mineral.

3. *Characters exhibited by sections under the Microscope.*—There appear to be two principal varieties of this mineral, one of a yellowish-brown colour, the other nearly black, these differences, however, are chiefly dependent upon the position the particular fragment selected for section occupied in the block. When the first variety is reduced sufficiently thin to be transparent, which can be done without much difficulty, it will be seen to consist of a tolerably uniform, yellow mass, whilst the darker variety is either of a rich brown, or of a pale-yellow colour, minutely spotted with black granules. When the first or yellow variety is examined with a power of 50 or 100 diameters, it exhibits an appearance of being made up of a mass of transparent rounded particles or spherules of a rich yellow or amber colour, varying in size from the $\frac{1}{4000}$th to the $\frac{1}{200}$th of an inch (as shown in Plate III., fig. 1), whilst the darker variety (fig. 2) is composed of two essential elements, one in the form of the transparent rounded particles, the other minutely granular, but black and opaque, and occupying the spaces between the yellow particles. In the first variety of this mineral, or that which is of a yellowish-brown colour in section, the yellow particles above alluded to are so very abundant, that they appear almost to make up the entire mass, whilst the dark granular element is small in quantity. In the second, or dark variety, the strictly granular opaque element is much more abundant; it sometimes occurs in large patches, having none of the yellow particles with it, but more frequently it is found in the form of a coating to the particles themselves. When the yellow particles are of large size, they always exhibit more or less of a radiated structure internally: this appearance, which is well represented in figs. 1 and 2, very much resembles that of a radiated fracture, or a species of crystallization. I shall now, for the sake of distinction, call all these yellow particles, or spherules, the bitumenoid or combustible portion of the substance, and the dark, granular part, I shall consider as the strictly mineral, or earthy ingredient.

In some specimens there is a tolerable regularity in the size of the yellow particles, and in the disposition of the black mineral ingredient around them, so much so that an unpractised eye might, at first sight, consider its structure to be cellular: that such mistakes have actually been made you will very soon have an opportunity of learning.

Having told you what is the usual structure of the substance in question, I must beg you to understand that it matters little in what direction the sections are taken; whether cut vertically, horizontally, or obliquely, there is no perceptible difference in the structure, and I say it without fear of contradiction, that no one, however skilled in microscopical observation could, from the inspection of a single specimen, state the direction in which the section had been made. Such is not the case with coal, as will hereafter be shown; a single inspection is sufficient to enable a practised microscopist to determine the actual direction of the section, whether transverse or longitudinal.

Examination of portions of the Mineral having Plants imbedded in their substance.—I have already stated that plants and the impressions of plants are not uncommon in this mineral; of these I have made numerous sections and chippings, and most instructive they all are. The plants appear to be principally *Stigmariæ*, and exhibit more or less of the three tissues known to botanists as the cellular, the woody, and the vascular; and should one or more of these be present in any section, the minutest fragment even of a cell or vessel can be readily recognised by a practised observer; they, as it were, stand out boldly from the mineral matter in which they are imbedded, and (as shown in figs. 3 and 4) can be distinguished in all cases by their rich brown colour; but such plants I not only consider as extraneous and not forming the bulk of the mineral, but such plants my investigations lead me to conclude rarely if ever form coal; at all events no coal that I have yet examined has ever exhibited the least trace of being made up of such plants as are so commonly seen imbedded in this mineral. Even the coal lying upon this mineral, and running through it in every possible direction, is composed principally of woody tissue, and not of plants such as these.

Examination of sections of the Mineral having Coal in juxtaposition.—The first specimen of this kind which I had the opportunity of examining was brought by Mr. Bowerbank himself from one of the Torbane-hill pits. From this specimen several sections were taken; one of them slightly magnified, is represented in Plate V., fig. 1. I regret I cannot show you the specimen itself, it being lodged in the Court of Session, in Edinburgh; but I have been favoured with a somewhat similar one through the kindness of Mr. Gratton. As the block lay in the pit, the coal was situated below the mineral in the position I now hold it, and you will readily be able to distinguish the one from the other by the naked eye; but when viewed with a power of at least 50 diameters (as shown in Plate III., fig. 5), the smallest fragment of the coal that may happen to be mixed up with the mineral may be readily traced; even a part so minute as a single woody fibre can easily be recognised.

In some specimens, the line of demarcation between the coal and the mineral is not very decided, owing to the coal and the plants found in connexion with it being so intimately blended; in all such cases recourse should be had to the streak, as the best guide to distinguish them. In every part of the block containing coal and coal plants, the streak is black; but in the smallest portions of the mineral it is brown. It is a curious fact, however, that in the specimen now before you, three kinds of structure are visible to the naked eye: 1st, true coal; 2nd, a mixture of coal with a few coal plants, principally *Stigmariæ*; 3rd, the mineral. When sections are made through the block in two directions at right angles to each other, the coal and the mixture of coal and plants will exhibit a structure corresponding with longitudinal and transverse sections of wood, but the mineral is the same in both sections. The yellow particles occupy all the interstices in the coal, and vary in shape, according to the spaces they have to fill (as shown in fig. 5); but whether they be elongated or of circular figure, more or less of the radiated structure is present in every particle. In such sections the vegetable tissues may be distinguished from the earthy ingredient by their rich brown colour.

Examination of the Powder.—When the Torbane-hill mineral is reduced to powder, and examined either in water or in Canada balsam, the combustible and incombustible portions can be well seen; the one occurring in the form of the yellow or amber coloured particles before noticed, and constituting full two-thirds of the mass (as shown in Plate III., fig. 6), whilst the remainder is made up of minute opaque granules, having occasionally amongst them some which are quite transparent, and probably siliceous.

Characters of the so-called Coke and of the Ash.—Three portions of the coke of the Torbane-hill mineral, each about 4 inches square, obtained from a gas-retort by Mr. Gratton, were of a greyish colour, and when scraped became perfectly black. The remains of plants were very visible throughout the substance of each, and were even more distinctly seen in the specimens of coke than in the mineral itself before being subjected to heat, for every part, however minute, had assumed a silvery appearance. When a flat piece of the coke, about half an inch square, is examined as an opaque object under a power of 50 or 100 diameters, it presents a peculiar sponge-like structure; and when contrasted with a portion of the mineral, it will be noticed that all the yellow particles have disappeared, and a pitted appearance is produced, the pits being nothing more than the cavities in which the yellow particles were lodged, and the walls of the pits being the granular earthy ingredient which at one time surrounded the yellow particles. When small fragments of the coke are scraped off and subjected to a power of 250 diameters, none of the yellow combustible principle is present, the entire bulk being made up of dark granular masses. If the mineral be burnt in an open fire, the ash will be nearly white; and when examined microscopically, no trace of the yellow combustible matter will be seen, and the granules (as shown in fig. 7) will be very minute, and of a light colour. These appearances will be constant, if care be taken to select a part of the mineral in which no traces of plants are visible to the naked eye; but if portions of plants be present, they will be readily recognised by their woody and vascular tissues. The principal distinction, therefore, between the coke of the gas-works and the ash is, that in the former the granules are larger and blacker than they are in the latter.

From these and numerous other observations, I conclude that the mineral in question is a clayey substance, impregnated with a combustible material occurring in the form of rounded particles of a rich yellow or amber colour, but whether these particles be bituminous or not the chemists must decide.

What I have already stated refers exclusively to the Torbane-hill mineral, and no mention has yet been made of the structure of coal. Under this head I could enter into a detailed account of most of the well-known varieties of British coal, my knowledge of which has been principally derived from a careful investigation of sections made by myself and by my friend Dr. James Adams, of Glasgow; and I am happy in having this opportunity of bearing testimony to the correctness of the observations of Dr. Adams, upon which his opinions had been formed prior to my having the pleasure of his acquaintance. Were I now to describe these, I fear you would be kept here many hours; but it is the intention of Dr. Adams and myself, at no very distant period, to read a paper on the minute structure of the principal kinds of British coal, before the Geological Society, as we deem *that* the most fitting place for such a subject. For our present purpose, therefore, it will be merely necessary for me to give, in as concise a manner as possible, the results of the investigations of Dr. Adams and myself on this point; but I would have you understand that although I give you a general description of the structure of coal, I have with me the specimens from which you will be enabled to judge for yourselves whether my statements be correct. I am fully aware that the prevalent opinion with geologists and botanists is, that coal is made up of fossilized vegetable matter, and that this vegetable matter may consist of stigmariæ, ferns, mosses, &c.; in short, of a great variety of vegetable substances. My investigations, however, lead me to believe that the basis of coal is essentially a peculiar kind of wood, and that when ferns, stigmariæ, lepidodendra, and other plants occur in coal or its neighbourhood, they should be considered foreign to the coal, as these plants, before noticed, are to the Torbane-hill mineral. However contrary this may be to our preconceived notions, yet all the sections on the table before you, on a careful examination by an unprejudiced observer, can lead to no other conclusion. I believe that there are in this room at the present time more sections of coal than any private individual has ever yet produced before a scientific assembly, and it is from these specimens, and from the study of these alone, that I am warranted in making this assertion. The botanist will remember that most of the plants generally considered as forming coal, are such as on section will exhibit more or less of the cellular, woody, and vascular tissue: now it is a remarkable fact, that most of the plants visible to the naked eye in the Torbane-hill mineral, as well as those lying in the strata above and below coal in general, are those which may contain spiral or other vessels; but, judging from all the sections of coal now before you, as well as chippings of others too numerous to mention, I am forced to the conclusion that such plants rarely if ever form coal, the basis of coal being essentially *wood*, of what kind, however, I will not at the present stage of the inquiry venture to mention, but I will state thus far, that it approaches more nearly to that of the Coniferæ than any other wood; because in the Coniferæ, as we know them in this country, there are few if any vessels or ducts in the woody part of the trunk, but occasionally cellular tissue in what are called the turpentine vessels, the entire bulk being woody fibre. Such is the case in coal. In all the sections that I have examined of undoubted coal, I have as yet found no trace of a spiral vessel or a dotted duct, but in one or two instances where the woody structure has been very evident, as shown in Plate V., fig. 3, the fibres were evidently dotted.

External Appearances of Coal.—These must be so well known to most of you, that I need not dwell further upon them than to particularise one or two kinds which approach nearest to the Torbane-hill mineral in general appearance. The most remarkable of these is from Methil, in Fifeshire, and known as the *Brown Methil.* So peculiar is it, that when scratched with a knife, the streak is brownish-black in colour, somewhat resembling that of the mineral. There is also another variety of coal, termed the *Black Methil*, but in this the streak is black, as in all other coals. Yet the microscopic characters of both these varieties are very similar, and differ in no respect from coals generally. A curious fact, however, I learnt from the chemists in Edinburgh, that the composition of the Brown Methil came nearer to that of the Torbane-hill mineral than any of the other known coals did; a fact which is borne out by the similarity in their external appearance.

Examination of Coal by the Microscope.—If a small cubical block of any kind of coal be examined under a power of 50 diameters, four of its six sides will exhibit more or less of a fibrous structure, precisely like that of wood; the other two sides, if perfectly flat, will appear bright and polished, and show very little structure: these correspond to the transverse sections of wood. Treat the Torbane-hill mineral in the same way, and how very different are the results! Nearly the same structure will be found on all its sides, but in none is there the least trace of a fibrous arrangement.

Examination of Sections of Coal by the Microscope.—If a section of any well-known coal, cannel or otherwise, be reduced sufficiently thin to be transparent, a work sometimes of considerable labour and difficulty, it will be found to exhibit one of two structures, according to the direction in which the section has been made. These, for the sake of distinction, may be called the cellular and the fibrous; the first corresponding with a horizontal section, the second with a vertical section, of wood. If it so happen that a section taken at random from any specimen of coal should exhibit one of these structures above named, by cutting at right angles, the other will be found. Thus, for instance, if the first section should correspond to a horizontal section of wood, the cut at right angles to it will correspond with the vertical one; and, of course, if the section be an *oblique* one, an intermediate structure would be observed. This remarkable fact is constant in all the coals I have examined, and a knowledge of it enables the observer to tell at once whether any section taken at random was a horizontal or a vertical one. How strangely different this from the Torbane-hill mineral! Cut that mineral in any way you please, and there will be little or no difference in appearance. The structure of the transverse sections of coal is so very peculiar and so characteristic, that I must briefly point out the means it affords of distinguishing coal from any other modification of vegetable tissue. The peculiarity consists in this,—that, in the midst of a black opaque ground, numerous brown transparent rings, each having a black dot in the centre, are interspersed; they appear like transverse sections of thick-walled cells or of woody fibres. In some coals they occur in close proximity to each other, as in woods generally: in other cases they are more or less separated, either by the black material before alluded to, or by a network of rather smaller rings, in which the central dot is absent. There are many coals, especially some of the common domestic kinds, in which it is difficult to recognise this structure in every part of the section; in these coals a rich brown structureless material—bituminous or not I cannot say—seems to be in excess, and so obscures the characteristic appearances of the rings. In longitudinal sections the woody fibres are generally well seen, and a tendency to split in the direction of their length (as shown in Plate V., fig. 5), may always be observed. Amongst the fibres may be noticed certain elongated cells, of a rich brown colour, having a dark line running down through the centre: these are constant in all coals, and when divided transversely, appear as the rings before noticed. Their size is tolerably uniform in many coals (as shown in Plate V., fig. 2). Mr. Witham was acquainted with the differences between a longitudinal and a transverse section of coal, as may be seen on referring to the 2nd edition of his work on the "Fossil Vegetables of the Carboniferous and Oolitic Deposits;" both the rings and the elongated cells are well figured, and his remarks on the value of investigating the microscopic structure of coal, are very excellent. The absence of vascular tissue in the numerous sections of coal, made both by Dr. Adams and myself, would lead to the supposition that the wood of which it is composed must approach very near to that of the Coniferæ.

Examination of the Powder of Coal.—When coal is reduced to a fine powder, and examined either in water or in Canada balsam, it will be found to consist principally of short opaque cylinders or fibres, occurring singly or in bundles, and of angular dark-brown plates of various sizes, probably composed of bituminous matter (as shown in Plate V., fig. 7); the remainder of the mass is made up of minute transparent particles of silica, with an occasional mixture of fragments of cells and fibres. Many blocks of coal have a fine dull black powder on two of their outer surfaces, which will make the fingers very black: this I call the charcoal layer, and in it will be found fragments of woody tissue of cells, and even of vessels. My investigations lead me to believe that this layer is derived from plants which existed at the same time as the coal-wood, but were not capable of being converted into true coal, but having been subjected to a great heat, their remains are left as a species of charcoal. Some specimens of the Torbane-hill coal have a large amount of this charcoal upon their upper and under surfaces, and in it, vessels of various kinds will occasionally be found, although such vessels do not occur in the solid coal itself.

Examination of the Ash of Coal.—The brown ash of coal, with the exception of particles, probably of silica, is almost wholly composed of vegetable remains, some of which properly belong to the coal itself, whilst others are derived from extraneous plants which have been mixed up with it. Every kind of tissue which has been described as proper to the coal may be met with in the ash, when not too much burnt. The remains of woody fibres and cells are the most common constituents, but flat, very opaque, irregular masses, such as are shown in Plate V., fig. 4, and which evidently correspond to portions of transverse sections of wood, are frequently found. Portions of siliceous cuticle, probably of grasses, as shown in fig. 6, from a drawing by Dr. Adams, are far from being uncommon. In short, when the indications of the woody structure of coal are very faint in sections, they are well exemplified in the ash. Sections of Welsh *Anthracite* (which I believe to be a fossil coke) are most difficult to obtain, and when made, afford very unsatisfactory evidence of vegetable structure: when, however, the ash is examined, the presence of woody tissue is unquestionable.

The Torbane-hill mineral has been most carefully examined by my friend Dr. Adams, and as his investigations were carried on independently of mine, it will be satisfactory that you should be made acquainted with the conclusions he has arrived at after a laborious series of examinations. They are as follow:—

"The most interesting example which could be adduced, illustrative of the differences in essential characters, as demonstrated by the microscope, between substances supposed by commercial men to be identical, is found in the Torbanehill mineral, known also by the name of Boghead coal. In the lawsuit previously alluded to, much of the scientific evidence regarding this mineral was of a very conflicting character, so much so that the court virtually set aside the scientific evidence, and decided on the legal merits of the commercial bargain.

"The importance of the interests involved, and the high character of the witnesses examined, have made this trial very celebrated; and it is from an excusable desire that the grounds of the opinion I expressed at the trial should be understood, that I now seek to place them on record. I will, however, confine my remarks to a very short summary of my observations made upon the Torbanehill mineral, leaving a fuller detail with my friend, Professor Quekett, who gave joint evidence with me, and with whom I have discussed and investigated the whole subject of my present communication with a most pleasing and perfect accordance of observation and opinion.

"The following are the principal results :—

"I. A very thin section of the Torbanehill mineral, when viewed by transmitted light, has a pale-yellow colour, is semi-transparent, and, with the exception of very slight variations in the depth of the colour, probably dependent on the varying thickness of the section, it appears to be a uniform homogeneous mass. *The same appearance is constantly presented notwithstanding that the sections are taken in various directions.* While this is the usual appearance of what may be termed the average specimens, viz., of portions taken from the centre of the block (or seam), yet, in sections taken from near the outside, or lower portion of the seam, I find a quantity of small opaque particles (evidently earthy matter) in the form of a fine powder, scattered through the yellow-coloured medium forming the mineral. In such specimens the transparent yellow substance forms irregular rounded granules, and the opaque powder is either sparingly diffused over, or forms an outline or partition, more or less perfect, around the exterior of the yellow granules. These granules vary much in size, being as small as 1-4000th of an inch, and of every intermediate size from that up to 1-200th of an inch in diameter.

"In sections taken from the outside, as above described, I have observed occasional patches of opaque material of every irregular form, and which I could not liken to any other substance, unless I spoke of them simply as specks of dirt. In the same sections I have also found stalks of plants and fragments of wood. These opaque patches and vegetable fragments are always *distinctly* isolated ; that is, they do not in any way resemble or form part of the substance of the mineral, otherwise than by being involved or contained in it, and their presence, therefore, can only be considered accidental.

"II. When reduced to a fine powder, and examined under water, all the particles of the mineral have a clear, and generally a sharp outline, are of an irregularly rounded form, and may be described as of a uniform granular appearance. About 7-10ths of the granules are very translucent, and of a light amber or yellow colour. About 2-10ths of the particles (also translucent) partake more of a flat, angular shape, and are quite colourless, probably consisting of siliceous matter. The remainder of the powder consists of dark semi-opaque particles.

"In specimens of powder taken from an outside portion of the mineral, there is observed a larger proportion of the semi-opaque particles, together with the occasional appearance of vegetable stalks, rough fibrous fragments, and delicate fibrils of microscopic plants. With these special exceptions, the powder gives no trace whatever of organic structure.

"III. The ash of the mineral, when examined under water, presents a considerable quantity of the colourless particles already described, lying at the bottom of the fluid, while a filmy particle of transparent particles floats on the surface. No trace whatever of organic structure is here observed.

"Polarised light does not in any way affect the appearances of the mineral.

I have, in consequence of these investigations, a firm conviction of the non-identity of the Torbanehill mineral with coal, setting aside those differences which may be found to exist under mineralogical, geological, or chemical investigation. I cannot conceive how the evidence of *Amorphisin* in the one case, and of intimate vegetable composition and of regular structure in the other, can be explained away, or any other view than that of non-identity of physical structure. In coal we find a well-characterized organization, or regular arrangement of its component parts so distinctly peculiar, that I should question the competency, at least, of any party who, after comparing the microscopic appearances of the two substances in question, could hint at a resemblance. The Torbanehill mineral, on the other hand, is as thoroughly devoid of organic structure, or of any regular arrangement of its component parts, as is a mass of jelly or a conglomerate of masons' mortar."

I will now, in the third place, proceed to read the evidence given by the witnesses for the pursuer and the defender.

Professor QUEKETT.—*Examined by* Mr. MACFARLANE.

You are one of the Professors in the Royal College of Surgeons in London?—Yes.

What chair do you occupy?—The chair of Histology.

What is the object of that study?—An examination of the minute tissues or structure of plants and animals.

I believe you have devoted a great deal of study and attention to that subject?—Yes, for the last twenty years.

You have published a catalogue of the preparations in the College of Surgeons of London, descriptive of the various tissues?—Yes.

And you have yourself a very extensive collection?—Yes, I believe the largest in Europe.

You conduct your investigations with the aid of the microscope?—Yes.

And have you made careful investigation into the structure of the various coals, as well as other minerals?—Yes.

Have you in this way had occasion to examine the most of the known coals in England and Wales?—Yes, about seventy varieties.

Have you also examined varieties of Scotch coal?—Yes.

What have you discovered to be the tissue of coals?—They show us a woody tissue.

Have you found structure of that description in all the varieties to which you have referred?—All the varieties of coal.

More or less distinct, I suppose?—Yes.

Now, have you examined the Torbanehill mineral?—Yes, in every possible way microscopically.

Were specimens of the mineral delivered to you?—Yes, some time ago.

By whom?—By Mr. William Forbes and a Mr. Rettie. I have the specimens here.

Now, did you subject those specimens to a very careful examination?—Yes, very careful.

You tried them in every possible way, and repeatedly?—Yes, and repeatedly.

Did you make a great many sections out of them?—Yes, an immense number.

So as to give you every possible opportunity in tracing their structure?—Yes.

What result did you come to?—That the Torbanehill mineral is different from anything that I ever saw in my life before.

Did you discover any trace of organic structure?—Yes, when plants are accidentally mixed with it.

You were enabled to ascertain when it was so?—Yes.

Perfectly?—Perfectly.

But in the substance itself?—No structure—that is, what the microscopists would term an organic structure.

Is it different in that respect from all the varieties of coal you have examined?—Decidedly so.

Did you get illustrations?—Yes, I have illustrations. (Produces same.) I think you will come to a better understanding of the thing from those illustrations than from the specimens.

Explain what that is (referring to illustration shown to the jury).—This is a section of the Torbanehill mineral, or rather a granular section, and in it you will observe *some yellow matter* that burns—whether bituminous or resinous you must go to the chemists for. The black part is the strictly mineral part.

What is the mineral matter to which you refer?—It is the dark granular matter.

Lord-President.—I understand that these illustrations show the bitumen and the mineral at different places?—Yes, my Lord.

Mr. Macfarlane.—Now, then, do the illustrations of your coal investigations exhibit a different appearance?—Decidedly.

Now, you say the mineral substance there is granular, is it so in the coal?—Not at all, except when visible to the naked eye. In coal, you can see mineral structure by the naked eye, but to that I do not allude; but under the microscope you can tell that is a totally distinct thing from the coal itself. What I mean is, that in specimens of coal you can often see crystallized matter with the naked eye.

Is that extraneous?—I would say so.

But when subjected to the microscope?—It exhibits a totally different structure. It is not granular; it depends entirely on which way the specimen of coal is cut. If cut in one direction you will either see a cellular or fibrous appearance.

Indicative of what?—Woody tissue.

You have, I suppose, made sections in all the specimens of the Torbanehill mineral—in all the various ways you have made sections of the coal?—Yes.

And you have found in all the different sections a decided difference, showing them in your mind to be different substances?—Certainly.

Then, judging from all your experience and investigation, do you consider this Torbanehill mineral to be a description of coal or not?—Certainly not.

Have you any illustrations of coal there?—Yes, I have a most remarkable illustration—perhaps the jury will understand better by this than anything else. I have here a section of the mineral and coal in juxtaposition; this has been cut by Mr. Bryson, lapidary, and you will be enabled to see whether coal or mineral. The woody section is shown by the dark colour, the mineral by the other.

Are those illustrations of longitudinal, or transverse sections, or what sections?—That of coal is longitudinal, and of the mineral it is supposed to be the same, because they are in juxtaposition.

Suppose a transverse section—what difference?—In the Torbanehill mineral a section at right angles would present precisely the same character, but the coal would present another character, that character being shown in this lower drawing (exhibiting it to the jury). You will notice that the coal runs through that mineral. You can trace it by its minute tissue.

You examined some of the Scotch varieties of coal?—Yes, many varieties.

Did you examine the Methil?—Yes, of two kinds, I believe known by the names of the brown and the black.

Did you discover vegetable structure there?—Yes.

Decidedly in both?—Decidedly in both.

And in that respect different from this Torbanehill mineral?—Certainly.

You mentioned at one time that you had observed the presence, in some of those coal specimens, of fossil plants?—Certainly.

Could you see them with the naked eye?—Yes, in those specimens of the Torbanehill.

Have you seen them in coal in the same way?—Yes, but I consider them extraneous or isolated examples.

Cross-examined by Mr. Neaves.—Is the structure of coal uniform in general?—It is so far uniform that the various transverse sections are uniform, and so are longitudinal.

Equally visible in all places of the coal?—Yes, in all places, except, as I have stated before, where you have mineral that is foreign to the coal.

What mineral matter do you allude to?—The chemists must decide that.

You only speak to appearances?—Yes.

And the same formation in all?—Yes; the plants differ; I believe there are two kinds of plants or tissue that essentially form coal.

But they present the same appearance?—Yes, but those plants are not traceable in the same specimens of coal. That in the neighbourhood of Glasgow may be different from the coal found in the neighbourhood of Edinburgh.

Can you distinguish the one plant from the other?—Yes, in the longitudinal section.

And you never found any portion of any coal without exhibiting the same permanent structure?—Certainly not.

Where did you get that specimen you showed us of the two coals together?—That was taken by Mr. Bowerbank from the mine two or three days ago, and the drawing was taken from a magnified representation of one of the sections.

Lord President.—Let us take down what those specimens are if they are to go in, but I thought they were to be taken away by the witness.

Dean of Faculty.—No. 25 represents that yellow matter of which the witness spoke; No. 26 is the drawing of that highly-magnified section; Nos. 28 and 29 are the specimen and the drawing; and No. 27 is the appearance presented by the two different sections of coal itself, the one longitudinal and the other transverse.

Witness.—There is one thing I would wish to state, this—I came here to speak the truth, and it may be testimony for or against my evidence, when I say that all that which may be supposed like vegetable structure in the Torbanehill mineral disappears when the structure is thin.

Dean of Faculty.—When you speak of that which appears as vegetable structure, you mean those isolated fossil plants?—Yes. I would also allude to the fact that a book was published in this city twenty years ago, by Mr. Witham, of specimens made by Mr. Nicol; and this was the first representation of this vegetable structure.

Dr. James Adams.—*Examined by* Mr. Macfarlane.

You practise as a medical man in Glasgow?—I do.

Have you devoted a good deal of time and study to observations by the microscope?—I have.

For a considerable time back?—For many years.

Have you subjected to examination by the microscope various minerals? —I have.

Extensively?—Extensively.

Varieties of Scotch coal?—Yes, a great many.

Most of the known varieties?—Most of the known varieties.

Have you examined the Torbanehill mineral?—I have.

Recently?—Recently.

And did you subject it to a very careful investigation?—Very careful.

In various forms?—Yes.

Now, will you tell me what those Nos. of process are, No. 259 to 263, both inclusive?—259 represents sections of various specimens of the Torbanehill mineral, as seen under the microscope.

From the centre of the same, from the outside or bottom, and also from the outside of block?—Yes.

What is the next No.?—260, representing two sections of coal, termed to me cannel coal—Duke of Hamilton's cannel coal; the one represents what I have termed a longitudinal section, and the other a transverse section, drawn by myself.

The next No.?—Is 261. This represents a drawing of what was termed to me Lesmahagow, Ferguson's cannel coal—two sections drawn from specimens made by myself; but the drawing made by an artist named Donald, of Glasgow, under my eye.

And you have no doubt they are correctly done?—No doubt; very faithfully made.

The next No.?—262, representing sections of—1st, what is termed Jordanhill cannel coal. The one is longitudinal of Jordanhill, the other is a transverse section of a coal called Cowdenhead, given to me. This one, 263, which represents three drawings—two transverse and one longitudinal; a transverse section of Jordanhill cannel coal, drawn by a medical gentleman of the name of Risk, under my eye, a faithful delineation; the other is a drawing of cannel coal procured from the Glasgow Gas Works, called Knightwood coal; and there is also a longitudinal drawing of Knightwood. Those three drawings were made under my eye by Mr. Risk.

Did you subject the powder of the Torbanehill mineral to the microscope?—I did.

Having applied a little water?—Yes.

What did you discover to be the particles?—Those particles have a clear granular shape, they are of an irregular rounded form, and I say may be described as exhibiting an uniform granular appearance.

Any further description?—About $\frac{7}{10}$ of those granules are very translucent, and of a light-amber colour. About $\frac{2}{10}$, also translucent, partake more of a flat or angular shape in their outline, and are quite colourless; and there are a few particles of a dark or semi-opaque matter.

Now have you examined coal specimens in the same way?—I have.

What were the results?—They differed very materially; the particles of cannel coal which I took as being the more compact coal, are found of various sizes, and in form generally flat, angular, or oblong, with fibrous character; the edges generally rough and as darkly opaque as in the centre.

Have you examined the ash of the Torbanehill mineral?—I have.

When you said that the coal particles were of different sizes, were the particles of Torbanehill mineral of various or the same size?—When I examined them under a high power I found the Torbanehill to be also of various sizes.

You examined them with the aid of a microscope carefully?—Yes.

What results?—I found it very difficult to describe the appearance, because it seemed to consist of a film or congeries of structureless particles. I got nothing tangible almost to lay hold of. I consider most of those consisted of the colourless particles which I have mentioned as having been found in the powder, viz., the flat, angular, and perfectly transparent particles.

I understand, Doctor, when you say perfectly structureless, that there was no organization ?—No organization ; they have form.

No trace of vegetable origin ?—None.

Nor the coal ash ?—In the coal ash examined under water, I found abundant remains of vegetable structure, examined in the same way.

Woody tissues in the coal ?—Yes.

Did you conduct your investigation of the ash of the Torbanehill mineral and of the coal both in direct and transmitted lights ?—By both.

And with the results which you have described ?—Yes.

Were they the best, most approved instruments ?—They were. I have used various instruments of all kinds, but I have used the best and most recent construction.

What were those ?—Those were prepared by two of the most eminent London opticians, Mr. Ross and the firm of Smith and Beck.

What conclusion do you arrive at in regard to this Torbanehill mineral, keeping in view your investigation of the sections, of the powder, and of the ash ?—That the two substances are totally dissimilar.

That the Torbanehill is a different substance from any coal with which you are acquainted ?—Yes.

Cross-examined by Mr. Neaves.—Are you in practice in Glasgow as a physician ?—I am.

Have you marked the magnifying power of the instruments used on those specimens ?—I have.

When did you first see this mineral ?—I think on 15th January last year.

Had you never seen it before ?—Never to my knowledge.

You had previously been in the habit of examining coals ?—I had.

And had seen all the cannel coals ?—Not then. I have since examined them.

What coal had you seen when in the practice of examining before ?—Chiefly domestic coal.

For many years ?—For several years.

With any particular view ?—None.

The body and ash of domestic coal ?—Yes.

You always see the woody structure in the ash ?—Always ; I have never failed.

And in the coal ?—Do you mean the sections ?

Yes.—I have never met with a piece of coal that had not those appearances.

Do you give it a name ?—I call it a fibrous section, from appearing like a bundle of fibres in one direction. I give it longitudinally, because it gives me the idea of length, and annular, that is, composed of rings, when seen in a cross cut with a longitudinal.

But are equally distinct in the same coal always ?—Not equally distinct.

Not equally distinct in all coals nor in the same coal ?—No, but remain always distinct in every coal.

Re-examined by Mr. Macfarlane.—Have you been at Torbanehill ?—Yes.

And made specimens ?—Yes.

Did you examine from those specimens ?—Yes.

Fair or average specimens of the mineral ?—I took them just as they were raised from the pit, and examined them from the centre, outside, and every way I could possibly conceive.

Your observations have been more recently directed to cannel coal ?—Yes.

Can you give me the names ?—I believe I have examined about forty or fifty different specimens, as far as I know, but I can give the names of different coals that I tested.

Just give us a few ?—These were Capeldrae, Wemyss, and Pirniehill, &c.

Your investigations had been previously chiefly directed to the ordinary coals ?—Yes.

Is it more difficult to trace the organic structure in the cannel coal than in the ordinary domestic coal ?—It is.

Perhaps requires more skill and practice ?—Yes, in conducting the investigation into the cannel coal.

What is the reason of that ?—The reason I believe to be, that the structure is much more compact in the cannel coal, and the section requires to be made exceedingly thin, and it is very difficult to procure that condition, from the excessive brittleness of the material, and also intense opacity, and containing particles of hard matter, which frequently tear out the specimens.

Mr. Bowerbank.—*Examined by* Mr. Macfarlane.

Mr. Bowerbank, you live in London ?—I do.

You have given a good deal of your time and attention to microscopical observations ?—I have for these twenty-five years past.

You are a fellow of the Royal Society ?—I am.

You were lately president of the Microscopical Society of London ?—I was.

And you have written on the subject, I believe ?—I have.

Have you made a great many examinations, with the aid of the microscope, of mineral substances ?—I have.

Of various descriptions of coal ?—I have. For many years, the subject, simply as a natural-history subject, was much inquired into.

And you have turned your attention to it ?—I have.

And have for several years been taking observations, with the microscope, of coal substances ?—Yes.

Have you been at Torbanehill ?—I have.

Recently ?—Yes, recently.

And you obtained specimens of the mineral that is working there ?—I did.

And subjected them to examination ?—I have.

Did you give a specimen last week to Professor Quekett ?—I showed him a specimen, and he desired to possess it for examination.

And did you give some specimens to Dr. Adams ?—I did.

What has been the result of your examination of coal substances ?—Every coal which I have examined, either by sections, or by external characters, or by the examination of the ash, has convinced me that it is an essential character of coal that it should be composed principally of organized vegetable substances and bitumen.

Lord President.—Of what, did you say ?—Of organized vegetable carbon and bitumen principally.

Mr. Macfarlane.—With a little earthy matter ?—Yes.

I think you said these examinations were of the sections of the substance, and of the ash as well ?—Of the sections of the coal matter, and of the ash as well. The practice generally adopted in examination is, first to observe its ordinary characters, and next its sections, so as to develop its structure.

Have you pursued the same mode of investigation in regard to the Torbanehill mineral ?—Exactly.

And with what result ?—I have found no organic structure in it, although I have examined it by powers varying from 40 or 50, up to very nearly 700 linear. I have also examined the ash with great care; and I may say that as to almost every specimen that has passed through my hands identified, and others as well, in no case have I found any indications of vegetable structure in the ash.

Then the results of your examination of the coal, and of this mineral, are very different?—Quite opposite.

I suppose, Mr. Bowerbank, you have used the best instruments?—Yes, Sir, I believe there are no better to be procured. Indeed, unless they were instruments of a high optical character, they would not develop the minutest portions of the tissue satisfactorily.

Who are the great London makers?—Ross, Powell and Smith, and Bett (or Beck).

You have examined, I suppose, different varieties of shales, have you? —To a very considerable extent.

Any traces of organic structure in them?—Not in the body of the shale itself, but a great intermixture of isolated plants. In fact, in coal shales isolated plants form a considerable portion of them.

We have had the word 'amorphous' used frequently, Mr. Bowerbank. Can you explain its meaning?—I understand an amorphous mass of that description to be a mass without crystallization—a mass which would cleave in any direction without any determinate arrangement. For instance, I would say a sandstone, although formed of granulated masses, is still an amorphous mass, as there is no determinate arrangement.

Where there is organic structure, the word amorphous would not, of course, apply?—Not to the structure itself, but it may apply to the medium in which that structure is imbedded.

Cross-examined by Mr. Neaves.—Where did you get your specimens? —Some from Torbanehill pits, which I visited within the last week.

And adjoining properties?—And some from the adjoining properties as well.

What property was that?—Bathgate pit, and another pit. I also received verified specimens sent from the country to request an examination of them.

You first saw the mineral there?—I first saw the mineral at Queenwood College, some time ago.

Some months ago?—About three months ago.

Re-examined by Mr. Macfarlane.—Among other coals have you examined various cannel coals?—Frequently.

And the statements you have made have had reference to them as well as to others?—The specimens which I have examined of the cannel coals, vary very considerably in character from this new mineral from Torbanehill.

You discovered the vegetable origin of the structure in them?—Oh, yes.

This closes the evidence of the microscopists on the pursuer's side. I will now proceed to read that given on the side of the defender.

Professor J. H. Balfour.—*Examined by* Mr. Neaves.

You are Professor of Botany in the University of Edinburgh?—Yes.

And I understand that you have devoted attention not only to the subject of botany as concerns existing plants, but also to fossil botany?—Yes.

Is that a part of the course that you teach?—Yes.

In the course of teaching that class, are you in the habit of examining mineral substances with a view to noticing their structure?—I examine fossil plants. I have a large collection of specimens of fossil plants.

Have you been in this case shown some specimens of different minerals with a view of examining them?—Yes.

What were they?—I have seen specimens of the Torbanehill coal, the Methil coal, the Capeldrae coal, the Lesmahagow coal, and several other parrot and other common coals.

Did you visit the ground at Torbanehill?—Yes, I went to the pits and examined the coal, and brought specimens from the place.

Did you visit the Methil pit?—Yes.

And got some specimens from Methil?—Yes, out of the pit.

And where did you get the other specimens that you refer to?—I got them from various sources. Some were sent me authenticated by Mr. Russel, some were given me by Dr. Maclagan, also by Dr. Redfern, Dr. Aitken, and Professor Harkness.

Did you make sections of these minerals with a view to a microscopical investigation of them?—Yes.

Did you make such a variety of sections as to enable you to judge in all directions?—Yes, so as to judge fully of the structure.

Now, from that examination, are you able to say whether you discovered in these specimens traces of organic structure?—Certainly organic structure.

In all the specimens?—In all the specimens more or less.

Now, in the Torbanehill mineral did you find marks of organic structure?—Certainly.

And in the Methil?—And in the Methil.

Was there any difference, or any resemblance, between the appearance of the Torbanehill mineral and the Methil mineral?—A remarkable similarity.

Was there some Lesmahagow coal?—Yes.

And some Capeldrae also?—Yes.

And I think some Kinneil coal?—Some Kinneil.

Which is a cannel also?—Yes.

Did you take the assistance of Dr. Greville?—I took his assistance in delineating what we saw under the microscope.

Did you see his delineations?—Yes.

Did they appear to you to be successful?—Most correct, I think.

You believe coal generally to be a vegetable formation, I suppose?—Certainly.

Of what species of plants is it generally supposed to be composed?—The coal plants are numerous. We have, in the first place, a mass of ferns, stigmarias, sigillarias, lepidodendrons, calamites, and various other genera.

The ferns supposed to form coal-beds are very gigantic ferns compared with the present ferns?—They are tree ferns.

Is it a cryptogamic plant?—Yes.

In such plants, what is the particular appearance or structure you would expect to find?—In all these plants, as well as in other plants of a woody stem, we have cells and vessels; but in the tree ferns we have a structure which may be said to be pretty regular, which is called scalariform, or ladder-like, from the bars visible upon it. They are vessels or tubes.

Did you see in the Torbanehill coal appearances that seemed to you to indicate cellular structure?—Certainly.

No doubt of that?—No doubt of that.

And also some appearances indicative of scalariform structure?—Yes.

The cellular appearances more generally diffused than the other ?—Yes, much more generally.

Do you consider you have in that way evidence of the vegetable composition of the Torbanehill mineral ?—Yes, certainly.

And of the same character generally as the other cannel coals that you examined ?—Precisely.

[Here several drawings were handed to the witness, and he was asked to explain them.

In the first drawing, which was of the Torbanehill mineral, witness stated the sections showed the vegetable structure, and also the scalariform vessels, with the bars upon it, very distinctly.]

Is that the kind of structure that is seen in modern tree ferns ?—Yes.

The next drawing exhibits three sections,—the Lesmahagow, the Capeldrae, and the Torbanehill coal,—showing precisely similar structure. They are a little different in colour, but the same in structure. There are also sections of the Torbanehill and Methil in the drawings, showing the same appearance and structure in both these. Another drawing of the separate individual shales shows distinctly the appearance of separate cells, both in the Torbanehill coal, in the Lesmahagow coal, and in the Capeldrae coal. And, in fact, we find these in various other coals.

The cell is the base of the organic structure of these vegetables ?—Yes.

It is the accumulation of cell upon cell that builds up the structure ?—Yes.

Judging microscopically, then, and also with your knowledge of fossil botany, would you draw the inference that the Torbanehill was of the same, or of a different class of substances from the other cannel coals that you have mentioned ?—The same class as of the cannel coals I have seen.

The only difference, I understand you to say, is the difference in the tinge of colour ?—Yes, and that occurs in many coals.

You don't think that essential in deciding the question ?—I do not.

Cross-examined by the Dean of Faculty.—These observations are made upon a thin section ?—Yes.

Who made the sections ?—They were made by Professor Harkness, Dr. Aitken, Dr. Redfern, and Mr. Glen.

Would you mark upon each the name of the gentleman who did them ? —Yes, to the best of my recollection.

[Here witness marked each section as requested.]

Have you yourself been accustomed to make such sections ?—I have made sections for the microscope.

Have you much practice with the microscope ?—Yes, it is part of my course.

In reference to existing plants ?—Yes, and also to fossil plants. I have a large collection of fossil sections.

With regard to this drawing here [holding up one of those previously described by witness], that represents the impression of an individual fossil plant ?—That represents only a portion of a plant, the vascular part of the vascular tissue of a plant, approaching nearly to the scalariform tissue.

Do you mean that the tissue is there, or the impression on the plant ?—The tissue is there.

In this other portion of the seam, then, which is coloured brown, you do not observe any structure ?—I did not examine particularly.

But does this represent what you saw on that occasion ?—Yes.

Then there is no appearance of structure there ?—I cannot say.

There is no structure represented there ?—No.

All that you found in this particular section is the representation of part of a fossil plant ?—Yes.

Part of an individual plant apparently ?—Part of an individual plant probably.

Do you know from what portion of this seam of Torbanehill mineral this slice representing the upper drawing is taken ?—I do not know the portion of the seam.

Do you know the portion of the seam from which any of them were taken ?—I have only seen the specimens. They seem to be the ordinary appearance of the Torbanehill mineral, and quite the usual appearance of the coal, so far as I saw.

Here the Dean of Faculty took up another drawing, and asked witness if he saw anything similar to that ?—I saw appearances similar to that.

Have you represented them ?—Represented them so far in some of these sections, only the dark colour between makes a difference in the appearances.

Lord President.—Is that in the Torbanehill mineral ?—Yes.

Dean of Faculty.—Did you see anything like that [showing witness another drawing, No. 25] ?—Something approaching to this. It wants, in some respects, the regularity of the structure I have seen in the other.

Shown No. 26, another drawing, and asked if he had seen anything like that ?—This also approaches to what I observed, but wants the definiteness and regularity of the structure I saw.

Did you see anything like that [showing No. 28, another drawing] ?—Yes, the yellow part is more like what we saw in the general structure.

What power did you use in making these observations ?—They are marked in diameters ; two of them were 200, and the other 70.

Have you ever examined shales in this way ?—I have looked at one or two shales. It is not so much in my way as plants.

Do you find marks of fossil plants in them ?—Yes, they occur ; but the structure is different in them. They have not the same marked definite form I have seen in the others.

I understand that in these you represent both the transverse and the parallel sections ?—Yes, we have taken them in two directions.

Which are the transverse ?—The three upper are the longitudinal, and the lower the transverse or horizontal.

What do you mean by horizontal ?—By horizontal we mean cutting off the ends of the vessels.

That is to say, you learned that from the gentlemen who made them ?—I have examined sections.

You did not see the sections made ?—No.

Then, of course, you could only get the information from those gentlemen who made them ?—Yes.

Are the three upper cut along the stratum, as it were, off the top of the stratum as it lies ?—I am talking of them as regards the appearances we see in the microscope. Judging from ordinary structure, in the one case we cut the ends of the vessels ; in the other, we cut along the line longitudinally.

Lord President.—The three upper are cut along the line of the vessels, and the three others are cut across the line of the vessels.

Dean of Faculty.—Do I understand you to say that you were told they were cut in this way, and that that is the ground of your saying so ; or do you form your opinion by the appearance they present ?—I was of course told so ; and on looking at them, I should say they are so cut.

Then it is from both these reasons that you say so ?—Yes.

Did you examine any part of the ashes of this mineral with the microscope ?—No.

Did you ever examine the ashes of coal with the microscope ?—No.

Did you use direct or transmitted light in these examinations?—I used generally transmitted light, but I also viewed some specimens by direct light.

Re-examined by Mr. Neaves.—There are several drawings here. Did you examine a great many more cuttings than these drawings?—A great number.

How many more, do you know?—I cannot tell the number of the sections of Torbanehill; at all events, some eight or ten, besides sections of other coal.

And then made a drawing of these?—Yes, as being average specimens.

Did you see some of these sections made?—Yes, these were the sections made under my direction by Mr. Glen.

The Methil section?—I cannot say I saw it made in the sense that I saw the whole process gone through, but it was done for me, by my direction, from a piece of Methil coal.

Lord President.—Did you see Mr. Glen make some of the sections?—I should rather say that the sections I allude to were made under my direction, and were authenticated by me at the time.

Mr. Neaves.—In the other sections of the Torbanehiil mineral which you have examined besides this, did you find the same appearances?—The same appearances.

I forget what you said as to this yellow part of No. 28?—I considered that to be a cellular structure.

The yellow part included?—Yes.

This cellular tissue is a magnified appearance of the separate individual cells?—Yes.

With the view of showing that they were at larger power?—These are cells which occur in these coals, and they are separated the one from the other. We took magnified drawings of them.

Occurring at Boghead?—Yes, and on the others.

And besides showing those things, you formed an opinion of what they were?—Yes.

That they were the indications of vegetable cellular structure?—Certainly.

Lord President.—That is, the appearances in the mineral seams?—Yes.

Mr. Neaves.—Including the Torbanehill?—Yes.

And of that yellow part of the representation of the Torbanehill mineral?—I believe it to represent vegetable cells.

In these plants I suppose the structure is but imperfectly understood?—I may say we do not know it so completely as we know all the plants of the present day.

The cells may be longer or shorter?—Yes.

They vary in their form?—Yes.

And that may affect the longitudinal appearance of the cells?—Yes.

I do not understand you to say that this is the mere impression of a foreign fossil, but the actual structure of the mineral at that place?—Certainly.

Dean of Faculty.—The individual plant is there lying in the mineral?—The structure of the plant—not the entire plant.

A part of a fossil plant is seen there?—Yes.

Mr. Neaves.—Forming a part of the coal?—Yes.

Dean of Faculty.—I understand, Dr. Balfour, that there is a part of the fossil plant here lying imbedded in something or other?—It is a quite dissimilar part as regards the appearance.

The plant must be there in order to give it that appearance?—It must be the structure appearing so distinctly as to be seen there.

Very well; a plant is lying here upon another thing, which is here represented by a dull-brown colour ?—Yes, a part of the plant.

Mr. Neaves.—What did you say ?—That that is part of the structure of a plant which is lying there in the mineral. When you make a section of the mineral you come upon this, showing you that there was a plant.

At that part the mineral consists of that plant ?—Yes.

Dean of Faculty.—You have seen fossil plants in stone quarries ?—Yes.

Mr. Neaves.—You do not consider that an example of such an appearance ?—No.

Dr. Redfern.—*Examined by* Mr. Neaves.

Dr. Redfern, you lecture on subjects connected with the microscope in connexion with the University ?—Yes; and teach the use of the microscope.

You are a Fellow of the College of Surgeons of London ?—Yes.

Have you been accustomed to the examination of substances by the microscope ?—Yes.

Principally of vegetable substances for some years ?—I have for many years been in the practice of examining vegetable structure by the microscope.

Both in recent vegetables and in fossil substances ?—I have.

Did you lately receive some specimens of different minerals, including some of the Torbanehill mineral ?—I did.

From whom did you get the Torbanehill mineral ?—I got some specimens from Dr. Fyfe, and some others from the Aberdeen Gas Works, in the presence of Mr. Leslie, the manager.

Did you subject these specimens of the Torbanehill mineral to microscopical examination ?—I did so.

How many sections of it did you take ?—Eighteen.

From the same piece, or from different pieces ?—From eight different pieces.

Did you or did you not find vegetable structure in these sections ?—I found vegetable structure in every section.

Have you examined different cannel coals with the same view ?—I have.

What cannel coals ?—I have examined Lesmahagow cannel coal, Capeldrae cannel coal, Wigan cannel coal, Methil cannel coal, and Halbeath parrot coal; and also the Kinneil coal from Bo'ness.

In what way would you speak of the examination of these minerals, and of the examination of the Torbanehill mineral, in reference to the vegetable structure ?—I am quite convinced, that in the sections of these different coals there are parts which cannot be distinguished from each other.

Vegetable structure in all ?—In all.

And in some parts this mineral undistinguishable from the others ?—Certainly.

The Boghead mineral has considerable varieties of aspect in itself ?—It has.

Different shades of colour ?—There are black, brown, and spotted pieces—black pieces with brown spots.

In the lightish-colour portions of the Boghead mineral, what is that you saw ?—I saw vegetable cells in these portions.

The structure that you saw is cellular structure ?—Yes.

Besides the cells that you saw, what else did you notice ?—I noticed also woody fibre, or woody tissue.

Are there some yellow spots in this light-coloured portion of the mineral?—There are.

What do you think these yellow spots indicate?—They indicate the existence of vegetable cells.

Have you applied any test to endeavour to find out whether they were vegetable or not?—I have, Sir; I have many reasons for concluding that they are vegetable cells.

Would you mention your reasons?—I find that they can be perfectly isolated—they project upon the edges of all sections of the mineral—they are rounded—they are as uniform in size as the cells of other vegetable structures—the general appearance of the section is that of a piece of vegetable cellular tissue—the yellow spots do not act upon polarised light, or act upon it very feebly.

Generally speaking, do you consider that the Torbane-hill mineral exhibits the same appearances of structure and position microscopically as the other cannel minerals?—It does.

Did you see Dr. Greville's drawings?—I not only saw the drawings, but I saw him make them.

You had long previously examined the minerals?—I had; long and carefully.

Do these drawings appear to you to represent the general character of the mineral?—They do.

And you believe these drawings to represent cellular tissue?—I do.

Your sections were taken at random from the general specimens that you had?—Certainly.

As fair specimens that you thought the mineral would exhibit?—That was my chief object in obtaining them from the Aberdeen Gas Works. I took the specimens for as fair average specimens of the Torbanehill mineral as I could obtain.

And they would have supplied similar representations as those Dr. Greville has given, in your opinion?—I am satisfied of that.

Cross-examined by the Dean of Faculty.—You say Dr. Greville's drawings represent the same thing that you saw?—They do.

Did you examine the ash of this coal?—Yes.

With the microscope?—Yes. I consider the examination of the ash as liable to great sources of fallacy, and place no dependence upon it.

Your reasons?—I should not look upon the ash to make out the structure it contains.

That is not your reason, but a repetition of your opinion. What is your reason?—Because I would expect the greater portion of vegetable structure, if it existed, to be destroyed by the process of combustion.

Did you ever examine the ash of ordinary coal with the microscope?—I have not.

Dr. R. K. Greville.—*Examined by* Mr. Neaves.

Dr. Greville, I believe you have devoted a good deal of your attention to the study of botany?—Yes, it has been the principal study of my whole life.

And in connexion with that to the use of the microscope?—I may say, without exaggeration, that for many years I have used the microscope almost every day.

Among other branches of the vegetable kingdom, you have studied and written upon the cryptogamic family, which includes the ferns?—Yes.

And which requires particular use of the microscope in order to illustrate its fructification?—Yes. I may add that I have made the drawings of everything I have published from my own microscopical investigations.

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I made drawings of the outline and structure of two or three hundred ferns alone.

Were you asked to assist some gentlemen using the microscope to represent the appearance of some sections of minerals?—Yes.

These are the drawings you made?—Yes.

Did you yourself look at various sections of the minerals besides those that you have represented?—I did, especially with regard to the Boghead mineral. I examined under the microscope eighteen different slices made from eight different specimens of the substance.

Were these Dr. Redfern's specimens?—Yes.

Did you discover vegetable structure in these?—Unquestionably, in the whole of them.

Did you examine some other minerals—some cannel coals that this gentleman had?—I examined all those coals of which the names are appended to the drawings. There is the Methil, Lesmahagow, and Capeldrae coals.

Now these are correct representations, to the best of your ability, of what they present?—They are; they might be more minutely finished, but they give, I hope, a fair representation of the structure.

Did it appear to you, from your examination of these different things, that they were the general structure of the mass, or any incidental structure?—I have no hesitation in saying that it was the general structure of every specimen, not incidental. I should consider it to be quite impossible it could be incidental.

Do you consider that there is a material difference or a substantial identity between these different bodies, as represented in these different minerals?—I do not. I examined the specimens of the three uppermost sketches, and the structure was so similar, that I considered them to be identical. There is a difference, but nothing amounting to anything essential in the structure. The Lesmahagow, Capeldrae, and Torbanehill are essentially the same. I may be allowed to add, that in each slice there is a difference in every part of that slice, so that you must be guided by the general view.

From your botanical knowledge, have you any doubt that these representations exhibit vegetable cells?—I have no more doubt of that than of my own existence at this moment.

Will you explain what that paper is?—[handing witness one of the drawings spoken to by Professor Balfour]—That drawing represents vegetable cells in an isolated state, scattered throughout the substance, and observable, I believe, in most coals—certainly in most coals that I have examined. It is difficult to say what they may be, but I have no doubt that they are vegetable cells, solitary cells. They may possibly be transverse segments of cells, but I would not venture to say anything more than that. I believe them to be vegetable cells.

Found in this mineral?—We have found these vegetable cells in the Boghead as well as in others.

Will you explain what these two drawings represent?—[handing witness two of the drawings spoken to by Professor Balfour]—The uppermost one represents cellular tissue in the Torbanehill mineral; and, upon the whole, I consider that as one of the most satisfactory specimens which I examined; the cellular tissue is so unequivocally marked, and so regular, that it may be compared to that of a recent plant. It is exceedingly well defined. What I have represented in the drawing is not in the least exaggerated. No person accustomed to botanical sections would hesitate in believing that to be cellular tissue. The lower drawing represents a beautiful specimen, but whether that is general in the mineral I could not say. It represents a modification of the vascular structure of plants called technically the scalariform structure. I can compare it best by comparing it with an old basket. It is an unequivocal vegetable structure.

What occurs in its neighbourhood in the rest of the section?—This was the whole that I saw. The other portion was not ground so thin, and I could not see what it consisted of; but judging from the traces of these vessels at the extreme edges, I have no reason whatever to doubt, that if the remainder of the section had been ground sufficiently thin, we would have seen the continuation of that structure.

But the other cells that you described here are diffused through the entire mass of the substance?—In all the specimens I examined it was uniform throughout the whole. It was exceedingly well marked in the one that represents the transverse section of the cells.

You get the width of the cells more distinctly when you cut the transverse section?—You get the area more distinctly shown.

Cross-examined by the Dean of Faculty.—Can you explain to me what are infusoria?—Infusoria represent minute animals invisible to the naked eye—visible only to the microscope.

Where do you find them?—It is very difficult to say where you do not find them. Generally they are sought for in fluids.

You find them in minerals also?—I am not prepared to answer that question. I am not sufficiently acquainted with the subject to venture to answer it.

Then you cannot tell me what appearance they present when found in minerals when examined under the microscope?—No, I am not aware of their occurring.

Professor HARKNESS.—*Examined by* Mr. YOUNG.

Professor Harkness, you are Professor of Geology in Queen's College, Cork?—Yes.

You succeeded Dr. Nicol?—About six months ago.

You have devoted considerable attention to the study of geology?—I have.

And also to the examination of objects by the microscope?—Yes, so far as relates to fossil plants.

You have visited Torbanehill?—I have.

You went down one of the pits?—I was down two of them.

And examined the mineral as it lay in the earth?—Yes.

And made yourself acquainted with its geological composition?—I found it to occur in the proper coal measures.

Exactly in the position you would expect to find coal?—Decidedly so.

You found nothing whatever in its geological composition to lead you for a moment to doubt that it was coal?—Nothing; on the contrary, everything to induce me to believe that it was coal.

Did you form any opinion upon the mineral itself?—I formed the opinion, that from the appearance of the mineral it was a coal.

Did you take some specimens of the mineral away?—Yes, I did, for the purpose of making a more careful examination.

And after that examination you retained your opinion?—I did.

And your opinion now is that it is a coal?—Decidedly so, without any manner of doubt.

Did you make some sections of the mineral which you took away with the view of microscopic examination?—So far as regarded fossil plants.

Did you find the structure familiar?—I found the structure peculiar, and the fossils characteristic of the coal formation.

How many structures are there in coal and coal plant?—There are two or three distinguishing characteristics, first the woody fibre, the scalariform tissue, and the cellular tissue.

Is this upon the examination of a great many sections?—Yes. That was generally, not mere accidental structure of particular pieces.

You saw a drawing made by Dr. Greville?—I was present when that drawing was made.

And that gave a sufficiently distinct idea of the course of examination? —Yes.

Of the Torbanehill and some other coals?—Yes; and the Lesmahagow, Kinneil, Capeldrae, and some other cannels.

I believe the drawing was made from a section furnished by you?—That is a most beautiful specimen of cellular tissue.

This is the most beautiful specimen you have seen of woody fibre?—I distinguish woody fibre from cellular on account of the more regular formation of the cells.

You have no doubt that this is a vegetable product?—Not the least.

[Witness was shown the drawings illustrative of cellular tissue and woody fibre, and distinguished each with great precision.]

You know what shales are?—Yes.

Do shales ever exhibit vegetable structure?—As shales they do not.

How would you describe a shale?—There are several forms of shales. Supposing the coal to be so mixed with earthy matter as to be incapable of being used for fuel, then that would be called a coaly shale.

And when the coaly matter is so great in proportion to the earthy matter that it will burn?—I should consider this a coal.

And more or less pure according to the admixture of earthy matter?—All coals contain more or less of earthy matter, and accordingly the coals run into shales as the earthy matter increases.

When you come to a substance beyond which a substance will not burn, you would call it a coaly shale?—Yes.

It is very difficult to draw the line at the exact place?—Very difficult.

Has this mineral anything of the character of a shale?—Not the least, so far as I have been able to detect.

You have seen specimens of Methil coal, and examined them with the microscope?—Yes.

And did you find anything to distinguish the Boghead mineral?—So far as external appearance went, I could scarcely distinguish the one from the other, and there was also a great similarity in internal structure.

There are a variety of cannels which approach each other very closely? —In regard to the distinction between the two there is not a more common one than this, the capability of burning and being used for the purposes of fuel.

If the substance would burn, and could be used as fuel, you would say it was a coal?—Yes, I would.

If any substance is sold in the market as a coal, is it a coal?—Yes, I should think so.

There is no science against this?—None that I am aware of.

Cross-examined by the Dean of Faculty.—I suppose whatever comes out of the coal measures and burns by itself is coal?—No; I would not say that. You might get a fragment of bitumen, which would not be coal, and that burns by itself.

Is that the only exception?—I am not prepared to say that there are any other exceptions.

Fragments of bitumen would be an exception?—Yes.

The way by which you distinguish a coal from a shale, or a shale from a coal, I understand is, that the one will burn, and that the other will not?—The one will burn without the mixture of any extraneous matter.

It will burn by itself?—Yes.

There are other distinctions; but this is the distinction upon which you rested?—Yes.

You were going to tell us that there were a number of kinds of shales. Tell me some of these?—There are some which are absolutely devoid of coaly matter—clay shales, which have no coal in them at all.

Any other distinction?—Yes; there are shales which I should characterize as bituminous shales.

How do they differ from coaly shales?—They differ inasmuch as they give a bituminous smell when struck by the hammer; and they yield bitumen to chemical solvents.

Do they burn?—Yes, they burn in some cases.

Where do you find most bituminous shales?—You find them in Cambridge and in Dorsetshire, in the higher beds of the oolite.

Do you find the Methil coal to be of a laminated and slaty structure?—I found some fragments that were laminated; but others present the conchoidal structure that you have in the Boghead, and is compost.

The Boghead is compost?—It is.

Is the Methil coal so?—It is generally so.

But portions are slaty and laminated?—Yes.

Will you explain what infusoria are?—I have not given any opinion as concerning infusoria.

But you can give one?—They are minute microscopic animals.

Where are they found?—I generally find them in water.

Are they not to be found in minerals?—I have not found them in minerals.

But are they not to be found in minerals?—They are found in certain mineral beds, but I have not found them in mineral beds.

Dr. William Aitken.—*Examined by* Mr. Penney.

You made some sections of the Torbanehill mineral, and of some other coals?—Yes.

Were they for your own examination, or some that Dr. Greville drew?—I did some, and also for my own.

You got the returns from Torbanehill?—I did.

From the pit mouth?—Yes.

You made the sections fairly for the purpose of testing?—Yes.

Mr. Neaves then stated that they would not require to examine Mr. Glen, as his sections were also admitted.

Having now read to you the evidence given by the microscopists on both sides of the question, I cannot refrain from making a few remarks on some of the statements of the defender's witnesses. The subject to me is a painful one, for it is always with feelings of regret that I venture to differ in opinion from any scientific observer; but, however contrary to my inclination, I have a public duty to perform, to say nothing of the character I have to sustain amongst you as a member of this society. I sincerely hope, however, that those gentlemen will take it all in good part, and believe that it is only for the reasons above assigned, and not from any public or private feeling of opposition to their opinions that I appear before you this night.

I will not dwell long upon the subject, as it must be very clear to you all—first, that the specimens examined by these gentlemen must have had more or less of plant structure imbedded in them; secondly, that they have evidently mistaken the peculiar arrangement of the combustible and earthy portions of the mineral for vegetable cellular tissue. Thirdly, they can certainly never have examined sections of many coals microscopically, as one and all tell you that they saw the same structure in the mineral as they did in coals. Had they made sections of coal in two directions, at right angles to each other, they could hardly have failed in seeing, almost at a glance, how much the sections differed in structure the one from the other. That such is really the case, even in the coals which they state in their evidence they have examined, may be shown by reference to Plate IV. In fig. 1 is represented a transverse section of the so-called brown methil; and in fig. 2, a longitudinal section of the same. The two structures are so different in appearance, that, had such sections been made, I feel confident there could not have been a second opinion on the subject. In fig. 3 is shown a transverse section of the black methil, and in fig. 4 a longitudinal section. The differences, if anything, are even more striking than in the brown methil. But what will be said of figs. 5 and 6, which represent a transverse and longitudinal section of Lesmahagow cannel coal? That anything at all resembling such a structure as this, can be found in sections of the mineral in question, except when coal is present, I emphatically deny.

Now, granting for a moment that the structure of the mineral be cellular, what plants, I would ask, could the cells have belonged to? Can any botanist produce a single instance of a recent or fossil plant of the same thickness as a seam of the Torbanehill mineral, which shall be made up of a mass of cellular tissue, that is, without vessels or woody fibres being present with the cells?

Again, if the structure be cellular, we should expect to find the most durable part of the cell—the cell wall—always present, which is not the case. If this view be correct, the yellow particles being solid must be the contents of cells, they certainly cannot be cells. The cell-wall also, as far as we know it, in recent and fossil plants, always presents on section a more or less uniform thickness and a homogeneous appearance; whereas the structure around the yellow particles in all cases, except where plants are present, is minutely granular, being in reality the clayey or earthy ingredient of the mineral.

None of the defender's witnesses, it appears, ever examined the ash of coal; and one witness in particular, Dr. Redfern, stated that the examination of "ash in general was liable to great sources of fallacy, and placed no dependence upon it;" whereas, it subsequently appeared that he had never examined the ash of ordinary coal with the microscope.

Were I disposed to be hypercritical, I could mention many other points in the evidence that I entirely dissent from; but I trust I have already said enough, and will therefore sum up my remarks by stating that I consider the mineral in question is not a coal, being structurally different from all undoubted coals, including those with which it appears it has been compared by the microscopists engaged by the defender. In order, therefore, that the scientific world in general may have an opportunity of judging for themselves whether this statement be correct or not, I have put specimens of the mineral and of these coals into the hands of the preparers of microscopic objects, and in a short time sections will be on sale by them and by the principal opticians in this metropolis.

I might by some persons be accused of unfairness in making even these few remarks upon the evidence of the witnesses for the defence, when they are all located in different parts of Great Britain, and therefore not able to be present this evening to answer for themselves. I wish, however, that they could have been here, and more especially if they could have brought with them the sections upon which their opinions were formed, and the drawings which were produced in court. They might say, perhaps, that it would not be fair play to send their specimens, their drawings, and their remarks into an enemy's camp; on my own part, however, I can venture to state that I am ready to appear before any tribunal of scientific men in this kingdom, and my drawings and specimens shall be open to all who may be interested in the subject, to examine for themselves. I beg it may be expressly understood, that should there be any one point in this paper which on subsequent investigation may turn out to be incorrect, I shall be as ready to come forward and acknowledge myself in error as I now am to express an opinion not hastily formed: my only object, as I said before, is truth; and by truth I will abide.

There is one other point that I would briefly allude to before drawing my remarks to a conclusion, and this is a portion of the Lord President's address to the Jury, in which, as before stated, Mr. Bowerbank and myself are placed in no very enviable position; it is as follows:—

"Besides those gentlemen who were examined as geologists and chemists, and who differ so widely, there was examined another class of men, and possessed of great attainments—I refer to the microscopists. One of them was the late President of the Microscopic Society of London—a learned body, who make it their object to pry into all things. Three of these gentlemen were examined for the pursuer, and four for the defender. The pursuer's witnesses told you that there was no trace of organic structure, no woody fibre or tissue, in short, no trace of vegetable matter in this substance, although occasionally there might be the incidental presence of vegetable remains. The witnesses of this class on the other side told you, on the contrary, that in every part of it there was the most clear vestiges of vegetable structure. I do not know, when I have so many geologists and so many microscopists telling me that it is not coal, and so many on the other side telling me the opposite, I say I do not know that I feel myself much the wiser, or further advanced in the inquiry. But if you have, in addition, a great number of chemists, and speaking with equal authority and equal contrariety, it is difficult to know what to make of the controversy. I do not know that I have anything to say against the skill of the microscopists, or the skill of any of those gentlemen; but one general remark may be made on the microscopic testimony, and it is, that there are those who see a thing, and also those who do not see it—those who do see it, cannot see it unless it is there, and those who cannot see it do not see it at all. But very skilful persons looking for a thing and not seeing it, creates a strong presumption that it is not there. But when other persons do find it, it goes far to displace the notion that it is not there. But there is another observation on the microscopic evidence that occurred to me. I do not know whether I am under any misapprehension, but I think that three, certainly two, of those examined by the defenders, are botanists also; and I do not think that any of those examined for the pursuer, two of them from London, represented themselves as botanists. Now, the defender's witnesses are accustomed to look for plants, and can understand them when they see them. The gentlemen on the other side again, looking for woody fibre or tissue, are not, as I understand, conversant or skilful in fossil plants. But finding such a difference of opinion, and such opposite conclusions arrived at by those persons, I do not know, unless you think that some gave their reasons more satisfactorily than others—I say I do not know that I feel my mind much relieved from the difficulties of this case by listening to all that evidence. It is very interesting no doubt, and if they were all standing on one side, and nobody standing on the other side, it might be very satisfactory to one's mind to listen to such evidence."

To such remarks I would briefly reply that, however severe a counsel may be in his cross-examination, and however strong his language in addressing the jury may be, I think it to a certain extent excusable, as he is endeavouring to do the best for his client; but I must confess my great surprise that a learned judge should see fit to single out one set of scientific witnesses from the pursuer's side, and hold them up, I would say, almost to ridicule; that he did so on the present occasion, the part of the address which I have just read to you will show. I think it will eventually turn out that the two members of the Microscopical Society of London, "that learned body who make it their object to pry into all things," *are* accustomed to look for plants, and *can* understand them when they see them; nay, I will assert that they can do more, for *they* can tell when a particular structure is not a plant. Had his Lordship been silent on the point, he would not have laid himself open to these truly justifiable remarks.

I would now, gentlemen, in conclusion, leave the matter in your hands. I think that the subject in question is one of the most important ever brought before the notice of this Society, and one which no set of men in this or any other country are so competent to investigate. Most of the members of this society are, as stated in the certificate for suspension, "attached to scientific pursuits," and most of them are in possession of the best instruments, and are accustomed to use them; let them, therefore, study the subject for themselves, and give independent testimony. Where, I might ask, can be found a correct definition of coal? I believe, at present, no such definition is extant, and it is on this account that I look upon the trial of Gillespie *versus* Russel, as one of the greatest importance to the geologist, the chemist, the mineralogist, and the microscopist; and I am of opinion that from it will spring, not only a perfect definition of coal, but of other combustible substances found in connexion with it, and, therefore, it is to be hoped that such contradictory statements as were made by the different scientific witnesses on the trial in question may in future be avoided. It remains, then, for the microscope, "that most valuable of all scientific instruments (to quote the words of Mr. Ross) ever yet bestowed by art upon the investigator of nature," to assist in deciding the true structure of coal, as it has already done that of many other organic substances of a previously-doubtful nature.

TABULAR VIEW of the PRINCIPAL POINTS of DIFFERENCE between the TORBANE-HILL MINERAL and COAL.

—	TORBANE-HILL MINERAL.	COAL.
1. External characters . . .	Colour, brown or black, tough, streak brown, without lustre.	Colour black, brittle, streak black and lustrous; in the Brown Methil, dark brown and lustrous.
2. Characters exhibited by a cubical fragment, viewed as an opaque object, under a power of 50 diameters.	Granular, those granules or particles being of light-brown colour, which are detached from the general mass: any part, however black, will become brown, either by scraping or by raising it from the mass.	Four of its six sides, in certain lights, will exhibit a fibrous appearance, like a longitudinal section of wood.
3. Characters exhibited by sections under the microscope.	Particles of various sizes, of circular figure, and amber colour, in some cases surrounded by a minutely-granular basis. When the particles are large, a radiated structure is present in the centre of each; no difference in structure whichever way the section is made. If plants, or portions of plants be present, they may be easily recognised by their rich brown colour, as well as by the form of their tissues.	Transverse and longitudinal sections totally different, but both agreeing with corresponding sections of wood. The presence of the so-called amber-coloured ring in the former, and the rich brown elongated cells and the fibres in the latter, are characteristic of all the coals which have been examined. In some coal, as the Brown Methil, a few yellow bitumenoid particles are scattered irregularly through the mass, but in most cases the bitumenoid portion is of a rich brown colour.
4. Characters of the powder .	Composed principally of the yellow particles, with occasional mixture of granules of earthy matter, some opaque, others quite transparent, and probably siliceous.	Composed principally of fibres or bundles of fibres; these are occasionally mixed with flattened angular plates of a rich brown colour; earthy matter sometimes present.
5. Characters of the coke and ash.	Coke, light and spongy, dull sound when struck, presenting a series of pits or cavities, from which the yellow particles have been removed by burning; the ash consists entirely of dark granules of earthy matter, and of angular pieces, probably of silica.	Coke emits a metallic sound, and presents more or less of a fibrous appearance, and the ash always shows traces of vegetable structure.

Some Observations on the DIATOMACEÆ *of the Thames.* By F. C. S. ROPER, F.G.S. (Read January 25th, 1854.)

IN the year 1843 M. Ehrenberg, read before the Academy of Sciences of Berlin a paper on the microscopical marine infusoria of the deposits of the Elbe,* in which he established the remarkable fact that at Glückstadt, a distance of forty miles, and even above Hamburg, upwards of eighty miles, from the mouth of the river, marine siliceous-shelled *Infusoria* were found alive, and their skeletons deposited in such abundance in the mud of the river, that at the former locality they form one quarter to one-third of the entire mass, and that the proportion is still about half that amount at Hamburg, as far as the flood-tide extends. All his observations gave a great predominance of marine over fresh-water species, even when the salt taste of the water was no longer perceptible.

In the lists which accompany this paper, M. Ehrenberg enumerates thirty-four marine species, under the style of siliceous-shelled *Polygastrica*, the whole of which would now be classed as *Algæ*, under the order *Diatomaceæ*. The local distribution of these organisms is a point of some interest; and as well-authenticated lists of species from the different localities in Great Britain have still been only partially attempted, I am induced to lay before the Society the results of some observations on the deposits of the river Thames, which accord in a great degree with those made by Ehrenberg in the Elbe, though the proportion of marine to fresh-water species is more equal at corresponding distances from the sea.

The abundance of the *Diatomaceæ*, and the facility with which the different species have been collected at Hull, Poole Harbour, and other well-known localities, where they may be gathered alive, and offer such advantages for acquiring an intimate acquaintance with their habits and modes of growth, has tended, in a great measure, to divert attention from those which are deposited by the Thames water; and, with the exception of some species of *Triceratium*, *Eupodiscus*, and a few other forms, the greater part of the list I shall hereafter mention has been hitherto, so far as I am aware, altogether unnoticed, or at all events no special detail of them has been given from that locality.

The chief cause, I imagine, for this neglect of the *Diatomaceæ* of the Thames and other rivers, has arisen from the fact, that observers have endeavoured to pursue the same plan

* Verhandl. der Königl. Preuss. Akad. der Wissenschaften zu Berlin, 1843.

which meets with such success in the localities I have before alluded to, that is, to examine them in a living state; but, as far as I can judge from my own experience, this affords a most unsatisfactory result; and after a careful examination of the mud deposited at different points in the Thames, any one might easily arrive at the conclusion that the varieties to be met with were comparatively few, and, except for the examination of some of the larger species, not worth the time necessary for extended observation.

Having, some months back, brought home a bottleful of the black mud from the extremity of the Isle of Dogs, taken about half-way between high and low water mark, and for several nights successively submitted it to a careful examination, the only species of *Diatomaceæ* I met with were a *Triceratium favus*, and several specimens of *Coscinodiscus radiatus* and *Surirella splendida.* I had laid it aside for some time, when it occurred to me that the same course of proceeding which is necessary to bring out the siliceous frustules from guano might prove equally efficacious with this Thames mud. Acting on this idea, I boiled a portion of it for some time in hydrochloric and afterwards in strong nitric acid, until the whole was perfectly clean: and, on mounting it, the result far exceeded my expectations; for though impossible to form an accurate conclusion, I should imagine that, excluding the coarse sand, nearly one-fourth of the finer part of the residuum was entirely composed of the siliceous valves of different species of *Diatomaceæ;* and the prevalence of marine forms also proves that, at the distance of nearly forty miles from the mouth of the Thames, their distribution is very similar to that previously described by M. Ehrenberg in the Elbe.

The only observations on this point of the inquiry, as regards British rivers, that I have met with, are notices of the species which occur in the Humber, and in a paper by Mr. T. F. Bergin,* read before the Microscopical Society of Dublin in 1842, who, from a careful examination of the deposits of the Liffey, after a perusal of Ehrenberg's paper on the Mud Banks in the Harbour of Wismar, was led to a different conclusion; and stated it as his opinion that a few species of *Navicula,* not comprising 1-1000th part of the mass, were the only organized forms that occurred in the mud deposited by that river. The cause of this he attributes to the fact of the source of the river being so short a distance from the sea, and, having its rise in the mountains of Wicklow, the rapidity

* Microscopic Journal, vol ii., p. 68.

of the current is so great, that the germs of these minute organisms have not time to increase and multiply as they do in more sluggish streams, flowing for a long distance through alluvial deposits.

A similar occurrence of marine *Diatomaceæ* at a considerable distance from the sea has, however, been noticed by Professor Bailey, in America, who, in his 'Microscopical Observations on South Carolina and Georgia,' published by the Smithsonian Institution, expresses the surprise with which he found in Lake Monroe, 200 miles from the mouth of the St. John's river, specimens of *Amphiprora constricta*, *Odontella polymorpha*, and *Navicula elongata*, which he considered decidedly marine, and which had often occurred to him on the shores of the Atlantic.

I now proceed to give lists of the species from different localities in the Thames, placing those from the Isle of Dogs first, and comparing them with the forms from Hammersmith and near Gravesend; and though I have been unable at present to examine the deposits of the two latter localities so as to give more than a general view of the species, yet these are sufficiently well marked to show the distribution of those peculiar to marine and fresh water.

In all the localities many species of *Melosira*, *Odontidium*, and other genera occur, which, from the want of good figures, I have been unable to name. The well-marked frustules of those figured in the first volume of the Rev. William Smith's valuable synopsis have been easily recognized, from the extremely accurate figures there given. In all cases where any doubt existed, I have referred to slides of the species authenticated by Mr. Smith himself. In some few instances I am indebted to his kind assistance, and also to his able coadjutor, Mr. West, for the determination of forms I was unable satisfactorily to identify, and in a few others I have depended on the figures of Kützing's work on the *Diatomaceæ*. One if not two species of *Dictyocha* occur in the mud from the Isle of Dogs, but I have excluded them from the list, as there appears some doubt if they can be correctly referred to the same order

Marine and Brackish Water Species from the Isle of Dogs.

1. Epithemia sorex	9. Eupodiscus argus
2. ,, musculus	10. ,, fulvus
3. Amphora affinis	11. ,, radiatus
4. ,, hyalina	12. ,, sculptus
5. Cocconeis scutellum	13. Actinocyclus undulatus
6. ,, diaphana	14. ,, sedenarius ?
7. Coscinodiscus radiatus	15. Triceratium favus
8. ,, eccentricus	16. ,, striolatum ?

17. Triceratium undulatum
18. ,, alternans
19. Cyclotella Kützingiana
20. Campylodiscus cribrosus
21. ,, bi-costatus
22. Surirella Brightwellii
23. ,, ovata
24. ,, gemma
25. ,, fastuosa
26. ,, salina
27. Tryblionella marginata
28. ,, punctata
29. ,, acuminata
30. ,, gracilis
31. Nitzschia sigma
32. ,, angularis
33. ,, parvula
34. ,, dubia
35. Amphiprora alata
36. Navicula elliptica
37. ,, didyma
38. ,, punctulata
39. Navicula Jennerii
40. ,, pusilla
41. ,, elegans
42. Pinnularia directa
43. ,, distans
44. ,, peregrina
45. Stauroneis pulchella
46. Pleurosigma hippocampus
47. ,, strigilis
48. Synedra gracilis
49. ,, crystallina
50. ,, superba
51. ,, tabulata
52. Doryphora amphiceros
53. ,, Boëckii
54. Odontella aurita
55. Podosira Montagnei
56. Grammatophora marina
57. Zygoceros rhombus
58. Melosira nummuloides
59. Achnanthes (a spec.)

Fresh-water Species.

1. Epithemia turgida
2. ,, alpestris
3. ,, argus
4. Cymbella Ehrenbergii
5. ,, maculata
6. ,, cuspidata
7. ,, helvetica
8. Amphora ovalis
9. Cocconeis placentula
10. Campyllodiscus costatus
11. Surirella biseriata
12. ,, pinnata
13. Cymatopleura solea
14. ,, elliptica
15. Nitzschia sigmoidea
16. ,, linearis
17. Navicula ovalis
18. ,, producta
19. ,, rhyncocephala
20. ,, inflata
21. ,, gibberula
22. ,, amphisbœna
23. Pinnularia acuta
24. Pinnularia viridis
25. ,, oblonga
26. ,, major
27. ,, radiosa
28. Stauroneis linearis
29. ,, Phœnicenteron
30. ,, anceps
31. Pleurosigma attenuatum
32. Synedra radians
33. Cocconema lanceolatum
34. ,, parvum
35. ,, cistula
36. Gomphonema acuminatum
37. ,, capitatum
38. ,, curvatum
39. ,, constrictum
40. ,, cristatum
41. ,, dichotomum
42. Odontidium hyemale
43. Fragillaria capucina
44. Tabellaria ventricosa
45. Diatoma vulgare

From this list it appears that out of one hundred and four species, fifty-nine are peculiar to marine and brackish water, of which thirty are decidedly marine. The following six species are, however, all that are identical with those included in M. Ehrenberg's lists from Glückstadt and Hamburg, viz.: *Coscinodiscus radiatus* and *eccentricus*, *Triceratium favus*, *Surirella gemma*, *Eupodiscus argus*, identical with *Tripodiscus* *germanicus* and *Actinocyclus undulatus*, probably identical with *Actinoptychus senarius.* This would seem to show that though the general results were similar, yet from some peculiarity, either in the water or the distribution of these minute organisms, the species abounding in the rivers of the north of Europe are marked with a distinctive character from those found in the Thames. The prevailing form in the Elbe appears to be the *Actinocyclus*, and its allied genus *Actinoptychus* of Ehrenberg, of which he enumerates no less than fourteen species out of the thirty-four marine forms that he recognised.

On comparing with the foregoing list from the Isle of Dogs, the species which occur in the mud at Hammersmith and near Gravesend, it appears, that though a few marine forms are still found at the former locality, yet the preponderance of fresh-water species is very great; whilst at the latter the marine and brackish water species, with a few exceptions, alone occur.

The following lists include all I have at present met with from those localities:—

Marine and Brackish Water Species from the Thames near Gravesend.

1. Epithemia musculus
2. Cocconeis scutellum
3. Coscinodiscus eccentricus
4. ,, radiatus
5. ,, marginatus?
6. Eupodiscus argus
7. ,, crassus
8. Actinocyclus undulatus
9. ,, sedenarius?
10. Triceratium favus
11. ,, alternans
12. Campylodiscus cribrosus
13. Surirella ovata
14. ,, gemma
15. ,, fastuosa
16. Tryblionella acuminata
17. ,, marginata
18. ,, punctata
19. Nitzschia sigma
20. ,, dubia
21. Nitzschia angularis
22. Amphiprora alata
23. Navicula Jennerii
24. ,, didyma
25. ,, punctulata
26. Pinnularia cyprinus
27. Stauroneis salina
28. Pleurosigma angulatum
29. ,, hippocampus
30. ,, Balticum
31. Doryphora amphiceros
32. Achnanthes brevipes
33. Grammatophora marina
34. Podosira Montagnei
35. Melosira nummuloides
36. ,, sulcata
37. ,, salina
38. Odontella aurita
39. Orthosira marina

Fresh-water Species from Gravesend.

1. Cocconeis placentula
2. Coscinodiscus minor
3. Nitzschia sigmoidea
4. Navicula cuspidata
5. Navicula minutula
6. Synedra ulna
7. Cocconema cistula
8. Cyclotella rotula

Marine and Brackish Water Species from the Thames near Hammersmith.

1. Amphora membranacea
2. Coscinodiscus eccentricus
3. Actinocyclus undulatus
4. Surirella Brightwellii
5. Tryblionella gracilis
6. ,, acuminata

7. Nitzschia sigma	11. Pleurosigma hippocampus
8. Nitzschia parvula	12. Doryphora amphiceros
9. Navicula elliptica	13. Gomphonema marinum
10. Pinnularia directa	14. Odontella aurita

Fresh-water Species from the same Place.

1. Epithemia turgida	16. Pinnularia viridula
2. Cymbella Ehrenbergii	17. ,, stauroneiformis
3. Amphora ovalis	18. Pleurosigma attenuatum
4. Cocconeis placentula	19. Synedra ulna
5. Campylodiscus costatus	20. Cocconema cymbiforme
6. Surirella biseriata	21. ,, cistula
7. Cymatopleura solea	22. Gomphonema acuminatum
8. ,, elliptica	23. ,, constrictum
9. ,, apiculata	24. Fragillaria virescens
10. Nitzschia sigmoidea	25. Diatoma vulgare
11. Navicula amphisbœna	26. Melosira arenaria
12. ,, crassinervia	27. ,, varians
13. ,, inflata	28. Fragilaria capucina
14. ,, cuspidata	29. Coscinodiscus minor
15. ,, amphirhynchus	

From these lists it appears that at Gravesend, out of forty-seven species, eight only are decidedly peculiar to fresh water; whilst at Hammersmith we find there are twenty-nine fresh-water species out of a total of forty-three; showing, however, that the influence of the flood-tide, even at that distance from the sea, gives a decided character to the *Diatomaceæ* deposited by the water. The following ten species are all that are common to the three localities:—*Coscinodiscus eccentricus*, *Actinocyclus undulatus*, *Tryblionella acuminata*, *Nitzschia sigma*, *Pleurosigma hippocampus*, *Doryphora amphiceros*, *Odontella aurita*, *Cocconeis placentula*, *Nitzschia sigmoidea*, and *Cocconema cistula*, of which the three latter alone are peculiar to fresh water. These are all forms which more extended observation on the deposits of other river and estuary deposits will probably prove to be most universal in their distribution. I have found most of them in the mud of the Avon from Bristol, and also in that deposited at Pembroke Harbour; but it will require a careful examination of many other deposits to prove that any have a purely local habitat, or are entirely confined to sea or fresh water.

The following species which occur in the Thames have also been found by Professor Bailey in America, recorded in 'Silliman's Journal of Sciences' for 1845, vol. xlviii. p. 337:—

In the Mud from Charleston Harbour.

Actinocyclus senarius	Rhaphoneis amphiceros
Coscinodiscus eccentricus	,, rhombus
Eupodiscus argus	Triceratium favus
Pinnularia didyma	Zygoceros rhombus
Pleurosigma Balticum	

And in the Mud from Newhaven Harbour.

Actinocyclus senarius	Pinnularia peregrina
Coscinodiscus eccentricus	,, didyma
Gallionella sulcata	Rhaphoneis rhombus
Grammatophora marina	

A proof of the widely-extended distribution of these species.

From the first of the foregoing lists I have selected a few species for more particular notice, and annex drawings of the most interesting, on the scale adopted by Mr. Smith, namely, 400 diameters.

There is a large species of *Cocconeis* (Pl. VI. fig. 1), elliptical in form, and marked longitudinally with undulating striæ, and also with faint transverse lines, concentric with the extremities of the valve, but only visible with a high power and oblique light. The perfectly elliptic form and peculiarity of the cross striæ seem to distinguish it from the *C. placentula* of Mr. Smith; but I am doubtful whether it may not be a variety of that species.

Of the four species of *Eupodiscus*, the most plentiful is *E. radiatus*, which, from one specimen, in which three frustules were conjoined, may probably sometimes occur concatenated, in a similar way to *Odontella aurita*. *E. sculptus*, the most peculiar in its markings, is rarely met with; and *E. fulvus* and *argus* are sparingly distributed. The latter shows the delicate hexagonal reticulations alluded to by Professor Quekett as marking the *Tripodiscus Rogersii* of Professor Bailey. The star-shaped cells appear, when seen by direct light, to be placed in the centre of small bosses or protuberances, in which it differs from all other *Diatomaceæ* that I am acquainted with.

The *Actinocyclus undulatus* of Mr. Smith's Synopsis occurs abundantly. This species appears to include the *Actinoptychus senarius* of Ehrenberg and Kützing; but after a careful examination of many specimens, I have been unable to make out any undulations similar to those of fig. 4, in Plate V. of the Synopsis, in the large species that occur in the Thames and elsewhere; and although a multiplication of species is a point carefully to be avoided without good grounds, it appears to me that the appellation *undulatus* should be confined to a small form, in which these undulations distinctly occur, and the large and well-known species retain the name originally applied to it by M. Ehrenberg, namely, *A. senarius*.

Sparingly distributed, I have another large and beautiful disc (fig. 2), with sixteen septa, the surface of which is covered with faint cross striæ, similar to those of *Pleurosigma*; and in

* See Histological Catalogue, p. 212.

that respect it resembles the valves from Natal, for which Mr. Shadbolt proposed the name of *Actinophænia;* but I find this striation is no distinctive character, as all the specimens of *A. undulatus* (or *senarius*) that I have examined have the same peculiarity, and the septa are plainly discernible, especially with the parabolic condenser. In the lists I have applied to it provisionally the name of *Actinocyclus sedenarius*, as it approaches very nearly to Ehrenberg's figure of that species in the 'Berlin Transactions' for 1839, tab. 4, p. 2. The septa appear to have their origin from the smooth central portion or pseudo-nodule, and to terminate at slight elevations or openings at the margin of the disc, and in perfect specimens those on one valve are opposite to the interspaces on the other. The front view exhibits slight traces of undulations, as in fig. 13, not in continuous waved lines, but rising to points at the extremities of the rays, giving the side view an appearance similar to that of a ridge-and-furrow roof. The diameter varies from 1-288th to 1-187th of an inch.

Of the genus *Triceratium* four species occur. A small one, by no means uncommon, is represented by fig. 3, which I consider the *T. striolatum* of Ehrenberg; it has convex sides, small horn-like processes at the angles, which are rather obtuse, and is marked with minute dots or cells, radiating from the centre. In the determination of this species I am, after a careful examination, compelled to differ from Mr. Brightwell, who, in his monograph of this genus in a late Number of the 'Microscopical Journal,' refers to a Paper by M. Ehrenberg in the 'Berlin Transactions' for 1839, in which there is a figure of *T. striolatum*, and the following description of the species:—"Testulæ lateribus triquetris convexis, angulis sub-acutis, superficie subtilissime punctato-lineata, dorsi cingulo mædio lævi;" and yet Mr. Brightwell describes it as with "concave ends," and figures it with concave sides; and in the frustules I have seen of his species, the central band on the front view is punctate or cellular, whereas it is described by Ehrenberg as smooth. The cellular structure of the side view is also so plainly apparent, that it would hardly have been described as "subtilissime punctato-lineata" by so careful an observer. Looking, therefore, at Ehrenberg's figure and description, I should conclude that the species figured by Mr. Brightwell cannot be the *T. striolatum*, but should receive some other appellation. The concave sides would seem to refer it to *T. pileus* of Ehrenberg; but I have not seen a figure or full description of that species.

Triceratium alternans of Bailey is rarely met with; and I have only one specimen of *T. undulatum*, in which the peculiar projection of the posterior valve beyond the undulating sides of the upper, as noticed by Mr. Brightwell in his Paper before alluded to, is plainly shown.

Campylodiscus costatus and *cribrosus* are frequently met with. Another small species is represented by fig. 4, which Mr. Smith informs me he has named *bi-costatus*, and that he will give a figure of it in the addenda to his second volume. In appearance it so much resembles *C. clypeus*, that I had applied that name to it, especially as that species is included in Ehrenberg's lists as occurring at Glückstadt, Hamburg, and some localities in Holland, and was found by Professor Bailey in Lake Monroe. The valve is nearly circular, saddle-shaped, *canaliculi* about forty, distinct, length at the sides about half the radius, at the ends much shorter. The central portion has two narrow bands of *costæ* parallel with the terminations of the side *canaliculi*. Diameter is about 1-384th of an inch.

The most abundant species in all the slides I have examined is represented by figs. 7 to 10, which I believe would all be included as varieties of *Doryphora amphiceros* by Mr. Smith, and as different species of *Rhaphoneis* by Ehrenberg and Kützing. The difference of form is so great, and the peculiarity of the cellular markings so apparent, that they appear to furnish data for specific distinction quite as good as are afforded in many species of *Navicula* and *Pleurosigma*. Not having Ehrenberg's figures or descriptions to refer to, I am guided solely by the "species *Algarum*" of Professor Kützing. Fig. 7, from its lanceolate form, strong granular markings, and well-marked median line, might probably be referred to *Rhaphoneis gemmifera*. The length varies from 1-319th to 1-320th of an inch; breadth, about 1-1090th of an inch: it occurs but sparingly. Fig. 8 is rarely met with, but is readily distinguished by its more robust form, the greater delicacy of its striæ, and the slightly marked and nearly parallel sides of its median line. The length is 1-349th of an inch, and breadth 1-779th of an inch. It would be referred to *Rhaphoneis fasciolata*. Fig. 9 is exceedingly common, and in the size of its markings resembles fig. 7, but differs in being more concentric, and nearly obliterating the median line at the acute extremities of the valve. The breadth of the valve is also much greater in proportion to the length. The length is 1-600th of an inch, and breadth 1-1224th of an inch. It agrees with *Rhaphoneis pretiosa*. Fig. 10 is widely different from any of the preceding, and is by no means abundant. The valves are very diaphanous, the markings faint, median line obscure, and form sub-orbicular, the apices being very short; the length is 1-588th to 1-779th of an inch, and breadth 1-1034th to 1-968th of an inch. I should refer it to *Rhaphoneis rhombus*. The only point of distinction between this genus and the *Doryphora* of Professor Kützing appears to be the presence of a stipes; and it would be a point of some interest to determine whether these forms are attached in a similar manner, or whether, as I imagine from the abundance with which they occur, and the absence of any direct negative observations, the frustules are free as in *Navicula*.

A large and well-marked species is represented by fig. 5, which has not, I believe, been hitherto figured as British. I have been unable to obtain a front view of a perfect frustule, though the single valves are by no means uncommon. By a comparison with some specimens of *Zygoceros rhombus* from Petersburg, Virginia, kindly lent me by Professor Quekett, I have little doubt that it can safely be referred to that species,* as the only difference is, that in the Thames specimens, the side view of the valves is rather broader in proportion to the length. The valves are nearly rhomboidal, slightly produced at the extremities, and terminate in a projecting tubular horn or spine. The surface is minutely punctate with small hexagonal cells, radiating from the centre, and has from three to six small spinous processes at the sides, with two rather longer at the extremities of the valve. The length varies from 1-300th to 1-183rd of an inch, and breadth from 1-375th to 1-260th of an inch.

Figs. 11 and 12 are, I believe, front and side views of *Zygoceros surirella* of Ehrenberg. I have only met with one specimen of the perfect frustules, represented by fig. 14, which agrees in form with the figure given by him in the 'Berlin Transactions' for 1839, tab. 4, fig. 12, and shows the smooth central band and striations, which distinguish the side view. Fig. 16, which I consider the side view of a larger specimen, somewhat resembles the genus *Rhaphoneis*, but differs, in the markings being nearly parallel, and though granular, so confluent as almost to appear as lines; the central smooth portion terminates in two lobes, corresponding with the projections, which appear at the extremities when the front view is obtained. The length is 1-714th to 1-1240th of an inch, and breadth 1-1500th to 1-2500th of an inch. I have met with the same species in the deposits of Pembroke Harbour.

Fig. 6 *a* and *b* represents a small cross-shaped valve that occurs sparingly, which Mr. Smith, from a drawing, thought

* The genus *Zygoceros* is included by Mr. Smith in that of *Biddulphia*; this will, therefore, be the *Biddulphia rhombus* of the 'Synopsis.'

might be referred to his *Odontidium tabellaria*: it is peculiar, from the strongly-marked cross *striæ*, which occur on each side of the valve; the length is 1-1385th and the breadth 1-1750th of an inch. The form of the valve is similar to Ehrenberg's figure of *Staurosira construens*,* which he describes "as a four-angled *Fragilaria*, separated from the nearly allied genus of *Amphitetras*, by the absence of openings at the four angles,† but without authentic specimens for comparison, it is impossible, from the small outline figure he gives, to refer it with certainty to this genus. Mr. West informs me he has met with it from many other localities.‡ From the Thames near Gravesend I have lately obtained a large and fine specimen of *Coscinodiscus*, about the 1-107th of an inch in diameter. It has a smooth spot in the centre of the valve, and with that exception is covered with hexagonal cells, radiating towards the circumference. Mr. Smith informs me it is quite new to him, but approaches somewhat to *C. marginatus*, but differs from the descriptions given of that species. I have at present no other forms, either from this locality or at Hammersmith, that call for special notice.

From the foregoing observations it appears that at the distance of at least fifty miles from the sea, the deposits of the Thames are still, to a certain extent, influenced by marine forms of life, and that at Greenwich, which is about forty miles from the mouth of the river, a most distinct marine character is shown by the examination of the species of *Diatomaceæ* which occur there. I think it very probable that many species are only brought up by the flood-tide, and being unable to exist in the slightly-brackish water, the siliceous skeletons are merely deposited in those parts of the river least subject to disturbing causes, and that they would rarely be met with in a living state. That they have a perceptible influence on the formation of shoals and mud-banks in the bed of the river there can be no doubt; and the great abundance and general distribution of species serve to illustrate the occurrence of similar deposits in a fossil state, at localities now far removed, by alterations in the earth's surface, from the streams or harbours in which they were originally deposited.

Another point, probably worthy of attention, is the influence these organisms have in the formation of deltas at the

* See Berlin Academy Transactions, 1847, tab. 1, fig. 44.

† See Berlin Academy Proceedings, 1843, p. 45.

‡ From specimens I have lately seen of *Odontidium Harrisonii*, W. S., I am inclined to believe that this may be a small form of that species rather than *O. tabellaria*. As it is a doubtful form I have not included it in the lists.

mouths of large and slowly-flowing rivers, such, for instance, as the Mississippi, in which the mean velocity of the current at New Orleans is only about one mile and a half per hour for the whole body of water. Sir Charles Lyell, from experiments on the proportion of sediment carried down by the river, has calculated that, taking the area of the delta at 13,600 square miles, and the quantity of solid matter brought down annually at 3,702,758,400 cubic feet, it must have taken 67,000 years for the formation of the whole.* Now, as the siliceous frustules of the *Diatomaceæ* are secreted from the water alone, and would most probably be extremely abundant in so sluggish a stream (especially as Professor Bailey has found both marine and fresh-water species abundant in the rice-grounds), there can be little doubt that, without taking the larger proportion noticed by Ehrenberg in the Elbe, even if it were considerably less, it would reduce the above period by several thousand years, and the same cause would probably apply with equal force to the Ganges and Nile. M. Ehrenberg considered that at Pillau there are annually deposited from the water from 7,200 to 14,000 cubic metres of fine microscopic organisms, which in the course of a century would give a deposit of from 720,000 to 1,400,000 cubic metres of infusory rock or Tripoli stone.

My principal object in the foregoing paper has been to direct the attention of microscopists more particularly to the *Diatomaceæ* deposited by rivers and in tidal harbours, not only in those localities where they occur in overwhelming abundance, on the surface of quiet estuary waters, but in the mud itself, in which many of the rarer forms, and doubtless many new species, are yet to be found. That such an examination is still a desideratum is, I think, shown by the fact that out of the 279 species described by Mr. Smith in the first volume of his 'Synopsis,' only six are given as inhabitants of the Thames, and a very limited number to the Avon, Orwell, and some other rivers; whilst the Severn, the Mersey, and many of our tidal harbours are altogether unnoticed.

That examinations of this nature may sometimes prove useful in an economical point of view is very probable, particularly as it has been noticed that the best samples of guano contain the greatest number of these siliceous skeletons, which doubtless serve to replace the large amount of silica abstracted from the soil by the cereal crops. Hence it is probable that the deposits of many of our rivers would have a beneficial effect if applied to the land, and it rests with the microscopist to point out the most favourable localities for

* Lyell's Principles of Geology, 8th edit., p. 219.

obtaining it. Ehrenberg notices an instance where this has been done in Jeverland, where a blue sand, abounding in calcareous and siliceous shells, is collected, and greatly increases the fertility of the arable soil to which it is applied; and Professor Bailey also states that the mud of Newhaven harbour is used as a fertilizer, and is found to contain 58·63 per cent. of silica.

The distribution of the lower forms of *Algæ*, particularly the *Diatomaceæ*, is probably more extended, both in point of time and geographical range, than any other class of organized beings. Thus we see associated with gigantic reptiles and other extinct forms, several existing species of *Diatomaceæ* occurring in the chalk formation before the deposition of the tertiary strata, proving that the *Eocene* group is not strictly entitled to that designation, but that the dawn of the world in which we live extends much further back in the history of our planet.* And with respect to their local distribution, Dr. Hooker, in alluding to the deposits of the Victoria Barrier in the Atlantic Ocean, remarks,† "There is probably no latitude between Spitzbergen and Victoria Land where some of the species of other countries do not exist. Iceland, Britain, the Mediterranean Sea, North and South America, all possess antarctic *Diatomaceæ*. The siliceous coats of species only known living in the waters of the South Polar Ocean have during past ages contributed to the formation of rocks, and thus they outlive several successive generations of organized beings. The *Phonolite* stones of the Rhine, and the Tripoli stone, contain species identical with what are now contributing to form a sedimentary deposit (and perhaps at a future period, of rock), extending in one continuous stratum for 400 miles."

With the distribution of these forms in our own country we are only at present partially acquainted, and the preparation, therefore, of carefully-compiled lists of species from different localities is still a point to be desired, and might probably lead to some interesting generalizations.

In conclusion, I have only to hope that this slight attempt to bring before the Society the results of a careful examination of the Thames deposits may induce other and more experienced observers to take up the same subject in other localities. The facts I have brought forward are sufficient to afford, in the words of an excellent observer and late member of this Society, "a striking proof of the important part which

* Humboldt's Cosmos, p. 265.

† Dr. Hooker, Flora Antarctica, vol. ii., p. 505.

‡ Mr. Edwin J. Quekett, in London Physiol. Journ. Feb. 1844, p. 145.

these minute organisms were created to perform in the deposition of materials for the earth's surface, and stamp upon reflecting minds that no creature, even the most minute, is formed without special purposes ; and that the least in size of all, by the organization given to them by the great Architect of the Universe, have been employed to carry out his unfathomable intentions."

REPORT

OF

THE FOURTEENTH ANNUAL MEETING

OF THE

MICROSCOPICAL SOCIETY.

THE Microscopical Society of London held their Fourteenth Annual Meeting, February 15th, 1854,—GEORGE JACKSON, Esq , President, in the Chair. The Assistant Secretary read the following Reports :—

Report of Council.—According to annual custom, the Council have to make a Report on the state and progress of the Society during the past year.

The number of members at the last anniversary was—ordinary members 198, associates and honorary 5, giving a total of 203. Since that time there have been elected 28, making the total number 231. This number must, however, be reduced by 3, who have retired, making a final total of 228, and being an increase of 25 upon the number at the last anniversary.

The cabinet of objects and the library have been increased by various donations; and there are also in the possession of the Society various drawings and diagrams relating chiefly to papers read at the meetings of the Society, together with copies of the several parts of the Transactions and of the Journal.

The Council have also to state that, in consequence of the great inconvenience of the present rooms, they have decided upon removing from them. The Society will return to the rooms of the Horticultural Society in Regent-street, if possible, by the meeting on the 29th March.

REPORT OF THE AUDITORS.

From February 15th, 1853, to February 15th, 1854.

RECEIPTS.

	£.	s.	d.	£.	s.	d.
Balance from the previous year				32	8	1
For Entrance of Members	60	18	0			
Compositions (which have been invested as *per contra*)	45	3	0			
Annual Payments for the years 1851, 1852, 1853, and 1854	88	4	0			
Copies of Transactions sold	12	6	3			
Two Dividends on 210*l.* 5*s.* 11*d.* Consols to January, 1854	6	2	6			
				212	13	9
				£245	1	10
Donation from Mr. Lawrence				5	0	0
				£250	1	10

PAYMENTS.

	£.	s.	d.
For Rent of Rooms	20	8	0
Salary of Curator	2	2	0
,, Assistant Secretary	21	0	0
,, Kirkman (Porter)	2	10	0
Attendance at Meetings, Gas, Oil, &c.	10	10	7
Refreshments, &c., at Soirée	12	15	6
Printing Transactions, Reports, Lithography, &c.	73	0	0
Commission to Collectors	9	16	0
Stationery, Postage, &c.	8	0	4
Purchase of 48*l.* 18*s.* 2*d.* Consols*	45	3	0
Balance in hand	44	16	5
	£250	1	10

Examined and approved this 13th February, 1854.

H. PEREGAL, Junior.
W. A. BOYLE.

* The amount of 3 per Cent. Consols belonging to the Society is now 259*l.* 4*s.* 1*d.*

The President delivered the following Address:—

Gentlemen,—I have much pleasure in again congratulating you on the state of our finances; for, although the balance in hand is only twelve pounds more than that of last year, yet we have been enabled to pursue our usual course of investing the compositions received from life-members, and have thus increased our funded property from 210*l.* 5*s.* 11*d.* to 259*l.* 4*s.* 1*d.* It is the opinion of some who have had experience in these matters, that a society which judiciously expends its yearly income is in a more healthy condition than one which hoards a large portion of it, and that therefore we ought carefully to avoid becoming rich. Until, however, our dividends form a much larger proportion of our annual assets than they do at present, we need entertain no fears on this head; while the possession of a reserve fund to fall back upon in case of need cannot be regarded as an evil.

By the arrangement which has been made with the editors for the supply of the 'Microscopical Journal' *gratuitously* to all our members, and by the prompt publication of our transactions, which have been brought down to the end of the year, a steady increase both of members and papers may be reasonably expected; of which I trust the experience of the past year is but the commencement. Twenty-eight new members have been elected, and twelve papers, many of them of considerable interest, have been read.

That of Professor Wheatstone, on the application of binocular vision to the microscope, has pointed out the advantages we may expect to derive from this principle, when certain optical difficulties have been overcome; and Mr. Wenham, by his ingenious contrivances and admirable workmanship, has vanquished some of these difficulties, and given us a glimpse of the benefits in store for us.

The modification of artificial light by the intervention of coloured glasses has often been attempted, but it has generally been found to impair definition. The combination proposed by Mr. Rainey, for the purpose of stopping the heating rays, although it sensibly diminishes the light, appears to answer remarkably well.

Dr. W. Gregory, Mr. Shadbolt, and Mr. Roper have contributed papers on Diatomaceæ. The latter, on those of the Thames, is particularly interesting, as opening a field of research in our own vicinity, the specimens being obtained from localities with which we are all acquainted.

Mr. Legg's paper on sponge-sand contains many hints which collectors and mounters of objects will find useful in their pursuits. Mr. Boswell has communicated an interesting fact on the mode of progression of Actinophrys Sol. His subsequent paper on the bird's-head processes in Polyzoa had been anticipated by the accurate and more extended observations of Mr. Busk, read two months before. The valuable paper of Professor Quekett, on " a combustible mineral from the coal measures of Torbane Hill," clearly demonstrates the presence, not merely of the remains of plants, but of a peculiar woody structure in every description of coal, and the absence of this peculiar structure in the mineral in question.

In microscopic botany we have been favoured with two interesting communications: one by Dr. Hobson, on the development of tubular structure; and the other on the disease affecting the vine, by Mr. T. West.

As most of these papers have been already published, a more extended analysis of them would only be tedious. I would rather occupy a few minutes in considering how far this Society, during the fourteen years of its existence, has accomplished the objects which its founders had in view at its formation.

On turning back to our " History, Constitution, and Laws," we find it recorded that one of these objects was the " promotion of improvements in the optical and mechanical construction of the microscope." With the improvements which have been made in the construction of object-glasses, the Society for many years had but little to do; although, by promoting the use of the instrument, and by keeping alive a spirit of rivalry between the different makers, it was not altogether without influence. Recently, however, an amateur among our own members has demonstrated the possibility of getting good definition with an angular aperture that admits of no appreciable increase; and has thrown out suggestions, which, if carried into effect, will be productive of still further advantages.

In the mechanical construction of the instrument, and in the different methods of illumination, so many improvements have been made by our members, that I should take up too much of your time were I to attempt to enumerate them.

The next object proposed for the Society, " the communication and discussion of observations and discoveries," has constituted the principal occupation of our hours of meeting; and for the interest and variety of the subjects, I need only refer to the volumes of our 'Transactions.'

These observations have been altogether the result of individual and self-directed researches; but it is worthy of consideration whether more might not have been done had we adopted the co-operative and systematic mode of proceeding recommended by our first President. That Professor Owen's suggestions may not altogether be lost sight of, I will, with your permission, quote a paragraph or two from his Address at our Second Anniversary!

After remarking on the importance of conceiving clearly the aim of our researches, and giving a right direction to our exertions, he says: " A slight glance even at the classes of natural objects, of which the intimate structure remains but partially, if at all, known, will suffice to show us how many are the subjects that might be profitably selected by an individual or a committee for a systematic series of microscopical observations. In the animal kingdom, for example, how little we know of the modifications of the microscopical structure of shells recent and fossil, of the stony habitations of the numerous class of polypes, of the crustaceous coverings of the annulose animals, of the calcareous coverings of the Echinodermata, or of the bones in different classes of animals, and in different parts of the skeleton of the same animal!

" In Mineralogy how much remains to be done in the microscopical investigation of different classes of rocks, as of oölites, of sands, flints, &c.

" If committees were appointed to take different subjects of minute research under their respective care, in how short a time might a vast body of microscopical facts be accumulated!"

Selecting a few subjects from the Professor's list, let us see what the systematic researches of three individuals, quite distinctly carried on, have effected. The structure of shells has been ably investigated both by Dr. Carpenter and by Mr. Bowerbank; the structrue of flints and agates also by the latter; and that of bones, developing general views of much importance, by our indefatigable Secretary.

Had committees been appointed, as Professor Owen suggested, and had their members worked with half the zeal and assiduity displayed by these three gentlemen, what a vast body of microscopical facts might by this time have been accumulated!

The " formation of an arranged collection of microscopical objects " was another of the ends proposed to be effected by the Society; but, considering the number of our Members, and that many of them are dextrous manipulators, frequently engaged in mounting specimens, the progress made in stocking our cabinet is by no means a subject of congratulation. The last object proposed to be attained was " the establishment of a library of standard microscopical books." Here also, although something has been done, we have no cause for boasting. But if every Member who wishes to refer to such works, and cannot find what he wants in our library, were to address a note to the Council, giving the title of the book needed, we should gradually have these deficiencies supplied.

There are one or two subjects of microscopical investigation that do not appear to have attracted our attention so much as might be wished.

Some communications from the Rev. J. B. Reade, in the early days of the Society, served to show that the microscope might be made of great utility in delicate chemical researches; and a paper by Dr. Bird Herapath, in the Fifth Number of the 'Microscopical Journal,' strongly confirms this view; but, with the exception of some incidental notices of the application of chemical tests to determine the nature of organic structure, very little of chemical microscopy has come before this Society. For this there may be a sufficient reason; the subject is a special one, and chemists may prefer bringing their microscopical observations before the Chemical Society, to the alternative of submitting chemical matters to the Microscopical. How far they are right I will not determine.

When we look over the list of our Members, and observe the number of medical men included in it, we may well be surprised that our 'Transactions' have been enriched with so few papers on Animal Pathology. Had this been the case only since the institution of the Pathological Society, a reason similar to that above assigned might account for it; but even now it may fairly be questioned whether the accuracy of discoveries in microscopical pathology would not be better tested by a body of men accustomed to use the instrument, and to examine matters of all kinds with it, than by those who have not had these advantages, however well they may be acquainted with the general subject.

A physician in a neighbouring country some years ago announced that, by the aid of the microscope, he had discovered a pathognomonic symptom of pulmonary consumption in a peculiar egg-shaped body occurring in the sputa. It was afterwards found that in the hospital to which he was attached the consumptive patients breakfasted on arrow-root, and the peculiar bodies were some of the unbroken starch granules that had stuck to their mouths.

In both the addresses of Professor Bell this want of papers on Animal Pathology is noticed; and his ideas are so just, and his language so clear and forcible, that I cannot better conclude the subject than by quoting the last paragraph from his Address at our Sixth Anniversary: "Let us remember," said he, "that the instrument which we employ is capable of elucidating subjects of far more importance than the distinction of species of animalcules, and the demonstration of the structure of a zoophyte. The relief of suffering, and the salvation of life itself, are amongst the legitimate objects of microscopic research. Let not our medical members, then, be satisfied with the mere amusement, or even the bare scientific information to be derived from it; but let them employ it as an important means of carrying out the great objects of their profession, in determining the nature of diseased structures, the distinctions between the healthy and morbid states of the tissues, and, consequently, in enlarging our means of restoring health to the sick, ease to the suffering, and life to the dying."

It only remains, Gentlemen, for me to express the satisfaction which I feel in resigning this Chair to one whose intimate knowledge of physiology in both its branches, no less than his general scientific attainments, so eminently qualify him to preside at our meetings.

It was unanimously resolved—That the Reports of the Council and Auditors be received; and that the Reports, with the President's Address, be printed.

The election of officers took place; when the following were declared elected:—

Officers.

President . . . . Dr. Carpenter.
Treasurer . . . . N. B. Ward, Esq.
Secretary . . . . John Quekett, Esq.

New Members of Council.

Dr. Lionel Beale.
Josh. Gratton, Esq.
M. Marshall, Esq.
Saml. C. Whitbread, Esq.

In the place of

Warren De La Rue, Esq.
W. Gillett, Esq.
John Lee, Esq., LL.D.
Robert Warington, Esq

Observations on the DEVELOPMENT *and* GROWTH *of the* WATER-SNAIL (*Limneus stagnalis*). By JABEZ HOGG, M.R.C.S., &c.

(Read March 29th, 1854.)

IN submitting the observations which I have the honour of bringing to the notice of the Fellows of the Microscopical Society this evening—on the Development and Growth of the Water-snail (*Limneus stagnalis*)—I do so with considerable diffidence. When I first gave the subject my special attention, and began to jot down the remarks that occurred to me as growing out of my experiences, I was not fully aware of the extent to which many able investigators had traversed the same ground before me. So far back as 1754, precisely a century since, Baker, in his book entitled, 'Employment for the Microscope' (p. 325), was the earliest to describe "a small water-snail and its spawn, or eggs, fastened in little masses, against the sides of the glass," in which he kept them. It also engaged the attention of the illustrious Swammerdam; and, more recently, that of Reaumur and Dr. Grant. Mr. Bowerbank's very interesting and careful observations on the 'Structure of the Shells of Mollusca and Conchifera,' and the scientific researches of Dr. Carpenter, have thrown great additional light upon this subject. A brief record of my own personal investigations, with regard to this department of microscopic observation can, therefore, present no signal feature of interest beyond that of confirming and enforcing the experience of the talented and eminent microscopists who have preceded me. It is with this view that I venture to lay before the Microscopical Society the few remarks which I have now the privilege of reading, happy if I shall have contributed, in however slight a degree, to add in any way to the store of knowledge already accumulated.

Into a glass vase, where my stock of *Chara. Vallisneria*, &c., is growing, I introduced last Autumn a single *Limneus*, for the purpose of observing its habits; I was then more especially curious to see its mode of creeping along, under the surface of the water, by means of its fleshy foot. Upon one occasion, as I sat watching the movements of the animal, attached as it then was to the side of the vase, near the surface of the water, it suddenly became uneasy, moving to and fro, and in a short time it began to deposit very slowly, through a fissure near its ventral aperture, a small gelatinous sac, filled with transparent specks, at the same time firmly gluing it to the glass. This sac I examined with a pocket magnifying-glass, and found it contained fifty-six ova. Each egg was of an ovoid form, and consisted of a pellucid membrane filled with a transparent fluid, having a very minute yellow spot, the yolk, adhering to one side of the cell-wall. Seen with the sunlight falling upon it, it had all the brilliant colours of the soap-bubble. Viewing it again on the second day, I observed that the yolk had a central spot, or nucleolus, rather deeper coloured than the rest. On the fourth day the yolk had changed its position, and doubled in size, as shown slightly magnified (Plate VII. fig. 1). Upon a closer examination, a central depression, or transverse fissure, could be seen, which, on the sixth day, plainly indicated the line of demarcation in the little mass, as represented at fig. 2. From this time it commenced to move round the whole interior of the cell, with a very slow rotatory motion ; the motion was increased when the sunlight shone upon it, from which I concluded, that, as it received more heat, its movements were thereby accelerated. The increase in size of the two parts of the animal appeared to be uniform up to the sixteenth day, when the shell apparently occupied the larger portion, represented at fig. 3 ; and the spiral axis, around which the calcareous lamellæ were being deposited, had a much darker colour than the soft, or cephalic extremity. On the eighteenth day the tentacle was visible, with a small black speck at its root, the eye ; this was seen to be protruded with the movement of the tentacle. Upon closely watching it, a fringe of cilia could be seen surrounding the tentacle and oral aperture ; and, from observing the direction of the currents, I am led to believe that the earliest rotatory motion is in a great degree, if not wholly, dependent upon the action of the cilia. A constant current being kept up in the cell-contents, we may conclude, that with this motion, we have the conversion of the cell-contents into the several tissues ; and probably the whorl-shape of the shell is likewise due to the same formative process. The rotation was, on every occasion of my observing it, from the right to the left, and this always combined with a motion around the egg ; the embryo performing a circuit, as represented magnified at fig. 4, and forcibly reminding me of M. Wichura's scientific investigations into the curious property possessed by the leaves of plants, of *winding* generally in a particular direction. He observes :—

"It is a very remarkable phenomenon, that the circularly or heliacally acting forces of nature follow an unchanging, definite, lateral direction in their course. In cosmical nature the planets describe heliacal lines, winding to the right in space, by virtue of their circulation from west to east ; since this is combined with the advance, in company with the sun, towards a point in the northern hemisphere. In the department of physics we meet with allied phenomena in the circular polarization of light, and in the course of electro-magnetic spirals. Organic life exhibits the same laws in the circulation of the blood, in all cases starting from the left side of the animal body; and in the heliacal windings of the shells of Mollusks, which follow a direction determinate for every species. But plants, above all, give evidence of a wonderful obedience to such laws, in the direction of the spiral vessels, the heliacally winding trunks of trees, winding stems and leaves, and probably also in the circulation of their saps." *

Professor Quekett has directed attention to this subject, especially with regard to plants, in his 'Histological Lectures.' To proceed:—

From the twenty-sixth to the twenty-eighth day the little animal was actively engaged in making its way out of the egg, in the advanced stage represented at fig. 5, leaving its shell behind it in the ova-sac, and immediately attaching itself to the side of the glass. The ciliary motion is then better seen; each tentacle being surrounded at the extreme edge by a row of cilia kept in motion by bands of muscular fibre: the cilia are protruded from beneath the shell, and kept incessantly at work, in conjunction with those surrounding the opening to the mouth; thus bringing a constant current of water for the æration of the branchiæ, situated above the oral aperture; and at the same time a due supply of nourishment for the growth of the little animal. And it is a remarkable fact, that, as soon as the gastric teeth are properly matured to enable it to cut the vegetable substances growing in the water, the cilia being no longer required, then disappear, and drop off, from the tentacles. The tentacles and oral fringe of cilia are represented magnified, in the drawing, at fig. 6. But if, on the other hand, the young animal be kept in fresh water alone, without vegetable matter of any kind, it still retains its cilia, and attains only to a small size; it then acquires gastric teeth, but of a very imperfect character, which never attains to perfection in form or in size. If at the same time it is confined to a small narrow cell, it will only grow to such a size as will enable it to move about freely; thus adapting itself to the necessities of its existence.

Dr. Grant, I believe, first pointed out the ciliary motion in the embryo of some salt-water species of Gasteropoda. In examining the embryos of *Buccinum undatum* and *Purpura lapillus*, which are also enclosed in groups within transparent sacs, he was struck with an incessant motion of the fluid in the sac towards the fore-part of the embryo; and he then noticed that this motion was produced by cilia placed around two funnel-shaped projections on the fore-part of the young

* M. Wichura, "On the Winding of Leaves," translated by Arthur Henfrey, F.R.S., 'Scientific Memoirs,' 1853.

animal, which form the borders of a cavity, in which he perceived a constant revolution of floating particles. He also observed these circles of cilia in the young of the species of *Trochus*, *Nerita*, &c., in which the embryo was seen revolving round its axis. He met with the same appearance in the naked Gasteropoda, as the *Doris*, *Eolis*, &c. The embryo of these revolve round its centre, and swims rapidly forward by means of its cilia, when it escapes from the ovum. Dr. Grant assigns various uses to these motions, but does not connect them with respiration or nourishment, although there can be little doubt that they are so.

In some six weeks, or two months, the flattened form of the shell becomes gradually changed into that of the conical form of the full-grown animal (fig. 7).*

That this little creature is hermaphrodite, like the common snail, is proved by my having only this solitary animal in my vase; and yet nearly all the eggs deposited by it arrived at maturity. Like the common snail it is also copulative, as I have seen two animals mutually pass a thin tongue-like organ into a fissure between the body and upper surface of the posterior portion of the foot.

I observed in the few eggs that did not come to maturity that the yolk only slightly increased in size, and then remained in that state until all the others were hatched, when the ova-sac became the prey of other animals.

This one snail deposited two and three of these ova-sacs in the course of the week; and in two months I calculated that upwards of 800 young would result therefrom; thus it will be seen, that the number of eggs deposited by each individual is very great; fully explaining the rapidity with which this class of animals increases, either on land or in the water.

The shell, as we have before seen, is begun at a very early stage in the formative process. It is first observed to have the shape of minute ovoid cells, which are deposited side by side around the axis, or central cell; and this may be described as a cytoblast, enclosing a certain quantity of colouring matter, just sufficient to give it a distinctive appearance, from the previously-formed basement membrane. The sides of one cell being in close contact with those of other cells, a gradual compression, or elongation, takes place, and we have, finally, resulting divisional ribs, hardened by the deposition of calcareous matter into a shelly covering. Subsequently all trace of the earliest cells and cytoblasts are lost.

* In warm weather the eggs arrive at maturity in a much shorter time, especially when exposed to the light and warmth of the sun.

My own observations upon the *Limneus*, in many important particulars, coincide with those of Mr. Bowerbank, made in 1843, and published in the Transactions of this Society, upon the Structure of Shells of Mollusca, &c. Mr. Bowerbank thus explains the development of the shells of these animals :—

"Let us suppose the rudiment of the future shell to have been the result of the excretion of some mucus or lymph (properly, albumen); it would then be nothing more than a very thin transparent membrane, with a determinate figure dependent upon the figure of its species. In this membrane organizing cytoblasts and cells are produced and multiplied in rapid succession, until, by their increase and opposition, a cellular structure is formed in it. On their first appearance the cells are transparent and globular, but pushed on by the law of growth, which regulates their development, they very soon begin to secrete, from their inner surfaces, carbonate of lime. The cells being filled with it, a solid structure is the result of their close aggregation; the pattern being modified only by the form and degree of condensation of the calcigerous cells, in which it has been secreted. * * * * * A layer or stratum of shell being thus formed, another is produced from its inner surface by the same means, and then others, until the normal set is completed: the whole being kept together as one by the living tissues."

Mr. Bowerbank believes that the truth of this mode of formation is proved, not only by the structures he has discovered, but also by the phenomena which occur in its reparation of injuries; for he says :—

"This reparation is not made by a coat of calcareous matter, spread over the wound by the *collar* or mantle of the animal, as has been maintained, but by an effusion of coagulable lymph, in which cytoblasts are produced in the first instance, and quickly succeeded by a cellular structure, in which the earthy basis of the shell is secreted, and by which the scar is filled up, or the fracture cemented together."

This I have repeatedly verified, and always found that after an injury to the shell of either an embryonic, or more perfectly-formed animal, in a few hours subsequently the process of repair has been commenced by a deposition of cells, less in size, and somewhat more irregular in form than the first. Upon breaking off an eighth of an inch from the edge of the shell of a full-grown animal, I observed that it first threw out a series of exudations of plasma, or albuminous matter; which, after some days, became hardened by a calcareous deposit, corresponding in appearance to the lines of growth of the old shell, but only to the extent required to convert the edge into a smooth and strong margin of about one-half the breadth broken off; and, ultimately, new lines of growth were thrown out beyond the edge of the mantle; this I clearly ascertained by scraping it with a fine knife. In reference to this part of my inquiry I may be pardoned for directing attention to the very interesting observations of Professor Paget:—

"That the reparative power in each perfect species, whether it be higher or lower in the scale, is in an inverse proportion to the amount of change through which it has passed in its development from the embryonic to the perfect state. And the deduction to be drawn is, that the powers for development from the embryo are identical with those exercised for the restoration from injuries: in other words, that the powers are the same by which perfection is first achieved, and by which, when lost, it is recovered. Indeed, it would almost seem as if the species that have least means of escape or defence from mutilation were those on which the most ample power of repair has been bestowed; an admirable instance, if it be only true, of the beneficence that has provided for the welfare of even the least of the living world, with as much care as if they were the sole objects of the Divine regard."

Dr. Carpenter differs in some particulars from Mr. Bowerbank, more especially with reference to the *vascularity of the shell*, which, I believe, he entirely denies, and somewhat inclines to the more generally received opinion of Reaumur; who, after careful examinations of the shells of *Gasteropoda*, came to the following conclusions:—

"That these calcareous defences are mere excretions from the surface of the body, absolutely *extra-vital* and *extra-vascular*, their growth being carried on by the addition of calcareous particles deposited in consecutive layers. The dermis, or vascular portion of the integument, is the secreting organ, which furnishes the earthy matter, pouring it out apparently from any part of the surface of the body, although the thicker portion, distinguished by the appellation of the mantle, is more especially adapted to its production. The calcareous matter is never deposited in the areolæ of the dermis itself, but exudes from the surface, suspended in the mucus which is copiously poured out from the muciparous pores, and gradually hardened by exposure; this calciferous fluid forms a layer of shell, coating the inner surface of the pre-existent layers to increase the size of the original shell, or else in furnishing at particular points for the reparation of injuries which accident may have occasioned." *

Now, if it be a mere excretion from the surface of a membrane, and neither *vital* nor *vascular*, how does Reaumur account for the deposit of the calcigerous cells, and subsequent formation into shell, so early seen in the embryo; and that long before these cells can become consolidated by exposure to air? Mr. Bowerbank has seen, as well as myself, that at a very early stage of embryonic life, calcareous matter is deposited, and hardened into shell; and this can be readily proved, by simply breaking up the egg, and submitting a portion of the contents to the action of a drop of very dilute acetic acid, when the carbonate of lime will be very quickly

* 'Article Gasteropoda.' By Professor Rymer Jones. 'Cyclopædia of Anatomy and Physiology.'

dissolved out, with a brisk effervescence; the basement membrane only remaining, as in the older shell.

If the young animal be viewed under a power of 150 diameters, the whole mass is sufficiently transparent, to show that the shell is an important part of the whole structure, and not "*suspended in mucus;*" but has a hardened and definite form long before it issues from the egg, or comes in contact with the external air, to produce any hardening effect upon it.

Mr. Bowerbank has observed, that in the fully-formed shell "the mode of effecting repairs in the periostracum, affords evidence of a high degree of vitality." As to the term *extra-vital*, I know not what it means; and, I believe, no one who has bestowed care and attention in the investigation of the works of the Great Creator, will for one moment assume the smallest speck to be an *extra-vital* production, or addition. Indeed, it appears to me that it would be as reasonable to deny the vitality of bone, or the growth of the lower organized cartilage, as to deny it to the shell of the pectinibranchial and pulmonated Mollusks.

Dr. Carpenter says:—

"It may now, however, be stated as an ascertained fact, that shell always possesses a more or less distinct organic structure; this being, in some instances, of the character of that of the *epidermis* of higher animals, but in others having more resemblance to that of the *dermis*, or true skin."

From repeated examinations, I believe, with Mr. Bowerbank:—

"That the structure of shell is analogous to bone in some respects, and is formed much in the same manner as in particular kinds of bony matter, by the deposition of carbonate of lime within the cells of the membranes, which enter into the composition of the shell, or by the aggregation and coalescence of the calcigerous cells when the membrane is very sparingly produced; and that it is made up of three strata. Each stratum being formed of innumerable plates, composed of elongated cellular structure; each plate consisting of a single series of cells parallel to each other. These plates of cellular structure are deposited alternately in contrary directions, so that each series of cells intersects the one beneath it, at nearly right angles."

If to a portion of the periostracum a small quantity of very dilute acetic acid be added, to dissolve out the calcareous matter, and it be then viewed under a magnifying power of 250 diameters, it will be seen to be composed of oval cytoblasts, exhibiting distinct nuclei, beneath which will be found a fine membrane studded with minute spots, apparently the escaped contents of the cells. This membrane has a regular series of corrugations or folds arranged throughout its whole extent, which gives to the shell in certain positions an iridescent lustre. Immediately beneath this is placed the transparent basement membrane of an even texture and very light amber colour, this is the albuminous or animal membrane; which with the layer before referred to, and above this, appears to me to be traversed by tubes, that no doubt run from the inner to the outer portion of the shell substance, and probably this net-work of pores have assigned to them similar duties to those in the human skin, viz., that of throwing off effete particles of matter, &c.

The very small proportion of animal matter contained in this shell is a marked characteristic; after the removal of the calcareous matter by dilute acid, we have the small residuum of a grain or two only; from this cause the shell is very brittle at all times. The shell of the fully-formed animal is ovate, whorls five or six, elongated and dextral; thus favouring, as before observed, the notion that the circular motion of the embryo when in the egg determines the whorl.*

The mantle of the animal partakes of the same character and structure as that of mucous membrane generally, more especially that portion of it lining the internal surface of the shell; thence it is reflected over the body, and forms a direct communication with the external shell and internal soft parts. Its other important use, besides that of depositing carbonate of lime, is the secretion of plasma, or a glazing fluid, which it spreads over the internal portions of the shell, and with which it lubricates the whole of the external parts, thus preventing any irritation that might arise from a drying up of the coarser particles of calcareous matter. Another use I have particularly noticed, is that of converting a large part of it, beneath the greater whorl of the shell, into an air-bag, or receptacle for holding a bladder of air, which must have considerable influence in rendering the shell buoyant and light, as by suddenly discharging it, the animal instantly sinks to the bottom. The animal is often seen to rise to the surface of the water for the purpose of taking in a supply of fresh air, which it does by opening a small valvular aperture, situated about the eighth of an inch above the ventral outlet. If the animal be removed from the water it immediately squeezes out this supply of air, at the same time it presses out the water from the body, for the purpose of enabling it to recede

* For further information and much interesting matter upon this subject I must refer to Mr. Bowerbank's researches upon the 'Structure of Molluscous and Conchiferous Animals,' most accurately and carefully illustrated, published in the Transactions of this Society, 1843. Also Dr. Carpenter's researches, published in the Reports of the British Association, 1844 and 1847; and his 'Principles of General and Comparative Physiology.'

into the interior of its shelly house for protection; in this act it is greatly facilitated by the action of retractor muscles, having a strong tendinous attachment to the *columella* of the shell. The shell of the young animal, and thin portions of the older shell, viewed by polarized light on the selenite stage, are interesting and beautiful objects. In the young animal the growth of the membranous part is effected by the gradual expansion of the vascular and cellular tissues, and we are soon enabled to define the expanded foot. This is a fleshy disc, broader anteriorly and divided into transverse segments; by a particular arrangement of the longitudinal muscular fibres it is enabled to perform a series of undulatory movements, by which means the animal glides smoothly along; its under surface is likewise studded over with a number of small orifices, which assist in causing a vacuum to be formed, and thus it suspends itself in an inverted position from the surface of the water, moving about in any direction. The muscular fibres, by their interlacements, greatly assist the animal in its progression, and in the performance of rapid movements; at the outer edge it is turned over, or returned upon itself, forming a smooth and strong margin of condensed tissue and muscular fibres, which take their course in broad fasciculi, and gradually taper off to a thin tendinous attachment on the pillar of the shell.

The mouth is situated at the under and fore-part of the head; it is a muscular cavity, enclosing a dental apparatus, semicircular in shape and provided with transverse rows of projecting spines, or teeth of a horny structure, or, more correctly, alternating rows of incisor and canine teeth, each being pointed with silica, and accurately fitted to cut against each other; they are thus admirably adapted for the scraping or stripping off the cuticle from the blades of *Vallisneria*, which the animal does without killing the plant, and leaves it more accurately divided, than at all possible to obtain by the usual mode of splitting for microscopic observation. The gastric teeth are immediately joined to the œsophagus or gullet, and to this succeeds the gizzard, a strong muscular apparatus, a quarter of an inch in length, and having a rugose appearance, with transverse and longitudinal fibres, by means of which every movement requisite for the conversion of the food is effected, and passes into a small membranous sac, the stomach; this is folded into longitudinal *plicæ*, and from it arises the large intestine of considerable length, having much of the appearance of intestine in the higher order of animals, excepting in colour; a narrow longitudinal band passes down on either side of the external coat, and internally it is apparently supplied with valves. In its course it takes a considerable turn around the inner whorls of the shell, terminating in a rectum which has its vent placed between a small portion of the mantle and the under edge of the last whorl of the shell.

The liver is not nearly so large as it is in the land-snail; it consists of two lobes, and is enclosed in a strong capsular covering; it pours a pale-coloured bile into the stomach by more than one duct, and is provided with a proper hepatic system of vessels.

The heart is a strong muscular apparatus, having both an auricular and ventricular cavity; it is surrounded by a very delicate membrane (the pericardium). In shape it is pyriform, with muscular cords stretching from side to side, of a highly elastic character, looking not unlike very fine bands of India-rubber alternately contracting and expanding; these cords are, no doubt, analogous to the *cordæ tendineæ* of the mammal heart. The heart receives the ærated blood from the respiratory organs, and propels it through the vessels at the rate of sixty times a minute. It is placed far back in the superior portion of the shell, near to the axis, where it is securely fixed without reference to the movements of the mouth or body of the animal.

Like others of this family of aquatic Gasteropoda, the breathing apparatus resembles the branchiæ of fishes in structure; they are pectinated, and placed in three or four rows near the roof of a cavity under the integuments of the head, or rather above the oral opening, which is peculiarly arranged with retractor and other muscles, for the purpose of permitting an uninterrupted æration of the blood as it is brought to the branchiæ.

The nervous system consists of many gangliæ, or nervous centres, in place of a distinct brain, but "each of these gangliæ may be considered as a distinct brain of the *heterogangliate* form." They are freely distributed throughout the body, but connected with each other by cords of communication; the nervous mass appears to be granular, and is somewhat yellow in colour, whilst the nerves themselves are white and smooth, and invested with a delicate membrane (*neurilemma*). Professor Jones observes that—

"One remarkable circumstance may be mentioned as peculiar to this class; the changes of position of the nervous centres obey the movements of the mouth, with which they are intimately connected; they are, in fact, pulled backwards and forwards by the muscles serving for the protrusion and retraction of the oral apparatus, and are thus constantly changing their relations with the surrounding parts.

"The ganglia, placed above the œsophagus, sends off branches to supply the muscles of the head, the tentacles, and give origin to the optic nerves;

and from the sub-œsophagial ganglion, which fully equal the former in size, arise those nerves which supply the muscles of the body, and of the viscera."*

The singular adaptation of the eye must not be omitted; this appears in the early embryonic stage to be situated within or on the tentacle, it is constantly retracted with it, which is due to the length of the pedicle, and to the retractile sheath of the optic nerve, enabling the animal to shorten it; at the same time the tentacle folds down over it, forming a protective cover at all times. The eyes are situated at the base of the inner side of the tentacle, and resemble two very small black spots. When examined with a power of 100 diameters, they are seen to be transparent spherical lenses, surrounded by a black zone or iris, the pigmental layer is continued some distance down the pedicle. It is pear-shaped, and evidently the little animal is very quick-sighted, as he avoids every obstacle placed in his way, or quickly withdraws himself into his house if one attempts to touch him; although in avoiding obstacles he appears to make great use of his tentacles as true feelers. The tentacles are composed of a dense elastic tissue, surrounded by a band of muscular fibre; in shape they are triangular, with the base attached to the body of the animal.

The *Limnei* are stated by Professor Forbes to have been found in the fossil state as far back as the Oolitic epoch; and the most ancient forms bear a striking resemblance to the common existing types. In England, at the present time, they are abundant in nearly all the waters where vegetable matter is growing, and in the slow running rivers, especially where the water-cress is found.

The *Limneus*, like every other living thing, is infested with its *parasite.* Reaumur observed a sort of mite infesting the snail (*Helix aspersa*), they were securely lodged in the pulmonary cavity. Müller also noticed in certain Gasteropods a *worm;* and Dr. Gould, examining a specimen of the *Physa heterostropha*, "found the neck of the animal beset with numerous little things, looking like short, minute, white lines, attached like leeches, and which derive their nourishment from the fluids of the animal without his having the power to dislodge them."

M. Bäer states that he discovered a *Filaria* in the abdomen of *Limneus stagnalis;* and in many of the same family of Mollusca he has met with a worm allied to the Naides, "living in the respiratory cavity, or hanging like little tufts of threads from the sides of the abdomen; whence he named it *Chælogaster.*" Besides these, he says, "a kind of Cercaria

* Professor Rymer Jones, *op. cit.*

finds an appropriate nidus for their evolutions in the body of the lacustrine snails; and the curious transmutations of form they undergo in the interior of the animals, and the circumfluent water, afford one of the most striking illustrations of Steenstrup's theory of alternating generations." *

Upon observing the *Limneus* in my glass rather closely, I noticed that its body was covered with the "*little white line-looking leeches*," described by Dr. Gould and M. Bäer; upon carefully detaching one or two, and viewing them with a half-inch object-glass, it had the formidable appearance represented in the drawing at fig. 8. It has an anterior mouth, surrounded with minute teeth or spines, over which it possesses great power. Suddenly it may be seen to dart out its body, at the same time projecting its mouth to some distance apparently for the purpose of seizing its prey, when it as quickly retracts itself within the shell of the animal, where it securely attaches itself to its body by a posterior sucker. It is possessed of a great number of hooklets or feet, by these it creeps from one part of the body to another, but is always found adhering to those parts affording security in times of danger. Eventually they become so numerous that the animal's life falls a sacrifice to its troublesome tormentors, having apparently no power to rid itself of them.

In conclusion, I would offer a word or two on the *cell;* the primordial wall of which does not enter into the formative process of the embryo. The cell contents only are required for the purpose of affording nourishment to the vital blastema of the nucleus, in which a cycle of progressive development once set up, goes on until the animal is sufficiently matured to break through the cell-wall, and escape from the ova-sac. At the same time it may be inferred, that this is in some way assisted by the process of endosmose, and in this way certain gases or fluids become drawn into the cell-interior, and thus materially aid in the supply of nourishment for the growth of the animal.

The cell-wall bears the same relation to the future perfect animal that the egg-shell of the chick does to it; it is but an external covering to a certain amount of gaseous and fluid matter, and for the purpose of placing the germ of life in a more favourable state for development, assisted as it is by an increase of temperature usually the result of a chemical action set up, or once begun, in an *organism* and a *medium.* The ovum, destined to become a new creature, originates from a cell enclosing a gemmule, from which its tissues are formed, and nutriment is assimilated, and which eventually enables

* Agassiz and Gould's 'Principles of Zoology.'

the animal successively to renew its organs through a series of metamorphoses, which give it permanent conditions not only different but even directly contrary to those which it had primitively.

In this one fact are we not furnished with a well marked or broad line of demarcation between that of animal and vegetable life? In the development of the animal, the cell-wall takes no part in the formative process; it is but an enveloping membrane required for a time, and then thrown off. On the contrary, in vegetable life it enters largely into the formative process, and ultimate development of all its tissues; it is ever to be found growing with its growth, cell-wall upon cell-wall intact, with or without its earliest contents.

Note.—June 6th, 1854.

My attempt to arrest the development of some young animals is still continued with perfect success. They have remained in the same *narrow glass-cell*, at the stage of growth before referred to, viz., about the size the animal usually attains during the first *two or three weeks* of its existence. They are now *six months old*, alive and well, the cilia are retained around the tentacles in constant activity; whilst other animals of the same brood and age, placed in a situation favourable to growth, have attained their full size, and have now produced young, which are of the size of their *elder* relations.

DESCRIPTION OF PLATE VII.

Fig.

1.—A magnified representation of the increase and change of situation occurring to the yolk of egg of *Limneus* on the fourth day.

2.—The change observed on the sixth day, showing the transverse fissure or divisional line in the mass.

3.—The formation of the shell proceeding more rapidly, it appears on the sixteenth day as the larger portion of the embryonic mass.

4.—The embryo performing its heliacal windings around the shell.

5.—The embryo, or young animal, seen soon after it has issued from the shell.

6.—The tentacles, with cilia, seen under a ½-inch object-glass; the arrows indicating the course of the current produced by the cilia.

7.—The natural size and form of the shell of a full-grown *Limneus*.

8.—Parasitic animal found on the body of *Limneus*, magnified 100 diameters.

Observations on some Deposits of Fossil DIATOMACEÆ. By WILLIAM GREGORY, M.D., F.R.S.E.

(Read April 19th, 1854.)

IN the series of microscopic objects issued by the Zurich Microscopical Association, there occurs a specimen of Bergmehl, stated to be from Lillhaggsjön in Lapland, which is very remarkable in several particulars.

First, there is a very great abundance of *Eunotia Triodon*, exhibiting the most astonishing variations of outline, so that the extreme varieties in opposite directions, those, for example, which are short, compressed, and have strongly-marked prominences, and such as are long, flattened, the apices being lengthened out, while the prominences actually disappear, or can only be traced by a hardly-perceptible waviness in the dorsal outline, would hardly be supposed to belong to the same species, and yet a perfect and gentle gradation may be traced from the one extreme to the other. This remarkable tendency to vary in form is peculiar, among the *Eunotiæ* I have seen, to this species, *E. Triodon* and to *E. begebba*, Kützing. It is totally absent in the common fossil forms of *E. Tetraodon* and *E. Diadema*, which hardly vary at all, save in size.

It appears to me that this fact, especially when we consider that all these species often occur together, as, for example, in the Mull deposit, where *E. Triodon*, though not frequent, is just as variable as in the Bergmehl under consideration, demonstrates that these species are really distinct, and not, as some have conjectured, varieties of one, which may present one, two, three, four, five, six, seven, or more prominences. If all belonged to one species, all should be alike variable or alike constant, whereas some vary *ad infinitum*, others not at all, in form at least. Nor can it be said that such a form as *E. Triodon* is developed, as a variety, only under certain circumstances; for in the Mull deposit it occurs with all its peculiarities, and therefore the supposed circumstances must have occurred; and yet, in that deposit, *E. Tetraodon* and *E. Diadema* are much more abundant, and show no tendency to vary in form. But these two last-named species are absent from this Lapland deposit, where *E. Triodon* abounds. It seems to me that these facts settle the question as to the species named, which must be held to be true and well-marked species; one of the characters of *E. Triodon* and of *E. begebba* being a tendency to vary in form, while fixity of form characterises *E. Tetraodon* and *E. Diadema*.

This Laponian deposit also contains *E. serra*, and I think

I have seen *E. heptodon.* *E. serra* seems to be confined to Scandinavian deposits.

Although differing from the Mull deposit in regard to the forms I have named, and also some others, this Bergmehl agrees with it in many points, as in the abundance of *Navicula rhomboides* and *N. serians*, that of many *Pinnulariæ*, of *Gomphonema coronatum*, of several *Cymbellæ*, *Stauroneides*, *Tabellariæ*, *Orthosiræ*, and other forms, but especially in the presence of *Eunotia incisa*, first observed by me in the Mull earth. The variety β is here the more frequent.

There is another form, common to these two deposits, which, so far as I know, has not been described. It is an aspect like a *Synedra*, long and narrow, straight in the middle, and having the ends curved opposite ways, which gives to it a sigmoid character. I am inclined, however, to suppose it to be a *Nitzschia*, for while I cannot make out the transverse striæ of *Synedra*, I can see a row of puncta on each margin in some specimens. It is, however, quite distinct from *N. sigmoidea.* In the Mull earth it is generally broken, so that we see only one-half; but I have found several entire examples. In the Lapland deposit it is more frequent, and often entire, although from its slender proportions it is apt to be broken, and fragments also occur. As we have already *Nitzschia sigmoidea* and *N. sigma*, this form, if it be a *Nitzschia*, may be called *N. sigmatella.*

I have still to notice a form occurring in this Lapland Bergmehl, which, so far as I have been able to ascertain, is undescribed. It is narrow and of considerable length, but bent into the form of a sickle, or nearly a semicircle. It is slightly attenuated at the rather acute apices, and has very strong and distinct, though rather fine, transverse striæ. It approaches more nearly to *Eunotia arcus*, as figured by Smith, but differs entirely from it, in being much more curved, in the absence of the characteristic prominence in the so-called ventral surface, and in its having much stronger and more distinct striæ, all of which characters combined give it an entirely peculiar aspect. Taking it, for the present, to be a *Eunotia*, I propose for it the name of *Eunotia falx*, or *E. falcata.*

I would now direct attention to a deposit, of which specimens were sent to me by Mr. Norman, under the name of Lüneburg. It is well known that there is an extensive deposit on the Lüneburg heath, in Hanover, and one part of it is known as the earth or Bergmehl of Oberrohe, near Lüneburg, another as that of the Lüneburg heath. These I find to be quite distinct from the deposit of which I now speak, as obtained from Mr. Norman; for this, as I have found, has a composition absolutely identical with that of the Lillhaggsjön Lapland deposit I have described. Not only the species are the same, but they are in the same proportions. In both *Eunotia triodon* presents in abundance its strange variations; in both the long sigmoid form, and also the sickle-like form, occur.

In short, I can detect no difference between these two deposits; besides the forms I have named, both contain *Eunotia incisa*, chiefly var. β; and both alike contain such forms as *Eunotia serra*, *Tetracyclus lacustris*, and others, inasmuch that I think it more probable that one of them has been misnamed, than that two deposits, in places so distant as Lüneburg in Hanover, and Lillhaggsjön in Lapland, should be identical in composition. Since all the specimens of earth from Oberrohe near Lüneburg, and the Lüneburg heath, that I have examined (and I have seen several different specimens in the natural state), differ from the Lapland earth, and since the Lapland earth is referred to its locality by the Zurich Association, I conclude that the earth in Mr. Norman's hand is really not from Lüneburg, but from Lapland. Perhaps there may be a place called Lüneburg in Lapland, near Lillhaggsjön; but this I have not been able to ascertain. In the mean time, this so-called Lüneburg deposit will supply observers with the two forms I have now described. I have noticed it in some other forms which I believe to be undescribed; to these I shall return on some future occasion.