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AN INTRODUCTION

TO

PHYSICAL MEASUREMENTS

WITH APPENDICES ON

ABSOLUTE ELECTRICAL MEASUREMENT, ETC.

BY DR. F. KOHLRAUSCH,

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Translated from the Second German Edition

BY THOMAS HUTCHINSON WALLER, B.A., B.Sc.

AND

HENRY RICHARDSON PROCTER, F.C.S.

LONDON

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PREFACE.

THE Author, in the preface to the second German edition, gives a sketch of the purposes which he hopes that the present book will serve. He says, a truth which all experience confirms, that the mere verbal teaching of physical laws is seldom of much use, tending frequently merely to confuse the student; while the simple performance of an experiment gives him confidence in himself and in the laws he is investigating. Another use of such a manual in the education of the scientific student is to lead him, by means of measurements which can be independently verified, to that knowledge of his powers which is so important when he has to do any original work. The greater part of the treatise is devoted to measurements of physical quantities. From this circumstance we have thought its object better expressed by the title we have placed at the head of it than by a literal translation of the German one.

Descriptions of apparatus are but rarely given, as students mostly have instruments provided for them, and seldom have to make their own apparatus, or to put it together.

The mathematical knowledge required is but very elementary, as the proofs of the formulæ are only given when they present no complex arguments.

The body of the work and the Appendix on Absolute Measure is, with little exception, an almost literal translation from the second German edition; but the Translators alone are responsible for the remaining Appendices and for several additional Tables.

THOMAS H. WALLER.
HENRY R. PROCTER.

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On the Protozoon Ophryodendron abietinum, *Claparède and Lachmann.* By the Rev. Thomas Hincks, B.A., F.R.S. (With Plate I.)

The very curious and interesting Protozoon which is the subject of the present paper was first characterised by Claparède and Lachmann in 1855, and placed by them amongst the suctorial *Infusoria*, in the group of the *Acinetina*.[1] It was subsequently obtained by Dr. Strethill Wright in Scotland, and described by him under the name of *Corethria Sertulariæ*.[2] At a later date he published a further account of it, and gave some interesting particulars respecting its reproduction.[3] I am not aware that any other naturalist has made it the subject of special investigation.

Some important points appear to have escaped the notice of the able observers to whom we owe our present knowledge of the *Ophryodendron;* indeed, what may be called the great peculiarity of its life is not referred to in any of the works which I have mentioned. I propose, therefore, to supplement the researches of my predecessors by recording the observations which I have had the opportunity of making on this remarkable animal.

The *Ophryodendron* has occurred to me twice ; I first met with it in the Menai Straits, near Bangor, where it formed groups on different portions of the common Hydroid *Halecium halecinum*. I afterwards found it at Ilfracombe on *Plumularia pinnata*.

Claparède obtained his specimens on some of the *Campanulariidæ*, and Wright on *Sertularia pumila;* so that it would seem to be a parasite of the Hydroida. The former states that the Protozoon manifests a preference for the

[1] It was first described in a memoir presented by these authors to the Academy of Sciences at Paris, in 1855, which ultimately formed the third part of their "Etudes sur les Infusoires et les Rhizopodes," published in the volume of the 'Mémoires de l'Institut National Genevois' for 1861.

[2] 'Edin. New Phil. Journal,' new series, for July, 1859.

[3] 'Annals and Mag. of Nat. Hist.' for August, 1861.

external surface of the calycles of the zoophyte as a residence, but was sometimes attached to the algæ growing upon it. My specimens were distributed over the stem and branches of the Hydroids, but occurred most frequently on the reproductive capsules and the hydrothecæ. I was fortunate in meeting with one exquisite colony, which had planted itself on a perfectly hyaline calycle of the *Plumularia*, from which the polypite had disappeared, so that I was able to examine the structure to the greatest possible advantage (Plate I, fig. 1).

I shall first describe the Protozoon as I have seen it, and indicate any points of difference between my own observations and those previously published.

The *Ophryodendron*, in what may be regarded as its perfect condition, occurs in colonies, each of which is composed of a number of individuals exhibiting two distinct types of form, which are grouped together, but not organically united. The first of these associated organisms (which I may call for convenience the *proboscidian*) (fig. 1 *a*, and fig. 4 *a*) consists of a somewhat pyriform or cup-shaped body, the interior of which is filled with a mass of minutely granular matter (sarcode), in which I was unable to distinguish either a nucleus or contractile vesicle. Claparède, however, observed within his specimens a body which he regards as probably a nucleus.[1] On the upper surface of this structure there is a depression or hollow, from which rises a proboscis bearing at its free extremity a tuft of delicate tentaculoid appendages. This proboscis is capable of very great extension and contraction; it can be drawn entirely within the body, but commonly projects to some distance from the top of it. Occasionally it is darted out with the greatest possible rapidity to several times the length of the body, and the tentacles, which ordinarily form a tuft at the extremity, are then seen to be distributed along the upper portion of it. When the proboscis is contracted the tentacles are generally motionless; but as soon as it is extended they start into lively and energetic movement, which continues until contraction again takes place. The proboscis tapers slightly towards its upper extremity, and, when retracted, is strongly wrinkled transversely.

In the specimens obtained at Bangor the body was perfectly sessile, as it is represented both by Claparède and Wright; but in the Ilfracombe examples it was supported on a delicate elastic pedicle (fig. 2). Other differences exist

[1] "Un corps obscur, dont on pouvait apercevoir parfois vaguement les contours," page 144.

between the Welsh and Devonshire forms, to which I shall refer more particularly hereafter; they probably indicate a specific distinction.

The second of the two organisms, which are grouped together in each colony, presents a striking contrast to the one which I have just described. It is of a graceful flask-like figure (fig. 1 *b* and fig. 3), and is mounted, at least in its adult state, on a slender setiform stem.[1] The body is some what ovoid, tapering to a point below, and above produced into a long and slender neck, which terminates in a small orifice. It is composed of a minutely granular sarcode, which occupies the whole interior with the exception of the upper portion of the neck, where there is a clear space extending for some distance below the oral aperture (fig. 3 *x*). Each adult colony of the *Ophryodendron* consists of a small number of the proboscidians and a much larger company of the lageniform individuals, the latter being grouped around the former and at some little distance from them (fig. 1). A large community may be composed of about eighteen members, of which six would belong to the one class and the remainder to the other. I have seen a single proboscidian with one attendant, and rarely one of the flask-shaped bodies is met with alone. A colony in full health and activity presents a most animated and interesting spectacle. The proboscidian members remain for the most part comparatively quiescent, with the extensile trunk contracted and motionless, or bending slightly hither and thither as if in search for something; occasionally, however, this organ is darted forth to an amazing distance with the speed of light, and the filamentary processes towards the extremity start into instant play. In a few seconds it is as suddenly and swiftly withdrawn. On the other hand, the lageniform bodies are in ceaseless motion, swaying themselves gracefully to and fro in all possible directions, now sweeping the surrounding water, now passing the oral extremity of the neck over the surface of the polypary on which the colony is planted, now reaching over towards the proboscidians, and bringing themselves into frequent contact with the trunk and tentacles of the latter. They are never still; the restless movement to and fro goes on without intermission. And it maintains the same character; there is first the searching here and there as if for food, and then the return towards the proboscidian zooids, against which they are almost sure to impinge. I have seen one of the flask-shaped bodies plunge the

[1] The lageniform body was furnished with a pedicle both in the Bangor and Ilfracombe specimens, but it differed in character in the two.

extremity of its long neck into the midst of the tentacles which crown the extensile proboscis. On the other hand, I am inclined to think that the somewhat eager movements of the trunk, which may be noticed when the lageniform individuals are in its neighbourhood, must be interpreted as a kind of feeling after them, and may indicate some peculiar relationship between the two.

Such is the ordinary life of the *Ophryodendron* colony, and, so far as I know, it is without parallel.

It is somewhat remarkable that neither Claparède nor Wright has given any full or accurate account of the flask-shaped bodies. The former seems to have confounded them with the proboscidians; he describes some of his *Ophryodendra* as having a vermiform body, whilst others were ovoid; but he evidently recognised no essential distinction between the two classes, and has not a word respecting their contrasted habits and their singular association in the colony. He has, indeed, figured two of the flask-shaped individuals (pl. v, figs. 2, 3), but the description of the plate informs us that they are *Ophryodendra, with the proboscis retracted.*

It is the more extraordinary that this most able observer should have confounded two forms so essentially distinct in structure and habit, as he has not only very accurately represented the lageniform body with its pedicle and small terminal orifice, but has also described its peculiar movements. Writing of his *Ophryodendra* with "vermiform" bodies, he says, "L'extremité antérieure était assez transparente tandisque la partie postérieure était en général beaucoup plus sombre et opaque. Cette extrémité antérieure s'agitait d'une maniere toute particulière et en tous sens, à peu près comme le bras d'un aveugle qui cherche avec inquiétude quelque chose à tâton."

There can be no doubt that when he wrote this passage he had one of the lageniform zooids in view.

Wright briefly notices these bodies, but did not determine the place which they hold in the life-history of the *Ophryodendron.* He seems to have observed them very imperfectly, and evidently knew nothing of the way in which they are associated with the proboscidian forms in groups or colonies.[1]

[1] The following are the only passages in Dr. Wright's papers referring to them:—"There exists, but not invariably, a long spindle-shaped and rather curved process of a granular tissue, similar to that of the cushion (the body of the *Ophryodendron*), also attached by one extremity to the upper surface of that body, and having at its unattached extremity a clear space which opens externally by a small oral aperture. This body is often absent, and I have seen it attached alone to the *Sertularia.* I am therefore inclined

I am unable to throw much light on the mode in which the *Ophryodendron* feeds. I have never observed the tentaculoid organs with which the proboscis is furnished capturing prey of any kind, but I have seen them applied, for a long time together, to the stem of the zoophyte, which was covered with a growth of Algæ, Polyzoa, Protozoa, &c. I can only suppose that they may obtain nourishment by suction, as Claparède has suggested. There can be no doubt that the active and incessant movements of the flask-shaped bodies are connected with the search for food.

Before proceeding to consider the very interesting question as to the relations existing between the two classes which I have now described as composing the *Ophryodendron* colony, I shall bring together the observations which I have made on the reproduction of this singular Protozoon.

Individuals of the proboscidian form are met with bearing buds in various stages of development. These originate on the surface of the body a little below the anterior depression from which the trunk rises (fig. 4, *b*); they are somewhat thickened at the base, and taper away towards the free extremity, and from a very early period are curved inwards towards the parent body. They exhibit the same minutely granular structure that is characteristic of the adult. There can be no doubt, I think, that the bud represented in fig. 4 would be developed into one of the lageniform individuals; it already bears a very close resemblance to them, while it differs altogether in shape from the proboscidian.

Claparède has figured a similar bud, and states that he had met with such not unfrequently. He will not "positively affirm," indeed, that the individual thus "fixé sur le dos d'un autre" is really developed from the latter by gemmation, but he seems to think it probable. I confess that I can see no room for doubt about it. The mode in which the nascent body is united to the substance of the proboscidian that bears it is such as to forbid the supposition that it is a mere parasite. Further, Claparède's specimens afforded an additional and very striking proof that it is a true bud, the product of the individual on which it is found. His *Ophryo-*

to consider it either a gemma, or a parasite belonging to the *Gregarinæ*."—'Edin. New Phil. Journ.,' new ser., for July, 1859.

"The body of the *Ophryodendron* frequently bears fusiform bodies, from one to four in number, which I have already described (in the foregoing passage) and which appear to be gemmæ."—'Annals of Nat. Hist.' for August, 1861.

I shall describe just now the gemmiparous reproduction to which Dr. Wright refers in these passages. The history of the "gemmæ" he did not succeed in tracing.

dendra were usually thickly covered with bodies bearing a general resemblance to the thread-cells of the Hydroida, which gave them a very marked and peculiar appearance, and similar bodies were present on the supposed buds, showing clearly an organic relationship between the two. Wright, as we have seen, noticed these "spindle-shaped" gemmæ, and has observed as many as four of them on a single proboscidian. I have seen a fully developed lageniform individual furnished with a pedicle attached to the parent (fig. 5), but I should suppose that the bud generally detaches itself before the appearance of a stem, and, fixing itself on the polypary of the zoophyte, developes one subsequently.

Whether the flask-shaped individuals are produced in any other way than as buds from the proboscidian, I have had no opportunity of determining.

Besides the gemmæ just described Claparède observed others on some of his specimens which were developed into the proboscidian form. They originate from the same part of the body as the lageniform buds, and soon exhibit the characteristic shape of the adult. In the stage of development represented by Claparède they are much swollen and rounded above, and taper slightly downwards; the proboscis is already distinguishable within the body, and in some cases it was observed in a state of extension before the connection between the bud and the parent had ceased.

I can see no reason for regarding these bodies as a gemmiparous product of the *Ophryodendron* which does not hold good in the case of the lageniform buds. Both are developed in the same region of the body, both blend at the base in the same intimate way with the substance of the individual that bears them, and both (in Claparède's specimens) were covered with the minute structures like thread-cells which gave such a peculiar appearance to the adult Protozoon.

I have also observed gemmiparous reproduction in the lageniform zooid. In this case the bud originates at some distance below the commencement of the neck (fig. 6), and assumes, when mature, the form of the parent.

In a single instance two individuals of this class were met with, the pedicles of which were united for a portion of their length. There can be little doubt that the pair, thus organically joined together, were produced by longitudinal fission.

I was not so fortunate as to observe the ciliated embryos which have been noticed by Claparède and Wright. They are developed within the proboscidian zooid, and are described

as oval or elongate bodies, flattened on one aspect which is clothed with cilia, and slightly convex on the other. They are furnished, according to Claparède, with a contractile vesicle and with a few of the bodies resembling thread-cells. Wright observed individuals "slowly moving on the zoophyte," while others, "attached, were putting forth the rudiments of the proboscis."

The questions remain, what is the precise nature of the dissimilar organisms associated in the *Ophryodendron* colony? and what relation does one class bear to the other? As to the former, two views are possible—(1) that the proboscidian and lageniform bodies are distinct animals, united in a species of "commensalism;" and (2) that they are different forms of one and the same animal, zooids belonging to a single life-series.

As I entertain no doubt, for the reasons previously stated, that the lageniform bodies borne so commonly on the *Ophryodendron* are *true buds*, I cannot hesitate to accept the latter of these views. Confirmatory evidence, too, in its favour is found in the close resemblance in general structure between the two bodies. In the Devonshire specimens, as I have before stated, both forms were furnished with a slender elastic pedicle, and in both this pedicle was curved in a similar way (figs. 2, 3). Both exhibited the same minutely granular substance, and in Claparède's specimens both contained the naviculoid corpuscles described by this observer.[1]

It is difficult to conjecture what the precise relation between these associated organisms may be, and I have no theory to offer on this point. While watching the energetic and unresting search, as it were, of the flask-shaped bodies, and their constant return to the neighbourhood of the proboscidians, against which they are almost sure to strike, one might almost fancy that the former were engaged in conveying nutriment to the latter;[2] but more extended observations are required to elucidate the internal economy of the colony.

If my view of the history, then, be correct, the *Ophryoden-*

[1] Claparède evidently had no suspicion that the two forms which he had observed did not belong to one and the same animal, for he includes them both in his diagnosis of the species:—"Corps tantôt vermiforme, tantôt plus ou moins ovoïde." It is true that he did not determine the peculiar structure of the lageniform bodies, but the general character of the two zooids and the mode of their association led him to unite them without hesitation.

[2] I merely give this as the fancy suggested by the appearances, and not as the rationale of the facts.

dron is a dimorphic animal, that which may be called the primary zooid giving origin by gemmation to bodies unlike itself, which, on becoming free, group themselves around the parent organisms, and lead with them an associated life. To some extent a parallel may be found in the observations of Haeckel on certain Geryonidan medusæ, which he represents as producing by gemmation medusæ of a different family type (*Æginidæ*). Indeed, amongst the Hydroida generally heteromorphism is a common condition; and the *Corymorpha* which produces free medusiform zooids, or the *Myriothela* which gives origin to fixed buds unlike itself, on which the true reproductive buds are borne, presents, so far as its heterogenetic tendencies are concerned, a strict analogy to the case of the *Ophryodendron*.

I have already identified the form which I obtained in the Menai Straits (figs. 4, 5) with the *O. abietinum* of Claparède and Lachmann (= *Corethria Sertulariæ*, Wright); but there is a point of difference between the two which must not be passed without notice. The curious structures bearing a resemblance to the thread-cells of the Hydroida ("petits corpuscules tout à fait semblables aux organes urticants des Campanulaires") which occurred in Claparède's specimens were altogether wanting in mine. They are described as scattered through all parts of the body, but extremely variable in number. In some cases they were so abundant as to render the body almost opaque; occasionally, but rarely, they were altogether absent. They were noticed in the ciliated embryos as well as in the adults. I could detect no trace of them in the specimens that came under my observation; and, whatever their precise nature may be, as they were sometimes wanting, they can hardly be regarded as a distinctive characteristic.

The Ilfracombe specimens (figs. 1, 2, 3) differed in some important particulars from those obtained in Wales. In the first place, *both* zooids are furnished with a pedicle, which was always curved in a peculiar way (figs. 2, 3); whereas in *O. abietinum* the proboscidian zooid is perfectly sessile, and the flask-shaped bodies are supported on a straight setiform stem, which penetrates the sarcode mass for some distance. The body of the proboscidian zooid was decidedly cup-shaped, and not elongate and somewhat pyriform, as in Claparède's species, and wanted the very marked cleft in the upper surface which distinguishes the latter. I believe, also, that the proboscis was furnished with a much smaller number of the tentaculoid processes, but of this I cannot speak with absolute certainty. The southern

form seems well entitled to rank as a distinct species, which may be characterised as follows:

Genus OPHRYODENDRON, Claparède and Lachmann.

O. PEDICELLATUM, n. s.

Zooids of both classes furnished with slender curved pedicles; body of the proboscidian zooid cup-shaped.

Hab. On *Plumularia pinnata.* Dredged off the Capstone, Ilfracombe.

It is impossible to speak with certainty of the systematic position of the *Ophryodendron* in the absence of definite information as to its mode of feeding. It seems probable, however, that the processes on the proboscis are analogous to the suctorial organs of *Acineta* or *Podophrya*, and that its place is amongst the *Infusoria suctoria.* At the same time, its dimorphism and the disposition of its suckers on a retractile trunk are characters which mark it out as the type of a separate family at least.

RECENT RESEARCHES *in the* DIATOMACEÆ. By the Rev. EUGENE O'MEARA, A.M.

IV.—CYMBELLEÆ.

PFITZER ranges the *Cymbelleæ* immediately after the *Naviculaceæ.* Some authors, as Smith and Rabenhorst, have placed these families widely apart; while others, as Kützing, Ralfs, Grunow, and Heiberg, have treated them as more intimately related, the connecting link between them being found in the common features of a median line and central and terminal nodules. Pfitzer places the two families in immediate proximity on account of the genera *Brebissonia* and *Anomœoneis,* which, while they resemble the *Naviculaceæ* in the symmetrical outline of the valves, are related to the *Cymbelleæ* by the unsymmetrical character of the cell-contents. Comparing the two families together in regard to the latter, the principal difference is that the *Cymbelleæ* have but a single endochrome-plate, while the *Naviculaceæ* have two. The group embraces the following five genera:—1. *Brebissonia,* Grun.; 2. *Anomœoneis,* Pfitz.; 3. *Cymbella,* Agardh; 4. *Cocconema,* Ehren.; 5. *Encyonema,* Kütz.

1. *Brebissonia.*—This genus was established by Grunow

to receive a single species, *B. Boeckii* = *Cocconema Boeckii*, Ehr. and Kütz. = *Doryphora Boeckii*, W. Sm. The author had the rare opportunity of observing this form in a living state, having found it abundantly in the harbour of Pillau; and he remarks that in the outline of the valve there is no want of symmetry, so that in this aspect it might be regarded as a stipitate *Navicula*. On account of this symmetry of form it is plainly distinct from *Cocconema*, in which genus Ehrenberg, Kützing, and Ralfs placed it; it also differs from *Doryphora*, in which it was placed by Smith, by the presence of a central and terminal nodule, formed similarly to those of the genus designated *Frustulia* by Pfitzer, and which has been referred to before. But as the internal structure of *Brebissonia* is unsymmetrical throughout, it stands in more intimate relationship to *Cocconema* than to any other genus. Besides the plasm-sac lying on the cell-wall and most highly developed at the extremities, we have, in *Brebissonia*, a large central plasm-mass which occurs in all the *Cymbelleæ* and *Naviculaceæ*. The mass of thicker plasm interposed between the endochrome-plate and the cell-wall which is found in *Frustulia*, is present also in *Brebissonia*; in the latter case, as in all the *Cymbelleæ*, only one of these occurs in each frustule, and lies across the middle of a girdle-band; in *Brebissonia* it is large and semiglobose. The single endochrome-plate covers the same girdle-band, folding itself over the valves on both sides till its free edges nearly meet on the opposite girdle-band. Division commences with the separation of the endochrome-plate into similar halves by an incision from the extremities.

As Grunow gives no special characters of the genus *Brebissonia*, the detailed description of its peculiarities by Pfitzer is the more valuable, and exhibits the relationship of this interesting form more clearly than has been done hitherto.

2. *Anomœoneis*.—The only form which with certainty can be assigned to this genus, according to our author, is *A. sphærophora* = (as he thinks) *Navicula sphærophora*, Kütz. It bears a strong resemblance to *Navicula ambigua*; its valves, however, are not furnished with transverse striæ, but with fine puncta arranged in quincunx. On one side of the central nodule the striation fails, so as to leave a smooth space reaching the margin; this smooth space on the under valve lies immediately under and parallel to the corresponding smooth space on the upper. *Anomœoneis*, therefore, like the *Cymbelleæ*, is unsymmetrical, notwithstanding its decidedly symmetrical outline. The internal structure corresponds; a

broad central plasm-mass is noticeable on both girdle-bands, but this is obviously narrower on the side of that girdle-band on which the middle of the single endochrome-plate is situated. Division in this case, as in that of *Brebissonia*, takes place by means of an incision proceeding from the extremities.

The unstriated space on one side of the valve which is characteristic of *Anomœoneis*, occurs in a similar manner in *Navicula sculpta*, Ehren., and to a slighter extent in *N. bohemica*, Ehren., so that for this reason the author considers these latter-named species may be included in *Anomœoneis*.

I have some doubt as to the identity of *Anomœoneis sphærophora*, Pfitz., with *Navicula sphærophora*, Kütz. The failure of the striation on one side of the central nodule extending to the margin of the valve, a feature so characteristic of *A. sphærophora*, does not appear in Kützing's figure of *N. sphærophora*, nor in that of Smith. Donkin ('Brit. Diat.,' pl. v, fig. 10) does not notice this peculiarity; it is not apparent in the forms so named by Eulenstein (Diats. p. typ.), nor in those in my possession, which in all respects agree with the figures referred to. I should have supposed Pfitzer had *Navicula sculpta* in view, were it not that he refers to this latter as a distinct form. While differing from the author as regards his opinion that the failure of the striæ on one side of the central nodule removes this species from the symmetrical forms and includes it in the unsymmetrical —in the sense in which Grunow and Heiberg used the terms symmetrical and unsymmetrical,—I cannot fail to express my obligation to him for the new light he has thrown on the relationship of this species, as well as that of *Brebissonia*, by his observations on the unsymmetrical characters of the contents of the frustule.

3. *Cymbella*. 4. *Cocconema*. 5. *Encyonema*.—These three genera correspond in this common feature, that the portions of the valves at the opposite side of the longitudinal axis are unsymmetrical. One margin is always more convex than the other; in many forms one margin is concave. The section of the cell is sometimes rectangular, sometimes strongly trapezoid; a longitudinal line and nodules occur as in *Navicula*, *Brebissonia*, and *Anomœoneis*. The longitudinal line is never straight except in the case of *Encyonema*, and always divides the valves into two very unsimilar segments. The striation is sometimes costate, for instance, in *Cymbella Ehrenbergii*, Kütz.; sometimes moniliform, as in *Cocconema asperum*, Ehrenb., in which case there are depressions arranged in linear order.

The generic distinctions depend on the circumstance that *Cymbella* is free, *Cocconema* stipitate, and the frustules of *Encyonema* enclosed in tubes. In *Encyonema* the girdle-band lies somewhat obliquely on the frustule, so that the transverse section is slightly rhomboid. The structure of the primordial cell in these three genera is very similar to that of *Brebissonia* and *Anomœoneis*. In all the forms investigated the single endochrome-plate lies with its middle portion on the more strongly arched broader side, and thence folds itself over the valves, its free margins lying on the concave or less convex, and at the same time smaller side. The division of the endochrome-plate takes place by means of an incision proceeding from the extremities. The single plasm-band described by Ehrenberg, and regarded by him as a spermatic gland, presents itself on the more strongly arched girdle-band, and thence passes, interposed between the cell-wall and the endochrome-plate, slightly over on to the valves.

The middle plasm-mass observable on the valves in *Cymbella gastroides*, Kütz, and *Cocconema Cistula*, Ehrenb., is broader on the convex than on the concave side, and the reverse in *Cymbella cuspidata*, Kütz. In the smaller forms *Cymbella scotica*, W. Sm., for instance, no distinction in breadth is noticeable. In the last-named species the endochrome-plate is separated into four portions by a roundish lacuna passing from side to side on each valve.

Pfitzer has never had the opportunity of observing the formation of auxospores in the *Cymbelleæ*, but gives an interesting summary of what other observers have recorded as follows:—Thwaites, in 1847, noticed that in *Cocconema lanceolatum* and *Cocconema Cistula*, two mother-cells invested with gelatinous matter produced two auxospores, which lie parallel with the mother-cells. Carter, in 1856, confirmed this observation in *Cymbella pediculus* (= *Cocconeis pediculus*), and Smith, not only in the two species investigated by Thwaites, but also *Cocconema parvum*, W. Sm., and *Encyonema prostratum*, Ralfs. Lastly, Lüders gave a full description of the process in *Cocconema Cistula*. The two cells co-operating in this instance appear generally if not always to issue from the division of a single mother-cell; they separate themselves according to Lüders into two plasm-masses superimposed one on the other, which then conjugate in pairs.[1]

[1] Fr. Schmitz has a paper on the "Formation of the Auxospores of Cocconema Cistula" in the 'Bot. Zeit.,' April 5, 1872.—Ed.

V.—Amphoreæ.

Nearly related to the *Cymbellæ* are the *Amphoreæ*, in which are included the two genera *Amphora* and *Epithemia*. These agree with one another, as also with the *Cymbellæ*, in the lunate and decidedly unsymmetrical form of the valves, and in the usually trapezoid transverse section of the cell. *Amphora* possesses on each valve three distinct nodules, connected together by two longitudinal lines which divide the strongly arched valves into two parts, so dissimilar that the smaller portion is sometimes scarcely apparent, although it possesses striæ precisely the same as the broader section. An *Amphora* may thus be regarded as a *Cymbella* with one portion of the unsymmetrically divided valve nearly obsolete.

In *Epithemia* the nodules and median line are absent, or but slightly observable. The two forms agree in this feature, namely, that the single endochrome-plate passes over the median line without suffering any interruption similar to that which occurs in *Cymbella*. In other respects the conditions of the endochrome-plate are the same as in the group last referred to, except that in *Amphora* and *Epithemia* the middle of the endochrome-plate does not lie on the broader but on the narrower girdle-band. Further, the plasm-bands situated on the cell-wall in this case fail altogether, while the central mass is distinctly observable. It appears necessary, therefore, to unite *Amphora* and *Epithemia* into a distinct group, the more so because they correspond in a remarkable feature of their mode of growth, namely, that they both usually attach themselves by the smaller girdle-band to larger Algæ or other water-plants. Kützing, Grunow, and Pritchard place *Epithemia* with the *Eunotiæ*, which latter Pfitzer observes are very differently constructed, as will be seen hereafter, as respects the cell-contents. *Amphora* was by Kützing wrongly referred to the *Naviculaceæ*. It stands near the *Cymbelleæ*, to which the two other observers named referred it; but it is still more intimately related to *Epithemia*, for the form and position of the endochrome-plate and the absence of the plasm-band is a character of greater importance in the estimation of the author than the more or less distinct development of the nodules and median line, the presence of which has nothing to correspond with it in the internal structure.

In *Amphora* the single endochrome-plate is only slightly scalloped. It separates at its middle portion by an incision

commencing at the ends. This in *Amphora ovalis*, Ehren., is very wide; in *A. salina*, W. Sm., on the contrary, it is very narrow. The valves are altogether brown. On the broad more strongly arched girdle-band each edge of the plate exhibits at the middle a deep indentation, which is noticeable when the valve is seen in profile. Two symmetrically situated oil-globules are found in the central portion of the frustule. A nucleus is also distinct, which separates into two considerably before the process of cell-division commences.

The newly-formed valves in *Amphora* are at first flat. In consequence of the peculiar structure the line along which the two corresponding points of the valves move away from one another, in the process of division, is not straight but curvilinear, and nearly parallel to the convex side of the girdle-band. When the *Amphoræ*, after division, do not separate from one another, the daughter-cells do not form filaments but ellipsoids, in which the individual frustules stand in the same position to one another as the sections of a melon.

The *Epithemiæ*, as respects their inner structure, distinguish themselves from the *Amphoræ* by the fact of their endochrome plate being much scalloped, so that the valves exhibit alternate light and brown transverse bands. This is especially the case on the side of the broad girdle-band, only that here the scallops of the endochrome-plate appear to be shorter. The division of the endochrome-plate takes place through an incision proceeding from the ends.

In the middle plasm-mass no nucleus was noticed in any of the forms examined—*Epithemia gibba* (Ehren.), Kütz., *E. ventricosa*, Kütz., *E. turgida* (Ehren.), W. Sm., *E. Zebra* (Ehren.), Kütz., *E. Sorex*, Kütz. But invariably one, usually two, more highly refractive corpuscles were found of a spherical shape, which treatment with perosmic acid proved not to be oil-globules, but plasmatic structures. Four and even more of these were sometimes met with in each cell; and as no single nucleus was present, there is reason to suppose that these structures should be regarded as nuclei. They increase in number by division, growing longer, and stringing themselves in a direction parallel to the longitudinal axis of the cell, but less frequently in a direction slightly inclined to it. The daughter-cells of *E. gibba* and *E. turgida*, while still remaining attached, possess each two such bodies, while in other cases there is only one long after separation. Probably these structures, as the author thinks, are analogous to the plasm-bands of *Anomœoneis*, *Cymbella*, &c., with which they

correspond in the feature, that they do not lie free in the granular plasm-mass, but on the plane of the convex girdle-band. The strong costæ which the valves of all *Epithemiæ* exhibit are not, the author informs us, canaliculi or tubes, as Smith supposed, but, as Pritchard correctly observed, solid strips projecting inwards.

The mode of development of the auxospores supplies a strong reason for uniting *Epithemia* and *Amphora* in a group distinct from the *Cymbellæ* and *Eunotiæ*. According to the observations of Carter in *Amphora ovalis*, of Thwaites in *Epithemia turgida* and *E. gibba*, of Smith in *E. ventricosa*, *E. gibba*, *E. Sorex*, and *E. Zebra*, of Lüders in *E. turgida*, and *E. Zebra*, of Itzigsohn in *E. Goeppertiana*, Rab., as well as those of the author in *E. gibba*, two auxospores always originate from two mother-cells, which do not lie parallel with the former, but at right angles to them. They grow also in a direction different from the *Cymbellæ*. In the case of the *Eunotiæ* only a single spore is usually formed.

The distinction between *Cymbellæ* and *Amphoræ*, founded on the relative position of the auxospores, would be untenable if the statement of Itzigsohn, in conflict with the observations above referred to, be confirmed, namely, that in many *Epithemiæ* the mother-cells and auxospores lie parallel.

The process of spore-formation, according to Thwaites and Smith, is as follows:—Each primordial mother-cell throws out two protuberances, which increase towards one another until they finally join, and ultimately unite between the parted valves of the mother-cells. According to Lüders, the parting of the valves of the mother-cells precedes the conjugation.—[*From the 'Journal of Botany.'*]

Contributions *to the* History *of the* Polyzoa. By the Rev. Thomas Hincks, B.A., F.R.S.

(Plate II.)

I.—The "Germ-capsule."

In a previous paper[1] I have given a brief account of my observations on the "germ-capsule" of the Polyzoa, with special reference to Dr. Nitsche's criticisms on the researches of the Swedish naturalist Smitt.[2] Since the publication of that paper I have had the opportunity of further investigation, and have both confirmed the results already announced and cleared up some points which were involved in doubt. I have also had the advantage of studying the complete exposition of his views, which Nitsche has since given us,[3] and of which his communication to this Journal was only "a preliminary sketch." Under these circumstances I propose to discuss more fully the very interesting question as to the true nature and function of the so-called "germ-capsule."

I shall first, however, bring together the accounts of this structure which have been given by various authors, so as to exhibit the history of opinions respecting it.

The dark, circular bodies ("germ-capsules" of Smitt), which are generally present in many of the zooecia, on any given polyzoary, are much too conspicuous to escape the notice of observers, and accordingly we find a reference to them in most works on the Polyzoa. Amongst the earlier writers Ellis describes and figures them under the name of "black spots." "These," he says, "are nothing but the dead polypes, or remains of the animals once inhabiting these cells, of which I had evident proof in my last journey to the sea-coast; for after I had examined this coralline (*Bugula plumosa*), with its polypes alive in sea-water, I laid the specimen aside, and upon examining it again some time after, I found the lifeless contracted animals exhibited the appearance above mentioned" ('Nat. Hist. of Corallines,' 1755, p. 34).

Dr. Grant, in his remarkable papers "On the Structure and Nature of *Flustræ*,"[4] has noticed the small red or brown spots in the centre of the cells of *Flustra foliacea*, which he considered to be "the last remains of the dead polypi."

[1] 'Quart. Journ. of Micros. Science' for July, 1871, p. 235.

[2] Ibid., for April, 1871.

[3] 'Beiträge zur kenntniss der Bryozoen,' ii Heft. 3. 'Ueber der Anatomie und Entwicklungsgeschichte von *Flustra membranacea*,' 1871.

[4] 'Edinburgh New Phil. Journ.' for 1827, pp. 107 and 337.

In describing *F. carbasea*, too, and comparing it with the former species, he speaks of "the same dark round spots in the centre of those cells which have lost their polypi." In his account of the development of the ova he remarks that they "first make their appearance at the narrow base of the cells as very small, pale-red, gelatinous spheres, and the polypi of such cells are generally removed, and only a small, round, dark brown spot is seen in their stead, in the centre of the cells;" and he adds the important observation—"When the ovum has escaped from the cell *the dark round spot in the centre of the cell enlarges, and a new polypus shoots out at that point* . . . . The same cells may repeatedly produce ova and polypi, and the whole zoophyte retain its energy for several seasons." It would seem, therefore, that he noticed a change taking place in the dark body, and connected with it the appearance of a new polypide in the zooecium, and so far anticipated the observations of Smitt.

J. V. Thompson[1] noticed that the lower portion of the stomach (which he calls "the viscus") acquired a spherical shape, and opaque yellowish colour, and persisted after the death of the polypide in many of the Polyzoa; and he regarded the body thus formed as "most probably an ovum or ovarium."

Farre, in his well-known paper on the "Ciliobrachiate Polypi,"[2] gives an account of these bodies, and conjectures that they are probably connected with the process of reproduction, though whether they were to be regarded as ovaries or immature ova he was unable to determine. "From their dark colour," he says, "they are generally very conspicuous, especially as they remain in the cells long after the animal has perished and disappeared from them. From this circumstance it might be imagined that they resulted from decomposition, were they not also frequently seen in the living animal. Moreover, they have a definite form and size, and, when removed from the cell and carefully examined, are found to consist of a delicate transparent membrane, enclosing a brown granular matter, to which their colour is due." This observation is an important one.

In a paper on *Cellularia* (Bugula) *avicularia*,[3] Pallas, Nordmann has described the structure to which Smitt gives the name "germ-capsule," and rightly regards it as formed

[1] 'Zoological Researches and Illustrations,' 1830, art. v.

[2] 'Philosophical Transactions' for 1837, p. 400.

[3] 'Voyage dans la Russie Méridionale et la Crimée, executé en 1837, sous la direction de M. Anatole de Demidoff,' 1840, vol. iii, 702, note.

out of a portion of the body of the polypide, though he mistook its function, and considered it to be an egg.

Van Beneden[1] regards the "round, dark-coloured bodies" which he noticed in *Flustra foliacea* through the walls of the zooecia as eggs. He describes them as destitute of cilia, and quite motionless, and says that they are hatched in the deserted cells, for he had seen very young individuals in the old zooecia. From some of his figures we may, I think, infer that he had observed the development of a polypide from the "germ-capsule," though he misinterpreted the appearances that came under his notice.

In plate viii, fig. 4 *n*, a germ-capsule is represented in a zooecium of *Alcyonidium gelatinosum*, from which an elongate body, which is clearly a rudimentary polypide, is sprouting. The description explains that it is a "young polypide originating from a bud;" but the supposed "bud" is evidently one of the dark bodies which Van Beneden has elsewhere described as ova. It may also be remarked that the figure very fairly represents the general appearance of the polypide in course of development from the germ-capsule.

In plate vii, figs. 15 and 16 are described "as young polypes attached to the valves of the egg," but are really polypides budding from the germ-capsule, which Van Beneden, as we have seen, identified with the ovum.

Reid[2] describes a prolongation of the stomach below, which he regards as an "appendix" of the digestive sac, or a "separate organ." "Its inner surface," he says, "is so thickly covered with reddish-brown granules, or, more properly speaking, minute cells, as to be quite opaque." He remarks that this "appendix" is, in some cases, much larger in proportion to the stomach than in others. He also observed many bodies, "each composed of reddish-brown nucleated cells, enclosed in a membrane" amongst the broken-down zooecia, which he considers to be ova.

Dalyell[3] notices the "dark globular substance" which is found in many of the cells of *Bugula ciliata*, and figures the same body in *B. avicularis*. He speaks doubtfully of the opinion that it is the residuum of the body of the polypide, but has no special observations of his own to record.

In a paper on *Flustrella hispida*,[4] Dr. Redfern has sug-

[1] 'Recherches sur l'Anatomie, la Physiologie et l'Embryogenie des Bryozoaires,' 1845, p. 58.

[2] "Anatom. and Physiolog. Observations on some Zoophytes," 'Annals Nat. Hist.,' xvi, 1845, p. 385.

[3] 'Rare and Remarkable Animals of Scotland,' vol. i, p. 240, 1847.

[4] "Flustrella hispida and its Development," 'Quart. Journ. Mic. Sci.,' vol. vi, p. 96, 1858.

gested a new interpretation of the "dark bodies," by naming them "unciliated ova or statoblasts."

In 1861 I published a "Note on the Ovicells of the Cheilostomatous Polyzoa,"[1] in which the "germ-capsule" was regarded as an egg, but an egg of different character from those which I supposed to be produced in the ovicell. It was described as a non-ciliated ovum developed in the zooecium,[2] and only liberated after the death of the polypide. The view which I then entertained was that it might prove to be the equivalent of the statoblast of the *Phylactolæmata*.

We come now to Smitt's observations on the "dark bodies," which were published in 1863 and 1865.[3] In his first paper, after an historical review of the literature of the subject, he gives the result of his investigation into the various modes of development by gemmation which prevail amongst the Polyzoa. Amongst these he places reproduction by the formation of a germ-capsule (groddkapsel). In the following passage he explains his use of this term:—"As will appear from what follows, the 'dark bodies' in their composition and significance approach most nearly the egg-formation ('äggbildning'), which Allman, in the case of the freshwater Bryozoa, has named *statoblast*. As, however, the former not only serve as receptacles for eggs, which escape as embryos, but also, as will appear hereafter, contain within them buds for the formation of a new polypide in old cells from which the original polypides have disappeared, I shall employ in what follows the name 'groddkapslar' (germ-capsules)." He then describes the germ-capsule as it occurs in various species, and points out that it originates from the digestive sac of the polypide, to which, indeed, it bears a striking resemblance in structure and appearance. He had seen one, definitely formed, which was still firmly attached to a portion of the stomach-walls. He had observed further (in *Alcyonidium parasiticum*), that in old zooecia, the polypides of which had vanished, the germ-capsule was often to be seen attached to newly formed buds. The relation, however, between the two was not so intimate but that the buds, especially when advanced in development, often broke loose from the germ-capsule and lay free in the zooecium. In this case he did not succeed in establishing the connection of the bud from

[1] 'Quart. Journ. Mic. Sc.' for October, 1861.

[2] Nitsche and Claparède were quite right in supposing that at this time I mistook the germ-capsule for an ovum. I shall refer to this hereafter.

[3] "Bidrag till Kännedomen om Hafs Bryozoernas utveckling," 'Ups. Univ. Årsskrift,' 1863; "Om Hafs Bryozoernas utveckling och fettkroppar," 'Öfversigt af Kongl. Vet. Akad. Förh.,' 1865.

its origin with the germ-capsule and its derivation from the latter. In *Lepralia Pallasiana* he noticed one portion of the germ-capsule of a lighter colour than the rest, and at the extremity of this part a process of a light-grey colour and homogeneous granular substance was budding. In another case a bud was attached to the side of the light portion, furnished with a crown of rudimentary tentacles.

In his second paper (1865) Smitt has embodied the results of further observation, in addition to those already obtained, and has supplied illustrative figures, which are most of them, however, much too small to exhibit satisfactorily the structural details. I shall bring together here some of his observations on the different species that came under his notice, as I shall have to refer to them hereafter when I give the results of my own investigations.

In *Scrupocellaria scruposa* a dark mass was noticed in the older cells, at whose side a bud was placed, which had already the rudiments of tentacles.[1]

In *Bugula fastigiata* the germ-capsule exhibited the same character as in the last species, but the buds in this case were much more distant from it, and their relation to it was difficult to determine.

In a zooecium of *Flustra membranacea*, a germ-capsule was seen from which tentacles and the cavity of the alimentary canal had been developed by budding.

In *Eucratea chelata* a kidney-shaped germ-capsule was met with, light-coloured at one end, and at the other thickly covered with dark spots. The new parts arising from it (digestive canal, with the crown of tentacles and muscles) were already fully developed.

In a specimen of *Alcyonidium gelatinosum* a germ-capsule was found attached to the stomach-wall, and another of lighter colour lying near it; and from this Smitt infers that several germ-capsules may be developed successively from the walls of the stomach.

In *Membranipora unicornis*, a germ-capsule of an oval form was observed *near a bud* which exhibited twenty-four rudimentary tentacles.

In connection with the development of the germ-capsule Smitt describes a mass of oil-globules ("fettkroppsmassa") formed in a particular way, which constitutes a foundation for the production of the new parts.[2]

[1] "Vid hvars sida ligger en Knop, som nyst fått de första anlagen till tentakler," p. 23, plate v, fig. 1 *g*.

[2] The following is the passage referred to:—"Ett bland Bryozoernas egendomligaste reproduktionssätt är deras Groddkapselbildning, vid hvilken lika-

In 1870 the accomplished biologist, Edouard Claparède, whose early loss every student of science must deplore, published in Siebold and Kölliker's 'Zeitschrift' a paper on "The Anatomy and Development of the Marine Bryozoa,"[1] in which he reviewed Smitt's theory of the germ-capsule, and offered a very different interpretation of the facts on which he supposed it to be grounded. The "brown body" he regards as a secretion from the endocyst, and in nowise endowed with any reproductive function. The supposed buds which have been noticed in adult zooecia, and which he had himself examined, he considers to be the result of a "retrogressive metamorphosis" of the original polypides, which, under certain circumstances, shrink back into this rudimentary condition, passing through the same stages in their decline as in their progress towards maturity, but in an inverse order.

With reference to the first point it is sufficient to remark that the development of the germ-capsule from the body of the polypide has been conclusively established by direct observation, and is placed beyond a doubt; while the extraordinary theory invented to account for the presence of buds within the adult zooecia is not only unsupported by any analogy or positive evidence, but gratuitously creates a difficulty where none is presented by the actual facts. The bud from the endocyst in the adult zooecium (to which Claparède refers, and *not to the germ-capsule bud*) exactly resembles that in the forming zooecium on the margin of the polyzoary, and has no doubt a similar history.

In 1871 Smitt's views were again subjected to criticism by Dr. Hinrich Nitsche.[2] He agrees with the Swedish zoologist in regarding the "brown bodies" as originating from the polypide, but denies that they have any reproductive function. He has found in them Diatomaceæ, sponge-spicules, thread-cells, &c., a fact which clearly proves that they once formed a part of the stomach of the polypide, and which is also fatal, as he thinks, to Smitt's view of their office. He contends that they never give origin to buds, as this observer affirms, and

ledes en fettkroppsmassa, om också på ett annat sätt upkommen, utgör grundlaget för de nya delarnes uppträdande." 'Om Hafsbryozoernas utveckling,' p. 23.

[1] "Beiträge zur Anatomie und Entwicklungsgeschichte der Seebryozoen," 'Zeitschrift' for December, 1870, p. 147.

[2] "Some interesting points concerning the mode of reproduction of the Bryozoa," 'Quart. Journ. Micr. Soc.' for April, 1871.

"Beiträge zur Kenntniss der Bryozoen," ii Heft. 3. "Uber die Anatomie u. Entwicklungsgeschichte von *Flustra membranacea*," 'Zeitschrift für wissensch. Zool.,' xxi Bd., 4 Heft.

have nothing to do with the appearance of fresh polypides in the adult zooecia. The buds which are frequently met with in zooecia from which the original tenant has disappeared, are a product of the endocyst and not of the "brown body;" but as they are commonly developed in close proximity to the latter they have been mistaken for a part of it, and hence the error of interpretation. As I shall point out hereafter the endocyst-bud is totally distinct from the germ-capsule-bud. Both are of common occurrence; Nitsche is correct in his account of the former, but has overlooked the latter.

In a paper in this Journal, already referred to, I have briefly replied to Nitsche's statements by detailing my observations on the formation and development of the germ-capsule which are confirmatory of Smitt's views. The latter has also published a short rejoinder,[1] in which he expresses his confidence in the results previously announced, and refers to my paper for corroborative evidence.

I may conclude this historical survey by mentioning that, in a paper[2] very recently published, Nitsche has recorded the occurrence of the germ-capsule in one of the freshwater Polyzoa (*Alcyonella*). He states that in a specimen which came under his observation the ovary and embryos occupied so fully the upper space in the zooecium that the polypide was pressed down, and, as it were, forced into a "retrogressive metamorphosis," which resulted in the formation of a "dark body."

It appears from this historical sketch (1) that the dark-coloured, more or less spherical bodies that occur in the zooecia of the Polyzoa have attracted very general attention; (2) that several of the earlier observers had determined their formation out of a portion of the body of the polypide; (3) that the definite structure which they exhibit had been noticed and accurately described by Farre; (4) that in one or two cases the connection of these bodies with the appearance of fresh polypides in adult zooecia had been surmised or imperfectly observed long before the doctrine of the germ-capsule was enunciated by Smitt.

I shall now proceed to describe the origin and development of the germ-capsule more fully than in my previous paper, and shall be able to render the details more intelligible by the aid of the accompanying figures (figs. 1—7.)

Every student of the Polyzoa must have noticed that the

[1] "Remarks on Dr. Nitsche's Researches on Bryozoa," 'Quart. Journ. Micr. Sci.' for July, 1872.

[2] "Betrachtungen über die Entwicklungsgesch. u, Morphologie der Bryozoen," 'Zeitschrift' for 1872, 4 Heft, p. 467.

life of the individual polypides is comparatively ephemeral. In most cases a large proportion of the zooecia are found to have lost their original tenants, while at the same time the polyzoary retains its full vitality, and along the margin fresh additions are constantly being made to it by the growth of new buds. Not uncommonly almost all the zooecia covering the lower or older portions of a tuft of *Bugula* or *Bicellaria*, for example, are thus emptied of their polypides; but towards the upper extremities of the branches all are occupied by an active population, and along the outer edge zooecia are to be seen in every stage of development. The disappearance of the polypide, however, is a comparatively unimportant incident in the life of the colony; and ample provision exists for securing new tenants for the deserted dwellings. The zooecium which has lost its polypide is, in time, filled by another, and may very possibly be the home of a succession of occupants.

The renewal of the polypides in the adult zooecia is effected in two ways—(1) by the formation of a germ-capsule, and (2) by gemmation from the endocyst or inner wall of the zooecium.

1. Renewal of the polypide by the formation of a germ-capsule.

If we examine a specimen of one of the erect, plant-like Polyzoa, we shall find, as I have stated, along the upper edge of the branches zooecia in course of formation, and exhibiting every degree of development; immediately below these will be zooecia in which the polypides are fully formed, but immature; below these, again, will extend a zone, inhabited by adult polypides, in full vigour and activity; further down still we shall probably encounter dwellings for the most part destitute of tenants. Of course these divisions are not invariable, nor are they separated by any hard lines; but such is the general character of the polyzoarium. In most of the tenantless zooecia dark, more or less spherical bodies occur, which occupy somewhat different positions, but exhibit a definite and uniform structure. They are generally placed about the middle of the zooecium, and are commonly attached to the (so-called) funiculus, by which the polypide is connected with the base of its dwelling (fig 1 *a*). It is a mistake to speak of these bodies, at least in their normal and perfect condition, as lying free within the cavity of the zooecium; they occupy the same position as the lower portion of the digestive sac of the polypide, and, like it, are firmly attached to the funiculus. Immediately below the "dark

body," to which I shall apply Smitt's name ("germ-capsule"), and in contact with it, is a small oval body, of a pale gold colour, and minutely granular structure, which is included within the funiculus (fig. 5 *c*). In zooecia containing a polypide it occupies a similar position in relation to the base of the stomach. The germ-capsule is found at times separated from the funiculus, but in the species which I have most closely studied (belonging to the genera *Bicellaria* and *Bugula*) it usually occurred as I have described it; and I am inclined to believe that when detached it has lost the power of further development, and probably decays away. This, however, is a mere conjecture; it is not a little remarkable that in their *figures* neither Smitt nor Nitsche has clearly indicated the connexion of the germ-capsule with the funiculus.

The germ-capsule exhibits a very definite structure; it consists of a mass of granular matter inclosed in a delicate membrane, the inner surface of which is covered with spots of a rich, reddish-brown colour. It bears the closest resemblance, in general aspect and in colour, to the stomach of the polypide, the reddish spots at once reminding the observer of the hepatic cells of the latter. If direct proof were wanting, it would be difficult to avoid the conclusion that it has originated in some way or other from this portion of the polypide. This point, however, has been conclusively established. J. V. Thompson and Nordmann[1] have both described the survival of a portion of the digestive sac, after the disappearance of the rest of the polypide, as a dark spherical body, which they supposed to be an egg or ovary. Smitt has seen a germ-capsule attached to a piece of the stomach-walls. I have met with a polypide in an early stage of decay, having a fully formed germ-capsule at the base of its body. Nitsche has also observed the development of this structure from the polypide in *Flustra membranacea*.

Further, I have traced the course of its development in *Bicellaria* and *Bugula*. This is so fully described in my former paper in this Journal that I shall only briefly refer to it now in connection with the illustrative figures. The first

[1] "En soumettant à un examen microscopique soigneux un certain nombre de cellules de Polypes des espèces *Cellularia*, *Bowerbankia*, *Flustra*, *Eschara*, &c., on ne tardera pas a découvrir que, dans beaucoup de ces cellules, le Polype est déja mort, qu'il ne reste plus une trace des tentacules, mais qu'une partie du corps du Polype (principalement la cavité digestive) s'est convertie en une masse ronde ou oblongue ayant des contours *parfaitement arrêtés*, et qui montrent, a ne pas pouvoir s'y méprendre, la forme et la nature d'un oeuf." On *Cellularia avicularia*, in Demidoff's 'Voyage dans la Russie Meridionale,' vol. iii, p. 702, note.

step towards the formation of the germ-capsule is a slight constriction of the walls of the stomach at a certain point (fig. 7 *c*, *c*). This constriction increases, until at length the lower portion of the stomach assumes a somewhat globular shape, and hangs at the base of the body like a distinct organ, communication with the rest of the digestive system being maintained through a narrow channel (fig. 6 *a*). When the polypide loses its vitality and decays, this globular appendage survives as a separate structure, and remains, as I have described it, attached to the funiculus (fig. 2 *a*).

In its first stage, after separation from the polypide, the germ-capsule is more or less surrounded by a mass of pale yellow globules (fig. 1 *a*), the "fettkroppsmassa" of Smitt, which this author regards, if I rightly understand him, as discharging a nutritive function and contributing to its further growth. As development proceeds they disappear.

At this point I may remark that the whole aspect and history of the germ-capsule would naturally lead us to conclude, apart from the developmental changes which I am about to describe, that it is much more than "the mere remains of a decaying polypide." Its definite form and structure, its constant occurrence, its connexion with the funiculus, its persistency, all suggest that it must have some special part to play in the life-history of the Polyzoon.

The first change that seems to take place is an increase of size. This was noticed by Grant, as before mentioned.

In one of the zooecia, on a specimen of *Bugula*, I have met with a germ-capsule which had attained unusually large dimensions; it was oblong in form, and occupied a considerable portion of the cavity. This was, no doubt, abnormal; but some increase of size appears to precede the actual commencement of development. After a time a light space makes its appearance on the upper surface of the capsule (fig. 2 *x*), and at this point a small swelling or bud is subsequently developed, which is of a light greyish colour, while the lower portion of the capsule retains its rich deep red. This bud enlarges (fig. 3), and the germ-capsule assumes an oblong form, tapering very slightly upwards. In a short time the tentacles are distinguishable in the anterior portion of the bud (figs. 1 *a'*, and 5 *b*), and below them the commencement of a pharynx and œsophagus, the lower and deeply coloured portion of the capsule occupying the place of the stomach.[1] As development proceeds the tentacles

[1] I am unable to describe more minutely the development of the various parts, as I have not examined the germ-capsule under compression. I can merely record the general appearance which it presented in different stages of growth.

lengthen, and the different portions of the alimentary canal are more distinctly defined, and, at last, a fully formed polypide fills the place of the one which has disappeared from the zooecium (fig. 4). *From the first the polypides thus developed from the germ-capsule are distinguished by the dark reddish-brown colour of the walls of the stomach*, while those which bud from the endocyst, whether in the new or adult zooecia, do not exhibit this character until they have attained maturity.

Nitsche considers that the "brown-bodies" arise in *Flustra membranacea* from the decay of the polypide, and are formed by a kind of encysting of the greater part of the products of this decay.[1] The observations which I have just detailed do not allow me to accept this statement as a correct expression of the facts. In *Bicellaria* and *Bugula*, at least, the germ-capsule (or "brown-body") consists of a portion of the stomach of the polypide that survives the rest of the organism as a separate structure.

In his criticism on Smitt's views the same able observer maintains, as we have seen, that certain buds, originating from the endocyst, which often occur in adult zooecia, had been mistaken for buds from the germ-capsule. When I first commented on this statement I had not seen these endocyst-buds, but my observations left no doubt on my mind that Smitt was substantially correct in his account. I have since, however, carefully examined them, *and can now state that they differ essentially in general appearance as well as in position from the budding germ-capsules*. It must be admitted, at the same time, that Smitt has given a handle to his critics; for I have satisfied myself that in several instances he has actually confounded the two bodies, and has thus been led to give a form to his statements which has naturally suggested doubts as to the accuracy of his observations. For example, in describing the "groddkapsel" of *Bugula fastigiata* and its development, he speaks of the bud as situated at some distance from it, and says that it was difficult to determine the relation between the two. On referring to his figure (pl. v, fig. 2) it is evident enough that the supposed bud from the germ-capsule is in reality one of the endocyst-buds described by Nitsche. So, again, in his account of *Scrupocellaria scruposa* he tells us that in one of the zooecia a bud with rudimentary tentacles was observed *by the side* of one of the "dark bodies." This would be a very incorrect description of the germ-capsule and its bud; and accordingly on reference to the

[1] 'Ueber die Anatom. u. Entwicklungsgesch, von *Flustra membranacea*,' p. 86.

figure (pl. v, fig. 1) it appears that in this case also we have to do with an endocyst-bud, the characteristic appearance of which is very accurately given! Elsewhere (as in his account of *Alcyonidium parasiticum*) Smitt remarks that the buds are sometimes so slightly connected with the germ-capsule that they break loose from it, especially as they advance in development, and lie free in the zooecia. Hence Claparède represents him as teaching that the young polypide creeps out of the capsule and attaches itself near it! Nothing of the kind could possibly take place in the case of the germ-capsule, which is not a structure distinct from its bud, but organically one with it. It is clear that in this case Smitt has seen the development of the endocyst-bud into the polypide alongside the "groddkapsel."

These mistakes on the part of this distinguished observer have certainly given some show of reason to the strictures of Claparède and Nitsche, but, on the other hand, these writers have not assigned their due weight to the passages in which he has correctly and clearly described, or to the figures (*e. g.* pl. v, figs. 5, 17, 18, 19), in which he has represented the development of the germ-capsule into the polypide.

It is hardly necessary that I should refer at any length to the objections and arguments of Dr. Nitsche. If the observations now recorded are worth anything they conclusively establish the doctrine of the germ-capsule as I have stated it.

One point is much relied upon by Nitsche; he states that he has found in the interior of the brown bodies diatomaceæ, sponge-spicules, thread-cells, &c., evidently the remains of the polypide's last meal before its dissolution, and this fact he considers fatal to Smitt's theory. I have never observed anything of the kind; but inasmuch as the germ-capsule is a portion of the digestive sac, it is not at all improbable that it may occasionally retain portions of the food at the time of its separation from the polypide. But I fail to see how this should interfere with its further development. If, however, in such cases development be arrested, it is nevertheless true according to my observations, that in others it takes place.

Many points in the history of the germ-capsule remain to be determined. Not unfrequently the zooecia over a large portion of the polyzoary, are found to contain "dark bodies," a number of which show no signs whatever of growth, while others are already budding. It would be interesting to know under what conditions development takes place; and whether the germ-capsule discharges any other function than that which has just been described. Smitt asserts that it sometimes acts as an ovary, and that he has observed the

ova in the interior of one taken from *Alcyonidium mytili*, Dalyell. One is tempted to imagine that there may have been an error of observation in this case, but the point should be thoroughly investigated.

Both Grant and Farre noticed that occasionally two of the "dark bodies" were present in a single zooecium, and Smitt has confirmed the observation; but it remains to be determined how the second originates.

It may be remarked, in conclusion, that the germ-capsule cannot properly be brought into comparison with the statoblast, from which it differs essentially in its origin, structure, and function.

I pass on to notice very briefly the second mode in which the polypide is renewed in the adult zooecium.

2. Renewal of the polypide by gemmation from the endocyst.

In adult zooecia, in which the polypides are already decaying, a bud is sometimes observed sprouting from the endocyst, in which the rudimentary crown of tentacles is soon traceable (figs. 8, 9), and which exactly resembles the polypide-bud of the newly formed zooecium on the margin of the polyzoary. It originates about the middle of one of the sides of the zooecium, and, no doubt, passes through the same course of development as the last named. I have met with such a bud in a zooecium containing a fully formed capsule,[1] and it is certainly not a little puzzling to find this double provision for the supply of a fresh tenant. What the precise relation may be between these two modes of reproduction, I cannot at present pretend to say; nor can I answer Claparède's query, "Wozu dann aber die Keimkapsel, wenn dieselbe so leicht entbehrlich ist?" But I have not the least doubt that the two modes exist, and that between them they keep up the succession of polypides so long as the life of the colony continues in vigour.[2]

3. Reproduction by ova—the ooecium.

The reproductive organs of the Polyzoa have been carefully studied, and the position of the ovary and testicle, the development of the ova, their fertilisation by the spermatozoa in the perigastric cavity, and their final conversion into cili-

[1] So far as my observation goes, the endocyst-buds are of rare occurrence as compared with the germ-capsules.

[2] Amongst the Hydroida we know that there may be a frequent fall and renewal of the polypides during the life of the zoophyte.

ated embryos, have been described by various writers. A question, however, has arisen respecting the precise function of the ooecium (ovicell) with which the *Cheilostomata* generally are furnished. Huxley was the first to suggest that it is a kind of marsupium, with which the ova pass from the zooecium, and are there matured into ciliated embryos.[1] This theory was challenged by myself in a paper,[2] in which I supported the view previously taken by Reid, and endeavoured to show that the ova found in the ooecia are produced there. The eggs which are developed within the zooecium I considered to be of a different kind; I supposed that they continue unciliated, and are only liberated at the death of the polypide. This paper has been recently criticised by Nitsche,[3] who adopts Huxley's opinion, and supports it by fresh observations. It is also referred to by Smitt, as affording confirmation of views at which he himself has arrived. Under these circumstances it seems desirable that I should state how far the opinions expressed in this paper have been modified by further observation. When it was written (1861) the true structure of the ooecium had not been determined, and we were ignorant of the real significance of the germ-capsule. The latter I undoubtedly mistook for an ovum, distinct in kind from that contained in the ooecium, and which I fancied might prove to be a sort of winter-egg. With respect to the marsupial theory, I now believe that the ova produced and fertilised in the zooecium are probably conveyed into the ovicell, and there complete their development, though I am still unable to understand how the transference is accomplished. At the same time I am by no means prepared to abandon the opinion *that ova are in some cases developed within the ooecium itself*. The observations which I have recorded in my paper of 1861, and which are supported by those of Reid, previously published, have lost none of their force to my own mind. The later researches of Smitt have led him to adopt a similar view.[4] He has observed the

[1] "Note on the Reproductive Organs of the *Cheilostome Polyzoa*," 'Quart. Journ. Mic. Sci.,' vol. iv, p. 191.

[2] "Note on the Ovicells of the Cheilostomatous Polyzoa," 'Quart. Journ. Mic. Sci.,' vol. iv, p. 278, 1861.

[3] "Beobachtungen über die Entwicklungsgeschichte einiger Chilostomen Bryozoen," 'Zeitsch. f. Wissensch. Zool.,' xx Band, 1 Heft.

[4] Nitsche, in his criticism on my paper, has hardly given a fair account of Smitt's testimony. He says that Smitt has arrived at the same results as Huxley in the case of *Scrupocellaria scruposa*, and that he merely quotes the observations leading to a different conclusion made by myself on other species. On this passage Claparède has remarked, "This is a misrepresentation of the actual state of the case, inasmuch as Smitt fully confirms both Huxley's statements and those of Hincks."

asexual development of ova both within the zooecium and the oœcium. In the ovicell of *Crisia eburnea* he has traced their formation by a kind of budding, and, in the absence of spermatozoa, their development into the embryo. In *Lepralia Peachii* and *L. Pallasiana* he describes the same mode of egg-formation as taking place within the zooecium. On this point Claparède has remarked that it is scarcely to be supposed that Smitt, to whom we owe such accurate observations on the male organs of many species, should have overlooked the spermatozoa, if present; and he adds, "It seems to me much more probable either that in the species observed the sexes are distinct, and that Hincks and Smitt were unacquainted with the male, or that Parthenogenesis has a place in the history of the Bryozoa."[1] More can hardly be said at present, but the subject is one of peculiar interest, and worthy of the special attention of students.

In connection with the development of the ova and their passage into the oœcium the following observations may be worth recording. In a specimen of a species of *Bugula* ova were met with in considerable abundance, appearing as small spherical bodies of a rather bright yellow colour. Only one was observed in a zooecium, which usually lay at the very bottom of it, below the base of the polypide. Many of the zooecia were furnished with fully-formed ovicells containing yellow bodies, bearing a general resemblance to the ova, but usually of larger size; these did not contain eggs. On other zooecia the ovicell was only partially developed, or, if completed, was empty, and in these an ovum was very generally present. It was difficult to imagine in what way it could be conveyed, in the absence of cilia or other means of locomotion, from its position at the base of the zooecium to the marsupium at its summit. In a single instance I observed an egg in a different situation; it lay beside the polypide, at a very short distance below the top of the zooecium, and was affected by all its movements. When the polypide was expanded it was drawn upwards and retained between the œsophagus and the wall of the zooecium; when it was retracted, it was drawn slightly downwards. It seemed as if a vigorous movement might at any moment discharge it into the zooecium. I could only conjecture that perhaps after all the action of the polypide might be mainly instrumental in effecting the transference to the marsupium, though I am bound to say that, considering all the circumstances of the case, the agency seems hardly adequate to the work.

[1] "Beiträge zur Anatom. u. Entwicklungsgesch. der Seebryozoen," 'Zeitschr. für Wissensch. Zool.,' xxi Band, 1 Heft, p. 165.

Amongst the *Ctenostomata*, which are destitute of ovicells, the egg is developed into the ciliated embryo within the zooecium, and is liberated after the disappearance of its tenant. In all probability the embryo is itself the cause of the death of the latter, for as it increases in size it occupies a large portion of the cavity, and the polypide is displaced and crushed down towards the bottom.

In *Vesicularia spinosa* I have observed an ovum in the upper part of a zooecium which was of a dark brown colour, and surrounded by a delicate envelope. Myriads of spermatozoa were swarming in the perigastric cavity, while the polypide was withdrawn to the very base of its dwelling. In other cases the ovum had attained a much larger size, occupying nearly two thirds of the cavity, and had acquired a fine rose colour. The polypide had either altogether or in great part disappeared. In other cases, again, the rose-coloured embryo was found in the empty zooecium, equipped with its cilia, and ready for escape.

The ooecium, which is so prominent a feature of the Cheilostomata, exhibits, in certain genera at least, a somewhat complex structure, which has been admirably demonstrated by Nitsche[1] in the case of *Bicellaria ciliata*. In this species the opening of the helmet-shaped marsupium is closed by a sub-globular membranous capsule (fig. 10 *x*) which is attached by its base to the polyzoary immediately in front of it. Through this capsule passes obliquely a muscular band (fig. 10 *m*) which is attached to its inner surface above, and below to a point near its base, and by means of this muscle the capsule can be withdrawn from before the opening of the ooecium, so as to permit the escape of the embryo (fig. 11 *x*). I have witnessed the action of the muscle, and the vigorous retraction of the capsular operculum, even when the ovum was as yet in a very rudimentary condition. The retractor is attached at the centre of the upper part of the capsule, and as it contracts the membranous wall is inverted, and the cavity above is proportionally enlarged. By this beautiful apparatus a sufficient space is provided for the embryo in all stages of its development, and its escape at last is secured without any injury to the structure of the ooecium, which may probably serve as the nursery of several generations.[2]

[1] 'Zeitsch. für Wissensch. Zool.,' xx Band, 1 Heft, pp. 3, 4.

[2] Reid noticed that the membrane enclosing the ova in *Bugula avicularis*, when they were fully formed, contracted and relaxed at intervals, and conjectures that in this way the escape of the embryo was facilitated; but he did not observe the muscular band.

4. The Embryo of Pedicellina echinata, *Sars*.

The embryo of *Pedicellina* has been studied more or less by Reid (in the paper already referred to), by Van Beneden,[1] and by Uljanin.[2] The reproductive organs have also been investigated by Nitsche, but he has not described the embryo. The observations of the last-named author, of Reid, and of Uljanin, were made on *Pedicellina echinata*, Sars, while the closely allied *P. belgica* was the form examined by Van Beneden.

I have lately had an opportunity of studying the embryo of the former of these two species, and as the results obtained, after careful observation, differ materially from those already recorded, I shall give them a place in this paper.

While examining some Devonshire dredgings my attention was attracted by a *Pedicellina* which bore two ciliated bodies within the tentacular ring, apparently attached to the lophophore. On examination they were found to be embryos in an advanced stage of development which had passed from the brood-chamber and taken up temporary quarters in the tentacular crater. The movements of the mantle and ciliary lobes, to be described hereafter, were distinctly visible. After a time they detached themselves and swam about freely in the surrounding water.

The body of the embryo (fig. 15) is turbinate, and is traversed by several transverse indentations or furrows. It widens upwards from the somewhat pointed lower extremity to the top, where it is encircled by a collar-like expansion clothed with vibratile cilia (fig. 15 *b*). This is formed by a sort of contractile mantle, which can be extended and folded over the anterior surface of the body and the organs which it carries, or thrown back so as to constitute a ciliated girdle, by means of which, in great part at least, the embryo is propelled through the water.

On the anterior surface of the body, in the space enclosed by the ciliated collar, is placed an organ, furnished with two opposite lobes ; one of these is somewhat produced and pointed (fig. 15 *c*), and finely ciliated round the margin, and bears an orifice from which I have seen fæcal matter ejected, and which I take to be the mouth ; the other is smaller, less elevated, rounded, and set with a number of long setiform

[1] "Recherches sur les Bryozoaires qui habitent la côte d'Ostend," 'Memoires de l'Acad. Roy. de Belgique,' t. xix, pp. 80-2, plate x.

[2] "Zur Anatomie und Entwicklungsgeschichte der Pedicellina," 'Bulletin de la Soc. Impériale des Naturalistes de Moscou,' t. xlii (1869), pp. 435-8, plate vi.

processes, which, when in motion, wave rapidly to and fro, and lash the water with much vehemence (fig. 15 *d*).[1] These lobes are very movable and contractile; they are sometimes extended, sometimes altogether withdrawn, the mantle in this case folding over them and enveloping the whole of the oral surface of the body. At the lower extremity of the body there is a small projection (fig. 15 *e*) by which the embryo attaches itself at pleasure, and I think (though I cannot speak with certainty) I have seen cilia at this point. I was unable to determine the internal structure, owing to the opacity of the cuticular covering. The embryo was active in its movements, but often brought itself to anchor; it would also reverse the body and creep along on the ciliated lobes as on a foot (fig. 16). I met with individuals that had permanently attached themselves, but was not able to follow the later stages of development.

It seems to me probable, however, that the oral lobes disappear, and that the tentacles are developed within the margin of the mantle-like envelope, in which we may, perhaps, recognise the membranous cup that surrounds the base of the arms in the adult polypide. But this is purely conjectural.

In comparing the foregoing account with the descriptions already published of the *Pedicellina* embryo, many important discrepancies will be noticed. There is a general agreement between the observations of Reid and Van Beneden. Both describe the embryos which they examined as presenting a somewhat funnel-shaped body, with a circle of long cilia round the upper margin. The latter noticed one or two indentations (échancrures), which divided the body into an anterior and posterior half. As development proceeded he observed the formation of rudimentary tentacles within the margin of the funnel, the disappearance of the cilia, and the growth of a short pedicle.

I cannot harmonise this account with my own observations, and can only suppose that the embryo of *P. belgica* differs essentially from that of its congener.[2] Nitsche, who was only able to make a very imperfect examination of the embryo of *P. echinata*, in specimens preserved in spirit, was, nevertheless, of opinion that it was more highly organised than the form described by Van Beneden.[3]

[1] A similar structure is of common occurrence amongst the Polyzoan embryos.

[2] The species investigated by Reid, however, was *P. echinata*; and unless the embryo is liberated in very different stages of development, I cannot but suspect some error of observation in this case.

[3] "Ueber die Anatomie von *Pedicellina echinata*, Sars.," 'Zeitsch. f. Wissensch. Zool.,' xx Band, 1 Heft, p. 28.

Coming now to Uljanin's observations (which, he tells us, were made under the direction of Prof. Rud. Leuckart), we find many points of agreement, but still very weighty differences. I should be inclined to distrust my own work if the circumstances under which my observations were made did not seem to preclude the chances of serious error. The escape of the embryos from the parent was witnessed, and their structure and habits were carefully noted. I was also able to sketch them in many attitudes and from various points of view.

Uljanin describes the embryo as consisting of a symmetrical, cuticular cup in which the true sac-like body remains quite free, except at its upper margin, by which it is attached to the rim of the cup.

This sac can be partially everted, as it always is when the embryo is in motion. In this state its inner wall, which is covered with cilia, is turned outwards and forms a collar-like "velum" by means of which the embryo propels itself. So far the two accounts show a certain amount of general agreement.

But Uljanin makes no mention of the ciliated lobes and oral aperture; while he does describe two "ganglion-like organs" placed near two orifices in the wall of the cup, one on its margin and the other at its base, which certainly escaped my notice. It must be left to future observers to clear up these differences; meanwhile, I may remark that, judging from analogy, the structures on the oral aspect of the body which I have described and figured, are very much what we should expect to meet with in the Polyzoan embryo.[1]

5. The colonial nervous system.

The existence of a common nervous system amongst the Polyzoa, by which the zooids composing a colony are linked together and brought into relation, was first demonstrated by Fritz Müller; his view has been adopted and confirmed by Smitt and Claparède, and opposed by Reichert[2] and Nitsche. Müller only succeeded in detecting the supposed nervous structure in the Ctenostomata, but Smitt in the first place, and subsequently Claparède and myself, have proved its existence in the Cheilostomata. It is most readily observed in the

[1] Since the foregoing has been in type I have found some notes on the embryo of *P. echinata*, accompanied by drawings, which I made several years since, and which confirm in all points the later observations recorded in this paper.

[2] "Vergleichende anatom. Untersuchungen üb. *Zoobotryon pellucidus*, Ehrenberg," 'Aus den Abhandl. d. Königl. Akad. d. Wissench. zu Berlin,' 1869.

former; in the stem of *Vesicularia*, *Valkeriâ*, *Serialaria*, &c., the nerve-trunk is very conspicuous, running along one side of it and terminating at the joints in distinct ganglia (fig. 14). In the neighbourhood of the zooecia it is sometimes overlaid by a complicated plexus of nerve-threads, from which filaments pass off to the ganglia that are situated at the base of each zooecium. This structure is beautifully displayed in a specimen of *Valkeria pustulosa* preserved in fluid, which I have in my possession, and my principal object at present is to direct attention to the figures which I have given of it (figs. 12, 13).

Near the base of the internode is a mass of nucleated cells (fig. 12 *c*), from which threads pass off in various directions. Towards their origin they consist sometimes of a single series of cells for a short distance, and afterwards assume the usual appearance of the nervous filament.[1] I suppose that this cellular structure forms a portion of the plexus, which overlies the main trunk and connects the various ganglia.

That Fritz Müller has assigned its true significance to this complicated system of cords and threads pervading the stem of the polyzoon, and penetrating into the several zooecia, I have little doubt. Its general appearance and arrangement naturally suggest this interpretation, which is not contradicted by histological research as far as it has been undertaken, and is supported by various independent considerations. The movements of the avicularia and vibracula, which are quite independent of the individual polypides, and continue after they have disappeared, seem to point to some common or central nervous agency as their source.

It has been observed by Smitt, Claparède, Lovén, and myself, that a branch from the (supposed) common nervous system passes to the base of each avicularium.

The same remark may be made of the simultaneous movements of the polypides of *Mimosella*, which I have noticed in the original account of this exquisite form, and to which Fritz Müller refers in his paper.[2] This author also cites the energetic movements of the peduncle of *Pedicellina*, which continue after the removal of the polypide itself, and commence when as yet it is a mere rudimentary bud, as further evidence to the same effect.

Perhaps the strongest point which has been urged against this view is, the alleged non-existence of the common nervous

[1] In *Serialaria* I find the supposed nerve-trunk to consist of a very delicate, transparent membranous wall enclosing a fine granular matter, each granule being made up of several minute bodies agglomerated together.

[2] 'Wiegmann's Archiv,' 1860, p. 311.

system amongst the *Phylactolæmata*, as showing that it is not physiologically necessary to the colonial life (Nitsche).[1]

The whole subject demands a more exhaustive treatment than it has yet received, which I trust it may soon obtain at the hands of competent observers.

Researches *on the* Connective Tissue *and* Vessels *of the* Nerves. By Louis Ranvier. (With Plate III.)

(Translated from 'Archives de Physiologie,' July, 1872, p. 427.)

The nervous tubes, which are the essential elements of nerves, are united into distinct bundles. Some authors, comparing these bundles to the primitive bundles of muscles, have described them as primitive nerve-bundles. This is, however, an error, for if there is any elementary part in the nerve which can be compared to the primitive muscular bundle, it is certainly the nervous tube, a conclusion which follows from what has been said in a former memoir.

A nervous bundle is rather comparable to an entire muscle; in fact, the nervous bundles, like muscles, differ generally in their diameter, and also, like muscles, are invested with an aponeurotic membrane. There is, then, no propriety in giving the name of primitive bundles to the bundles of nerves, and we shall call them simply nervous bundles.

A considerable nerve may be formed, in part of its course, of a single nervous bundle, or else made up of several. Each bundle is surrounded by a special connective sheath, which gives it its individuality. When several bundles combine to form a nerve they are united together by loose connective tissue, containing blood-vessels and lymphatics. In the interior of each bundle the nervous tubes are also united by loose connective tissue, traversed by blood-vessels. In order to become acquainted with these preliminary notions it is

[1] "Dumortier mentions a peculiarity of *Lophopus*, which indicates the existence of a colonial nervous system, such as has been discovered by Fritz Müller in *Serialaria*. He remarked, in fact, the same phenomenon in *Lophopus crystallinus* which led Müller to begin his investigations, namely, that when the cœnoecium was touched all the polypides were alarmed, whereas when a single polypide was disturbed it alone retracted. I have examined with care all parts of the cœnoecium vaginable endocyst in other genera in order to find this colonial system, but without success." Hyatt, "Observat. on Polyzoa, Sub-order Phylactolæmata." From 'Proc. Essex Institute,' vols. iv and v, 1866-8, pp. 46-7.

only necessary to examine in glycerine with a magnifying power of twenty diameters the transverse section of a nerve which has been hardened in a solution of chromic acid (in the proportion of two or three parts to a thousand). But by the application to the study of nerves of the histological methods known at the present day we arrive at facts which are much more interesting both from the morphological and from the physiological point of view. The connective tissue of nerves and the vessels of nerves will be successively spoken of.

Connective tissue of nerves.

This tissue presents itself in the nerves under three forms. Immediately around the nervous bundles it is condensed so as to form laminæ ; this I will call laminated or ensheathing connective tissue, and the sheaths which it forms will be called laminated sheaths of the nervous bundles. It unites the different bundles either to one another or to the neighbouring connective tissue, and is composed of thick fibres mingled with adipose cells and elastic fibres. In the interior of the nervous bundles the fibres of connective tissue are extremely thin, and are never, in the normal state, mingled with fat-cells or elastic fibres. Each of these forms of connective tissue requires a special description.

Laminated sheaths of the nervous bundles and ensheathing connective tissue.

The description which Bichat has left us of the sheaths of the nervous bundles is remarkably exact. In the chapter on the neurilemma (laminated sheaths of the nerves) he thus expresses himself:—" This membrane forms an actual canal for each nervous thread, containing in its interior the medulla (bundles of nerve-tubes) as the veins and arteries contain the blood, with this difference, that the medulla stagnates while the blood circulates." Bichat adds that the neurilemma is continuous with the fibrous envelope of the spinal marrow in such a manner that the whole nervous system is contained in a system of channels analogous to the arterial system. Finally, he says that the anatomical relations of the parts alone concern him, since he knows nothing about their nature. What is surprising is that since Bichat we possess no more exact notions on the nature of this sheath.

Henle[1] has described in the neurilemma a series of elements belonging to the connective tissue, but without exactly point-

[1] Henle, 'Anatomie Générale,' 1843, t. ii, p. 164.

ing out their situation and shape. Still, he draws attention to a sheath in the form of a membranous tube existing on the finest nerves which can easily be studied in the fresh state. This sheath is "structureless, hyaline, or faintly granular, and showing on its surface elongated nuclei." I have seen, he says, tubes which contain only two primitive fibres.

M. Charles Robin,[1] in a memoir published in 1854, entitled "On the Perineurium, a New Kind of Anatomical Element," repeats exactly the description given by Henle for the small nervous twigs, and applies it to all nerves, whatever their size.

Since that time histologists have neglected the study of the connective tissue of nerves.

In reality, the immediate sheath of the nervous bundles is made up, in large nerves, such as the sciatic, the brachial plexus, &c., of a series of superimposed laminæ.

If transverse sections are made of the sciatic nerve of the dog, previously hardened in chromic acid, these sections coloured with carmine and examined in glycerine or Canada balsam, we see a large nervous bundle, and by the side of it several small bundles, varying in number and size according to the place at which the section is made. Each of these bundles is surrounded by a circle deeply stained by the carmine, wider around the larger bundles than the smaller, and presenting parallel circular bands. If alcohol or a saturated solution of picric acid be used to harden the nerve instead of chromic acid, the sections are more easily coloured by carmine, and acetic acid acts on them as it does on fresh tissues. The connective tissue uniting the different nervous bundles swells up under the influence of the acetic acid and loses its colour, while the laminated sheath of each bundle remains of a bright red colour and preserves its striation. These simple methods show that the tissue which composes the striated sheath of the bundles differs from the neighbouring connective tissue. To show the striation of this sheath and study its composition I have had recourse to more complicated methods, which, however, do not present any great difficulty in the carrying out.

In the first place I injected into the interfascicular connective tissue, by means of a Pravaz syringe, the following mixture:—Gelatine, swelled in distilled water, and melted by the salt-water bath, two parts; one-per-cent. solution of nitrate of silver, one part. This mixture is liquid at 35° or 40° C., the temperature at which the injection should be made. The injection runs along the nerve and surrounds it

[1] Robin, 'Mémoires de la Société de Biologie,' années 1854, 1855, p. 87.

like a sleeve. I shall explain afterwards, in speaking of the interfascicular connective tissue, why the injected matter spreads in the direction of the nerve, and not by the side of it.

When the envelope of gelatine surrounding the nerve is solidified by cooling, the nerve is removed and put into alcohol. After twenty-four hours it becomes hard enough to make thin sections, which, placed in glycerine and exposed to the light, exhibit interesting relations. In the sciatic nerve of the dog, for instance, the perivascular sheath is deeply stained black, while the rest of the preparation presents a light brown tint; each nervous bundle appears accordingly surrounded by a very dark circle, the outlines of which are perfectly distinct. The sheath of the nervous bundles accordingly possesses in a high degree the power of fixing and reducing the nitrate of silver. If transverse sections of the nerve are then removed with a power of 600 or 800 diameters, the sheaths are seen to be formed by a series of laminæ bounded by black lines. These laminæ are ten or twelve in number on the large nervous bundle of the sciatic nerve in the dog. They are very regular, and their general arrangement recalls the section of a hydatid membrane (Pl. III, fig. 3).

If longitudinal sections are made from the same nerve the fascicular sheath is often seen to be displaced to the right or left by the pressure of the lamina covering it, and presents itself in the form of a ribbon. By teasing this ribbon out with needles it may be decomposed into a series of distinct lamellæ (fig. 4). These lamellæ present on their surface very clear black lines, which form epithelial outlines, so that one is led to suppose that each of these lamellæ possesses an epithelial or endothelial investment. This fact will be referred to again.

It often happens that the silver-gelatine insinuates itself between the laminæ of the laminated sheath of the nervous bundles, separates them, and produces a regular separation. In transverse sections of such nerves each nervous bundle, instead of being surrounded by a single-banded black circle, appears enveloped in a series of black concentric circles, separate one from another and united by oblique laminæ. This separation is produced with difficulty in the sciatic nerve of the dog. In the rabbit, on the other hand, the sciatic nerve is very easily separated by interstitial injections of silver-gelatine, beautiful and instructive preparations being thus obtained (fig. 2).

When these preparations are coloured with picrocarminate of

ammonia we observe on the surface of the laminæ and the bands which unite them endothelial cells, which are detached either singly or in groups in the form of shreds. I shall return further on to the forms and relations of these endothelial cells, but it is necessary first to point out the fundamental structure of the laminæ of the laminated sheath.

When a large nerve, like the sciatic of man or of the dog, has remained for fourteen days in a solution of chromic acid (two parts in a thousand), the nervous bundles can easily be isolated on short pieces of the nerves. One of these bundles is placed in a saucer of water; we can then remove from its surface with needles a certain quantity of connective tissue which floats away the moment it is removed, and forms flakes in the liquid. When it seems as if the nervous bundle were altogether separated from the connective tissue surrounding it, bundles of the same tissue may still be removed from its surface, and, still more, may be obtained from the exposed tubes. If the separation be not carried too far and the sheath of the nervous bundle be split up lengthwise, the two edges of the incision may be flattened out and the nervous tubes easily separated from the envelope surrounding them. The membrane thus obtained remains rolled up and shows on its internal aspect a peculiar lustre. Sometimes it may be divided into two or three lamellar shreds, which, examined microscopically, are found to be composed of bundles imbedded in a granular or homogeneous substance. This substance is coloured by carmine, not swollen up or softened by acetic acid, and shows all the chemical characters of the envelope of connective-tissue bundles or of the annular and spiral structures known as the annular or spiral fibres of Henle. The inner surface of the lamina nearest the nerve shows very delicate fibres connected with the intertubular connective tissue; these are, however, easily torn, so that the whole nervous bundle may be withdrawn from its sheath—for instance, a piece ten or fifteen centimètres long of the pneumogastric.

Some elastic tissue was also observed in the sheath, which presented, especially in animals or man advanced in life, certain peculiar characters. It might be said to exist in three forms, that of fibres, plates, and granules. The elastic fibres, which were variable in diameter and nodulated (fig. 5 *b*), formed a dense network, and were seen to be continuous with plates of identical composition, while both fibres and plates were also continuous with strings or chains of granules of highly refractive substance (fig. 5 *c*). The same characters may be seen elsewhere, and, indeed, the striated appearance

already observed in elastic fibres by several histologists, as Quekett, Kölliker, and Robin, is really due to the fibre being made up of strings of brilliant granules imbedded in a less powerfully refracting substance. These appearances are best shown by the following method. A solution of osmic acid, one part in 200, is injected in the manner before described into the subcutaneous or other cellular tissue, so as to form a lump. This is cut out with scissors and placed in a solution of osmic acid of the same strength. The elastic fibres which were stretched by the injection are by this means fixed in their position. Small pieces are then removed with the scissors and placed in glycerine, when the elastic fibres appear, under a magnifying power of 600 to 800 diameters, distinctly striated (fig. 6). In other parts, as in the cartilages of Santorini from the larynx, the *formation* of elastic tissue *from* such granules may be distinctly traced. The latter observation was, indeed, made by Heinrich Müller,[1] who thus refuted the theories of Donders and Virchow on the development of elastic fibres from cells.

These characters of the elastic tissue in the nervous sheaths are nothing more than a special case of the general morphology of this tissue, but confirm the general conclusion that it exists in the organism under three forms, viz. isolated granules, fibres, and plates or masses.

We have now to study the relations between the endothelium and the laminæ. The simplest method, direct impregnation by nitrate of silver, gives the clearest results as to the form and extent of the endothelium. In a former memoir a figure was given of the single layer of endothelium, distinguished for the extreme thinness of its intercellular substance, which is seen on the thoracic nerves of the mouse. A similar continuous endothelial covering, composed of a single layer of cells, has been shown to exist on the nerves of the cornea by an Italian histologist, Dr. Durante.

On the larger nerves, such as the large thoracic nerves of the mouse or the thoracic nerves of the rat, two layers are seen ; the lines of separation of the cells cross one another at various angles, and appear to be situated on two neighbouring planes.

I am therefore of opinion that these two endothelial coverings are applied one over the other. On still larger nerves, such as the small nervous bundles of the sciatic in the dog or the rabbit, the black intercellular lines of the

[1] 'Wurzbürger Nat. Zeitschrift,' vol. i, p. 162, quoted by Kölliker, 'Elements of Histology.'

endothelium are situated on different planes, and compose an inextricable network.

By another method preparations may be obtained which show that between the most internal lamina and the next there is a space the two surfaces of which are covered with endothelium. A nerve which is kept in a state of extension is placed in a saturated solution of picric acid for twenty-four hours, then in a syrupy solution of gum. After remaining in this for forty-eight hours the nerve is placed in strong alcohol, in which it becomes very hard, and extremely thin sections may then be made. These are placed in water to dissolve out the gum, then stained with carmine and examined in glycerine. Round each bundle of nerve-tubes, and beneath the laminated sheath, is then seen a circular space (fig. 1 *a*), traversed by bridges of connective tissue, and with a very high magnifying power a lining of endothelial cells may be recognised on all the walls of this space.

All that has been said about the laminated sheath of the nervous bundles shows that we have to do with a particular kind of connective tissue, characterised by laminæ formed of bundles united together by a substance absolutely similar to that which in the lax connective tissue envelopes the bundles of that tissue. This laminated form of connective tissue is met with in other organs, and especially round organs, for which reason I call it "*ensheathing connective tissue.*"

In the nerves the bundles of connective tissue which unite to form laminæ are not directly in connection with the flat cells of the connective tissue. Accordingly the laminated or ensheathing connective tissue differs in that respect also from the ordinary fasciculated connective tissue, the structure of which I described in a former memoir,[1] and which is now admitted by most histologists. The endothelial cells occupying the surface of the laminæ represent here the flat cells of the lax connective tissue.

The bundles of connective tissue which are sunk in the laminæ of the sheath are continuous on one side with the fibres of the perifascicular connective tissue, and on the other side with the fine fibres of the intrafascicular connective tissue.

Perifascicular connective tissue.

The connective tissue which envelopes the laminated sheath and unites together the different bundles of a nerve does not differ from the lax connective or cellular tissue,

[1] "The Cellular Elements of Tendons, &c.," 'Archives de Physiologie,' 1869, 'Quart. Journal Mic. Science,' 1870.

except in the direction of the elements which compose it. The bundles of connective tissue have a direction parallel to that of the nerve, or only slightly oblique, so that in examining a transverse section of a nerve, made by one of the methods previously pointed out, almost all the bundles appear cut across, and in preparations coloured with carmine and treated with weak acetic acid we have the appearance of what was formerly called, in accordance with Virchow's views, a plasmatic network. The longitudinal direction of these bundles explains why the interstitial injections which are sent in spread so as to form, not a gradually extending lump as in the subcutaneous tissue, but a cylinder surrounding the nerve. Elastic fibres of moderate size and islands of adipose tissue are associated with the bundles of connective tissue; the blood-vessels and lymphatics will be spoken of further on.

Intrafascicular connective tissue.

It was shown above that from the external lamina of the sheath there proceed extremely slender fibres of connective tissue which penetrate into the bundle of nerve-tubes. These immediately bend inwards and take a direction parallel to the axis of the bundle. All the other fibres of connective tissue interposed between the nerve-tubes have the same direction. These fibres are small connective bundles, for each is composed of fibrillæ, and a large number of isolated fibres, less than ·0005 mm. in diameter, may also be obtained by breaking up the nerve. When a nerve which has been twenty-four hours in osmic acid is broken up the greater number of medullated fibres are found to be surrounded by a layer of these little bundles. A continuous layer of the same bundles is also observed between the internal lamina and the nerve-tubes, so that the bundle of nerve-tubes is invested with a layer of connective tissue in the same manner as a single nerve-tube.

A nerve treated with osmic acid, chromic acid, or nitrate of silver, and then broken up with needles, shows, along with the connective fibres, flat cells of irregular outline, or furnished with prolongations and containing flat oval nuclei. These cells accordingly exist on the surface of the nervous tubes in direct relation with Schwann's sheath, or rather they are separated from it by a group of fibres. Flat cells, similar to those just described, cover all the vessels which are contained in the thickness of the nervous bundle. Along with these flat cells are also always found some lymphatic cells, similar to the white globules of the blood, which travel

in the spaces left between the bundles of intrafascicular connective tissue. Such cells exist in the fasciculated connective tissue of all parts of the body.

What characterises this intrafascicular connective tissue is, then, the slenderness of its bundles, their longitudinal direction, and the existence of flat cells spread out either on the surface of the tubes, or on the surface of the vessels, or on the groups of small connective bundles.

If a solution of Prussian blue be injected, by means of a very fine pointed canula, into the thickness of a nervous bundle, the coloured matter makes its way for some distance in a straight line, and if only moderate pressure be employed and the nerve afterwards hardened in chromic acid it may be seen that the injection makes its way among the nerve-tubes, not in any closed vessel, but surrounding some and pushing aside others till its lateral extension is limited. The distribution of the injected material is thus different from that which takes place when a similar injection is made into the subcutaneous tissue. On cross sections each of the nerve-tubes enclosed in the mass is seen to be surrounded by a ring of injection.

Long ago Bogros[1] succeeded in injecting nerves with mercury, and thought that he was thus filling the system of canals so remarkably described by Bichat.

Vessels of nerves.

The arterial and venous vessels which arise in large numbers along the course of a nerve form, in the perifascicular connective tissue, an arterial and venous network, the meshes of which are rectangular and elongated in such a way that they are cut transversely in transverse sections of the nerves. From the arterial network arteries arise, which traverse the laminated sheath perpendicularly or obliquely, and penetrate into the interior of the nervous bundle. There they give off branches, which take the direction of the nervous tubes and break up into capillaries. The capillaries also form a network composed of a series of much elongated loops, so that the whole is arranged like a chain. All these vessels present externally, beside the special elements composing them, an investment of flat cells, which at many points forms a continuous epithelium. The same flat cells may be observed, but less easily, on the vessels of ordinary connective tissue.

A certain number of lymphatic trunks following the course

[1] "Mémoire sur la Structure du Nerf," 'Repertoire d'Anat. et de Physiol.,' 1827, t. iv, p. 63.

of the nerve are always found in the perifascicular connective tissue. The connection of the intrafascicular connective tissue with the lymphatics has not yet been demonstrated.

Some physiological considerations conclude the paper.

The Luminous Organs *and* Light *of* Pyrosoma. By Paul Panceri, Professor of Comparative Anatomy in the University of Naples.

The memoir presented by me to the Academy[1] is intended to make known the results of the researches which I undertook during last winter, and which I continued in December and January of this year on *Pyrosoma giganteum*, in continuation of those which I have already made on animal light. In the historical sketch which forms the introduction to my memoir I have quoted, first, the observations made by Peron in the Atlantic between 19° and 20° W. long. (from Paris) and 3° and 4° N. lat., when, in December of the year 1800, on his way to Australia, he met a bank of Pyrosoma, which, during a dark night, strongly illuminated the waves tossed by the storm.

Bennet, in 1833, in the Atlantic, close to the equator, saw the sea all on fire owing to the Pyrosoma, and he gave some valuable information on this phenomenon. I afterwards analysed the memoir of Meyen, in which he describes a luminous organ, which was in consequence of his assertion so generally believed. My observations demonstrate the error into which Meyen and Bennet fell in attributing a luminous power to the red pigmentary cells, which are spread over the surface of the œsophagus and stomach.

The work of Huxley, although the author's principal object was to make known the parts of the Pyrosoma and some natural analogies with the other Tunicates, contains some data on the light which emanates from that animal, and makes mention of the luminous points which are spread and propagated on a Pyrosoma; that is the light which traverses the colony from one end to the other. These authors, as well as others who have written on the Pyrosoma,

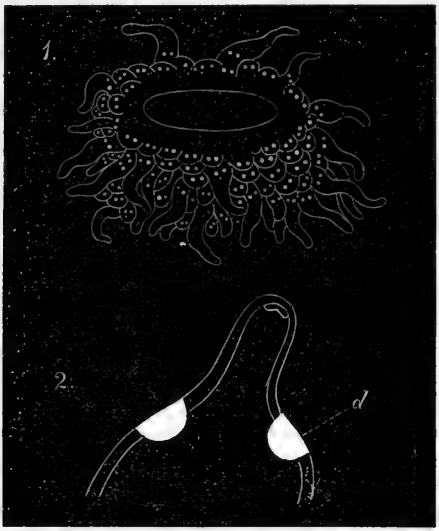
[1] We must refer the reader to the fifth volume of the 'Atti della R. Accademia della Scienze Fisische e Matematiche di Napoli' (1872), for longer memoirs by Prof. Panceri on the Phosphorescence of Pholas, Pyrosoma, Pennatula, and Phyllirhoe, illustrated by numerous beautifully executed plates.

have not succeeded in indicating where the luminous movement of the animal has its seat, and this is why, as preliminary to my memoir, I have said by what means I succeeded in determining with certainty what were the luminous organs of the animal. Every time that I have had at my disposal any Pyrosoma I have been able to prove that its light takes its origin really from thousands of brilliant points or spots placed at nearly equal distances one from another in the inner surface of the tube: but I perceived also that these spots were disposed in couples.

At first sight it is difficult to determine exactly the seat of these brilliant points, yet in the direction of the largest conical tubercles there are sometimes seen a couple of luminous points more raised than the others (which make the surface of the tube rough). For determining, in the first place, the precise spot where these luminous points are found, I thought of making use of fresh water, which has the power of fixing the light in the phosphorescent animals of the sea; and whilst a whole colony was all alight by this means, I cut the tube through (Fig.1). By this proceeding I was able to observe that the couples of luminous points are found very near to the exterior wall of the tube, and nearly in the same position where the ganglia of the ascidia are found. In observing the conical tubercles with which there always corresponds an ascidium larger than the others, with a very long neck, so as to reach two thirds of the height of the tubercle, I perceived that the two luminous points belonged to that ascidium, and owing to the lengthened form of the neck they were far more raised than the others. By this observation I was convinced that in the Pyrosoma the light has its source in determinate parts, which are in the proportion of two to each ascidium.

In subsequently examining the little ascidia in the place in which the luminous points ought to correspond, I found nothing but two bodies, which Le Sueur and Savigny had declared to be the ovaries. Yet, before studying these organs, it was necessary to be assured whether it were really from them or from some other source that the light sprung. Having made some transverse sections with the razor through the walls of the tube, so thin that they could contain but one layer of ascidia, I again employed fresh water, and I placed these sections under the microscope. The observations were made in the evening, and when there appeared under a magnifying power of eighty diameters and even less, in the field the two aforesaid bodies, I extinguished the lamp, and at the same place and in the same forms, the two luminous spots appeared

(Fig. 2). I say under the same form, for if it happened that one of the organs was on its side, and, consequently, with a very different outline to the other, the luminous image, nevertheless, seen in the darkness repeated the same outline. By this proof I assured myself that the luminous organs of the ascidia of the Pyrosoma are really those bodies which the two above-named naturalists considered to be ovaries.



As one Pyrosoma, which is, for example, eight centimeters long, contains about 3200 ascidia, there will be then in the whole colony about 6400 brilliant points. The phosphorescent organs, which I recognised as such in the Pyrosoma, have not then been ignored by anatomists, and they were thought to be ovaries until Huxley, in 1851, proved that the ovary is placed near the testicle, and is composed of an ovisac and of a single egg, as is the case with the Salpa. In consequence of these observations the functions of these organs having become problematical, Huxley confined himself to calling them " cell masses," and he figured them under the same name, both in the adult and in the young born by budding, expressing, however, a suspicion that they might be urinary organs.

Vogt also, in his drawings, annexed to the information which he gives on the Pyrosoma, represents these organs without making mention of them, and Keferstein and Ehlers, under the title of " brissenförniger Körnershaufen,"

describes them exactly without speaking of their signification. These organs, then, are found in each ascidian at the base of the back, in correspondence with the upper edge of the two branchiæ at the bottom of each of the lateral arches of the vibratile bands, and immediately below the two nerves, which form the first or upper part of the lateral nerves of the ganglion. The form of these organs is oval and sometimes triangular; and if they are observed from one side, one can perceive how they are placed in the blood-lacunary space placed between the two tunics of the teguments, and are exclusively attached to the external tunic. As to their structure, they are made up entirely of spherical cells of 0·2mm. diameter on an average which are not found shut in a common membrane, but are bathed directly by the blood of the lacuna. These cells have no nucleus, and contain a substance soluble in ether and an albuminous substance. Notwithstanding the proximity of these organs to the nerve already spoken of, no filament is seen which on leaving that nerve proceeds to end in the organ; the organ in question receives very probably nerves from cutaneous filaments.

The luminous organs once recognised in their structure, I occupied myself with their origin in the embryos. From the researches of Savigny and from those of Huxley it is known that the Pyrosoma has two sorts of embryos; the composite embryos, which proceed from the egg, or rather from a nurse or generative larva, that Huxley called "cyathozooid," which produces the four twins which become the founders of a new colony, and then it appears that there are other embryos produced by budding on a special tubercule which is found at the base of the endostyle. These last are destined to remain in the colony, which thus grows and enlarges. I have followed the development of the two sorts of embryos at the same time, and I have observed that the luminous organs are formed from the external layer of the blastoderma of which they are a part. The cells which compose this organ are distinctly seen when the first traces of the branchial apertures are perceived.

I was able to prove by using fresh water that the luminous organs of the embryos of the young colonies, which are on the point of being laid, have already the power of shining, so that these organs are of such a nature that from the embryo of the two sorts to the adult condition they change neither their form nor their functions. Having ascertained that the phosphorescence may be manifest from the earliest age of the young colonies, I then occupied myself in studying it in the adult, first making known the varying states in which the animal

may be found, especially on account of the weakness to which it is subject on being submitted to experiment; after that I described the luminous currents. In the Pyrosoma these currents may be compared to those of the Pennatula, because the light starts from the point excited, and spreads through the whole mass; they are not, however, so rapid nor so flaming as those of Pennatula, and are not repeated spontaneously after a single stimulation, and the two convergent currents have never been observed to pass one another.

It is also very important to remark the fact of the different colours which the light can exhibit in the Pyrosoma. Whilst in the species studied by me (*P. giganteum*), as well as in that studied by Huxley in the Pacific, the light was clear azure,—in the *P. atlanticum*, studied by Péron and then by Bennet, the light appeared red at first, and then gold colour, orange, and then greenish, and finally ultramarine blue. This phenomenon of the changing of the colours of the light in the same individual can only be compared to the tricolour phosphorescence of the Appendiculariæ observed by Giglioli in crossing from Montevideo to Batavia.

The special studies made for the purpose of explaining the transmission of the excitement, which produces progressively the light in the different ascidia of the Pyrosoma-colony, have led me to the discovery of a particular social muscular system, by which all the ascidia are united with each other. Having, then, described the muscles of the diaphragm which are found at the entrance to the common cloaca, mention must also be made of the special muscular ribands which, by interlacing, join together the ascidia, and are attached to them where there corresponds in each the constrictor muscle of the cloaca. These muscular ribands are not always very regular in their course, nor always of an equal number with the ascidia. They can, nevertheless, be classified in two categories, according to their direction.

There belong to a first category those which are observed in a section of the tube of the Pyrosoma, which is made perpendicular to the axis, and these pass usually from one ascidium to another, crossing so that the fasciculus which is found at the back of one goes and encircles the ventral aspect of the other, to return to the back of the third, and so on.

There belong to a second category those which go from one ascidium to another, parallel to the axis of the tube, thus reuniting the ascidia of one horizon with those of another by uniting their homonymic sides. After having spoken of the special conformation of those organs which are used to

dilate the orifice of the diaphragm, the muscles of the social system are examined, which are formed of long fibres and nuclei, resembling the smooth fibres of vertebrate animals. Since there exists between the ascidia a special muscular social system, it can well be believed that the nerves of this system are those which, passing from one to another, are used for the transmission of the excitement, which is capable of producing the general illumination of the colony.

The researches that I have made up to the present have not yet given any certain results as regards these nerves. My supposition, however, does not cease to be reasonable.

In the last place, I shall explain the results serving to determine the various agents which may cause the appearance of the light:

A sudden shock, rubbing, or touch are enough to excite the light and the currents in fresh specimens, and if, as did Pliny with the Pholades, one masticates a fragment of Pyrosoma, the mouth becomes shining, and when it is opened, the light which comes out of it is sufficient to render easily recognisable the features of a person at hand. Fresh water, as has already been proved, has an energetic action, and if a Pyrosoma is steeped in it, after some minutes it will be seen to be alight, and the light will last several hours, until the death of the animal. The diminution of the temperature of the fresh water in no way diminishes its intensity, inasmuch as having placed two individuals, one in melting ice, the other in fresh water of 35° C., the same effects were obtained on exciting the animal by touch as if the water were of the usual temperature.

In fresh water heated up by degrees, the luminosity of the Pyrosoma is extinguished at 45°.

Alcohol and ether excite the light immediately in the whole Pyrosoma, and it is extinguished together with the life of the animal, about a quarter of an hour after its immersion, but if these two agents succeed in coming in contact with the luminous material of the organ, the light disappears immediately. This fact has been demonstrated equally in the Medusæ and in the Pennatulæ, as in the Pyrosoma; it is also demonstrated by using the liquid which results from pressing the body of the animal through a cloth. This liquid, which pours out in a luminous state contains without doubt the material of the crushed luminous organs, which, a little while after it is extracted, becomes non-luminous; and if, after it has lost its light, it is mixed with fresh water, the light will return very brilliantly, but if, on the contrary, alcohol is used, the light

will not again appear, or if it has been obtained by means of fresh water, it will be immediately extinguished.

Electric currents have no special action on the Pyrosoma in the way of rendering it luminous, and there is every reason to believe that this happens from the want of conductivity in the mucous tissue of the common mantle. Neither daylight nor the action of solar rays on Pyrosoma lessen the luminous power, as in the case of the Beroes.

On diminishing the temperature of sea water to 1° the luminous power of the Pyrosoma will not on that account be visibly weakened, and if, on the contrary, it is heated, the light will appear at about 28°, and will not leave off shining till about 60°.

My investigations on the light of the Pyrosoma have led me to conclude that the photogenic substance of the Pyrosoma is, in all probability, fatty matter. In every case it presents the same phenomena as the matter found by me in the luminous organs of the Pennatulæ, in the cells of the exterior epithelium of the phosphorescent Medusæ, as also in the special organs in the Pholades, in the Chætopterus, as well as in the Beroes, and it reacts with stimulants just as that contained in the Noctilucæ and Thalassicollæ.

When once the Pyrosoma is dead the light can no longer be made to shine from its body, which is then on the way to putrefaction; nevertheless, the matter extracted from the animal, whilst still alive, by means of pressure (of which we have already spoken above), retains for a certain time the power of again becoming luminous by means of mechanical stimulus and of fresh water, even after it has been dried.

Notes *on* Hanstein's Researches *on the* Development *of the* Embryo *in* Monocotyledons *and* Dicotyledons

(Die Entwicklung des Keimes der Monokotylen und Dikotylen, von Dr. Johannes Hanstein, 'Botanische Abhandlungen, Heft 1). By W. R. McNab, M.D., Professor of Botany, Royal College of Science for Ireland.

In Cryptogams the tissues at the growing-point of a stem or root always form by division from a single apical cell. Hanstein, in his researches on the development of the tissues at the growing-point of the stem of Phanerogams, found that the tissues were developed, not from a single apical cell, as in

Cryptogams, but from a tissue of similar cells. In only two cases did he trace back the development to the embryo in the seed. The question, therefore, still remained to be answered as to when the separation of the different meristem layers took place in the embryo, and whether the cells formed from a tissue or a single apical cell.

The growing-point of a branch consists of three meristem layers. The outer is the epidermis, the second the primary (parenchymatous) cortical layer, in which the leaves and branches develope, while the third forms the fibro-vascular cylinder with the pith. None of these layers grow with an apical cell, but by means of a tissue of nearly similar cells.

The three layers of meristem have been distinguished by Hanstein under the names Dermatogen, Periblem, and Plerom, while the first cells of each he calls the "initial" cells. The cells of the Dermatogen develope all the hair or scale-like structures which German botanists group together under the term Trichomes, and by division in one direction only, form the epidermal cells. In the Periblem tissues the lateral branches and leaf-structures (phyllomes) originate, the Plerom also assisting at a later stage. The Plerom forms all the inner tissues, and in it all the chief permanent "secondary" tissues form, In the formation of the different series of cells no geometrical arrangements are to be seen, neither are definite numbers of cells observed in any case.

1. *Dicotyledons.*

As examples Hanstein takes *Capsella Bursa-pastoris*, *Œnothera nocturna*, *Nicotiana Tabacum*, &c.

Capsella is well adapted for observing the different facts recorded by Hanstein. The young embryos are easily separated from the seed by opening the micropyle end of the ovule in a dilute solution of caustic potash, when the pressure caused by the fluid and a little gentle pressure with a needle soon liberates the young embryo. The embryos are rendered transparent by the caustic potash, and by placing them in glycerine and water. If they have been rendered too transparent by this treatment, then a dilute solution of alum will again render the walls distinct, while the contents have been got rid of. When the proembryo consists of about six cells the formation of the embryo begins by longitudinal division of the large end-cell, the embryo mother-cell (Pl. IV, fig. 1). Next transverse division takes place—the embryo mother-cell now consisting of four quarters (fig. 2). In all the

four cells a peripheral division now takes place, not simultaneously, but in rapid succession, the young embryo consisting of four central quadrant cells, and four outer Dermatogen cells (fig. 3). The first division of the embryo mother-cell indicated the position of the cotyledons, while the transverse division indicates the separation between the upper stem or cotyledonary part of the embryo from the lower or hypocotyledonary portion. The outer cells are the mother-cells of the Dermatogen, and the epidermis and its structures are at once and for ever thus separated from the inner tissues.

The Dermatogen cells begin to multiply before the inner cells (fig. 4). In the hypocotyledonary portion the inner cells first divide longitudinally, and then division in the cotyledonary portion takes place. In the hypocotyledonary part the outer cells of the inner portion develope the Periblem mother-cells, the inner the Plerom (fig. 5).

During these divisions in the embryo mother-cell the cells of the suspensor have also been increasing, the number, however, varying and not being definite. The penultimate cell of the proembryo, *i. e.* that next the embryo mother-cell, plays an important part in the development. This cell becomes more convex, and soon forms a portion of the embryo, appearing as if it were an integral part of it. This cell divides into two, the upper forming part of the interior of the young embryo, the other closing it below. This forms what Hanstein calls the "hypophyse." The hypophyse cells sooner or later develope further; division also taking place in the interior cells, while from the outer cell (formed by division) of the Plerom the procambium cells develope at a later period (fig. 6).

At this stage it will be observed that the Periblem and Plerom cells are bounded below by the upper part of the upper hypophyse cell, while the Dermatogen cells are in contact with the lateral walls of the lower hypophyse cell. Up to this stage the young embryo is in the form of a sphere, without any trace of external differentiation, and here the first stage of development ends.

The cells of the upper half of the embryo now become active, the embryo flattens, becoming triangular and then cordate, by division of the inner cells. These as yet show no separation into Periblem and Plerom (figs. 7 and 8). In the hypocotyledonary part the Periblem and Plerom cells multiply, the procambium now becoming distinctly marked. Between the procambium cells the pith or axile tissues of the root take their origin.

The cells of the hypophyse now divide so that three layers

form, the upper still forming part of the Periblem and Plerom, while the lower lie between the Dermatogen cells. The Dermatogen cells next the hypophyse cells divide, and a double covering to the base of the embryo is formed. The four lower cells are the mother-cells of the Pileorhiza, while the upper four form the Dermatogen initial cells of the root end (fig. 9). The upper hypophyse cells by division enclose the Periblem covering the end of the Plerom, and are most important in the further development of the root. The second stage of the development of the embryo is now complete.

In the next stage the cells divide, and the embryo enlarges until it is fully developed, the Periblem and Plerom-cells becoming more differentiated, and the pileorhiza more complete.

The next example is *Œnothera*, in which the proembryo consists of only two or three cells. The process of development, however, agrees in all essential points with that of *Capsella*. The same may be said of *Nicotiana*, *Viola*, *Veronica*, &c., of which figures are given.

2. *Monocotyledons.*

The embryo of Monocolytedons has only a single phyllome, which often hardly resembles a leaf, instead of two symmetrical ones as in Dicotyledons. As examples Hanstein takes *Alisma*, *Liliaceæ*, *Atherurus ternatus*, and *Gramineæ*.

The first cell of the proembryo of Alisma is so large that the young embryo appears as a kind of stalked parasite attached to it (fig. 10). The embryo is club-shaped, the earlier stages showing a two-celled stalk with a head consisting of five horizontal layers, the lowermost layer being either an individual cell or a pair of cells, while the others are either single or double pairs of cells. These layers arise from three cells which formed the apex of the row of cells forming the proembryo. The two upper cells divided first transversely and then longitudinally. The third lowest cell represents the hypophyse of the Dicotyledons, and appears at first undivided, but soon divides longitudinally. When the embryo consists of sixteen cells the Dermatogen begins to form, and its formation exactly resembles that in the dicotyledons (fig. 11). Further division of the cells in the interior takes place, and a second hypophyse cell is to be observed. The separation of the Periblem and Plerom, and the closing in of the layers below, is to be observed here, although not quite so clearly as in Dicotyledons. The first stage ends here.

A depression now becomes visible between the cells formed from the original cells *a* and *b* (fig. 12). The outer differentiation of the embryo is thus produced, the upper half *b* is the origin of the cotyledon, the under half is the hypocotyledonary part. The growth of the cotyledon is very rapid up till the ripening of the seed, while the lower part developes very slowly. The depression becomes deeper, and as the embryo enlarges by the increase of its cells, the deeper the apex of the stem sinks in the hollow, the apex being protected by the tissue at each side (fig. 13).

The second phyllome, the first stem leaf, is also formed and is well developed before the seed is ripe. The formation of the pileorhiza in the Monocotyledon resembles that of the Dicotyledon in all its essential details (fig. 14).

The development of the embryo of *Funkia* and Allium is also described, but for details we must refer to the paper itself.

Brachypodium is also described. In it every cell of the proembryo (except, perhaps, one very small one) forms part of the embryo. The end cells multiply and form the peculiar appendage to the grass embryo, the hypophyse being deeply seated and the pileorhiza formed deep in the tissues of the embryo. The first phyllome, the cotyledon, forms the so-called scutellum of grasses.

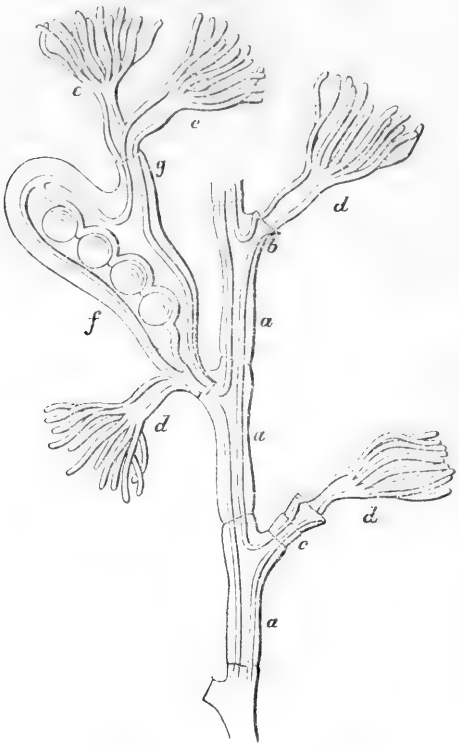
The paper next gives a *résumé* of the more important facts, then reviews the whole literature of the subject, and, lastly, discusses the general morphological deductions from the facts observed ; but for fuller information on these points we must refer to the paper itself.

The whole work is a most valuable contribution to botany, and, as but a few forms have as yet been examined, a large field is opened up in which British observers might labour to advantage, and employ some of the microscope power of which so much is going to waste in this country.

On the Homology *of the* Gonangium *in the* Genus Halecium. By Professor Allman, F.R.SS. L. & E., F.L.S., &c.

In the hydroid genus Halecium (see woodcut) the hydranths, as is well known, are carried on the extremities of lateral processes of the stem. These processes are sometimes simple (*b*), but they are frequently extended by one or more

tubular prolongations, when they assume somewhat the appearance of the drawn-out tubes of a telescope (*c*).



Portion of a female colony of *Halecium Beanii*, with hydranths and gonangium.

a, *a*, *a*. Internodes of the stem. *b*. Simple lateral processes of an internode, supporting a hydranth. *c*. The same prolonged by two accessory tubes. *d*, *d*, *d*. Cauline or ordinary hydranths. *e*, *e*. Gonangial hydranths. *f*. Gonangium. *g*. Tubular orifice of gonangium.

From the obvious relations of the species of Halecium with the Calyptoblastic rather than with the Gymnoblastic Hydroids, the processes which support the hydranths are referred to as hydrothecæ in the various publications which treat of the descriptive zoology of the genus. No true hydrothecæ, however, are ever developed in Halecium; the hydranths are incapable of retraction within the tubular processes which support them, whether these are prolonged

by accessory tubes or not, and are thus as truly naked as in any of the gymnoblastic genera.

That the supporting processes are not hydrothecæ is obvious, not only from the non-retractility of the hydranth but from the very important fact that the fleshy annular projection, by which in the campanularidæ and certain other calyptoblastic hydroids the hydranth is fastened by its base to the bottom of the hydrothecæ, is here attached just within the extreme margin of the lateral process. Retraction within the process is thus rendered impossible, and the sole representative of a hydrotheca is the scarcely perceptible rim which extends beyond the fleshy diaphragm of attachment.

Though hydrothecæ are thus never developed in Halecium, we find on the other hand that the generative elements are, as in all the other calyptoblastic hydroids, enclosed within well-developed chitinous receptacles or gonangia (*b*). Now, these gonangia in other Calyptoblastea are homologically identical with hydrothecæ. They are hydrothecæ modified for a definite purpose, namely, the protection of the generative buds. In all Calyptoblastea, with the exception alone of Halecium, that part which in the trophosome becomes a hydrotheca becomes in the gonosome a gonangium.

Now, I regard it as otherwise with Halecium, and I maintain that in this genus not only is the proper hydrotheca suppressed, but that it does not appear even in the modified condition of gonangium. I regard the gonangium of Halecium as possessing an entirely different homological significance from that of a hydrotheca, and I believe it can be shown that, instead of being a modified hydrotheca, it is a modified segment of the stem.

In species like the common *Halecium halecinum* the real nature of the gonangium is not at once apparent, but those species with the so-called slipper-shaped gonangia, such as *H. Beanii*, afford a key to the determination of the relations in question.

A glance at a female colony of *Halecium Beanii* renders apparent the resemblance between the gonangium (*f*) and a hydranth-bearing segment or internode (*a*, *a*, *a*) of the stem, and this resemblance is something more than a mere similarity of form, for it is the expression of a deep-lying homology. In both we can recognise the main body of the internode which in the gonangium becomes swollen and otherwise altered in form so as to constitute a receptacle for the ova, while the lateral process is also obvious in both. This in the trophosome (*b*, *c*) carries a hydranth and in the gonosome becomes the projecting orifice (*g*) of the gonangium.

This view of the homology of the gonangium affords at once a solution of the hitherto enigmatical phenomenon of the gonangium carrying hydranths (*e, e*) which in no respect differ from the true hydranths (*d, d, d*) of the trophsome. The position of these gonangial hydranths is exactly the same with respect to the gonangium as that of the cauline hydranths is with respect to an internode of the stem. The only point which remains unexplained is the fact of the gonangial hydranths being invariably in pairs—a fact, however, which in no degree lessens the validity of the relations here insisted on.

Though the relations between the gonangium and an internode in other species of Halecium is not at once so obvious as in those with slipper-shaped gonangia, it is plain that the only difference consists in the fact that the gonangial orifice in the former is carried upwards, so as eventually to assume a terminal position on the summit of the gonangium; while in the latter it retains in the gonangium the position which belongs to it in the internode.

According to the view here maintained, then, the gonangium in Halecium would represent a lateral branch which had never passed beyond the development of a single internode.

It is the gonangium in the female colony which I have thus made the subject of comparison. In the male the gonangium presents nothing which would suggest any other significance than that which belongs to the gonangium of other hydroids. It differs in no essential point of form from these, nor does it ever carry the gonangial hydranths which are so characteristic in the female.

It may be that while in the female the gonangium represents, as just maintained, an internode, in the male it represents a hydrotheca. There is nothing impossible in this view, but it is more in accordance with morphological law to regard both as referable to the same fundamental type of form from which the female departs but slightly, while modification is carried much further in the male—to such an extent, indeed, as to conceal the true relations which the study of the female colony reveals to us.*

* Another question connected with this subject here suggests itself. I have never succeeded in finding a true sporosac in the female gonangium of Halecium. When the ova were here recognisable I always found them within a common chamber which lay along the side of the blastostyle (see woodcut). I had regarded this chamber, however, as belonging to a late condition of the generative structures, and believed that the ova, originally formed within a true sporosac, had become ultimately discharged, and were then, after the absorption of the sporosac, confined in a cavity formed by the extension over them of a layer of the ectoderm of the blastostyle. This may be the

REVIEWS.

The Beginnings of Life: being some account of the Nature, Modes of Origin, and Transformations of Lower Organisms. By H. Charlton Bastian, M.A., M.D., F.R.S. Two vols. London: Macmillan.

The publication of this book is a very serious matter, as well for the reputation of English science as for the cause of the diffusion of sound knowledge among the wider public. It contains either the exposition of a great discovery and the final demonstration of the truth of views on the nature and origin of living forms which have long been urged on men of science, and as such is a grand triumph for its author, placing him in the position which he would doubtless claim for himself, not second even to Darwin—or, it is an inexcusable compilation of trash, and a disgrace to its author and his position.

That is the simple issue. The manner in which the work has been brought out, the success with which the author has met in attempting to enlist the interest of general readers in his speculations, and the unanimous condemnation with which the book has been received by those men of science supposed to be competent authorities in the matters of which it treats, combine to make the position no less grave than that described above.

Dr. Bastian adopts a confident tone; he does not suggest hypotheses, but boldly appeals from the circle of scientific

true interpretation, but another now suggests itself; for it is possible that no true sporosac has been ever formed, and that the ova have had their origin in the walls of the blastostyle, which here represents that portion of the cœnosarc which in the trophosome is contained within an internode of the stem. We know that in some rare cases, as in *Sertularia pumila*, ova are formed directly in the walls of the blastostyle, though here there exists also a true sporosac; and there is reason to believe that in some very exceptional instances ova may also originate in the walls of the cœnosarc within the cavity of the stem. May it not be that in Halecium they are also produced directly in the walls of the blastostyle without the intervention of a sporosac?

In the male colonies of Halecium it is different, for there the generative elements are formed within a true sporosac, which is developed as a bud from the blastostyle.

men whom he has entirely failed to convince, to a larger body of judges. He asserts that he has proved the truth of his statements over and over again, and he bitterly attacks those who have refused to accept them. The responsibility which rests with Dr. Bastian is all the greater, since cultivated but uninstructed persons have been led by the strength of his asseverations, and the remarkable power of expatiation which he possesses, to believe that he is what he claims to be—the author of a vast advance in biological science, and that the disapprobation which his writings have met from eminent biologists is only a form of that persecution to which we all know so well, great discoverers are liable to be subjected.

The main question of which Dr. Bastian treats, viz. the *de novo* origination of living things, is one which in the present state of science it is utterly impossible for the labours of one man definitely and finally to settle. A host of confirmatory witnesses of long-continued and thoroughly trustworthy investigations would be necessary before men could be called upon definitely to pronounce in favour of the existence of such a phenomenon as the so-called "spontaneous generation." We do not, therefore, propose to look at Dr. Bastian's book with the view of considering whether he settles the question or not, but simply to ask does he make out a *primâ facie* case? Is he justified in urging his views, not only upon the more limited body of scientific students, but upon the public generally? Has he brought the discussion, by means of carefully devised experiment and patient research, to such a point that he is justified in saying to his opponents, you must examine this and that, you must perform this experiment and carry out that investigation, before you venture to differ from me or object to the truth of my views? There are three startling natural processes which Dr. Bastian declares he has conclusively demonstrated to occur, and of which he offers proof, or records of proof, in these volumes, the rest of their bulk being occupied by an introduction intended to prepare the mind of the reader for a belief in the existence of these processes, and by a final chapter in which the most astounding general views are promulgated as natural deductions from the existence of these processes. The processes are—(1) the transformation of saline substances in solution into living things or organisms; (2) the transformation of organic matter, *i. e.* dead matter which has formed the substance of organisms, into new and living organisms; (3) the direct transformation of organisms of one kind into organisms of another, such, for instance, as the transformation of the spores of an Alga or a Euglena into Nematoid worms or Rotifers.

Dr. Bastian, in coming forward to assert that he has proof of the existence of these processes, is in much the same position as an alchemist. The alchemist used to declare that he possessed the power of forming gold from other elementary substances—that is, from matter which is not in the condition of gold. Dr. Bastian claims to have the power of converting solutions of saline and organic substances into living matter or protoplasm. Let us reflect for a moment upon the delusions of the alchemists, their enthusiasm, earnestness and conviction, all based upon the very wildest error—error due to the want on their part of the scientific method. Let us consider what kind of proof we should to-day demand from any person reviving the exploded pretensions of the alchemists—what rigid adherence to method and to a step-by-step demonstration of his position — and then let us turn to Dr. Bastian. Though the phenomena would not be precisely parallel, there is but little greater intrinsic improbability in the conversion, say, of chalk into gold, than of tartrate of ammonium into protoplasm. It is conceivable that chemists should find methods for both. But why should we require a less strict proof of the latter than the former? Is it because we know so little of the chemical constitution of these complex bodies—such as protoplasm—that we are to accept statements about them parallel to those made concerning the metals in past ages, when chemical knowledge was equally incapable of dealing with them? If we expect from Dr. Bastian a methodical account of the manner and conditions in which 'dead' matter becomes converted into 'protoplasm' in his experiments, we shall be disappointed. The evidence he offers on the subject is—when looked at from this point of view—altogether deficient and curiously like that of the old alchemists. A lot of rubbish (composition unknown) is put into a crucible—in the modern experiment it is a glass tube—this is heated, shaken, subjected to pressure or relieved from it, and gold or protoplasm, as the case may be, comes out. We are justified in demanding from any one who attempts to establish what Dr. Bastian attempts, a knowledge and control of conditions and the power of elimating chance, which he does not pretend to possess. In so far as the results of manipulations of solutions recorded by Dr. Bastian are to him matters of 'chance,' they are not scientific experiments, but alchemy. Whilst, then, we must at once reject all pretensions on Dr. Bastian's part to the position of one who has succeeded by using the scientific method in following up and demonstrating the formation of protoplasm from its elements otherwise combined, we

may nevertheless waive a point for him and demand a less severe proof—in fact, look at his observations on, what are to him, fortuitous combinations of the elements, for what they are worth. Some persons who accept the doctrine of evolution bring forward cogent arguments to lead *à priori* to the conclusion that the combination known as 'life-stuff' or protoplasm, once formed on the surface of the globe, has never been re-formed; the conditions, they say, were quite peculiar to that period of the earth's history and are most unlikely ever to recur. Other equally staunch evolutionists say, why should those conditions not recur? and endeavour to show, from the existence of very low organisms at the present day, that the conditions necessary for archigenesis or archebiosis—as Dr. Bastian well terms it—are in some parts or other of the earth's surface ever recurring. There is no means of deciding between these two views as the evidence at present stands, and the latter has as much in its favour as the former, quite sufficient to warrant us in adopting it as an hypothesis for guiding further inquiry. That being the case, it must be conceded that there is just a chance that out of all the myriad variations of natural conditions on this world's face, those conditions presented by the glass tubes containing solutions heated, closed with diminished pressure, and otherwise manipulated by Dr. Bastian, are the conditions necessary for archebiosis. There is this bare chance, though it must be observed that the most salient features presented by the conditions were contrived with a view to excluding the access of germs of organic life from the tubes, and not at all with a reasoned-out purpose of favouring archebiosis—we allude to the heating and the diminution of pressure.

The first portion of Dr. Bastian's book treats exceedingly well of the correlation of the modes of force, and fairly sets forth the case in favour of the chance of the evolution of living matter in the solutions of which he afterwards treats. Remembering then that there is a bare chance of obtaining positive results with these particular solutions and conditions —but that it is only a chance and not a reasonable result to be legitimately anticipated from the results of previous experiment—let us ask what is the value of the evidence which is put forward in this book to induce us to admit, as a subject for further investigation, the occurrence of archebiosis in saline and organic solutions?

A necessary factor in the value of this evidence is the personality of the witness. Dr. Bastian has not a number of corroborating witnesses who have worked with him, and

who by their interchange of criticism might serve to remove the personal element from results obtained. It is, therefore, necessary to ask—how far is there reason to have confidence in the precautions taken in the experiments recorded in these two volumes, in the descriptions of them, and in the reasonings upon them? However distasteful it may be, it is necessary to ask this question; it is one which is continually asked in the case of work like the present by readers, and must be answered simply and truly. Dr. Bastian is a graduate in medicine and arts of the University of London, and is, we believe, some fifteen years past his student days. He has been in the East, and for some two years held an appointment at Falmouth, when he devoted his attention to the forms and the minute anatomy of the Nematoid worms. The two large papers which he has written on this group show great industry; but that in which the anatomy of these organisms is especially treated, though a thoroughly acceptable piece of work, does not bear striking evidence of power of observation and interpretation of facts, and some of its chief conclusions are considered to be quite untenable by other more authoritative observers. In whatever way that matter may be settled there is no doubt that these were able papers. For this work Dr. Bastian obtained the Fellowship of the Royal Society, he has been pathological lecturer at St. Mary's Hospital, and is now in a similar position at University College. It is now nearly four years since Dr. Bastian began writing on the spontaneous generation question. He appears, on his own showing, to have published his first conclusions on this deeply involved matter a few months after his first experiments, and he has very consistently adhered ever since to his first statements. A series of articles first published in our contemporary 'Nature' were afterwards in a somewhat modified form brought out as a little book, and we now have these two volumes of a thousand pages, containing the old woodcuts and many of the old paragraphs, with much additional matter and illustration.

Dr. Bastian has *not*, previously to attacking this question, given any evidence of a familiarity with the lowest forms of life; his evidence, therefore, has not the weight which that of a professed botanist or zoologist—a De Bary or a Haeckel—would have. We mean that the supposition of mistakes in the nature of the objects seen by him is not excluded as a possible explanation of some of his results, and, moreover, the silence of all the great zoologists and botanists (notwithstanding the very unfair list of believers in heterogenesis given in the preface) who are devoting their lives to studying the very forms which Dr. Bastian gets by archebiosis, stands

out the more prominently as an indication of the repressive effect of truly scientific study upon the genesis of theories like Dr. Bastian's, from the fact that he is not by any means a follower, however humble, in their ranks. Nor has Dr. Bastian, previously to his researches on spontaneous generation, made himself known as a chemist nor as an experimentalist. We have therefore no reason to place especial confidence in, nor to treat with the respect due to proved ability his statements and manipulations as biologist, chemist and physicist. On the other hand, whilst engaged in urging his views on the spontaneous generation question, Dr. Bastian has given the following evidence of his fitness for the work—evidence which we consider it is very important to bear in mind in deciding whether he in this book makes out such a case as to call for further attention from biologists.

1. He writes in 'Nature,' vol. ii, p. 195:

"We are told in Watts's 'Dictionary of Chemistry' that after an aqueous solution of cyanate of ammonia has been prepared the 'liquid exhibits the reactions of a cyanate, but when heated or left to evaporate spontaneously it is converted into urea.' This would seem to show that the passage from the crystalloid to the colloid mode of molecular collocation is by no means a difficult one—that it may be brought about, in fact, by very slight determining causes."

Hence Dr. Bastian argues upon the supposition that urea is a colloid! The physiological implication is startling.

2. At p. 98 of the same volume: Dr. Bastian writes:

"At the time I thought I possessed a pure potash-alum, and by some strange oversight I had failed to recognise that if this had been the case I should have been employing a solution which contained no nitrogen. * * It was not, therefore, till my attention was called by Dr. Sharpey to the assumed absence of nitrogen in the above solution that I became aware of this. * * I then had some of my supposed potash-alum carefully tested, and it was found to contain a considerable quantity of ammonia."

We could have wished that Dr. Sharpey had always been by Dr. Bastian's side to help him to become aware of 'strange oversights,' and to ensure the proper analysis of all the materials used by him.

3. At page 473 of the same volume Professor Huxley gives a contribution towards the determination of the personal value in Dr. Bastian's researches. "He (Dr. Bastian) will recollect that he wrote to me asking permission to bring for my examination certain preparations of organic structures, which he declared he had clear and positive evidence to prove

to have been developed in his closed and digested tubes. Dr. Bastian will remember that when the first of these wonderful specimens was put under my microscope I told him at once that it was nothing but a fragment of the leaf of the common Bog-moss (Sphagnum), and he will recollect that I had to fetch Schacht's book 'Die Pflanzenzelle' and show him a figure which fitted very well with what we had under the microscope before I could get him to listen to my suggestion, and that only actual comparison with Sphagnum, after he had left my house, forced him to admit the astounding blunder which he had made."

Of these three pieces of evidence the last is the most important, for, whilst it places us on our guard with regard to Dr. Bastian's accuracy generally, it at the same time furnishes a key to the explanation of a number of his experiments in which, according to that precipitate discoverer, 'organisms' were found on opening tubes containing infusions which had been boiled and sealed hermetically.

Having thus, we trust dispassionately, formed an estimate of the trustworthiness of the witness who addresses us in the two bulky volumes before us, we may look a little further into the book itself. The book is, as we have before mentioned, divisible into an introduction, to the merits of which we have already given our tribute of praise, a central piece, and a very adventurous terminal chapter of speculations and suggestions. The central piece records experiments intended to demonstrate the occurrence of archebiosis from (*a*) saline solutions, (*b*) from infusions of organic matter—that is, matter which was but shortly before in the living condition; further, it records observations intended to establish the existence of the process termed "Heterogenesis," or the *per saltum* development of organisms of one kind from organisms of another kind. The two subjects of archebiosis and heterogenesis have little in common; the one might be truly a fact of nature without affecting in any way the probability of the other being also a fact. The question of the existence of archebiosis is the one which has been in these days the more seriously raised. For the probability of its occurrence under some conditions or other a great deal may be said, and we therefore regard it in some measure as a matter *before* the scientific public (though the particular case of the asserted tube-produced archebiosis has no such *à priori* claims on our attention). The doctrine of heterogenesis (as understood by Dr. Bastian), on the other hand, cannot be said to have ever been either seriously advanced or attacked in recent years. Certainly no one has professed to have witnessed facts

demonstrating this *per saltum* transformation of organisms whose evidence has been considered worth a moment's serious attention. The names of genuine supporters of this doctrine whom Dr. Bastian quotes—and with whom he most unpardonably associates the names of eminent observers who would repudiate such an interpretation of their observations as gratuitous nonsense—are names which carry even less weight than his own—*e. g.* Gros, Pineau, and Pouchet. The claims of the doctrine of heterogenesis to discussion being then so slight, we shall inquire how Dr. Bastian handles this less pressing matter, in order still further to develop our estimate of his competency as a scientific witness in the case of the more immediately important problem of archebiosis, or the *de novo* genesis of organisms. We do not hesitate to say that the statements contained in the latter half of Dr. Bastian's second volume amply confirm the suspicion of carelessness and precipitancy which the three blunders already quoted from his other writings justly arouse in the mind of an impartial critic, and are, as they stand in his own unvarnished story, enough to render any assertions, which he may have to make on the results of experiments on the production of organisms in solutions, of very little value. It is a distressing thing to be obliged to express so strong a condemnation of the work of one who has claimed to rank among scientific men; it must, however, be said that it is not possible to find an excuse for Dr. Bastian for having put out such manifest absurdity and misrepresentation as that which fills his chapters on heterogenesis. We do not say that adequate evidence would fail to convince us of the truth of Dr. Bastian's assertions as to the transformation of milk-globules into Fungi, Euglenæ into Nematoid worms, &c.; we are not unfairly prejudiced in the matter. But we cannot, and no reasonable persons even generally instructed in the nature of the things dealt with could for a moment, regard Dr. Bastian's conclusions as other than purely gratuitous,—taking his own statement of facts as accurate. Many microscopists are well acquainted with the appearances presented by Euglenæ, Vaucheria, Chlorococcus and Nitella. A large number of most able and approved botanists have devoted much of their lives to the study of these forms, giving far greater care and patience to it and bringing larger previous experience to bear on the matter than Dr. Bastian has done, and they have not, in spite of the misleading quotation of some eminent names by him, interpreted

the phenomena seen by them in a sense in any way consistent with heterogenetic views.[1]

When Dr. Bastian tells us that he saw on the field of his microscope the normal form of an Alga-spore, or of Chlorococcus or Euglena, and then afterwards or side by side an intermediate condition, and also finally the egg of a Rotifer or Nematoid, or even the perfect condition of these highly organized beings, and coolly tells us that because these things occur together the latter are *unmistakably* developed from the former, we are naturally enough astonished, not so much at the nature of the supposed transformation, as at Dr. Bastian's logic. We have read most earnestly his account of numbers of such observations, and have studied his illustrative drawings, and venture to say that they prove simply nothing, excepting that Dr. Bastian is in an abnormal state of mind. We hold that no person making normal use of his reasoning powers would, on seeing the objects drawn in fig. 95 of Dr. Bastian's book, come to the conclusion that they demonstrated the conversion of the spores of the Alga Vaucheria into Nematoid worms, nor would any number of such observations as those recorded and illustrated lead a normally active mind to such a conclusion. They would to a man of ardent imagination possibly suggest an hypothesis of such a genetic connection, and this he would proceed to test most carefully and repeatedly if he had a desire for the truth—a true scientific spirit—before mentioning it at all, even privately. He would isolate such spores and watch for hours, days, and nights, if necessary, by the aid of trustworthy assistants, the same individual spore, until he had satisfied himself what really became of it. Then, if he got such a result as that, which Dr. Bastian would have us accept on the strength of his own imagination, it would be received with respect, other observers would endeavour to repeat the inquiry, and Science would not have to resent the unbridled indulgence of a fantastic imagination. On the other hand, what opinion are we to form of the man who does not make the least attempt to check or

[1] It is an inexcusable thing on Dr. Bastian's part to persist in quoting (as he has done in a recent number of 'Nature,' in reply to an exposure of his fallacies by Mr. Moseley, published in the 'Academy') the name of such an eminent botanist as Pringsheim as an upholder of the doctrine of Heterogenesis, which he cannot but know he is not, as well as the name of our countryman, Mr. Carter, who has expressly repudiated the doctrine. In the same letter to 'Nature,' Dr. Bastian speaks of his "friend Dr. Burdon Sanderson" in such a way as has led readers, to our knowledge, to suppose that Dr. Burdon Sanderson was favorable, instead of hostile, which he is, to the doctrines propounded in the 'Beginnings of Life.'

test the suggestions of his fancy, but dogmatically asserts that they are demonstrated as true, without having made use of the ordinary methods of inquiry known to students of such matters, and suggested by even the most ordinary common-sense?

Dr. Bastian in this year of grace 1872 actually adopts the intellectual attitude—almost the words—of old John Gerarde, the herbalist, who wrote in the beginning of the seventeenth century, and described among other things the transformation "by an unmistakable process of heterogenesis" of the buds of an oak-tree firstly into barnacles, these becoming in their turn transformed into barnacle geese. There is absolutely nothing more incredible in Gerarde's story than in Dr. Bastian's, and the evidence which they bring in support of their views is of equal value. It is scarcely possible that Dr. Bastian believes Gerarde's case of heterogenesis to be a well-based one, but hesitates at present to introduce him into the list of those who have "witnessed unmistakable processes of heterogenesis" on account of his having already some notoriety, which MM. Gros, Pineau, Trécul, have not. Gerarde tells us how he opened some barnacles which he found growing on wood, and therein "found living things that were very naked, in shape like a bird; in others the birds covered with soft downe, the shell halfe open and the bird ready to fall out." "That which I have seene," he adds, "with mine own eyes and handled with mine hands, I dare confidently avouch, and boldly put down for verity." Then he tells us how the barnacle is bred from a certain spume which collects on trees at an island off the Lancashire coast, how the young bird grows in the barnacle and is in due season thrown out unto the waves, when it swims off and, growing, acquires its feathers. As one proof of the truth of this story we are told that "these geese do so abound that one of the best may be bought for threepence." Additional evidence is forthcoming in a very telling woodcut, equal to any of Dr. Bastian's, and exhibiting (1) the young barnacle, (2) the barnacle fully grown and opened, and (3) the young goose.

Dr. Bastian, more bold than Gerarde, is very indignant with those who refuse to accept seriously such accounts of transformations, and actually anticipates the charge which must assuredly be made against himself, by styling the conduct of these persons—eminent men such as Professors Huxley, Haeckel, Milne-Edwards, Agassiz, &c.—"unwarrantable." With more courage than candour Dr. Bastian declares that "unmistakable processes of heterogenesis have been *watched* over and over again." We find on looking at the particular

cases recorded in his book that 'watching' has some special signification for Dr. Bastian, for he has no more 'watched' processes of heterogenesis than old Gerarde watched his geese grow up from barnacles. One would suppose from his confident tone that Dr. Bastian had made use of some of those methods already applied by histologists to the 'watching' of processes of inflammation—methods with which as a pathologist he should be acquainted—for the purpose of watching some of his supposed cases of heterogenesis. We might expect as an accompaniment to his positive statements highly finished and detailed drawings representing the various steps of the process. We find in his book nothing of the kind. So far from this, he naïvely remarks, "My avocations during the day have generally rendered it impossible for me to carry out attempts at development satisfactorily." And yet he considers himself justified in making assertions at variance with the experience of all the most competent observers, and with the most widely received biological doctrines.

Briefly it is to be said that the chapters in this book on heterogenesis, contain a reckless attempt to revolutionise biological doctrine without a single demonstration of fact to justify it, even if it be admitted that the observations and drawings cited are accurate. Revolution in science as in politics can only be justified by success—a wanton attempt in either sphere must deserve the severest condemnation. Dr. Bastian by his exhibition of himself in dealing with heterogenesis writes himself down as incapable—as inadmissible in the character of a witness in a scientific investigation. The Sphagnum delusion is now explained, for it becomes evident that we have to deal with an individual with whom such delusions are no rare exceptions.

We should indeed be sorry to believe that Dr. Bastian is himself aware of the injury which he is doing to the cause of science, by promulgating these rash assertions as to the beginnings and changes of living things; we altogether decline to entertain the notion that he is himself conscious of the baselessness and flimsy character of his startling discoveries, and is nevertheless willing at the expense of injury to the cause of intellectual progress, to obtain for himself a temporary notoriety. On the contrary, we believe that he is under the influence of a paradox, similar to those which from time to time obtain notoriety in the case of 'spiritualists,'[1] 'circle-squarers,' and such victims of belief in the

[1] It is interesting to observe that an eminent traveller and naturalist, also a believer in psychic force, has written a lengthy notice of Dr. Bastian's

marvellous. The origin and mode of growth of such delusions form a very interesting psychological study, and it is only when we have obtained a proper conception of Dr. Bastian as an abnormal psychological phenomenon that we can hope rightly to appreciate the whole of the statements made in his book.

Delusion and self-deception are much commoner things than the world is generally accustomed to consider them. In a very well-known and often quoted remark we have a recognition of the wide-spread occurrence of delusions and an attempt to explain their origin; the saying to which we allude is "The wish was father to the thought." There cannot be the least doubt that men are unconsciously hindered or misdirected in their estimate of fact by previously formed desires. Such a desire acts on the mind like the suggestion of the mesmerist to an individual who has allowed himself to be brought into the hypnotic condition. In this way many misconceptions and strange contradictions of testimony are to be explained. Those only who have used the microscope and endeavoured by the aid of its highest powers to attack unsolved problems of minute structure, know how rigid a purpose, how strict a self-criticism, how constant a suppression of the tendency to see what one wishes to see, is required in order to make observations that will bear the examination of other unprejudiced investigators. There is no finer test of intellectual honesty, no better training for its development, no surer detective of an unconscious want of loyalty to the higher faculties of observation and reason, than work with the microscope. Very few of the large circle of readers and critics of Dr. Bastian's book know that these are the conditions of the observations which he narrates to them, and there is accordingly a probability that his statements will be largely received without due and, indeed, necessary reserve. We venture therefore to press the consideration of this matter upon the reader.

We have pointed out that in treating of heterogenesis, Dr. Bastian argues in a way which must appear to all persons acquainted with lower organisms, hopelessly unsound. He deliberately abstains from anything like criticism or testing of the suggestions of the imagination, and this is explicable on the supposition that he is terribly biassed. We do not doubt that the desire, legitimate in itself, to make a great discovery, to eclipse the reputation of even our greatest biologists, has so to speak mesmerised Dr. Bastian, as similar desires have in-

book in a contemporary, expressing his adhesion to the views put out therein.

fluenced other men before him, and that hence he has become the victim of delusion.

With this estimate of the psychological condition of our author, we now turn to that portion of his work in which he adduces evidence to prove the occurrence of archebiosis, or the *de novo* development of living things from solutions of saline and organic matters. We find a series of experiments—not on the whole very numerous—recorded as evidences which we have already pointed out are divisible into those intended to prove archebiosis from solutions of simple salts, and those intended to prove archebiosis from solutions of matter which has been derived from organisms, and the elements of which are in a high condition of combination as in the albuminoids.

It is not necessary to say much about that series of experiments relating to the saline solutions. It is now clear that foreign matter of all kinds, bits of fungi, vegetable fibre, &c., were in the crystals of tartrate of ammonium and other salts employed in making the solutions. These matters settling in the tubes were, on detection, at once set down as archebiotic products by the expectant investigator. To such a degree had he resigned all exercise of reasoning discrimination, that he was led to declare that a head of spores such as the moulds produce on their aërial branches—and which only are related to that aërial condition—was produced archebiotically in the liquid of one of the tubes. And this fancy, once ventured on, has been persistently adhered to by Dr. Bastian. Mr. Hartley, working in the laboratory of the Royal Institution, has performed a very large series of experiments in order to test the truth of Dr. Bastian's views with regard to archebiosis in saline solutions. The experiments were most carefully carried out, and have been published in the 'Proceedings of the Royal Society.' They furnish a complete answer to Dr. Bastian's assertion on this matter. They establish most fully that archebiosis does *not* occur in the particular saline solutions and under the particular conditions in which Dr. Bastian was led to suppose that it did. Though Dr. Bastian has not the courage to acknowledge the compliment which Mr. Hartley has done him in taking up his experiments in this manner, and in laboriously dispelling the false notions due to careless experimentation and inference, he yet appears inclined, to judge from his most recent utterances on the matter, to modify very considerably the strength of his statement *quâ* saline solutions. In fact, he may be considered as no longer disposed to combat on that field, and stakes now the question of his justification (in asserting the occurrence of

archebiosis in tubes prepared by him) on those cases in which he has made use of solutions of the organic elements in a complex condition of combination, such as, for example, infusion of hay, or infusion of turnip with a little cheese.

The number of cases adduced is by no means sufficient, and the description of the conditions under which the infusions were prepared, the state of the materials used, and the precautions taken, is altogether incomplete—by no means giving confidence that even as far as our present knowledge of the lowest forms of life goes adequate care was used in each particular experiment cited, to exclude access of living things or of bodies which might subsequently be mistaken for such.

The experiments in which anomalous "organisms" of a green colour were discovered in solutions containing salts of iron and bits of wood—besides other matter—after prolonged boiling and hermetical closure, may be at once dismissed as extremely childish. Like the Sphagnum exhibited to Professor Huxley, and like the fragments of fungi in the saline solutions, these 'organisms' are in all reasonable probability fragments (perhaps of the wood put into the tubes) which have settled down and collected after the tubes had been allowed to remain undisturbed for some time. Dr. Bastian does not in his uncritical state of mind think it necessary to ascertain anything definitely about these bodies—anything incompatible with the above suggestion—before publishing them to the world as created by him *de novo* in a closed tube.

The question on which it is most desirable to meet Dr. Bastian, since he records most experiments intended to answer it affirmatively, is with regard to the archebiosis of *Bacteria*. Do *Bacteria* develop in solutions prepared with precautions to exclude living *Bacteria* and their germs, and yet fitted to support the life of those organisms? With regard to the development of yeast-cells or *Torulæ* by archebiosis, Dr. Bastian records so very few experiments which have even a superficial appearance of value, that we may pass them by. *Bacteria* or *Torulæ*, when they multiply in a liquid, make it cloudy. It is only with regard to *Bacteria* that Dr. Bastian pretends to record cases in which, due precautions being taken to exclude the entrance of living *Bacteria*, there was such a development of life in a solution as to cause a cloudiness. He does not appear to have ever obtained this, in duly guarded experiments, with *Torulæ*, and hence it is probable that his *Torulæ* were present in the dead condition from the first.

It is impossible to determine exactly how Dr. Bastian got into error with regard to the development of *Bacteria*

in tubes which he supposed to be properly guarded. But we have already pointed out his mental attitude, and therefore a number of *possible* sources of this error might be urged. One thing is certain—that Dr. Bastian at the commencement of his researches knew very little about the life-history and characteristics of *Bacteria*, nor did his predecessors in this kind of experiment. An eminent physiologist, Professor Burdon Sanderson, determined some two years ago to investigate the natural history of *Bacteria* or Microzymes a little more closely than had been hitherto done. He made a very important discovery (see this Journal, October, 1871) as to the mode of their transference from substance to substance—viz. that it was *not* through the air, but through water, or surfaces which had been wetted. Now, the discovery of a fact like this—which was in contradiction to what was previously supposed, and which Dr. Bastian, we believe, admits as true—is a signal proof of the ability of the investigator who has made it and gives great weight to statements made by him as to the result of other experiments on these same organisms. He has proved himself to have a more intimate knowledge of these things and their ways, than other persons have had, and what does he tell us about the spontaneous generation of *Bacteria* in solutions of organic matter such as Dr. Bastian's? He emphatically asserts that in a number of experiments similar to Dr. Bastian's, he failed to obtain either *Bacteria* or *Torulæ* when due precautions were taken to prevent the contamination of the experimental solution by the germs of the former from dirty surfaces, or of the latter from the atmosphere.

This evidence is overpowering, but Dr. Bastian does not yield. He says now that it is necessary to employ peculiar solutions to get an archebiotic result. Dr. Sanderson failed because his solution was of the wrong sort. Dr. Bastian has injudiciously and in a manner which convinces us that he is carried away by his theory, and does not wish to consciously elude a decisive testing of his assertions, declared in the work before us, that an infusion of turnips, prepared with water not above 45° C., sufficient in quantity just to cover the sliced turnip, to which a fragment of cheese is added, has rarely if ever failed to give in his hands positive results; that is to say, a development of Bacteria has been obtained in the solution after it has been boiled in a tube previously heated to redness to destroy germs, and sealed by fusion whilst the infusion is boiling, the tube having been then kept for some days at a temperature of 30—35° C.

We set ourselves at the commencement of this notice the

task of determining whether Dr. Bastian had made out a *primâ facie* case. We cannot say that the various considerations adduced above allow us to hold that he has. He has not adduced sufficient evidence or evidence of a quality, to warrant him in inviting other persons to make this decisive experiment with turnip and cheese solution. Biologists would, we hold, be perfectly justified in refusing to be troubled by him any further. Time and skill are not to be wasted in confuting the statements manifestly uncritical.

Nevertheless, in consequence of the interest which Dr. Bastian's work has excited, we have made the experiment (and that repeatedly) as directed by him. This is not the occasion on which to give the details of the experiments in question. It will, however, perhaps add some value to the remarks which it has been our duty to make when we state that, carefully following Dr. Bastian's directions, using at the same time great care as to cleanliness and due boiling, we have obtained results which *in every single instance*, out of more than forty tubes closed on four separate occasions, simply contradict Dr. Bastian.[1] We believe, then, that Dr. Bastian's last dogma in archebiosis—his belief in turnip solution with a fragment of cheese—must be placed in the same category as his colloidal urea, his spontaneously generated bog-moss, his fungi born in crystals, his unmistakable processes of heterogenesis, and his 'watching' and 'experimentation.'

The Micrographic Dictionary. Parts I—IX. Van Voorst, 1871-72. Third edition, edited by J. W. Griffith, M.D. assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and T. Rupert Jones, F.G.S.

We have received from the publishers several parts of the new edition of this dictionary, but have deferred noticing them critically until a sufficient number had appeared to enable us to judge fairly of its character and merits. Having now examined what must be considered a fair example of the work, we feel constrained to say that the new edition is by no means what might be expected of the editors, the publishers, or the great reputation which the dictionary at one time enjoyed; and that in certain important departments

[1] The details of these experiments will shortly be published, when it will be possible for the reader to judge if they are satisfactory or not, as refutations of Dr. Bastian's assertion.

very little has been done towards bringing it up to the standard of the present day. It is true that the branches of microzoology and botany, for which Mr. Rupert Jones and Mr. Berkeley are reponsible bear evident traces of the care with which these gentlemen have applied their remarkable knowledge in the improvement of the dictionary. Of these branches we do not now speak; but of histology on the other hand we must say that this subject appears to have been treated with conspicuous neglect. We have, in fact, virtually the histology of 1856, several very important articles remaining almost textually the same as in the first edition completed in that year;—as if science had stood still in the interval.

In order to test what has been done in bringing the work up to the state of knowledge at the present day, we turn first to the introduction. Here the descriptions of methods and instruments remain almost the same as in the first edition. Of the modern microscopes little seems to be known; for instance, we read that "the English objectives are incomparably superior to the continental in every respect," a statement which however true it may have been sixteen years ago, could not be made at the present day by any one practically familiar with (for instance) the high powers of Hartnack. The general technical directions have also undergone but little change. A fair example is the table of reagents (pp. xxiv and xl), where, though some additions have been made, nothing is said of reagents which are now familiar to every student of histology. No medium is given in which fresh animal tissues can be examined in a truly normal condition. Serum and salt solution are things unknown; so are gold and silver solutions, osmic acid, acetate of potassium, and bichromate. Chromic acid is mentioned, but not its use; all processes for hardening tissues being quite passed over. Even to the familiar carmine we have only a slight allusion, but are referred to the article "Dyeing" (new in the present edition), where directions are indeed given for preparing carmine solutions. Chloride of gold is here mentioned and nothing more, while the remarks about the use of nitrate of silver though occupying some lines are actually misleading, and could not possibly teach any one unacquainted with this reagent to use it properly; nothing for instance being said about exposure to light. If this article were all that the student had to teach him the art of staining tissues, his resources would be limited indeed. Nor are these deficiencies supplemented in the articles devoted to the special tissues. Turning to that on epithelium, we are anxious to see whether justice is here done to the use of silver,

which has added so much to our knowledge of those structures. The silver method is not even mentioned, and, as might from this omission be expected, the article is equally silent on the distinctions between epithelia from mucous surfaces and those of serous membranes or vessels (endothelia), a distinction highly suggestive and important, whether or not it should turn out to be fundamental. In fact, a student might, so far as can be seen at present, follow out the whole chain of references without learning anything about some of the most striking methods of modern technical histology; methods, let us add, by no means so abstruse as to be known only to German histologists, but which have been fully described in this Journal, and largely written of by American authors.

Without discussing all the histological articles (which by the way are not numerous), it may be stated without injustice that the same defects are to be found in most, if not all of them. That on the blood, for instance, is singularly defective.

If the above remarks should be thought hypercritical, we can only say that we judge the 'Micrographic Dictionary' by a high standard, and assume that it aspires to be a scientific and not merely a popular guide to the subjects on which it treats. If the aim is merely to supply amateur microscopists with some account of the objects they are likely to meet with, there is no point in our remarks. On the other hand, a book which has been placed on university programmes as a standard work ought to aim at presenting every serious student with an accurate though compendious outline of the sciences which it embraces. Omissions such as we have pointed out can be justified only on one ground—viz., a belief on the part of the writers that the so-called progress of histology during the last sixteen years is altogether vain and illusory, or, at all events, quite inconsiderable. Otherwise it is difficult to explain an indifference which is extended to English and foreign research alike; which ignores Dr. Lionel Beale as much as modern German histologists. We only trust that the scientific public, either at home or abroad, will not take this new edition of what was once at least a standard work as representing, even approximately, the real state of knowledge on the subject of histology in this country.

NOTES AND MEMORANDA.

KING'S COLLEGE; *December 9th*, 1872.

To the Editors of the 'Quarterly Journal of Microscopical Science.'

GENTLEMEN,—I was somewhat struck by the fact that in the number of your journal for October last two papers upon the preparation of the organ of Corti both began with a reference to me. One paper was by Mr. Moseley, who expressed his surprise that my "Notes on Practical Histology" published in this journal a year ago contained nothing about Corti's organ. Mr. Moseley imagines that it is generally considered very difficult to demonstrate the structure of this organ, and so to help us over these difficulties he gives us the benefit of his experience.

The students of King's College know very well that for the last three years they have had microscopical demonstrations of the structure of Corti's organ. It need not therefore be supposed that this subject has been neglected by us.

The other paper was one by my assistant, Dr. Pritchard, to whom I gave the subject of the cochlea for a graduation thesis for the University of Edinburgh two years ago. A gold medal was awarded for that thesis by the senate of the university. We may therefore safely venture to say that not only is the minute structure of the ear demonstrated in this school, but that it is also being investigated successfully.

From my "Notes" I intentionally omitted a great many things which will be found in my 'Text-book of Practical Histology' in process of preparation. I am glad to find that, notwithstanding the omissions, the "Notes" have been of service to many. I may, however, take this opportunity of reminding some persons that, inasmuch as I have treated them with absolute fairness, they ought not to transfer my notes to their text-books and publish them as if they were their own. I am obliged to them for the compliment which their plagiarism implies, but I scarcely think that they will find the practice they indulge in profitable in the long run.

Mr. Moseley is startled by my statement that histology for

medical students may be gone through in twenty-four lessons, each lasting from one and a half to two hours. I would remind him that the time at the disposal of medical students is limited. They are under the necessity of studying a great many subjects within a short period. To one who devotes himself chiefly to histology it may seem very startling that a subject of such vast importance should be gone through in twenty-four lessons, but by those who have had some years' experience in the tuition of medical students another opinion is likely to be entertained.

The College of Surgeons was the first to render attendance upon a course of practical physiology compulsory. The length of the course prescribed by them is *thirty* days, to include tuition in practical histology, practical physiological chemistry, and experimental physiology. I devote *twenty-four* days to histology, *twenty-four* days to chemistry, and an indefinite number to experimental physiology. The importance of the subject has therefore induced me to double the number of lessons prescribed by examination boards. I think that it would very much startle medical students if one were to magnify this subject still further.

In this school we teach histology in the following manner:—I lecture upon the whole subject in my ordinary course of winter lectures on physiology. Once a week a microscopical demonstration is given of the structures described in the lectures. During the following summer the students attend practical histology. In this course all the tissues are not demonstrated, as in the winter, but the student occupies himself with the preparation and demonstration of such tissues as can be prepared in a class. The laboratory is open to him for the prosecution of inquiries into any subject omitted in the general course. I trust that this explanation will in some measure neutralise the "startling" effect of my "Notes" upon Mr. Moseley's mind. I have now been puzzling for eight years over the problem regarding the manner in which practical histology may be best taught to medical students, and I am labouring under the notion that it is not possible to improve upon the method which we adopt. I should be very glad, and I am sure there are many others who will rejoice, if Mr. Moseley will communicate to us the results of his experience regarding the tuition of this subject to large numbers of students.

WM. RUTHERFORD.

Professor Harting.—We are compelled to defer till our next number a notice of Professor Harting's letter, published in

our last, and the detailed memoir on 'synthetic morphology,' which we have received from him.

Medical Microscopical Society.—We wish to direct the particular attention of our medical readers to the report published under the head of Proceedings of Societies, in the present number, of the preliminary meetings for the establishment of this society, of which we hope in the future to publish regular accounts. It will supply we believe a want really felt by the more scientific members and students of the profession.

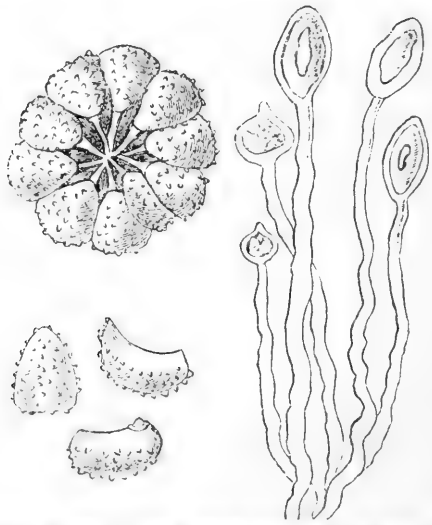
Schwendener's Theory of Lichens.—Mr. Berkeley, in the 'Academy' (vol. iii, p. 328), calls attention to a memoir by Mr. Thwaites ('Ann. and Mag. Nat. Hist.,' ser. 2, vol. iii, p. 219) on the gonidia of lichens as anticipating the views of Schwendener in 'Nägeli's Journal,' though taking a far more rational view of the subject than that which supposes that lichens are fungi parasitic on algæ. Cohn has lately ('Bot. Zeit.,' March 22, 1872) called in question the justice of Schwendener's views, but he is wrong in supposing that the threads in *Palmella* are always extraneous. A comparison of Mr. Thwaites' observations on *P. botryoides*, in the same journal (vol. ii, p. 312), as confirmed in 'Berk. Intr. to Crypt. Bot.,' p. 399, will be sufficient to show the connection of the threads and spores. A similar structure prevails in the gonidioid cells of the anomalous genus *Emericella* (l. c., p. 341).

Coffee-leaf Fungus of Ceylon (*Hemileia vastatrix*).—In May, 1869, a few coffee plants were noticed to be attacked by a fungus upon the leaves. In July following two or three acres of coffee plants were found to show it. Of its advances from that time a letter from Mr. Thwaites, Director of the Botanic Gardens at Peradenia, printed in the 'Gardeners' Chronicle' for March 30th of the present year, give the following particulars:—"Since then (1869) its progress has been quite extraordinary, and it is now to be found in nearly all, if not all, the estates in the island, doing apparently a good deal of mischief in some, whilst in others its effects have not been very alarming. Estates which were the first to be attacked by the fungus have subsequently given very good crops, and it is hoped, notwithstanding the check to growth and partial loss of crop which must ensue from premature fall of the leaf resulting from the attack of the fungus, that the trees will recover and not be materially injured. . . It seems, from observations made by myself and others, to cling to trees it has once attacked, and to exhibit a much less active vitality than at first, and we hope it will not resume its

original alarming powers of rapid diffusion with severe action upon the trees. Seedling plants, even so young as to have merely their two cotyledon leaves upon them, are affected with it equally with the older plants. Trees very severely attacked lose not only almost every leaf upon them, but the young branches die off, and any unripe berries upon them of course perish."

The most curious circumstance, Mr. Berkeley states, in the same journal (Nov. 6th, 1869, p. 1157), is that amongst a thousand species of fungi received from Ceylon this parasite of the coffee does not occur.

"It is not only quite new, but with difficulty referable to any recognised section of fungi. Indeed, it seems just intermediate between true Mould and Uredos, allied on the one hand to *Trichobasis* and on the other to *Rhinotrichum*. Though the fungus is developed from the parenchym of the leaf, there is not any covering to the little heaps, such as is so obvious in *Uredo* and its immediate allies, while the mode of attachment reminds one of *Rhinotrichum*." It was necessary, therefore, to provide a new genus for its reception.



One of the tufts as seen from above, spores in different positions, and group of threads with young immature spores, all highly magnified.

Hemileia,[1] Berk and Broome ("Gard. Chron.,' 1869,

[1] Briefly alluded to in 'Quart. Journ. of Mic. Sci.,' 1870, p. 84, but inadvertently quoted as *Hermileia*.

p. 1157).—Sori somewhat circinating, hypophyllous, naked; flocci distinct, inarticulate, flexuous; spores somewhat kidney-shaped, at first smooth, then granulato-verrucose on one side, attached obliquely at the base by a little papillæform point.

H. vastatrix, Berk and Broome.—Forming little white orbicular patches on the under side of the leaves, consisting of minute tufts of flexuose threads surmounted by a single subreniform spore attached obliquely at the base, rough externally with wart-like papillæ, quite smooth on the side nearest the flocci. The upper portion of the leaf above the patches looks as if it were burnt.

As the fungus is confined to the under surface of the leaves, and the mycelium is not superficial, it may be difficult to apply a remedy; but Mr. Berkeley would be inclined to try sulphur by means of one of the instruments which are used in the hop grounds in Kent, or syringing with one of the sulphurous solutions which have been recommended for the extirpation of the hop mildew. The singular character of the spores, being perfectly smooth on the side of attachment and rough externally, is found also in *Badhamia*, a genus of *Myxogastres*.

A letter recently published in one of the Ceylon papers gives the experience of the fungus during the past year. "It is now very evident," says the writer, "that this scourge is neither partial nor temporary, though it may not be so bad at some times or places as in others. I believe that there is quite as much fungus and disease spots this year as there were last. Possibly there may not be so much in some few places, but generally I hear there is more of it and more distributed, and so it may be less noticeable. Its injurious effect on the trees this year is not probably quite so observable; there is not such a loss of leaf, nor has the young wood received such a check in its growth, but this is entirely due, I expect, to the absence of our usual high south-west winds, and to a wet season. . . . "Analogy would lead us to believe, or at any rate hope, that our trees may in time as thoroughly recover from leaf disease as they have done from black bug, but present prospects are gloomy. . . This troublesome pest continues and spreads without abatement; it does great damage in more ways than one, and manure only secures a temporary respite."

In a note appended to this the editor states (much to the same effect as Mr. Thwaites quoted above) that, on the other hand, he had heard from an extensive proprietor that on his properties leaf disease seemed merely to have the effect of a wintering. In the case of a small property in Hewahette

leaf-disease laid the bushes bare; but at the time he was writing these very bushes were loaded with coffee, at the rate of fifteen cwt. per acre. This is a quite likely result, inasmuch as it is a very common thing in plants for a check to the vegetative to give a great impulse to the reproductive organs. The danger, however, arises from the imperfect development and ripening of the *young* wood when the leaves are lost prematurely; this would produce injurious effects in succeeding years.

Des Preparations Microscopiques Tirees du Regne Vegetal, et des differents procedes a employer pour en assurer la conservation. Par Johannes Grönland, Maxime Cornu, et Gabriel Rivet. (Paris: F. Savy. London: Williams and Norgate.) —Of the seventy-five pages of which this book consists, only the last twenty-five properly relate to the subject which is indicated by the title; all the rest are occupied by descriptions, of a very detailed and apparently accurate kind, of apparatus and various accessories to microscopic work, such as all but the most inexperienced are necessarily perfectly familiar with. A classification and account of the various kinds of turntables fills eight pages at the beginning; diamonds and scalpels are afterwards treated of, with the method of sharpening the latter. A simple plan of mounting needles for dissection, which consists in inserting their blunt ends into the pith cavity of pieces of fresh twigs cut of the proper lengths, and then allowed to dry, and consequently to shrink tightly upon them, will, no doubt, be found useful. The handles, however, for crochet-needles which are sold at Berlin-wool shops achieve the same end by a simple mechanical contrivance. The triangular needles, by the way, mentioned by the authors, are known in England as glovers' needles, and are kept by some instrument-makers. Microtomes are discussed very minutely; they are, no doubt, very useful, but excellent sections are habitually made by those who use no contrivance of any kind. Imbedding in stearine is recommended in the case of Rivet's most ingenious section cutter; but when this or paraffin is used it will be found that, with a little practice, the instrument can be quite dispensed with. It will hardly be worth while, therefore, for any one wishes seriously to work at vegetable histology to expend who twenty-eight francs upon it. A good hint is to coat the object to be cut with a thick solution of gum-arabic, which is to be allowed to quite dry before putting it into the melted stearine. By this expedient, when the section is thrown into water as soon as cut, the stearine is said to detach itself, and gives no further trouble. The manufacture of a slide and

covering glass (pronounced *slaïde* and *coveur*) requires an explanation of sixteen pages. It is, perhaps, a doubtful compliment to find only the mechanical side of English microscopy getting any recognition. It may possibly be all we deserve; still, no serious worker in England would waste his time in carrying out the directions given here for cutting, trimming, and polishing the edges of glass slips, which can be so easily purchased ready-made. Directions for making preservative solutions form the last chapter, and these are probably of some value. A medium prepared by adding 4 to 5 parts (by weight) of glacial acetic acid to 100 parts of distilled water, with which 2 parts of chloroform have been agitated for some time, is stated to preserve the endochrome of minute algæ without contraction, and to have the enormous merit, when vegetable tissues are worked with, of absorbing bubbles of air. Another liquid, composed of 75 parts of water saturated with camphor, an equal quantity of distilled water, and 1 part of glacial acetic acid, is recommended in the warmest terms for the preservation of fresh-water algæ. A great deal still remains to be done in the methods of vegetable histology. No one in England has probably as yet tried perosmic acid for plant tissues; and staining, which has proved so important an aid to animal histologists, never enters into the minds of the authors, even to the extent of mentioning the familiar carmine, much less the solution employed by Hanstein for colouring the cell-wall, consisting of equal parts of rosaniline (magenta) and aniline-violet (mauve) dissolved in alcohol.[1] Schulz's process for demonstrating the "intercellular substance" characteristically concludes what the authors have to say. On the whole, any person wishing to practise the preparation of vegetable microscopic objects merely as a matter of business on a large scale will find it useful to possess this book.—(W. T. T. D., in "Nature."

Microscopes of Winkel of Göttingen. — These microscopes are very highly praised by Merkel, the prosector of anatomy in Göttingen, who should be a competent judge ('Schultze's Archiv,' vol. ix, p. 126). He especially notices the very large amount of light obtainable even with the highest powers, the great power of resolution, and the considerable distance of the front lens from the object which can be gained even with high powers. Thus, Winkel's No. 9 allows the use of a cover-glass which would be too thick for Hartnack's No. 8. The latter strikes us as a very valuable quality. These glasses have also received the suffrage of Dippel, and

[1] 'Bot. Zeitung,' 1868, p. 708.

seem at all events to be very reasonable in price. A number *eight*, magnifying 400 to 1000 times, which may be, perhaps, regarded as equivalent to an English eighth, costs thirty-eight shillings. Number *nine*, which seems about equal to a one twelfth, costs (with correction) seventy-eight shillings.

Draw-tubes *versus* Deep Eye-pieces.—Hartnack's and most of the foreign microscopes, are, as is well known, distinguished from those of English makers by the shortness of the tube, even when fully drawn out. This is practically convenient, but one is tempted to ask, is there not a loss of power? since an increase in length of tube so much increases the magnifying power. M. Prazmowski, the manager of Hartnack's Paris works, has answered this implied criticism in an article published in the 'Lens.' He admits that since the magnifying power with the same objective depends upon both the length of tube and power of eye-piece, the latter must be increased if the tube is not lengthened, but contends that, with a given magnifying power, the image obtained by a short tube and high eye-piece is more perfect, clearer, and brighter than that obtained by the use of a long tube and low eye-piece. He thinks he is able to show that these different results do not depend upon any defect in the correction of the spherical or chromatic aberrations of the objective combination, but are produced by the variable amount of interference of the rays of light in the two given conditions, the fringes of interference becoming larger and more injurious to the image as the tube is lengthened and the luminous cone becomes more acute. By the luminous cone is meant the figure formed by rays passing from the back glass of the objective to the focus of the eye-piece. In proportion as we increase the length of the tube of the microscope, that is, the distance between the objective and eye-piece, the angle of this cone becomes more acute, for the base of it is unaltered while the height is increased. Now, interference is produced by rays of light crossing one another at very acute angles, so that as the angle of intersection is diminished by lengthening the tube the interference increases, or the fringes of interference become wider, and, mingling with the details of the image of the object, serve only to mask it. When the tube is shorter, or the image less, on the other hand, the fringes are less considerable and the image less affected by them, so that even when enlarged to the same extent it will be found more sharply and clearly defined, and more exact in all its minute representations of real structure.

On Deep-sea Dredging round the Island of Anticosti in the Gulf of St. Lawrence. By T. F. Whiteaves, F.G.S.—Depths

of from 100 to 250 fathoms were successfully explored during July and August, 1871. The temperature of the deep-sea mud was found by using a common thermometer to be almost invariably 37° or 38° F. About 100 species of invertebrata new to the Gulf of St. Lawrence were collected. These included a remarkable foraminifer, *Marginulina*, with spinous processes from the first chamber, *Grantia ciliata*, a new *Pennatula*, &c. Two rare Echinoderms were collected —the well-known *Schizaster fragilis*, and the curious *Calveria hystrix* of Wyville Thomson. Nearly all the marine invertebrates of the northern part of the Gulf of St. Lawrence are purely Arctic species. Three fourths of the mollusca of Greenland range as far south as Gasté Bay. The species which belong exclusively to the deep sea in Canada have a decidedly Scandinavian aspect.

Injection of the Blood-vessels and finest Gland-ducts of Insects.—H. N. Moseley, in the 'Berichte der Kön. Sächs. Gesellschaft der Wissenschaften, Mathem. Physische Classe,' 1st July, 1871, gives an account of some experiments on the injection of insects through the large vessels of the wing, which he carried on in Ludwig's laboratory at Leipzig. He has already in this Journal (October number, 1871) described his ingenious method of injection. In this paper he mentions that he found an aqueous solution of indigo-carmine a better injecting fluid than carmine or Prussian blue, but with this substance he obtained the same interesting result as did Chrzonszczewsky in mammalia, namely, a rapid excretion of the colouring matter, in such a way as to colour the intercellular passages of the excretory glands—viz. the Malpighian tubes and the simple intestinal glands in Hydrophilus.

Cohn's Beiträge.—The second part of Cohn's 'Beiträge zur Biologie der Pflanzen' has appeared too recently to admit of anything beyond mention in the present number. It contains papers by Dr. F. Cohn on parasitic algæ, with a plate illustrating the structure and development of a species of a new genus *Chlorochytrium Lemnæ*, Cohn, parasitic on *Lemna trisulca;* by Dr. J. Schroeter on colouring matters produced by *Bacteria;* and one also by Dr. F. Cohn on *Bacteria*, of nearly 100 pages, with a plate.

De Brebisson's Collection of Diatomaceæ.—This is now for sale "en totalité." It is advertised as containing the types of Kützing, Walker-Arnott, W. Smith, Ralfs, as well as those of De Brébisson himself. For particulars, application should be made to M. R. de Brébisson, at Falaise; or M. Buchinger, Grande Rue, Strasbourg.

QUARTERLY CHRONICLE OF MICROSCOPICAL SCIENCE.[1]

HISTOLOGY.[2]

I. Text-books and Technical Methods.—1. *Pathological Histology.*—Cornil and Ranvier have published the second part of their manual, comprising the pathological histology of the *tissues* and *systems*.

Ranvier promises a manual of normal histology, from which we expect great things.

2. *Logwood as a Staining Material for Animal Tissues.*—Professor Arnold, of New York, recommends a new method of preparing logwood solution for this purpose, which, he says, has been for some time in use in his laboratory. The ordinary extract of logwood is finely pulverised in a mortar, and about three times its bulk of alum (in powder) added; the two ingredients are well rubbed up together and mixed with a small quantity of distilled water. The complete admixture of the alum and hæmatoxylin is necessary, and this will require fifteen to twenty minutes' vigorous stirring. More water may now be poured on, and the solution, after filtration, should present a clear, somewhat dark violet colour. If a dirty red is obtained more alum must be incorporated, and the mixture again filtered. By always having an excess of both alum and hæmatoxylin in the mortar, a saturated solution can be obtained, which, after filtration, may be combined with alcohol, one ounce of the logwood fluid with two drachms of 75 per cent. alcohol. A much better colour is produced by allowing the mixture, after thorough trituration, to stand for several days before filtering and adding the alcohol. Should a scum form on the surface of the liquid after it has been some time made a few drops of alcohol and careful filtering will be all that is required. With a strong solution, such as has been described, the colouring

[1] The editors will be glad to receive, for the purpose of making this record more complete, copies of separate memoirs or reprints from periodicals, which must otherwise often escape notice.

[2] The articles in this division are arranged under the following heads:—I. Textbooks and technical methods. II. The cell in general. III. Blood. IV. Epithelium. V. The connective tissues. VI. Muscle. VII. Nervous system. VIII. Organs of sense. IX. Vascular system. X. Digestive organs and glands. XI. Skin and hair. XII. Urinary and sexual apparatus.

is very rapid, requiring but a few minutes; whereas if a slower tinting be desired, the fluid may be diluted with a mixture of one part alcohol and three parts water. Whether the tissue has been previously hardened in alcohol or in any of the chrome compounds, it is coloured equally well, the nucleus of a most brilliant purple, the cell-body of a distinct neutral tint. An over-staining causes an almost perfect blackening of the nucleus, while the protoplasm becomes purple. The great advantage of this substance, as compared with carmine, is, that it succeeds well even with tissues hardened in chromic acid, which are so badly coloured by carmine. The sections may be placed in alcohol and oil of cloves, and mounted in Canada balsam or Dammar varnish. —*Lens*, July, 1872.

3. *Transferring Objects from Glycerine to Canada Balsam.* —Dr. J. Woodward, in the 'Lens,' describes a process for transferring those objects (not previously hardened) which may be preserved in glycerine to the more permanent medium, Canada balsam or Dammar.

The preparation selected, already laid on glycerine, on a glass slide, under a thin cover, and found satisfactory, is placed between the jaws of a delicate spring clip of steel wire, which holds the cover firmly upon the preparation, and the whole is immersed in a bottle of alcohol of about 75 per cent. strength. Here it is allowed to remain a few days, when the slide is lifted out, and, without other disturbance, transferred to a bottle of absolute alcohol; a few days subsequently it is taken out, the clip loosened, and the cover removed, when it will be found that the preparation is sufficiently dehydrated to be mounted on balsam by the usual process. By this plan all corrugations and irregularities are reduced to a minimum, and the piece, as finally mounted, so nearly resembles its appearance in glycerine that only the most experienced microscopist would suspect the mode of mounting, if his knowledge were limited to the appearances observed through the tube.

4. *New Method of Imbedding Tissues.*—Flemming (Max Schultze's Archiv, vol. ix, p. 123) recommends *transparent soap* as a medium for imbedding soft tissues, without the necessity of first dehydrating them. The soap should not contain glycerine, and must, therefore, we suppose (though Flemming could not learn the mode of preparation), be the same as that sold in this country as Pears's, and made by dissolving pure soap in alcohol. At all events the soap, whatever it be, is dissolved in one half or one third its volume of ordinary alcohol, a liquid being thus formed which soon solidifies, but

is readily melted by heat. The melted substance is poured into a convenient mould—a small metal box which opens at one end is best—and the object to be imbedded properly disposed in it. In a quarter of an hour it solidifies, and the mass is then removed and exposed to the air till it is perfectly dry, which requires one or two days. In drying there is great contraction, but the object expands to its original size on treatment with water. Sections are made with a *dry* knife, and melted with a drop of distilled water, more water being afterwards added to dissolve away all the soap. The advantages claimed for the method are that the object being always visible can be arranged at discretion, and that it suffers no shrinking from the action of reagents intended to dehydrate it, as is the case in other methods of imbedding.

II. The Cell.—1. *Cell-wall of Leucocytes.*—J. G. Richardson contends for the existence of a distinct cell-wall or limiting membrane in leucocytes.—('Report on the Structure of the White Blood-Corpuscle,' Philadelphia, 1872.)

2. *Endogenous Formation of Cells.*—Bizzozero (Medizinische Jahrbücher, Heft ii, 1872) has taken up again the question whether the cells containing other cells, found in certain pathological conditions, represent endogenous formation, as first supposed by Buhl, or are produced by the *invagination* of some cells into the substance of others, as believed by Steudener and Volkmann. Bizzozero's observations have been made on the pus which is produced by various causes in the anterior chamber of the eye. In this fluid were contained numerous pus-corpuscles (some still contractile), red blood-disks, and the large compound cells, which were especially the subject of observation. They were irreglarly shaped or roundish bodies, 20 to 40 μ in diameter, and enclosed in their substance, beside fatty granules and nuclei, a variable number of pus-corpuscles, sometimes as many as twenty. These appeared to have made their way into the large cells, not to have been formed there, for the following reasons. They had the characters of mature pus-cells, and no early stages were found. They were also observed, not at the beginning of the process of suppuration, but chiefly when it had lasted for some time, and were not rapidly reformed when the liquid was evacuated. The large cells, also, sometimes contained red blood-disks, especially when hæmorrhage had occurred in the chamber. Moreover, large contractile amœboid cells were seen furnished with contractile processes, which appeared precisely suited to "devour" or absorb the pus-corpuscles. Experiments on animals gave the same results still more distinctly; and the conclusion

drawn from the whole is, that the hypothesis of endogenous cell-formation is in this instance, at all events, unnecessary, since the appearances are better explained on the theory of invagination.

III. Blood.— 1. *Models of Blood-corpuscles.* — Welcker (Schultze's 'Archiv,' viii, p. 472) describes a series of models intended to show the form and relative size of the red corpuscles of vertebrata, which are made of plaster of Paris and coloured. They may be obtained of Mr. Klantzsch, anatomical assistant at Halle, at the price of six thalers.

2. *Action of Pyrogallic Acid on the Red Blood-corpuscle.*— Wedl ('Proceedings of the Vienna Academy,' vol. lxiv, Div. 1, 1871) describes remarkable changes observed on adding a drop of concentrated aqueous solution of pyrogallic acid to a drop of fresh human blood.

IV. Epithelium.—*Preservation of Ciliated Epithelium.*— Moleschott and Pisoborme (Moleschott's 'Untersuchungen') recommend, for preservation of the cilia, a mixture of five parts by volume of a 10 per cent. solution of sodium chloride with one part of absolute alcohol. It is best to immerse the fresh organ in this mixture for twenty-four hours. Microscopical preparations thus obtained last for years. It answers better for mammalian structures than for those of the frog.

V. Connective Tissues.—1. *Structure of the Serous Membranes.* —Lavdowsky ('Centralblatt,' No. 17, 1872, p. 257) has confirmed, by quite independent observations, published in a Russian journal in 1870, most of the views of Klein and Burdon Sanderson on the normal anatomy of the serous membranes, as to the lymphatics, stomata, plasmatic canals, &c.

2. *Distribution and Significance of Multinucleated Cells in Bones and Teeth.* — Kölliker, in a preliminary communication ('Würzburger Verhandlungen,' new series, vol. ii; abstract in 'Centralblatt,' Nos. 23 and 24, 1872), gives an account of some researches which are especially concerned with the absorption of bone and tooth substance. His fundamental observations are as follows :— The surface of all bones or teeth which are undergoing a process of absorption has a rough or excavated character, due to the presence of a number of minute hollows, which are generally known as Howship's lacunæ. These lacunæ are, moreover, occupied by giant cells (myeloid cells or myéloplaxes), in such a manner that usually there is one such cell in each lacuna, corresponding to it in shape and size. However, sometimes one myeloid cell will fill two lacunæ, or larger lacunæ may contain several cells. These myeloid cells do not originate in a transformation of the wasting hard structures, but by a

metamorphosis of the formative cells of the osseous tissue or the osteoblasts. They are the organs by means of which the absorption of osseous and dental substance is effected, and hence may be called *osteoclasts or osteophaga.*

Such lacunæ and their corresponding osteoclasts are found very widely distributed in the skeleton, sometimes on the inner, sometimes on the outer surface. *Internally* they are found, for instance, close behind the ossifying margin of ossifying cartilage; in this situation they are always found when cavities are formed by absorption of the first-formed layers of bone or of calcified cartilaginous substance. Kölliker cannot decide whether this is the means by which the formation of the medullary cavity is regularly or normally effected. They are found also wherever large medullary cavities become developed in bones, as in the diplöe of the cranial bones, the walls of the medullary cavities of large bones, &c.; also in the walls of certain special cavities which are formed in certain bones, as the frontal, ethmoid, and maxillary sinuses; the origin and extension of which must be attributed entirely to the destructive action of the osteoclasts.

Externally, Kölliker finds lacunæ and osteoclasts with the accompanying absorption of bone tissue in many parts, viz. in the dental grooves of the embryonic jaws; at many spots on the cranial bones, especially round their foramina and impressions; on the walls of the vertebral canal; on the wall of the orbit, and of the nasal cavities. They are also seen on the anterior bodies and surfaces of the coronoid and condyloid processes of the lower jaw, and on the corresponding part of the zygomatic process of the upper jaw. By the action of the osteoclasts bone is removed from these parts, while a new growth takes place on the posterior aspect, and thus the whole process is shifted backwards. The same thing is seen round the holes or canals which perforate many bones, the osteoclasts being found especially on one side, and absorption taking place here, while new bone is added on the opposite side. The opening thus gradually shifts its place, as is seen, for instance, in the foramen ovale, foramen rotundum, foramen opticum, &c. Other openings are simply enlarged by the same method.

With regard to the *teeth*, the osteoclasts play a very important part at the time of the change of dentition, the milk-teeth showing at this period genuine Howship's lacunæ and osteoclasts, and their absorption being, in fact, effected by this means.

Bone or tooth absorption, in general, is believed by Kölliker (in opposition to Virchow and others) to be quite inde-

pendent of the activity of the cellular elements of the bone or tooth itself. They simply disappear, and have nothing to do with the formation of the osteoclasts, as is shown by the following considerations:—The bone or tooth substance in the neighbourhood of the osteoclasts shows no trace of metamorphosis of its cells in the way of enlargement or multiplication. The cavities are simply opened up by the destructive action of the osteoclasts; moreover, the osteoclasts are always easily separated from bone, and sometimes small fragments of bone are left, which contain no more cells, and consequently the final absorption of which could not be due to the activity of bone-cells. Finally, there remain the results of experiments on pegs of ivory inserted into living bone, as previously done by Billroth, who found that they underwent absorption.

On the surface of such pegs Kölliker found perfect lacunæ and osteoclasts, while small portions of new-formed human bone were even in some cases deposited upon them.

The origin of osteoclasts is believed to be from transformation of osteoblasts, the two structures being, in fact, so similar that they evidently belong to the same class, and transitional forms are found between them. Moreover, the surfaces on which osteoclasts occur are originally covered by osteoblasts, and the two may occur together. Again, bone formation and absorption may be traced alternately on the same surface, in which case the supervening new growth of bone is preceded by a breaking up of osteoclasts into osteoblasts. The jaws during dentition show this alternation of processes.

The precise manner in which absorption is effected is not perfectly made out; but it is certainly not preceded by any general chemical solution (as in osteomalacia) of the calcareous portion, since the organic and inorganic parts disappear simultaneously. Again, the surface of wasting bone is smooth, not rough, as it would be from the action of solvents, and does not give an acid reaction. Thus, though it is difficult to conceive of the action as anything else than chemical, it can only take place at imperceptible distances, especially since areas of new growth are often immediately in contact with areas of destruction, and osteoblasts with osteoclasts.

The tendency of these observations is decidedly against the theory of "interstitial bony growth" lately proposed.

2. Development of Bone.—Levschin has published ('Centralblatt, Nos 18 and 19, 1872) an abstract of his researches on the ossification of the long bones in the frog. He finds that the elements of the cartilage (which forms the provisional structure) take no part in the formation, either of the external

bony cylinder or of the central medulla. The cartilage remains quite passive, only disappearing before the growth of the vascular medullary tissue, processes or loops of which penetrate the intercellular substance, and find their way into the cavities occupied by the cartilage-cells, as previously seen by Levschin in the bones of new-born children.

3. *Growth of Bone.*—Lieberkühn ('Centralblatt,' No. 27) contends against the theory of interstitial growth brought forward by Wolf and others.

Strelzoff contributes a preliminary notice on the normal growth of bone ('Centralblatt,' No. 29, 1872).

VI. Muscle.—1. *Structure of Striated Muscle.*—The memoir of Wagener ('Centralblatt,' No. 29, 1872, p. 453) discusses the questions raised by Hensen, Krause, Merkel, and others, as to the nature of transverse striation, and adds some new observations.

2. *The Muscles of the Kidney.*—Eberth ('Centralblatt,' No. 15, 1872) describes a plexus of smooth muscle-fibres from the superficial portion of the cortex of the kidney, under the capsule. Remak had previously described smooth muscle-fibres from the capsule of the kidney in the ox, sheep, &c., but Eberth had failed to find them in that structure in man, cats, pigeons, &c. Recent writers describe both the capsule and the stroma of the kidney itself as purely fibrous. The structure described by Eberth was not found wanting in any human kidney which he examined. It consists of a wide network, composed of bundles as large as the superficial veins. The individual cells are spindle shaped and various length, easily isolated by solution of potassa or dilute acetic acid. The network is quite superficial, sending only short and narrow processes into the cortical substance. These muscle-fibres are not found in the stroma of the kindey itself nor in the capsule, and are quite unconnected with the vessels. They are demonstrated by first stripping off the capsule, then hardening the kidney in alcohol, and making superficial sections. The network was not found in the kidney of the ox, sheep, or pig.

VII. Nervous System.—1. *Structure of the Roots of the Spinal Nerves.*—Rudanowsky has published an important Russian memoir on this subject, an abstract of which is given in 'Centralblatt,' Nos. 10 and 11, 1872. He also describes at length his method of investigating the nervous tissue, which consists chiefly in making sections in various directions from frozen tissue. The temperature recommended is abont 10° to 20° Fahr. Sections are made with a double knife, the blades of which can be approximated or separated by a screw.

They are then examined fresh, without any reagents, and also when dried or treated with various colouring matters, such as cochineal (decoction of one or one and a half parts in twenty-four parts water; also prepared with acetic acid, picric acid, &c.), and various anilin colours. The preparations are mounted in Canada balsam, Dammar varnish prepared with turpentine, or glycerin and isinglass. The figures are executed by photography, sometimes with ordinary, sometimes with monochromatic light.

By these methods he investigated both the general structure of the whole nerve and that of its elementary parts. Transverse sections of nerve-fibres, without the addition of any reagent, were especially instructive. From these it appeared that in the higher animals, with the exception of man, the primitive tubes often have polygonal outlines, being either pentagonal or, more generally, hexagonal; their size varies from 4 μ to 20 μ. The author believes every primitive fibre to be a tube enclosing a smaller tube, the axis cylinder, which is surrounded by myelin. The spaces separating the primitive tubes from one another form stellate cavities. In sections of nerves, wholly or partially dried, the structure is found to be much altered; the wall of the axis cylinder contracts, and its cavity cannot be made out, or only with great difficulty. The myelin finds its way into the folds and hollows thus produced, and hence the area occupied by it appears enlarged.

Nerves which have been acted on by hardening fluids show the following alterations:—The volume of the tubes much diminished; the outer tubular spaces almost vanished; the myelin forming either a coarsely laminated ring (in chromic acid) or finely granular mass (in alcohol or potassium bichromate; the axis cylinder much reduced in size, and having the appearance of small dots, while its cavity is unrecognisable.

Isolated nerve-fibres were also examined moist, without any preparation. They have sometimes a single, sometimes a double outline, but this difference cannot (according to the author) be taken as the distinction of two kinds of nerves, for the same tube may sometimes be seen with a double outline at one end and a single outline at the other. The tubes have two membranes; their outer surface appears sometimes longitudinally striated, sometimes nucleated, sometimes with transverse folds. The axis cylinder is a tube which, even when bare and deprived of myelin, as well as of the outer membrane, still has a double outline formed by its special membrane, which is distinguished by its transparency and

brilliancy. Many assume a varicose appearance, even when the primitive tube is unaltered, and when quite bare may show longitudinal striation.

The colouring methods served to show very well the transverse markings, which are not to be confused with the appearances produced by mere contraction of the primitive membrane.

The intertubular spaces form a series of canals. As to myelin, it is found to be best coloured by picric acid, but is also well characterised by its action on polarized light, which also reveals its complicated intimate structure.

2. *Distribution of Peripheral Nerves.*—Lavdowsky ('Centralblatt,' No. 17, 1872, p. 259) has, by independent observations, obtained results which precisely agree with those of Klein as to the distribution of nerves in the tail of the tadpole; he differs from Klein, however, as to their relation to the connective tissue.

3. *Movements of Nerve-fibres under the Influence of the Induced Current.*—Th. W. Engelmann (Pflüger's 'Archiv,' vol. v, pp. 31—37) has observed, on irritating the medullated fibres of the sciatic nerve of the frog with shocks of an induced current, either singly or with several in succession, changes of form in the medullary sheath, this becoming thicker and its outlines notched. The nerves of the web of the foot and other parts, as also dead nerves and isolated fragments of medullary substance from the nerve-centres, showed the same phenomena. These changes of form are simply consequences of the heating effected by the current, and are produced also by simple heating to 33° or 35° C.

4. *Termination of Nerve-fibres in the Cortex of the Brain.*—Rindfleisch (Schultze's 'Archiv,' viii, p. 453) publishes the following note on this subject:

If small portions of the cortex of the brain of the rabbit be macerated for ten or fourteen days in a one-tenth-per-cent. solution of osmic acid, and then preserved for about a week in pure glycerine, they pass into a condition very well adapted for the mechanical separation of their structural constituents. In the first place, they must be crumbled to pieces with the utmost possible delicacy, and such fragments selected as separate in the form of a roundish bundle about the thickness of a large pin. Such a fragment is placed on a glass slip in a drop of glycerine and covered with a piece of thin glass, which is provided at each of the four corners with a small support of wax. These supports must be high enough to prevent the space under the cover-glass from being completely filled with glycerine, and to shield the object abso-

lutely from pressure. Momentary gentle pressure is then made with a needle on the cover-glass, just over the object, the needle immediately removed again, and this process repeated until the to-and-fro movement of the glycerine has produced such a loosening (without crushing) of the object that it falls to pieces of itself, and its parts become distributed through the drop of glycerine. The high degree of perfection with which the ganglionic cells (for instance) are by this means obtained in an isolated state is truly astonishing. All their processes are distinctly seen, and the branched processes may be followed till they break up into lines of dots, so minute that the character of "threads" is completely lost, and their continuity with the "granular" cement of the nervous structures becomes obvious.

Rindfleisch finds, moreover, in such preparations a large number of remarkable terminations of medullated nerve-fibres, some of which are figured. Each of these terminal fragments shows at one end the varicose appearance produced by drops of myelin, which hang on to them like dew-drops on a spider's thread; at the other end it loses its medullary sheath, and is drawn out into an extremely delicate thread, which, after tapering for a little distance, suddenly breaks up into a bundle of the finest filaments, showing the same transition from a "thread-like" to a "granular" character as the branched processes of ganglionic cells. These nerve-fibres, then, also terminate in the "granular cement" of the nervous tissue.

There are, then, two ways in which the medullated nerve-fibres terminate in the cortical substance of the rabbit's brain. Some pass directly into the axis-cylinder processes of the ganglionic cells (or nervous processes of Deiters); others become lost in the same "fibro-granular" substance, into which the protoplasma-processes of the ganglionic cells also dip. If we were to attribute to the one an "afferent," to the other an "efferent" function, the intermediate fibro-granular substance would assume great importance, and would, in fact, appear as the chief link in the whole chain, as the "central nervous substance," while there would remain for the ganglionic cells only the function already attributed to them by Max Schultze, of acting as a collective and commutative apparatus for nervous excitement.

5. *Cortical Substance of the Brain.*—Gerlach ('Centralblatt,' No. 18, 1872, p. 373) has arrived, by the use of the gold method, at results, with respect to the human brain, very similar to those of Rindfleisch. He finds that, beside the well-known, radially arranged, white medullary fibres

which run in fasciculi nearly to the surface of the cerebrum, there are other medullated fibres which have a horizontal course and form with each other and with the radial fibres a coarse network or plexus of medullated fibres, which can be seen with a power of sixty diameters. This plexus contains in its meshes (beside the ganglionic cells) a second very close reticulation of exceedingly minute non-medullated fibres, which can only be seen with moderately strong immersion lenses.

Two elements are concerned in the formation of this second plexus; on the one hand the finest prolongations of the protoplasma-processes of the nerve-cells; on the other hand certain fibres which start from (or end in) this plexus as non-medullated fibres, and terminate in (or proceed from) the first-mentioned larger plexus as medullated fibres. Accordingly, in place of the granular substance which Rindfleisch supposes to be interposed between the commencement of the second or finest plexus and the protoplasma-processes of the cells, Gerlach thinks that he has by his method demonstrated an actual continuity of the plexus up to the protoplasma-processes.

Gerlach has observed the "nervous process" of ganglionic cells described by Deiters, but whether it is really present in all of them or not he has only seen it in certain larger cells. He concludes that medullated nerve-fibres arise from (or terminate in) the grey cortical substance of the human brain by two methods, either direct from cells or else from a plexus. This arrangement is quite analogous to that in the spinal cord, where both modes of origin are found, and the fibres directly springing from cells pass into the *anterior* roots, while those connected with a plexus run in the *posterior* roots. The physiological importance of this analogy, when applied to the two modes of origin in the brain, need hardly be pointed out.

6. *Connective Tissue of the Brain.*—Golgi has published an Italian memoir on this subject, of which an abstract appears in the 'Centralblatt,' No. 21, 1872.

VIII. Organs of Sense.—1. *Gustatory Organs.*—A. von Ajtai (Schultze's 'Archiv,' vol. viii, p. 455) has examined the tongue of man and other mammalia with reference to the existence of *papillæ foliatæ* and goblet-shaped organs.

2. A similar research by Hönigschmied appears in the 'Centralblatt,' No. 26, 1872.

3. *Sense Organs in Snakes.*—Leydig, in a very elaborate memoir (Schultze's 'Archiv,' vol. viii, p. 317), describes certain organs of the viper corresponding to Jacobson's

organs in mammalia; also the goblet-shaped sense-organs from the mouth of various snakes, which he had previously observed in the skin.

4. *Gustatory Organs of the Sting Ray* (*Trygon pastinaca*).—Todaro ('Centralbatt,' No. 15, 1872, p. 227) found in the papillæ covering the rudimentary tongue of this fish a number of round or club-shaped bodies connected with the ultimate ramifications of the glosso-pharyngeal nerve, which he regards as organs of taste. They are similar to the organs described by Schwalbe and Lovén from the tongue of mammalia, and by F. E. Schulze from other animals.

5. *Retina.*—Retzius has published, in a Norwegian journal, a memoir on the retina, partly agreeing with, partly differing from, the results of Max Schultze (abstract in 'Centralblatt,' No. 2, 1872).

6. *Development of the Eye.*—Semoff ('Centralblatt, No. 13, 1871) has investigated the development of the lens and its capsule, especially with reference to its supposed formation, according to Kölliker, from epithelial elements. He finds that the capsule is formed in birds by an invagination of the same connective tissue to which Kölliker ascribed the formation of the membrana pupillaris in mammalia.

IX. Vascular System.—1. *Development of Blood-capillaries.*—Arnold continues ('Virchow's 'Archiv,' liv, p. 1 and p. 408) the researches on this subject noticed in this Journal, vol. xi.

2. *Lymphatics of the Capsule of the Liver.*—Wedl ('Proceedings of the Vienna Academy,' vol. lxiv, Div. 1, 1871; 'Centralblatt,' No. 17, 1872) finds that the superficial lymphatic vessels break up into a capillary network somewhat larger than the network of blood-capillaries. Even under considerable pressure he never found extravasation from these vessels when injected, and hence doubts the existence of stomata. The same was true of the lymphatics of the *pericardium*. No such vessels were found in the *endocardium*.

3. *The Lymphatics of Fasciæ and Tendons* are elaborately described by Ludwig and Schweigger-Seidel (Leipzig Hirtel,' 1872; 'Centralblatt,' No. 22).

X. Digestive Organs and Glands.—1. *Glands of the Gizzard of Birds.*—Wiedersheim, in an inaugural dissertation (also Schulze's 'Archiv,' vol. viii, p. 435, and 'Centralblatt,' No. 18, 1872, p. 278) discusses the formation of the horny cuticle which lines the gizzard of birds, and finds it to be formed by the apposition of a number of minute parts, each of which is a product of the secretion of an individual cell of glandular epithelium.

2. *Spleen.*—Wedl ('Proceedings of the Vienna Academy,' vol. lxiv, Div. 1, 1871; also 'Centralblatt,' No. 17, 1872, p. 261) has investigated the vascular systems of the spleen. The venous system in the sheep consists of vessels with numerous diverticula, but not forming a network or anastomosis. The diverticula are often incompletely divided by processes or trabeculæ of cytogenous connective tissue. No venous twigs originate in the Malpighian follicles (on this point Wedl agrees with Gray and Frey). The vascular system throughout forms a system of closed channels. Wedl has (in opposition to Teichmann) traced lymphatic vessels in the interior of the organ.

XI. Skin and Hair. — *Ear of the Hedgehog.* — Schöbl (Schultze's 'Archiv,' vol. viii, p. 295) finds attached to the roots of the hairs in this organ nervous structures similar to those already described by him from the ear of the mouse and the wing of the bat, consisting of a nervous knot and a nervous ring, the latter being, in this case, most conspicuously developed.

XII. Urinary and Sexual Organs.—*Tubuli Seminiferi of the Testicle.*—Sertoli published, in 1865, some observations on certain peculiar stellate branched cells from the interior of the tubuli seminiferi, quite distinct from the ordinary cells (spermatic cells) of those parts. He now contributes 'Centralblatt,' No. 17, 1872, p. 263) new observations, with special reference to the views of Merkel, who regarded Sertoli's cells as merely supporting cells, and Ebner, who ascribed to them the formation of the spermatozoa. Sertoli finds a gradual transition between these stellate cells and the cylindrical epithelium, so that they are, he thinks, distinctly epithelial, and represent, in fact, the true glandular epithelium of the seminal tubules. The spermatic cells are found enclosed in the network formed by the processes of these stellate cells, but the relation between the two is unknown. In atrophic conditions, however, the stellate cells were entirely wanting.

PROCEEDINGS OF SOCIETIES.

Dublin Microscopical Club.

July 18th, 1872.

Mr. Crowe exhibited the little alga which seemed to be doubtless that named by Currey, *Monostroma rosea*. It is in reality not a Monostroma—not a chlorophyllaceous alga at all, but phycochromaceous. Its *pink* colour, when fresh, to the unassisted eye, renders it readily discernible. It is, no doubt, close to the plant named Clathrocystis by Henfrey.

Mr. Crowe also showed the always pleasing spectacle of the circulation of the blood in the branchiæ and tail of the young water-newt.

Dr. J. Barker showed one of Mr. Wenham's new dark-ground illuminators, described in a recent number of the 'Monthly Microscopical Journal' (a detail of which also appeared in a recent number of 'Science Gossip'), but, as he had not his own large stand present, could not show it in use. He, however, explained its principles, and adverted to previous experiments therewith in relation to the comparative performance of it and his own illuminator; he had found Mr. Wenham's easy in use, but the results attained with his own, in the resolution of the markings of *Navicula rhomboides* and other "difficult" diatoms, he candidly thought (and Mr. Archer, to whom he had shown these, coincided with him) were in favour of the latter.

Mr. Archer exhibited a gathering made near "Toole's Rocks" containing various Conjugatæ, including Desmidieæ, remarkable for the great number of distinct species simultaneously presenting themselves therein in the conjugated state (twenty-four in all, of which, as it afterwards turned out, twenty-two were Desmidieæ). The following were on the table, many of which presented remarkably elegant zygospores, of which some were novel. The following is an enumeration of the zygospores, only the new or striking ones, however, being specially exhibited:—*Hyalotheca dissiliens* (Bréb.), (very commonly, however, met with conjugated), *Sphærozosma excavatum* (Ralfs), *Micrasterias denticulata* (Bréb.), (as is known this has a noble zygospore), *Euastrum pectinatum* (Bréb.), *E. elegans* (Bréb.), *E. binale* (Ralfs), *Cosmarium truncatellum* (Perty); (this is not the same as *Cosmarium pygmæum*, Archer; at least *that* species has a large smooth zygospore, *this* a comparatively smaller one, thickly beset with minute conical spines, these somewhat suddenly narrowed into a very slender

and very acute apex) ; *Cosmarium Brébissonii* (Menegh.), (but only the commencement of conjugation, or rather the partially advanced zygospore, and in a single instance only seen. This was quite of irregular figure, covered by a very thick and quite smooth wall. Quere, an abortive zygospore? This may, indeed, be a smooth-spored species, but from one example only it would be premature to form an opinion, but this may be worthy this passing note) ; *Cosmarium punctulatum* (Bréb.) ? (This is at least the form Mr. Archer had been for some time disposed to identify with de Brébisson's species rather than to regard it as new, but on looking over Lundell's fine work ("De Desmidiaceis, quæ in Suecia inventæ sunt, observationes criticæ") he found a form with a quite different zygospore referred to that species by the author. It is clear that Herre Lundell and Mr. Archer have quite different species in view. Most likely *he* is right, and the present would thus be a distinct species. The present zygospore is, probably, amongst the most beautiful and ornate of any known. It is orbicular and elegantly beset with a number of rather short, somewhat thick processes (12—13 being usually visible round the margin when focussed equatorially), these are rounded above and margined by a number of very short acute spinelets, in a kind of whorl ; from the centre of these there starts up a slender elongate but slightly tapering process, minutely bifid or trifid at the extremity. Thus the whorl of short spinelets looks not unlike a kind of *involucrum* or *calyx*, surrounding a *pistil-like* process, which stands up vertically, terminated by a minutely-subdivided *stigma*, Mr. Archer had taken this zygospore on three or four occasions) ; *Staurastum dejectum* (Bréb.), *St. lanceolatum* (Arch.), *St. alternans* (Bréb.), *St. armigerum* (Bréb.), *St. Brébissonii* (Arch.), *St. polymorphum* (Bréb., Ralfs, 'Br. Desm.,' Pl. xxii, fig. 9 *c*), *Penium Brébissonii* (Ralfs), *Closterium Leibleinii* (Kütz.), *Closterium juncidum* (Ralfs), and (better than either) *Closterium calosporum* (Wittrock). (This latter Mr. Archer had only once before seen, that is, with its remarkable zygospore, for doubtless the species may be common, though in this country at least rarely seen conjugated, for, as Dr. Wittrock justly observes, this species, in the unconjugated state, would scarcely be distinguishable from *Closterium parvulum*, Näg.) This list of conjugated Desmidieæ would close with the singular zygospore of *Spirotænia condensata* (Bréb.), already described by Mr. Archer ('Quart. Journ. Micr. Sci.,' vol. xv. n. s., p. 186, since met with by Itzigsohn (in 'Botanische Zeitung,') and by Lundell ("De Desm., &c.," p. 91). Further, the gathering gave some examples of the conjugated state of *Navicula serians*, already described by Carter, also of *Zygnema stellinum* and *Staurospermum viride* and *S. quadratum*. Thus, almost any dip from the present rich gathering offered a charming picture, and one not often readily attainable.

To the list of Conjugated Desmidieæ shown on the same occasion is to be added *Arthrodesmus convergens* (Ehr.) exhibited by

Dr. Barker, taken from the "Rocky Valley" near Bray, by himself and Mr. Crowe. This zygospore, though not an ornate form, is remarkable amongst its "relatives," by being wholly smooth, which fact Mr. Archer had, ere now, used as an argument in contesting the point with Prof. Reinsch as to the actual specific distinctness of the form in question from *Staurastrum Dickiei* (Ralfs), that author contending that these two, not at all uncommon, forms are but "varieties" of one and the same species —if, indeed, *they* be not absolutely distinct, then *no* differences are available as diagnostic characters, which surely will not be maintained.

Rev. M. H. Close recorded the occurrence of *Staurastrum pileolatum* (Bréb.) at Leenane, Co. Galway, thus extending the distribution of this very scanty species from the east to the west of Ireland.

Mr. Archer showed some examples of a very minute form of Euastrum which he thought was decidedly new; this occurred in a gathering made at "Toole's Rocks." By some, no doubt, it might be regarded as a "variety" of *Euas. binale*, but it appeared to Mr. Archer that that species and *Euastrum elegans* were made to contain already within their bounds several forms which were most likely really independent. Others might, indeed, be found to hold this was a "variety" of some form of the affinity of *Cosmarium* (*Euastrum*) *sublobatum*, or of *Euastrum lobulatum* (Bréb.). But, of course, for the matter of that, there are, no doubt, really several species to which it offers certain resemblances, and yet it has a *tout ensemble*, minute and simple (for a Euastrum), as it really is, which Mr. Archer thought rendered it distinct. You could see with a one-inch glass, minute as is the form, that it was something before unseen that was under the microscope. This little form is about one third longer than broad, basal angles rounded, above which there is on each side a minute concavity, the lateral rounded angles projecting somewhat wider than the basal, whence the concave sides somewhat rapidly taper to the truncate end, which is in width about half that of the base of the segment, the terminal notch reduced to a mere shallow depression; in side view each segment is broadly-elliptic, slightly widened below and considerably inflated at the middle at each side; in end view broadly-elliptic, with a somewhat prominent inflation at each side; the membrane quite smooth.

Mr. Archer also drew attention to a very pretty Cosmarium (taken in the "Rocky Valley") which seemed to accord with *Cosmarium speciosum* (Lundell); the elegant markings of the form could of course be seen only on the empty cell, and then rivalling in beauty of tracery that of many diatoms.

August 15th, 1872.

Rev. E. O'Meara brought under the notice of the Club the fact that he had met *Navicula rhomboides* occurring plentifully

in pellucid tubes, the frustules arranged in single rows. The gathering was made in Lough Awn, a small lake near the summit of Slieveaneerin Mountain, Co. Leitrim. He showed a further new and pretty diatom from the prolific Sulu material—a Triceratium.

Professor Perceval Wright submitted to inspection four slides containing diatomaceous forms prepared by Prof. Cleve, of Upsala. One contained a form, which appears new, and will doubtless be fully described by Prof. Cleve. Two slides from Davis' Straits contained *Coscinodiscus oculus-Iridis*, and *C. radiolatus*. The remaining slide contained a new species of Synedra named by Prof. Cleve, *Synedra thallasiothrix*; this is a very remarkable form, being so extremely long and slender, quite distinguishable to the unaided eye, to which it presents the appearance of a fine hair.

Mr. Archer showed a drawing of a rhizopodous form, just met with, which *seemed* to be new, but it was one of the most intractable he had ever encountered. It may possibly appertain to the genus Amphizonella, but much more observation of it would be requisite ere any definite opinion of its true position could be arrived at, and for that purpose probably much time must elapse, for the supply of material in which it was met having run out, the form must be refound for future examination. Suffice it here to mention that it is about the same general dimensions as *Amphizonella vestita* (Arch.). He might mention, by the way, that this pretty form seems to be rather generally distributed, as he had taken it lately from several localities, but latterly without chlorophyll-granules, and without the hair-like processes—(quere, may these characteristics be connected with the season of the year?) The present form, however, was, he might say, decidedly not a state of that species, though invested like it by an outer envelope, which is of a distinctly double contour (never showing any hair-like covering) and coarsely dotted; nor can it be the same as Auerbach's *Amœba bilimbosa*, in which the colour of the body-mass is from yellow to a brick-red, pseudopodia extremely scanty, very slender, and seemingly sometimes very sparingly ramified. It may be quite possible ere any further sufficiently exact particulars may be afforded Mr. Archer by this, even when found (either as under reagents or as in the normal state to evince any vital powers), most obdurate of rhizopods, it may be discovered elsewhere, and that it may be so, and fittingly elucidated, is much to be hoped.

Dr. V. B. Wittrock (of Upsala), with whose company the Club was honoured on this occasion, showed some preparations of certain species of *Œdogonium*, embracing monœcious, gynandrous, and diœcious forms, recently collected by him in England, with a view to his forthcoming monograph on the Œdogonieæ; and he pointed out the various kinds of fructification nicely displayed.

Mr. Archer showed a drawing of the minute Euastrum brought forward by him at last meeting of the Club, and stated that Dr.

Wittrock had informed him he had some time ago taken this form in Gotland, and had shown him a sketch and told him that description of it was now in the printer's hands, from which it appeared indubitable that they had both, indeed, got hold of quite one and the same thing.

Mr. Archer showed, finally, a sketch of a remarkable zygospore, hardly to be doubted to be desmidian, but, unfortunately, he could not identify to what species it belonged. It possessed an hexagonal figure, sides gently concave, angles obtuse and rounded, intervening surface smooth, each angle surmounted by a tassel-like or tuft-like bundle of slender, somewhat elongate, scarcely tapering, nearly straight processes, like a little rosette at each angle. By "focussing up" (requisite, owing to the density of the mass of green contents) it could be made out that each angle towards the observer was similarly decorated. (Diam. excl. "rosettes" $\frac{1}{2000}''$, incl. $\frac{1}{700}''$.) There is, apparently, no zygospore known ornamented with these pretty "rosettes"—to what species, then, does it appertain?

East Kent Natural History Society.

President.—The Reverend John Mitchinson, D.C.L., &c., Oxon.;
Honorary Secretary.—George Gulliver, F.R.S., &c.

October 3rd, 1872.

Garnet Sand.—Colonel Horsley exhibited specimens of this sand, which he had collected at Cape Comorin, the extreme southern point of the continent of India. The characteristic colour of the garnet was very fine, and so brilliant under the microscope as to appear like an effect of polarized light.

Raphides of Tamus and Epilobium.—He also displayed these plant-crystals, which had been the subject of observation at the meetings of May 2nd and July 18th, when they were shown to be so characteristic of indigenous Dictyogens and Onagraceæ that these orders or groups are most easily distinguished from their next allies of other orders.

Parasites and Nettle-cells of Polyps.—These were shown in *Hydra vulgaris* by Mr. Fullagar. The parasites were seen, under the microscope, to move rapidly within narrow limits by means of the vibratile cilia with which their bodies are covered like *Paramecium*. Slides of dried nettle-cells and threads of the polyps proved how well they may be thus prepared as interesting objects for the microscope.

The Hop-dog.—Mr. Frank Wacher brought to the meeting living specimens of this larva, when the hairs thereof were micro-

scopically examined. They proved to be very delicate and translucent, many somewhat plumose, others composed of parallel cells, projecting in teeth directed towards the pointed and free end of the hair; thus the hairs of this caterpillar are not club-shaped at the tips, as they are generally described to be in the "woolly bears." It was noted as singular, in the present activity of research concerning the Lepidoptera and other insects, that we have not yet any sufficiently exact and extended observations on the comparative characters of the hairs of caterpillars, since, independently of their intrinsic beauty as microscopic objects, they would probably afford useful diagnostics in classification. The "hop-dog" is the caterpillar of a nocturnal moth, *Dasychira pudibunda*, belonging to the family Arctiidæ.

Evils and Benefits of Insects.—A discussion ensued on the evils and benefits of insects. Among the numerous hairy caterpillars which feed on the leaves and other parts of plants is the hop-dog; and the devastation of such insects is too well known. For example, in the year 1782 such were the ravages of *Porthesia auriflora* that prayers were ordered to be read in our churches to arrest its devastations, as related by Mr. Curtis in his 'Short History of the Brown-tail Moth,' published during that disastrous year; and our agricultural annals abound in similar accounts. But while we lament the manifold injuries inflicted by insects, we should not be unmindful of their benefits. Thus to insects we owe honey, wax, and silk, some valuable medicines, abundant food for birds and many other animals, and even for man—"his meat was locusts and wild honey;" the conversion of vegetable matter into nitrogenous compounds for manure; and, above all, the fertilisation of countless plants. In short, though the damages done by insects may be part of the primeval curse, in our present state these creatures are so essential to our welfare that, were they all completely swept from the face of the earth, there would be more lamentation for their absence than has ever been caused by their presence; and, indeed, without the beneficent agency of insects it is probable that numberless plants and animals, including the human race, would fade from the face of our planet.

Stenopterix hirundinis.—This parasite, though commonly described as infesting the swift, occurs frequently at Canterbury on the swallow. The Rev. C. W. Bewsher submitted to the meeting specimens from the swallow. They belong to the Pupiparæ, a family of dipterous insects, which, however insignificant singly, are very formidable when occurring in numbers. Thus, the *Hippobosca equina*, though scarcely larger than a small house-fly, has prevented the assembly or operations of armies; even lately intended reviews and bivouacs of cavalry in one of our forests were said to have been defeated by the mere demonstrations of these insects.

On examination under the microscope the compound eyes of *Stenopterix hirundinis* were found to be large, with the hexagonal facets of proportionable size—a structure of which the function

in a creature passing its life buried among the roots of the bird's feathers is not very obvious. The pigment behind the corneal facets was red. The transverse striæ of the muscular fibres of the legs were large and distinct, and sometimes presented an approach to a spiral form, recalling the more evident appearance thereof in a mounted specimen which is in the possession of the eminent zoologist, Dr. Bowerbank, and which was prepared from an amputated human limb. The magnitude of these transverse markings is noteworthy, because it has been regarded by Leydig and others as related to the activity of the muscles. But the legs of *Stenopterix* are not remarkable for activity; and the Hon. Secretary had long since proved of its host, the swift, that the transverse striæ of the wonderfully active pectoral muscles are much finer or smaller than the corresponding striæ of the comparatively idle crural muscles of the same bird. Indeed, the different characters of these muscular striæ is a subject deserving of further research throughout the different subdivisions of the Arthropoda and of the vertebrate subkingdom; and this would be an addition to the objects for a rational employment of the microscope.

November 7th, 1872.

Habits and Economy of the Fresh-water Polyps.—Mr. Fullagar, who has devoted much attention to these creatures in his aquarium, read a paper on the subject, illustrated by the living specimens and numerous instructive drawings. As it is understood that these will be engraved, and published with the whole text, in 'Science Gossip,' the present abstract will be very brief. The spermatozoa of *Hydra vulgaris* were discharged in the autumn, as noticed by previous observers; but *Hydra viridis* discharged its spermatozoa in the summer, in one instance as early as the first day of June; and in this species the sperm-cells and germ-cells were in the same individual. The development of the bud of germ-cells took three days from its first appearance, on the lower part of the body, until its separation therefrom and sinking to the bottom of the vase of water. In about fifteen days thereafter the germs or ova were hatched in the form of minute microscopic creatures, slowly growing, until the tentacles appeared, one or two at first, and gradually increasing in number and size, four only very short ones appearing at an early period of the development. The author repeated his observations, made at the meeting of Oct. 3rd, on the nettle-cells and parasites of the Hydra, and further illustrated them by drawings.

November 21st, 1872.

English Anchovies.—Mr. Gulliver gave an account of the distinctive characters of *Engraulis encrasichlus*, illustrated by specimens which he had lately procured during a visit to the coast of South Devonshire, and with the hope that some of the members

of the society might be induced to look for this fish on the Kentish and Sussex coasts. At Dawlish, Teignmouth, Torquay, and the neighbouring fisheries, he had seen it so plentifully as to raise the question, why should we not catch and cure our own anchovies? To the well-known characters by which the anchovy is distinguished from the sprat, he added that in the former the maxillary teeth are much larger than in the latter. And while explaining that these teeth, though characteristic of our Salmonacei and Clupeidæ, are neither described nor depicted in some of our great works of ichthyology, he added that this remarkable feature is commonly ignored by our best artists; as more fully explained in the lecture on the Smelt, at the meeting of the society on January 18th, 1872. The teeth of the anchovy are pretty objects under the microscope, and by them this fish may be easily distinguished, even in bits of the maxillary, from the sprat.

December 5th, 1872.

Blood-disks of Salmonidæ.—The Hon. Sec. having been afforded, by the courtesy of Mr. Frank Buckland, an opportunity of examining some of the living specimens in the museum of economic pisciculture, at South Kensington, exhibited slides of the red blood-corpuscles of *Salmo fontinalis* and *Salmo ferox*, and compared them with the corresponding corpuscles of other species of the same family of fishes and with several more osseous fishes of distinct orders. The results, in conformity with those described and depicted in Mr. Gulliver's memoir, read at the Zoological Society, November 19th, 1872, showed the pre-eminent largeness among osseous fishes, so far as is yet known, of the blood-disks of the Salmonidæ; while those of *Salmo fontinalis*, having a mean length of $\frac{1}{1455}$th and breadth of $\frac{1}{2286}$th of an inch, are the largest at present measured of this family. Hence it may be concluded that it is characterised among the osseous orders by the large size of its blood-disks; but in the Smelt (*Osmerus eperlanus*) this character is not maintained.

Sphæraphides of Caryophyllaceæ.—Mrs. Dean presented specimens of *Silene maritima*, of which the intimate structure was examined at the meeting, when the tissue of the leaves and stalks was found to be studded with sphæraphides, very variable in size, but having a mean diameter of about $\frac{1}{1333}$rd of an inch. This is an admirable British example of these bodies, and really a beautiful microscopic object. These sphæraphides, which are common in Caryophyllaceæ, were well shown in the Deptford Pink (*Dianthus armeria*), at the meeting of the society on August 3rd, 1871, reported in the 'Quarterly Journal of Microscopical Science,' January, 1872.

Medical Microscopical Society.

A PRELIMINARY MEETING to constitute this society was held at St. Bartholomew's Hospital, on Friday, Nov. 1st, 1872, W. Morrant Baker, Esq., F.R.C.S., in the chair.

The chairman made a few introductory remarks, and called upon Mr. J. W. Groves to give the reasons for the formation of such a society, and the objects which were in view for it when formed.

Mr. Groves proceeded to state these briefly, and then read extracts from some of the communications on the subject with which he had been favoured by Drs. Carpenter, Burdon Sanderson, Lionel Beale, and Rutherford; Messrs. R. B. Carter, E. Ray Lankester, and others. A discussion followed, in which Drs. Lawson, Heywood Smith, Payne, and Woodman, Messrs. Jabez Hogg, T. C. White, B. T. Lowne, E. C. Baker, and others, took part.

It was ultimately decided that the society should exist, and a provisional committee, composed of the honorary secretaries of the Royal Microscopical Society and the Quekett Microscopical Club, besides representatives from each of the London hospitals, was elected.

The next meeting for sanctioning rules, electing officers, &c., was fixed for December 6th.

AT the second General Meeting of this Society, held at St. Bartholomew's Hospital, on Friday, Dec. 6th, Mr. W. M. Baker in the chair, the minutes of the previous meeting were read and confirmed. Mr. Jabez Hogg gave some account of the work done by the provisional committee, of which he was chairman, and then the secretary (Mr. Groves) read a code of rules which it was proposed to adopt, all of which were carried with but few amendments.

The following gentlemen were elected as officers:

President—Mr. Jabez Hogg.

Vice-Presidents { Dr. H. Lawson. / Dr. F. Payne.

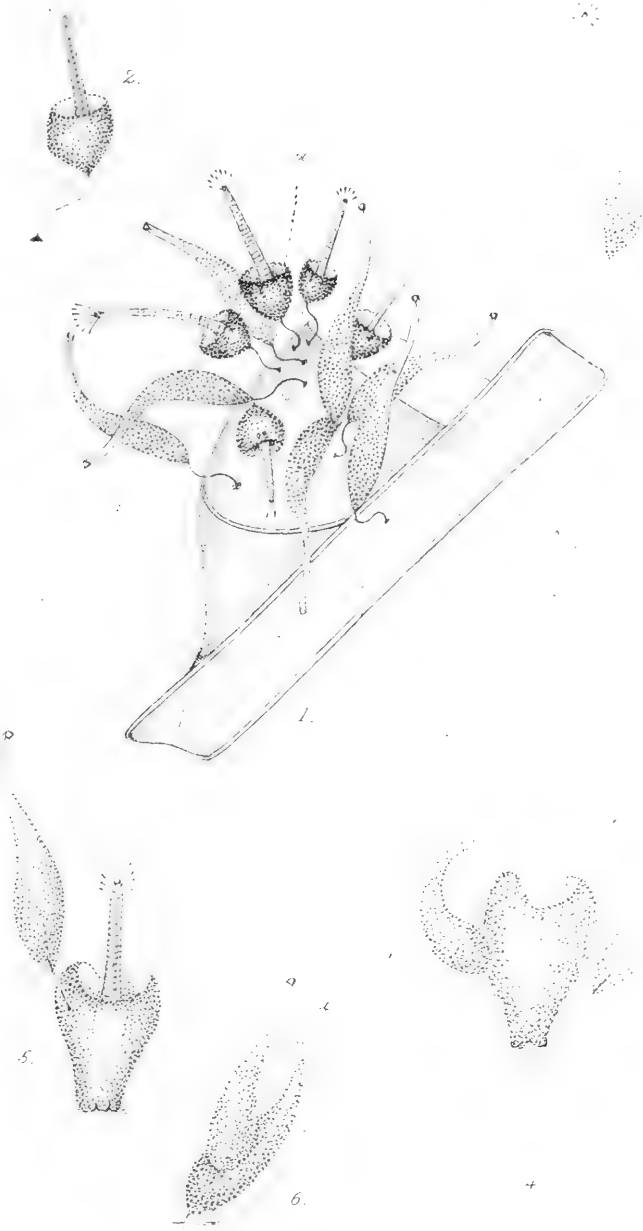
Treasurer—Mr. T. C. White.

Hon. Secretaries— { Mr. C. H. Golding Bird. / Mr. J. W. Groves.

	Committee.
St. Bartholomew's	Mr. H. E. Symons.
Charing Cross	Dr. M. Bruce.
St. George's	Mr. E. C. Baker.
Guy's	Mr. A. E. Durham.
King's College	Dr. U. Pritchard.
London	Mr. J. Needham.
St. Mary's	Mr. Gibs.
Middlesex	Mr. B. T. Lowne.
St. Thomas's	
University College	Mr. E. A. Schäfer.
Westminster	Mr. Geo. Cowell.
Cabinet and Exchange Committee	Dr. U. Pritchard. Mr. E. C. Baker. Mr. J. Needham. Mr. F. H. Ward.

The place of meeting was not decided upon, but the meetings will be held on the third Friday in each month, from October to July inclusive, and the subscription will be 10*s.* per annum, without any entrance fee. Intending members are requested to forward their names, qualifications, or medical schools, and addresses, to Mr. T. C. White, 32, Belgrave Road, Pimlico, S.W.; or to Mr. J. W. Groves, St. Bartholomew's Hospital, Smithfield, E.C.

The next meeting, of which due notice will be given, will take place on the third Friday in January (*i. e.* January 17th, 1873).



T. Hincks del.

DESCRIPTION OF PLATE I.,

Illustrating the Rev. Thomas Hincks's paper on the Protozoon *Ophryodendron abietinum*, Claparède and Lachmann.

FIG.

1.—A colony of *Ophryodendron* (Claparède and Lachmann), on a calycle of *Plumularia pinnata*, *a* the proboscidian zooid; *b*, the lageniform zooid. All the figures are very highly magnified.

2.—*Ophryodendron pedicellatum*, Hincks, the proboscidian zooid, showing the pedicle.

3.—*Ophryodendron pedicellatum*, the lageniform zooid with curved pedicle; *x*, the clear space below the oral aperture.

4.—*Ophryodendron abietinum*, Claparède and Lachmann; a single sessile proboscidian, bearing a lageniform bud (*b*) and one of the flask-shaped zooids beside it, with a straight pedicle.

5.—*Ophryodendron abietinum*, the proboscidian zooid bearing a fully developed lageniform individual.

6.—The flask-shaped zooid of *Ophryodendron pedicellatum*, with bud.

DESCRIPTION OF PLATE II,

Illustrating Rev. Thomas Hincks's paper, "Contributions to the History of the Polyzoa."

Fig.

1.—Two zooecia of *Bicellaria ciliata*, Linn., containing germ-capsules in different stages of development. *a*, a germ-capsule surrounded by a mass of pale-yellowish globules; *a'*, a germ-capsule partially developed into the polypide.

2.—A zooecium of *Bicellaria ciliata* containing a germ-capsule (*a*), in which the formation of a polypide-bud is just commencing at *x*.

3.—A zooecium of *Bugula plumosa*, Pallas, in which the polypide-bud on the germ-capsule has reached a more advanced stage.

4.—A zooecium of *Bugula plumosa* containing a perfect polypide recently developed from the germ-capsule.

5.—A germ-capsule removed from the zooecium. *a*, the capsule, of a dark reddish-brown colour; *b*, the bud, in which the tentacles of the polypide are traceable (the bud is of a light greyish colour); *c*, an oval body, of a pale golden colour; *d*, the (so-called) funiculus.

6.—The digestive sac of a polypide removed from the zooecium. *œ*, the base of the œsophagus; *i*, the intestine; *s*, the stomach; *a*, a globular appendage of the stomach, formed by a constriction of its walls at *y*.

7.—The lower part of the stomach, showing the commencement of the constriction (*c*, *c*) by which the globular appendage is ultimately formed.

8.—An adult zooecium of *Bugula*, in which a polypide is budding from the endocyst (*b*).

9.—A zooecium, with a bud from the endocyst, in a more advanced stage.

10.—The oœcium of *Bicellaria ciliata*. *o*, the ovum; *x*, the membranous capsule which closes the opening; *m*, muscular band by which the capsule is retracted.

11.—The same. *o*, the ovum in an early stage; *x*, the capsule partially withdrawn; *m*, muscular band.

12.—A portion of the stem of *Valkeria pustulosa*, Ellis and Solander, showing the colonial nervous system. *a*, points at which the zooecia are attached; *b*, *b*, nerve threads forming part of the plexus; *c*, mass of cells; *z*, a zooecium *in situ*; *g*, the ganglion of the branch.

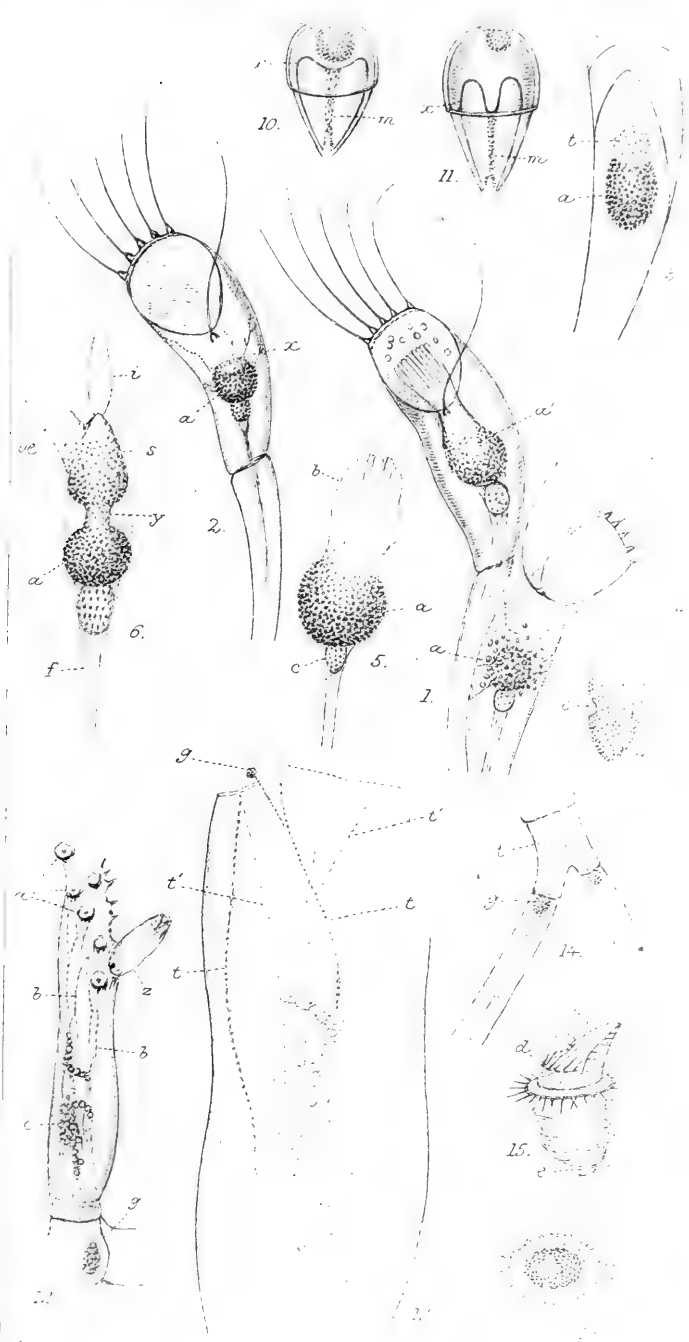
13.—The same more highly magnified. This figure is diagrammatic, the parts represented not being all in focus at the same time. *t*, *t*, thicker nerve-cords; *t'*, *t'*, nerve-threads forming part of the plexus; *g*, ganglion at the base of a zooecium.

14.—Portion of the stem of *Vesicularia spinosa*, Linn. *t*, the nerve-trunk; *g*, *g*, the ganglia at the base of the branches.

15.—The embryo of *Pedicellina echinata*, Sars. *b*, the ciliated edge of the mantle; *c*, the lobe bearing the oral aperture; *d*, the opposite lobe furnished with long and flexible setiform processes; *e*, the projection at the base of the body by which the embryo attaches itself.

16.—The same as it appears when creeping.

All the figures are very highly magnified.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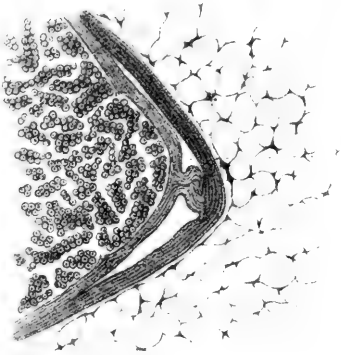

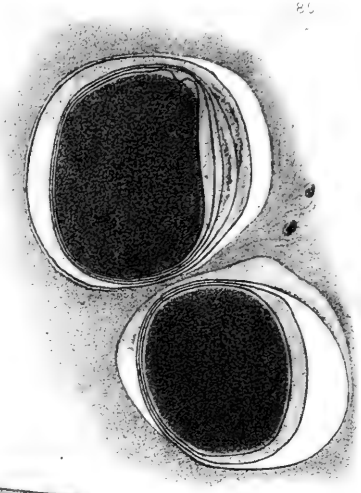

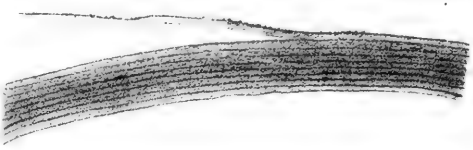

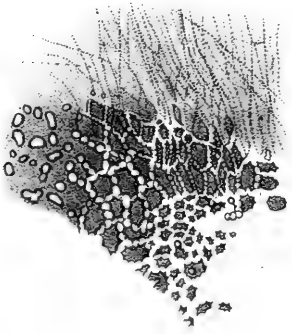

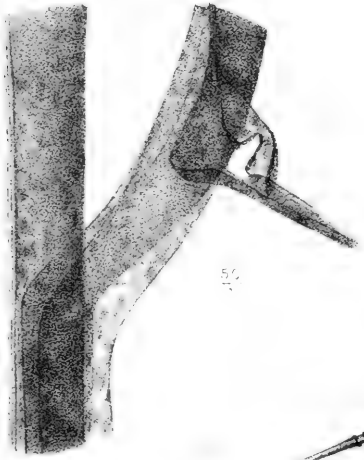

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DESCRIPTION OF PLATE III,

Illustrating M. Ranvier's paper on the Connective Tissue and Vessels of Nerves.

Fig.

1.—Transverse section of the large bundle of the sciatic nerve of the dog; prepared with picric acid, gum, and alcohol. *a*, circular space lined with endothelium. (100 diameters.)

2.—Transverse section of the sciatic nerve of the rabbit, in which the laminæ of the sheath have been separated by an injection of silver gelatine. (80 diameters.)

3.—Transverse section of the laminate sheath of the sciatic nerve of the dog after impregnation with nitrate of silver. (80 diameters.)

4.—Laminæ of the laminated sheath of the sciatic nerve of the dog, dissected with needles from a longitudinal section, after impregnation with nitrate of silver. (50 diameters.)

5.—Elastic plates and reticulations of the internal laminæ of the laminated sheath in the dog. *b*, network of nodulated fibres; *c*, chains of refracting granules. (600 diameters.)

6.—Elastic fibre from the lax subcutaneous connective tissue of the dog, after the action of osmic acid. (600 diameters.)

DESCRIPTION OF PLATE IV,

Illustrating Prof. McNab's paper on Hanstein's Researches on the Development of the Embryo in Monocotyledons and Dycotyledons.

The letters apply to all the figures.

vk, proembryo; *vk*^{1}, basal cell of proembryo, attaching it to apex of the embryo-sac; *km*, embryo mother-cell. *a*, lower daughter-cell or cells, forming hypocotyledonary portion of embryo. *b*, upper daughter-cell or cells, forming cotyledonary portion of embryo. *c*, hypophyse-cell and cells formed from it. 1, 2, 3, with *a*, *b*, *c*, indicate daughter-cells, formed by transverse division in order from above downwards, *i. e.* from cotyledon to root. *s*, cells formed from the hypophyse; *s*^{1}, hypophyse-cell ending the periblem; *s*^{2}, hypophyse-cell ending the dermatogen; *s*^{3}, first cell of pileorhiza. *h*, cells of pileorhiza. *v*, portion of stem-bud. *q*, quarter cells formed by division of embryo mother-cell. *m*, first longitudinal division-wall of embryo mother-cell. *d*, dermatogen and its mother-cells. *pe*, periblem and its mother-cells. *pl*, plerom and its mother-cells. *pr*, procambium mother-cells. *ks*, embryo-sac.

Figs. 1 to 9.—*Capsella Bursa-pastoris.*

Fig. 1.—Embryo and proembryo of Capsella, with greatly enlarged basal cell; the mother-cell of the embryo showing the first division-wall (longitudinal).

Fig. 2.—Embryo showing division of the embryo mother-cell into four quarters.

Fig. 3.—Embryo in which the four dermatogen mother-cells have formed.

Fig. 4.—The same, showing further division of dermatogen cells.

Fig. 5.—Further stage; the hypophyse-cell has divided by a transverse wall. Indications of periblem and plerom.

Fig. 6.—Embryo still further advanced; the form is still spherical; the plerom-cells have divided, and the first cell of the procambium (*pr*) formed; hypophyse-cells divided by a longitudinal wall.

Fig. 7.—Cotyledons beginning to form by being elevated, the position of origin of the stem-bud remaining unchanged; the cells in the interior rapidly dividing, the dermatogen increasing, and the hypophyse divided into three series of cells.

Fig. 8.—Embryo becoming more cordate; the tissues of the hypocotyledonary portion distinctly separated into periblem and plerom; no such change seen in the cotyledonary portion; the dermatogen-cells dividing and forming the pileorhiza, with part of hypophyse.

Fig. 9.—Section of root end, showing formation of pileorhiza and the position of the dermatogen, periblem, plerom, and procambium cells (plerom and dermatogen shaded).

Figs. 10 to 14.—*Alisma Plantago.*

Fig. 10.—Embryo-sac of *Alisma Plantago*, with large basal cell of proembryo; the two embryo mother-cells (*a* and *b*) have divided by a transverse wall, and the hypophyse-cell (*c*) is divided by a longitudinal wall.

Fig. 11.—Division of the inner cells and separation of the dermatogen; hypophyse-cell divided by a transverse wall.

Fig. 12.—Separation into cotyledonary and hypocotyledonary part indicated; the development of the hypophyse and differentiation of inner cells proceeding.

Fig. 13.—Entire embryo, showing cotyledon and stem-bud.

Fig. 14.—Section of root end; periblem and dermatogen shaded, plerom and pileorhiza not shaded.

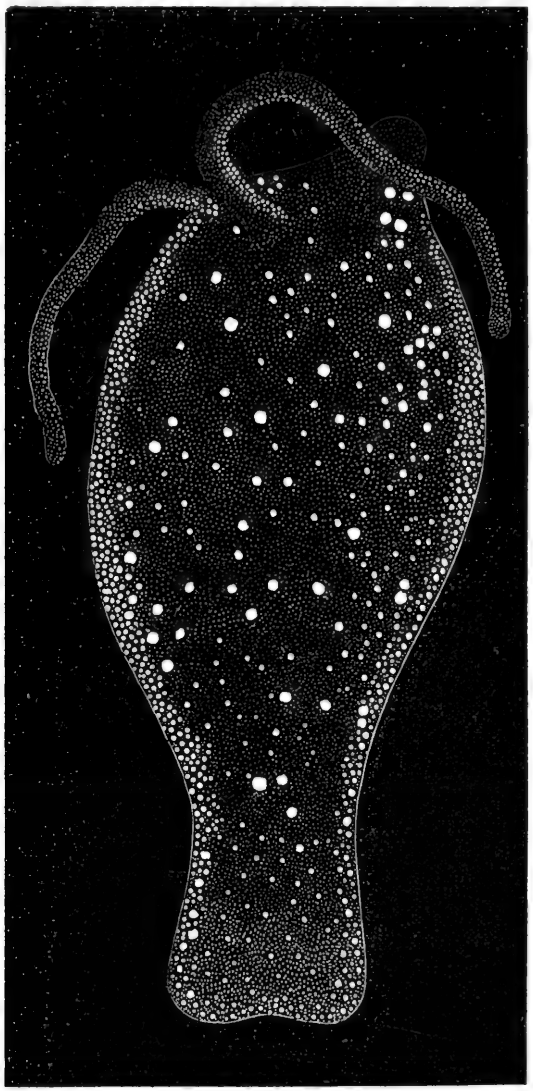
MEMOIRS.

On the Light *emanating from the* Nerve-Cells *of* Phyllirrhoe bucephala. By Dr. Paul Panceri, Professor of Comparative Anatomy at the University of Naples.

The observations of Péron, Eschcholtz, Quoy, Gaimard, Contraire, Souleyet, Leuckart, Krohn, Henry Müller, and others, have made fully known to naturalists those little pisciform sea molluscs which are denoted by the name of Phyllirrhoe, which in the Mediterranean are represented by *P. bucephala.*

The vitreous transparence of the Phyllirrhoe is such that, whilst their little bodies can with difficulty be distinguished from the currents of the sea, as they swim in the water, all the organs to the least cell can be clearly seen. The naturalists who have studied this mollusc have not perceived that it is phosphorescent, but, in fact, if the water in which they are found be agitated, or if they are touched, flashes of light will be seen to come from their body; and if for the purpose of provoking the complete illumination of the phosphorescent elements of the Phyllirrhoe, one stimulates it with a drop of ammonia, the surface of the body and of the gigantic tentacles shines immediately with a brilliant azure light. The upper and lower edge of the body are the parts where the light is most bright and abundant, so much so that the outline of the animal can be perfectly seen. The light is *not* communicated to liquids nor solids put in contact with it, as happens in many other luminous animals. If the drop of ammonia is dropped on the Phyllirrhoe, when extended on the stage of the microscope, and the lamp used for illumination is immediately after extinguished, even with a small magnifying power, it will be seen that the light escapes from myriads of shining points, which are more or less large and brilliant, and more abundant at the upper and lower edges of the animal. In looking for the real seat of the luminous movement in this animal, it is useful to exclude, in the first place, the idea that it can be, as in the Medusæ, the

cutaneous epithelium, which is very thin, and which would



communicate its luminosity to exterior bodies. Neither can there be any doubt about those cells, which contain a golden-yellow substance, and which are found scattered about

the edges of the Phyllirrhoes, and which are chromatophores, since the light is seen in this animal in every part of its body. It may, however, be believed that the seat of the luminous movement is deeper, that is to say, placed in organs which may be found everywhere, and which lie in the very transparent fundamental tissue of the animal below the elastic membrane which represents the derma. Thanks to this transparence, anatomists can study the internal parts in the living animal, and can observe even the nerves and their finest networks; and it was thus that Leuckart, in 1853, described the peripheral nerves of the Phyllirrhoes, pointing out the great number of those destined in part for the skin, and in part for the muscles, and working at their disposition and their course.

After having indicated the course of the fibres which go to the muscles, he pointed out the copious swellings which are found on the passage of the cutaneous nerves in the form of nervous cells placed at the bifurcation of the filaments, as well as on their course, and more or less large, and containing a nucleus.

In the same year Henry Müller wrote also that to the external tegument, which is furnished with a more or less visible epithelium, there are distributed a quantity of nerves with very numerous ramifications, to which are attached an abundance of cells of different sizes, granular in the interior, as is also found in other transparent molluscs. Besides this there are to be seen scattered quite close to the surface of the body, and almost everywhere, spherical cells, which have a very marked dark outline, and which are placed on the finest nervous ramifications, and which, besides possessing a nucleus, contain also a spherical body more or less large, which is yellow and refrangent. These cells, which I propose to name after their discoverer, were afterwards quoted by Gegenbaur, and also by Leydig.

In studying the Phyllirrhoes which are found in our seas during the winter and spring, it was very easy for me to confirm the observations of Leuckart and of H. Müller, especially when I made use of the living animal, in which the finest fibres are plainly seen, so that the connection of the nerves with the peripheral cells is in no wise doubtful.

The peripheral nerves of the Phyllirrhoes are of three sorts:

1. The branches, which coming directly from the ganglia are usually simple, and ramify dichotomously.

2. The little branches, which approaching the surface of the body become tufted, insinuate themselves among the

fasciculi of the longitudinal muscles, and which are varicose, and covered with copious pedunculated swellings, provided with a nucleus, and having the character of nervous cells. Commencing with little fusiform swellings, situated on the course of a fibre, and the simple triangular varicosities placed on the bifurcations, every form and fashion of swelling up to the special spherical cells mentioned by Müller are seen.

3. In the last place, fine motor fibres, without any swelling, and which may be traced to the superficial muscles, running at right angles to the principal longitudinal fibres, of which Leuckart had already made mention.

In the tentacles the nerves take their origin from the two central trunks, which start from the tentacular ganglia placed at the base of these organs (called the olfactory ganglion).

These nerves are so rich in swellings that the tentacle appears under a small magnifying power to be formed of a mass of granulations, which are nothing but nervous cells disposed beneath the fine cutaneous elastic membrane. The diameter of these cells is at the most 0·02 mm., and this dimension is the same as that of the spherical cells discovered by Müller.

The spherical cells, which have been discussed above, are observed to be more copious at the upper and lower edge of the animal. If these spherical cells be examined in specimens which have been kept several days in alcohol or ether, the matter contained in them will be found dissolved. For the purpose of proving whether the seat of the luminous movement in the Phylirrhoe is in the nervous peripheral cells, it is necessary to gain an exact knowledge of these cells. Whilst they are under observation with a small magnifying power, the animal must be wetted by a drop of ammonia and the lamp immediately extinguished. If the magnifying power be small, so that the light is able to travel through the lenses, it will be seen that at the same place and at the same relative distance where were seen by ordinary light the cellular swellings on the course of the nerves, there now are luminous points, more or less brilliant and large.

If the lenses are at a too great or too small distance from the Phyllirrhoe, the points will become pale and confused, whilst they will resume their brilliance when the aforesaid swellings, with the spherical cells, are in the focus of the microscope. And if the suspicion should suggest itself that the light has its seat in some other tissue or part of the body, it will be useful to reflect that where the cells of Müller are found

most abundantly, there, too, the luminous points are most numerous and largest, as at the upper and lower edge of the animal. The doubt will disappear altogether after having examined in the same manner a tentacle, in which nothing is found but the fundamental mucous tissue, the muscles, and the nerves, with their cellular swellings. If one of these organs is cut off from a living Phyllirrhoe, and is observed first by the usual light and then in darkness by the help of ammonia, it is easy to assure oneself that a great number of luminous points correspond perfectly to a great number of ganglion cells.

This experiment on the tentacle proves that the light in the Phyllirrhoe does not come from the cells of Müller alone, but that it emanates also from the ordinary ganglion cells dispersed on the nerves, since it is the latter that are found in the tentacles, whilst they are altogether deprived of the former.

Several trials and repeated experiments have made it certain that the luminous movement in the Phyllirrhoes has its seat in the peripheral ganglionic cells, and I have also observed individuals writhing in the tweezers emit light from a body which corresponded exactly in its position with the ganglionic collar of the œsophagus. I ought to add that, on other occasions, I have seen the two tentacular ganglia shining.

Whilst the luminosity of the peripheral nerves is a fact not yet observed, the light which comes from the ganglia may be the same as that which is said to shine from the ganglia of certain crustacea, and of certain ascidians, if any positive importance may be given to the observations recorded on that matter.

Having established the fact that the light of the Phyllirrhoes emanates from the nervous elements described as above, we may consider this remarkable phenomenon as established.

In the Phyllirrhoes is it the movement of the nerves which is transformed into a luminous movement, or is there in the terminal cells of the nerves a special matter which can be be isolated, and which is annexed in a certain manner to the nervous substance, and which could shine even when it is separated from the influence of the nerves?

The first case might be compared to the Torpedo and to the electrical fish, in which the electrical movement is developed in the terminal cells of the electric nerves, only in this case the organs of light, in the Phyllirrhoes, would not be conglomerated, but broken up into elements near the surface of the body.

In the second case a phenomenon belonging to another category would be presented, and, in all probability, a chemical one; the shining matter, which has its place in the nervous cells, would, perhaps, be the same as we have found in the epithelia of other marine animals, or in the special organs of the Pyrosoma and the Pennatulæ.

It was in view of this alternative that I entered upon my experiments with the persuasion that if the light were a property of the nerves it ought to show itself at each stimulation, and to cease with death; whilst if it were proved that the phosphorescent matter was in a certain manner associated with the nerves, the light would show itself irregularly and continue outside of the animal, and, probably, after death, as I succeeded in demonstrating in the Pyrosoma and the Pholades.

Having tried to stimulate the animal by various means, I perceived that the mechanical means that I employed acted, first, in provoking luminous discharges which then ceased, although there was a reaction in the animal, since it was agitated and contracted itself. If it was left a little while at rest, and was again stimulated, the luminous discharges were repeated, growing weak immediately, as if something were consumed in the nerves which required a certain time to renew itself and to shine again.

Since in the Torpedo and Gymnotus the discharges grow weak if they have been provoked and repeated one after another, the weakening and cessation of the light, in the Phyllirrhoes, was not sufficient argument to solve the problem.

I then tried the action of electricity, and, having placed an individual on a glass, between two little plates of platinum, so as to avoid the action of the salts which are found when electrodes of brass are used in seawater, which would have acted in their turn as stimulants, I tried it by means of feeble currents, but they gave no result, so I began to doubt whether the fundamental mucous tissue of the animal was not, as in the Pyrosoma, and the Medusæ, a bad conductor of the electric current. Having thought of augmenting the force of the current I obtained no greater luminosity, although the current passed undoubtedly through the body of the animal, as one might be assured by observing that the heart ceased to beat when the circuit was closed, and that it beat again when it was broken.

I must admit that I expected a very different result, that is to say, I thought I should always obtain the light by the

electric stimulation, but all the experiments I have been able to make have assured me of the contrary.

In crushing, nearly whole, a living individual a pale and dim light was observed.

I then employed fresh water with other individuals, and in this experiment the light escaped first in flashes, and then became fixed, even when the water was at zero. If a Phyllirrhoe were crushed in fresh water the light was very brilliant.

The effect of heat was then tried, and for that purpose a small tube containing a live Phyllirrhoe in sea water at 35° C., was heated in the water bath; a light was observed, at 44° the light became fixed, though pale, and was maintained up to 61°.

Alcohol and ether fix the light, and then extinguish it after some minutes, but so much light will never be obtained as by using ammonia, or even a solution of caustic potash. Having dipped some very lively individuals, one into sulphuric acid, and another into nitric, I obtained not the smallest spark, as I remarked also in the Pyrosoma. The light of day, and even the direct rays of the sun, in nowise modified the phosphorescence of the Phyllirrhoes.

With the intention of trying whether the luminous matter could be extracted from the animal I pressed some individuals in a cloth and there escaped from them, as they were shining, a few drops of a bright liquid, which became dark immediately, but fresh water soon made it shine afresh.

With the object of observing whether light could be obtained, even after the death of the animal, I let a Phyllirrhoe dry quite naturally on a glass. The following day the animal was already dry and fragile, and I thought of wetting it again with fresh water. By rubbing it thus with the finger I saw that the water which dropped from it began to shine with a uniform dim light. As the animal absorbed the water it became detached from the glass, and then, on pressing and rubbing it between my fingers, dipped into fresh water, a luminous matter began to spread, very like a pale cloud, which, as it dilated, soon illuminated the whole vessel.

Another dried individual was wetted with ammonia, and a very brilliant light was also obtained, as, too, the same result was obtained with Phyllirrhoes, which were already in a state of putrefaction and decomposition.

Without further reasoning on the subject, it seems to me that after all that has been said, the following conclusions have been arrived at:

1. There exists in the nervous cells of *Phyllirrhoe bucephala* a luminous matter.

2. This matter is found equally in the peripheral nervous cells of the ordinary form, and in those of the central ganglia, as well as in the special peripheral cells, which contain a refringent yellow matter, soluble, to a great extent, in alcohol and ether.

3. The light in the nervous cells is manifested during nervous excitement; it is, however, extinguished, little by little, to reappear after rest.

4. Whilst electricity has no apparent action on this matter, fresh water, ammonia, potash, alcohol, ether, and heat cause it to shine, as happens in other phosphorescent marine animals.

5. The animal once dead or dried, or in putrefaction, this matter can be again rendered luminous by means of fresh water and ammonia.

From these conclusions it follows that there is no evidence of a luminous movement, which has its seat in the nervous matter, properly so called, but rather of a matter associated with the nervous elements, which shines in consequence of "stimulus" during life, but which shines equally by means of certain special reagents, when it is extracted from the animal, as also after its death.

After having measured, in certain nerves, which anatomy has not yet demonstrated, the rapidity of the propagation of the excitement so clearly displayed by the luminous currents in the Pennatulæ and the Pyrosoma, I am gratified at having discovered a case in which the luminous matter is so closely united to the nerves as to form part of the ordinary ganglionic cells, and of the special cells in connection with the nerves, such as are the cells of Müller. I should be in nowise surprised if I or others succeeded in discovering, in many other animals, that the nervous cells are luminous in particular cases.

I am, of course, referring to the *living* nervous matter. I have often seen the light of the brain of *dead* fishes, which any one may observe in the brain of the *Mullus*.

The FRESHWATER ALGÆ *of* GOTLAND *and* ÖLAND (*with an Enumeration of the Species and Remarks*). By Dr. VEIT BRECHER WITTROCK. Translated in Abstract, with Notes, by WILLIAM ARCHER, M.R.I.A.

A VALUABLE memoir ('Om Gotlands och Ölands Sötvattensalger'), 'On the Freshwater Algæ of Gotland and Öland' (islands in the Baltic Sea), from the pen of Dr. Veit Brecher Wittrock, of the University of Upsala, communicated to the Royal Academy of Sciences, Stockholm, so far back as January, 1872, but which appears to have been only recently circulated, has just come to hand. As it possesses (notwithstanding that the algal-flora of those islands is not a rich one) considerable, not only local but general interest, and as the valuable remarks interspersed (with the exception of the descriptions of new species, which are in Latin) are in the author's native tongue, it has been thought that a *résumé* thereof could not but be welcome to those algologists of this country to whom the Swedish language might be more or less a barrier to the full employment of the original memoir.

The author commences by remarking that, though most departments of the natural history of these islands have been made objects of study by distinguished naturalists, yet some fields have hitherto remained to a considerable degree neglected. One of these being that of the Freshwater Algæ, the author determined to visit the islands, and make good that deficiency by devoting a portion of the summer (1871) to their investigation. Their algal-flora, he remarks, might be said, on the whole, to have been unknown, except so far as is due to Professor Cleve's researches. These relate, however, only to the Conjugatæ of the single island of Gotland, as set forth in his extremely valuable works, 'Bidrag till kännedomen om Sveriges sötvattensalger af familjen Desmidieæ,' as well as his 'Försök till en monografi öfver de svenska arterna af algfamiljen Zygnemaceæ.' The freshwater Algæ collected by Professor Cleve in other groups were kindly placed at Dr. Wittrock's disposal, and these, so far as they relate to the Œdogonieæ, have been already published in Dr. Wittrock's work, "Dispositio Œdogoniacearum Sueciarum" (in "Öfversigt af Kongl. Vet. Akad. Förhandl.,' Stockholm, 1870), which excellent though comparatively brief record of long-continued and most exact observations, along with his more recently published 'Œdogoniaceæ

novæ in Suecia lectæ" ('Botaniska Notiser,' 1872), are but the precursors, it is to be earnestly hoped, of a complete and fully illustrated monograph of this interesting group from the practised pen and pencil of the skilled author. Dr. Cleve's further material has been incorporated in the present work. The island of Öland had never been hitherto visited by any algologist.

It had been the author's desire to visit these islands about the end of May, the best season for studying the aquatic spring-vegetation, but circumstances prevented his undertaking the journey till the middle of June. In consequence, however, of the cold and rainy weather which had prevailed, he was fortunate enough to find, on his arrival at a later period, that the spring-vegetation was still in full vigour; the season was a late and cold one, and all vegetation, phanerogamous included, was considerably retarded. Still, the season was, by and by, favorable enough to the growth of algæ, although one effect of the late rains was to flood the larger bogs and marshes with water, preventing access except to the margins (a state of things which, alas! during the past season (1872) was a serious bar to algological as well as many other departments of microscopical pursuits in other countries far away from Gotland!).

The author then proceeds to describe the sorts of pools which were best for algæ; the running water he found poor in them. He examined only two lakes, one poor, the other rich, but he gives a number of special localities, and expresses the hope of revisiting the islands to investigate the larger water-collections and to perfect his acquaintance with their algal-flora.

The geological nature of the islands is limestone, generally covered by a rather thin stratum of earth, in some places sandy, in others clayey, but always highly calcareous. Hence, taken on the whole, the algal-flora of the islands as a consequence is poor, both in number of individuals as well as forms, with the exception, indeed, of Mesocarpeæ and Diatomaceæ. The latter family is omitted from the present work, as the material collected was not worked up; it will doubtless, however, be made the subject of a future communication.

The classification or arrangement of the larger groups is not only to some extent different to that generally employed, but certain of the species are likewise relegated to somewhat unusual positions.

So far as relates to the arrangement and limitation of the principal groups, the author states that he has followed the system

taught in his academic course of lectures by Professor J. E. Areschoug; whilst, as relates to the families, he has mainly followed Nägeli, de Bary, and Pringsheim. As to the limitation of the Chlorozoosporaceæ, he deviates from the other authors to an extent such as he holds to be in accordance with the results of the most recent morphological and physiological researches. Perhaps the position assigned by him to certain genera which he places here may be more or less open to question.

The nomenclature of species and groups employed by the author in his memoir deviates also in several instances from that most generally current, mainly arising from his being a staunch upholder of the "law of priority." In all cases, when the species is itself removed to a new position, he rightly quotes the original author within brackets, adding, however, the name of the subsequent author under whose auspices and owing to whose researches the transfer became requisite or advisable.

As, however, an adequate conception of the nature of the system adopted will be most readily conveyed by transcribing in a consecutive form the various orders, families, and genera from the work itself, I proceed to do so. The nomenclature employed as regards the "orders" will be found sufficiently self-explanatory. No doubt a considerable interest will attach to this *résumé*, as embodying the views of an algologist so experienced and so thoughtful, and reflecting too, as no doubt they do, to a great extent, those of his fellow-countrymen who have likewise made the freshwater algæ a subject of study.

ENUMERATION OF THE FRESHWATER ALGÆ OF GOTLAND AND ÖLAND. (*Note.*—The Diatomaceæ collected on the journey have been left for future examination.)

Order 1.—OOZOOSPORACEÆ, Aresch.

Family I.—*Coleochæteæ*, Näg.

Genus I.—*Coleochæte*, Bréb.

C. scutata, Bréb.;

(also a partially developed form supposed to be probably *C. irregularis*, Prings.)

Family II.—*Œdogonieæ*, de By.

Genus I.—*Bulbochæte*, Ag.

[Seven species found, including that curious one, *B. mira-*

bilis, Wittr. It would be beyond the purpose of this *résumé* to enumerate these, or to give the characters of the three new species described, as this would be of little advantage without the rest of the author's species, which will be made the subject of a future monograph from his pen.]

Genus II.—*Œdogonium*, Link.

[Nineteen species, one—*Œ. nodulosum*—being new, that is, not described in the author's previous works. This is a species with undulate vegetative cells, and shows very plainly that Kützing's genus *Cymatonema* has no proper foundation. Besides the nineteen species thoroughly identified, some others were found either sterile or with imperfect fructification, and hence must be left undetermined. As regards any enumeration of the species or special observations thereon by the author, the above remark equally applies here.]

Order II.—VAUCHERIACEÆ, Aresch.

Family I.—*Vaucherieæ*, Decaisne.

Genus I.—*Vaucheria*, D. C.

Two species, *V. geminata* and *hamata*.

[Though so distinct in structure of phycoma and in details of reproductive organization, still, taken in the *strict sense* of the word, the Vaucherieæ would appear to be equally entitled to be regarded as Chlorozoosporaceous as the members of Order III following; in many of the latter, indeed, zoospores do not seem yet to have been observed.]

Order III.—CHLOROZOOSPORACEÆ, Aresch.

Family I.—*Chætophoreæ* (Harv.), Hass.

Genus I.—*Draparnaldia*, Bory.

D. plumosa and *glomerata* (Vauch.), Ag.

Genus II.—*Chætophora*, Schrank.

Ch. cornu-damæ (Roth), Ag. (= *Ch. endiviæfolia*, Auct.), *Ch. pachyderma*, n. s., Wittr., *Ch. elegans* and *Ch. pisiformis* (Roth), Ag.

Genus III.—*Herposteiron*, Näg.

H. repens (Braun) = *Aphanochæte repens*, Braun.

Family II.—*Conferveæ* (Ag.), Wittr.

Genus I.—*Hormiscia* (Fries), Aresch.

(= *Ulothrix*, Auct. Three species.)

Genus II.—*Cladophora*, Kütz.
C. glomerata and *C. fracta*, Kütz.

Genus III.—*Microspora*, Thuret.
(Two species.)

Family III.—*Tetrasporeæ* (Näg.).

(The author would exclude from this family the genera *Porphyridium* and *Polyedrium*.)

Genus I.—*Oocardium*, Näg.

O. stratum, Näg.

(The author finds this alga established epiphytically upon *Zonotrichia calcarea*; as ordinarily known, though met with on other algæ, it grows upon stones. The plant found by the author differs somewhat from Nägeli's figure ('Gatt. einz. Algen,' t. 3 A), in not only the top cells, but also the remaining ones containing chlorophyll.) [But quere the correct location of this plant under Chlorozoosporaceæ, or, in other words, under an Order founded on the occurrence of zoospores, for there does not seem to exist any record of a mode of development in *Oocardium* otherwise than by self-division of the cells; still the propagation by zoospores may be possible.]

Genus II.—*Palmodictyon*, Kütz.
P. viride, Kütz.

Genus III.—*Tetraspora*, Link.
T. explanata, Ag., *T. gelatinosa* (Vauch.), *T. lubrica* (Roth), Ag.

Genus IV.—*Palmella* (Lyngb.), Näg.
P. miniata, Leiblein.

Genus V.—*Schizochlamys*, Braun.
S. gelatinosa, Braun.

Genus VI.—*Nephrocytium*, Näg.
N. Agardhianum, majus, Näg.

[Quere the propriety of the position of this plant here at all any more than *Polyedrium*, under the order Chlorozoosporaceæ or under Fam. Tetrasporeæ? The contents of the very thick-walled parent cell seem to divide into two, four, or eight (or more) young cells within the primary membrane, of considerable thickness, which *pari passu* with their growth expands and finally disappears, setting free the young group of cells, which do not, as far as appears known, seem to "swarm." Quere, too, if the *two* forms referred to here be,

strictly speaking, generically distinct from *Oocystis* (Braun), Näg., ? to which the foregoing description of the mode of growth applies; very much the same, too, in *Polyedrium*, though the young cells appear to be more numerous. No *Oocystis* was met with in Gotland.]

Genus VII.—*Raphidium*, Kütz.

R. fasciculatum, Näg.

[= *Ankistrodesmus*, Auct. Again quere the right place of this plant under Chlorozoosporaceæ ?]

Family IV.—*Pediastreæ* (Näg.)

The author would place under this family the genera *Hydrodictyon* and *Staurogenia;* [neither, however, seem to have occurred in Gotland.]

Genus I.—*Pediastrum*, Meyen.

[Five species: *P. biradiatum, tetras, duplex, Boryanum, integrum.*]

Genus II.—*Scenedesmus*, Meyen.

Three species: *S. quadricauda, acutus, obtusus*—[quere chlorozoosporaceous ?]

Genus III.—*Cœlastrum*, Näg.

S. cubicum, *S. sphæricum*, Näg.

[Quere chlorozoosporaceous ?]

Genus IV.—*Sorastrum*, Kütz.

S. spinulosum, Näg.

[Quere chlorozoosporaceous ?]

Family V.—*Characieæ* (Näg.)

[In this family the author unites *Hydrocytium* and *Codiolum*, but would exclude *Cystococcus, Dactylococcus, Botryocystis*, and *Gonium.*]

Genus I.—*Ophiocytium*, Näg.

Two species, *O. arbuscula*, Braun (= *Sciadium arbuscula*, Auct.), *O cochleare* (Eichwald), Braun.

Genus II.—*Characium*, Braun.

C. ornithocephalum, Braun.

Family VI.—*Protococceæ* (Menegh.)

Here the author would include *Polyedrium*, and would exclude *Cryptococcus* and *some* species of *Protococcus* and *Chlorococcus*, Auct.

Genus I.—*Polyedrium*, Näg.

Four species: *P. enorme* (Ralfs) de Bary, *P. tetraedricum*, Näg., *P. majus*, Reinsch, *P. muticum*, Braun, and a new species, very large, irregularly 5- or rarely 6-hedral, and sides concave—*P. gigas*, n. s., Wittr. [This is doubtless the same plant as that exhibited at the meeting of Dublin Microscopical Club, by myself ('Quart. Journ. Micr. Sci.,' vol. XI, N. S., p. 96), but not named by me. Quere, if this genus be chlorozoosporaceous?]

Family VII.—*Volvocineæ* (Ehr.).

Genus I.—*Volvox* (L.), Ehr.
V. globator (L.), Müll., Ehr.

Genus II.—*Pandorina* (Bory).
P. morum, Müll.

Genus III.—*Eudorina*, Ehr.
E. elegans, Ehr.

Genus IV.—*Gonium* (Müll.), Ehr.
G. pectorale, Müll., Ehr.

Order IV.—Conjugatæ, de By.

Family I.—*Mesocarpeæ*, de By.

[The observations of the author on the species falling under this Order are so interesting and important, that the best course is not to curtail or condense what he has given us, but to offer a translation of it as it stands:]

"As is known, the Mesocarpeæ have been divided by authors into the following genera:—*Staurospermum*, Kütz., *Mesocarpus*, Hass., *Craterospernum*, Braun, *Pleurocarpus*, Braun, *Sphærospermum*, Cleve, and *Plagiospermum*, Cleve. Of these, at least three, namely, *Mesocarpus*, *Staurospermum*, and *Plagiospermum*, have been generally regarded as in a high degree natural and well grounded as being based on physiological characters, drawn from the dissimilar course of the formation of the spores. Seeing, then, that these characters have been regarded by all algologists as specially important and good, and hitherto, indeed, have been always found perfectly constant, it could not but be in a high degree surprising to find a form of Mesocarpeæ (*Mougeotia calcarea* (Cleve) nobis) in which the formation of the spores takes place in all the different modes which are regarded as characteristic of the different genera of the family.

Under the circumstances that a species of Mesocarpeæ has now been found, which unites in itself the characters of all the genera which have been regarded as most certain and best circumscribed, there appears scarcely any other course possible but to combine them all into one genus. This I have ventured to do below, and as regards the name for it, it seems to me that I ought to use C. A. Agardh's generic name, published in 1824—*Mougeotia*—under which he comprehended all the then known forms of Mesocarpeæ, which represent at least two of the groups subsequently established as special genera, and these just the two most distinct, *Staurospermum*, Kütz., and *Mesocarpus*, Hass."

Genus I. *Mougeotia* (Ag., 1824), nob.

Characters the same as those of the Family (Ag., 'Syst. Alg.,' p. xxvi, char. emend.; *non* de Bary, 'Untersuch. üb. d. Conj.,' p. 78).

"According to what has been above mentioned, the formation of the spores in Mougeotieæ takes place in three different ways. (1) The spore may be formed by the tripartition of the cell resulting from the conjugation, and two sterile cells are then produced beside it, which, in transverse conjugation, while more or less parallel in regard to mutual position, become separated from one another by the spore (*in orig.* Pl. 3, f. 1 m, and Pl. 2, f. 1 and 2 m, m') ; on the other hand, in longitudinal conjugation (that is, taking place between two cells belonging to the same filament and abutting on one another), the sterile cells are not separated by the spore, but are persistently united directly with one another, as well as arranged in a direct line (Pl. 3, f. 1 pr). Thus takes place the formation of the spore in *Mesocarpus-*, *Craterospermum-* and *Pleurocarpus*-forms. (2) The spore may be formed through the quadripartition of the conjugation-cell, whereby three sterile cells are formed besides the spore, which in transverse conjugation (the only mode which has been observed in this case, although longitudinal conjugation is indeed conceivable) become separated from one another by the spore, and are disposed two at one side of it, and one at the other (Pl. 2, f. 2 pg, and f. 4 pg). Such is the condition in *Plagiospermum*, Cleve. (3) The spore may occur by quinquepartition of, in this case, the always cruciate or H-shaped conjugation-cell; four sterile cells are formed around, and mutually separated by the spore, and arranged two on each side (Pl. 3, f. 5, and Pl. 2, f. 1 and 2 s, s', s''). Only transverse conjugation is in this case

possible. Such a spore-formation takes place in the species of the groups *Staurospermum*, Kütz., and *Sphærospermum*, Cleve. Since in all the known species of the genus *Mougeotia*, with a single exception, the formation of spores occurs in each individual species exclusively in one of the three modes just described, I have regarded it as expedient to divide the species into three groups, each group characterised by its particular mode of spore-formation, adding a fourth to receive *Mougeotia calcarea* (Cleve) nob. and the species which may possibly be discovered in the future, in which the formation of the spores takes place, in one as well as in others of the three modes referred to. To preserve all the six old genera above enumerated of Mesocarpeæ as primary subdivisions of *Mougeotia* (Ag.), nobis, has appeared to me to be less suitable, since the characters which separate these so-called genera from each other are of very unlike, and in certain cases of rather subordinate, value."

[The author does not omit to take notice of the fact that the revived, and thus very comprehensive, genus proposed by him under the old name *Mougeotia* would, so far as the name is concerned, come into conflict with the well-grounded and interesting genus *Mougeotia*, de Bary (non Ag.) (see De Bary's 'Untersuch. ueber die Fam. der Conjugaten,' p. 78, also 'Quart. Journ. Micr. Sci.,' vol. VI, N. S., p. 268, and vol. VII, Pl. VIII, figs. 1, 2, 3), and in a note he suggests the name *Debarya* for that genus, in honour of Professor A. de Bary, a proposal which, under any circumstances, would be, without doubt, carried by the universal acclamation of all algologists.]

Section I.—*Mougeotiæ mesocarpicæ*.

Spore formed by the tripartition of the pair of conjugated cells (=*Mesocarpus*, Hass., *Craterospermum*, Braun, and *Pleurocarpus*, Braun).

1. *M. mirabilis*, Braun, *M. genuflexa*, Auct.

[Characters, p. 36, with the following remarks by the author]:

"No accurate description of this fine species having previously been given, I have here endeavoured to present one. The actual conjugation productive of the formation of the spore in this species is almost always longitudinal; very rarely is it in the ordinary way, or transverse. The transverse conjugation which very often indeed occurs here is but an apparent one; the cells have grown together with one another in the ordinary way, but there is no

fusion of the cells so grown together or resorption of the septa in the canal of conjugation takes place, nor any formation of spores. The formation of spores without resorption of the septum between the conjugating cells has, on the other hand, been observed by A. Braun as an exceptional case in longitudinal conjugation, and there are then formed two spores opposite to one another, one on each side of the septum (Pl. 3, f. 2). This mode of formation of spores strongly calls to mind the condition in certain Desmidieæ, *e. g. Closterium lineatum*, Ehr., and *Cylindrocystis diplospora*, Lundell."

[The probably abnormal case adverted to here by Dr. Wittrock we venture to think has only an apparent, not a real, parallel in the Desmidieæ cited by him. In *Closterium lineatum* (also *Cyl. diplospora*, Lundell, as well as others, *e. g. Penium didymocarpum*, Lundell, *Spirotænia condensata*, &c.) an actual and true *conjugation* takes place: the contents of the two parent-cells become divided into two equal portions, the two opposite moieties of the contents of each neighbouring parent-cell mutually conjugating, each with the corresponding moiety of the other, and two separate, more or less closely juxtaposed spores resulting, whereas in the abnormal case referred to in *M. mirabilis* no transfusion of the contents, segmented or otherwise, of *distinct* cells takes place—it *cannot* be accomplished, owing to the barrier offered by the still existent septum in the canal. Similar abortive efforts at conjugation, due to the same cause, are sometimes seen in various genera of Conjugatæ.]

"The Gotland, or upon the whole, the Swedish forms of the species in question, appear, judging from authors' descriptions, to be somewhat variable, from the silence touching the form of the spores. By Braun the spores are said to be nearly globular, and by de Bary globular or oval, whilst in one form they are always more or less angular, namely, either cubico-globular or short cylindrical, with a gentle thickening at the middle."

2. *M. scalaris*, Hass., var. β.

[The author figures a conjugated example in which the connecting canal is excessively long, being, instead of about the same length as the diameter of the cell or less, here many times longer. He seems inclined to suggest the possibility of this condition having been due to the collection lying several days in wet moss before being examined, as if the exceptional treatment might have brought about the exceptional condition to which he draws attention.]

3. *M. intricata* (Hass.).

4. *M. megaspora*, n. s.

(Description on page 38.) [The author remarks as follows:] "This species is distinguished by its large spores as well as by its long and, even when conjugated, straight cells. Of previously known forms, it calls most to mind Cleve's *M. nummuloides*, which certainly is not the same as Hassall's species of the same name. The likeness to *M. nummuloides*, Cleve, is so great that I scarcely hesitate to identify both these forms."

5. *M. Gotlandica*, Wittr. (=*Mesocarpus Gotlandicus*, Cleve).

[This species not certainly identified, as the examples found had not mature spores.]

6. *M. parvula*, Hass.

Section 2.—*Mougeotiæ plagiospermicæ.*

Spores formed by quadripartition of the pair of conjugated cells=Plagiospermum, Cleve.

No species appertaining here has been found in this district. The only hitherto known species, indeed, belonging here is *Mougeotia tenuis* (Cleve), Wittr. [=*Plagiospermum tenue*, Cleve].

Section 3.—*Mougeotiæ staurospermicæ.*

Spores formed by quinquepartition of the pair of conjugated cells (=*Staurospermum*, Kütz., and *Sphærospermum*, Cleve).

7. *M. viridis* (Kütz.) (=*S. viride*, Kütz.).

8. *M. gracillima* (Hass.) (=*S. gracillimum*, Auct.).

9. *M. elegantula*, n. s.

(Description on page 40.) [The author remarks:] "Amongst all known Mesocarpeæ this is the most minute. The width of the cells in *M. gracillima* (Hass.), and *M. parvula*, Hass., var. *tenuissima*, de Bary, which otherwise are the most slender forms, always exceeds $5\frac{1}{4}$ micromillimeters; here it hardly reaches $4\frac{1}{2}$ micromillimeters. Otherwise the most marked character of the species lies in the fact that the part of the spore enclosed by the mesosporium (when seen in the ordinary position) is almost perfectly quadrate, and without the slightest concavity at the sides. All the nearly related species have the sides of the mesosporium more or less hollowed in, with the exception only of *M. quadrata* (Hass.), which, however, is easily distinguished by its comparatively considerable size, and by its distinctly minutely scrobiculate mesosporium."

Section 4.—*Mougeotiæ polymorphæ.*

Spores formed by tri- or quadri- or quinquepartition of the pair of conjugated cells.

10. *M. calcarea* (Cleve), Wittr. (=*Sphærospermum calcareum*, Cleve).

[Description on page 40. The following are the author's remarks on this most remarkable form:] "In the whole of the freshwater algæ hardly any species has been found which possesses to so high a degree the power of variation as this. As well in regard to the mode of formation of the spores as to their form (to say nothing of their size and the length of the vegetative cells), there occurs, indeed, here the greatest possible polymorphism.

"The formation of the spores occurs in most cases as in the Staurospermum-group, or by quinquepartition of the cruciate cell formed by the conjugation (in orig. plate 2, fig. 1, 2, 3, s, s', s''). The spores are most frequently angular, sometimes of strictly Staurospermum-figure (f. 2, s), but ordinarily, angular-globular (f. 1 and 3, s), or oval-globular (f. 1, s'); in others, again, of quite irregular outline, for example, rounded on one half and on the other angular like *Staurospermum* (f. 1, s''). This mode of formation of the spores is that which has been observed by Cleve in this species, and in combination with a more or less globular form of the spores, as well as a coloured mesosporium, has been regarded as affording the characters to establish the genus *Sphærospermum*. Next to the mode of formation of the spores by quinquepartition, as in *Staurospermum*, the formation by tripartition of spores, as in *Mesocarpus*, is the most usual (f. 1 and 2, m, m'). The spores are here almost always globular, but exhibit variation even here in the respect that they sometimes occupy *only* the conjugation-canal, but most frequently, even a part of the original space of the conjugating cells (f. 1, m'). Least usual is the spore-formation in the Plagiospermum-mode by quadripartition. This occurs, however, most frequently under the form shown at fig. 2, *p*, less often, as at fig. 4, *p*, in a mode which, as to its details, calls to mind *Plagiospermum tenue*, Cleve. The form of the spores is along with this variable—oval, angular-globular, or almost quite globular.

"In this species there occur thus all the different modes of spore-formation, and almost all the forms of spores, which are regarded as characteristic of the various genera of Mesocarpeæ

established by authors, and it forms therefore the connecting link which combines them all into *one* genus.

"But it is not enough that the spores in this species may be formed in any of the three modes mentioned, for a fourth mode occurs which, however, must doubtless be regarded as abnormal. Spores, or at least spore-like cells (whether they have power to germinate is not yet discovered), are sometimes formed without conjugation by the instrumentality of a single cell (analogous to the condition in *Zygnema mirabile*, Hass.). And the species even here evinces its power of variation, for the cell divides sometimes into *three* daughter-cells, of which the middle one becomes spore-like (fig. 7, s), in *Mougeotia quadrata* (Hass.), Prof. de Bary has observed quite a similar mode of spore-formation ('Untersuch. üb. d. Conj.,' p. 22). Or it becomes divided into *two* daughter cells, of which one, the spore-like, occupies a projection from the side, as it were a conjugation-outgrowth (f. 8 m); the first is without doubt to be regarded as an abortive effort to Staurospermum-conjugation, the latter to Mesocarpus-conjugation. Another abnormal case, which may deserve to be mentioned, is that sometimes three cells, and not two only, conjugate with one another (f. 5); the spore-formation has in this case shown itself to take place by quinquepartition of the conjugated cell."

[The discovery of this remarkably curious and most unexpected alga must no doubt be considered by algologists as highly interesting, and the account will no doubt kindle a general desire to find and to examine so perplexing a form. Still, seeing that at least the species falling under the genera *Mesocarpus*, Hass., *Staurospermum*, Kütz., and (probably, too, *Plagiospermum*, Cleve) are so universal, so marked, so characteristic, and so constant, it does seem a hard case to relinquish them *as genera* (we are by no means compelled to do so *as species*), and we might venture to suggest that the difficulty might have been possibly escaped by leaving those genera intact, and instituting a new genus (under some such name as "Allospermum," or "Heterospermum"), for the reception of the extraordinary form, *M. calcarea*, described by Dr. Wittrock. It must be conceded (and we have thought so ere now) that the form of the spore is not sufficient ground for the distinction of *genera*, and therefore that *Craterospermum* and *Sphærospermum* could not be justly regarded as natural genera, however individually *specifically* autonomous the forms may be for which those genera were instituted. But the genera *Mesocarpus*, *Plagiospermum*, and *Staurospermum* do not depend on the figure of the spore,

but on the plan of spore-formation, that is, whether by tri-, quadri-, or quinque partition of the conjugation-cell. Now, the species falling under the revived (but far more comprehensive genus than when established by Agardh) *Mougeotia* have to be divided by Dr. Wittrock himself into four sections, founded on the mode of spore-formation, for the purpose of facilitating the discrimination of the individual species, that is to say, into groups distinguished by the circumstance whether during spore-formation the conjugation-cell be constantly divided into three, four, or five (the middle one being the ultimate *spore*), or whether all these modes be combined in the one species. Of this latter section Dr. Wittrock looks forward (we may gather from his words) to finding other species besides *M. calcarea*. Now these four groups or categories, falling under the proposed broader genus *Mougeotia*, Wittr., are precisely equivalent to, and are neither more nor less than the smaller genera *Mesocarpus*, *Plagiospermum*, and *Staurospermum* (as generally understood), as well as a "new" one, with as yet *one* species only—*M. calcarea*. Would it not then disturb our conceptions less, facilitate reference more, and equally meet the requirements of the case, to do as is here very deferentially suggested, rather than to upset an existing and universally understood nomenclature? It must be unnecessary to recal the fact that *Mougeotia*, de Bary, is quite a different thing, and Dr. Wittrock's proposed name *Debarya* for that genus would most fittingly honour a justly renowned algologist, and remove any chance of confusion of that genus with *Mougeotia*, Ag. (in any sense), although, if the modern genera composing the broader genus *Mougeotia*, Wittr., were still to stand, the name instituted in honour of Mougeot would fall to the ground (unless retained for *M. calcarea*), and it was just Prof. de Bary's regret at the fact that caused him to wish to perpetuate it, even though it was in an altered sense; a step, however, to some extent calculated to lead to confusion and ambiguity.]

[The author adds that he had found a sterile Mesocarpus-form of quite unusual dimensions. The diameter of the vegetative cells reached 35 to 40 micromillimeters; it was also distinguished by the circumstance that the starch-granules did not lie in a straight, but a zig-zag, line; and he thinks it is probably an undescribed species.]

Family II.—*Zygnemeæ* (Menegh.), De By.

Genus I.—*Sirogonium*, Kütz.

[The author says:] "Since the essential character of this genus appears to me to lie in the fact that the cells of the conjugating filaments are of two kinds, namely, fructificative, which by conjugation produce spores, and vegetative, which always remain sterile, I have without hesitation referred hither Cleve's *Spirogyra punctata*, notwithstanding that the details of the process in the formation of the fructificative cells are not known in this species. Cleve has already (in his 'Monograph of the Zygnemeæ,' p. 23) suggested its transfer to the genus *Sirogonium*."

1. *S. punctatum* (Cleve), Wittr. (*Spirogyra punctata*, Cleve).

[The author continues:] "A peculiarity which deserves remark is that the conjugating cells in this species are almost never of like size. The cell in which the spore is formed, or the female, is, as a rule, notably longer than the male. In this the species calls to mind *S. stictitum*, Kütz., in which the dissimilarity between the female and the male cells is even more conspicuous."

Genus II.—*Spirogyra*, Link.

1. *S. bellis* (Hass.).
2. *S. majuscula*, Kütz.
3. *S. adnata* (Vauch.), Kutz. = *S. hyalina*, Cleve.

[The author writes:] "The agreement between *Conjugata adnata*, Vauch, and *Spirogyra hyalina*, Cleve, is so great that I do not hesitate to regard them as identical. They resemble each other, not only in the diameter of the vegetative cells, the form of the spores, and other essential characters, but even in regard to the character of habit depending on the fact that the filaments are always in a high degree mucous."

4. *S. porticalis* (Vauch.), Cleve.
5. *S. condensata* (Vauch.), Kütz.

[The author states:] "This and the foregoing stand very close to one another. Amongst those Spirogyra-forms collected (at 'Rosendal') there were found some as to which I could not definitely decide how far they ought to be referred to one or other species."

6. *S. communis*, Hass. (=*S. longata*, Kütz.).

[The author's remarks hereupon are as follows:] "This species is regarded by Kützing and Cleve, and several of the

later authors, as Vaucher's *Conjugata longata*. A closer examination of Vaucher's description and figures leads one to understand that that author had before him quite a different species (see observation immediately below). Variable as is indeed *Spirogyra longata*, Auct., it has never, however, been observed with the spore-bearing cells so wholly without inflation, and, compared with the spores, so long as Vaucher (loc. cit.) describes and figures them."

7. *S. parva* (Hass.), Kütz.

8. *S. longata* (Vauch.), Wittr.

[The author remarks:] "If the name *Conjugata longata*, Vauch., can be applied to any species, without doubt it ought to be to this one. In it there occur long, not inflated, spore-bearing cells. The agreement in regard to other essential characters, such as the dimensions and the form of the spores, are indeed found here."

9. *S. subventricosa* (Hass.), Wittr. β. *inequalis* (Hass.), Wittr.

[The author writes:] "Evident intermediate forms between *a* and β, which two I had formerly considered as distinct species, were observed (at 'Burge'). In the same place was observed the form *Hilseana*, Rabenh., distinguished by the two chlorophyll-bands in the cells."

10. *S. inflata* (Vauch.), Rabenh.

11. *S. tenuissima* (Hass.), Kütz.

Genus III.—*Zygnema* (Ag.), de By.

1. *Z. stellinum* (Vauch.), Ag.

2. *Z. cruciatum* (Vauch.), Ag.

3. *Z. immersum* (Hass.), Wittr.

[The author remarks here:] "In 'Observations on Mougeotiæ,' Hassall, in 1843, described amongst others two new Zygnemeæ, one under the name *Tyndaridea immersa*, the other under the name *T. conspicua*. Subsequently, in 'Brit. Freshw. Algæ,' he reversed the nomenclature of these two species, so that he names the species *T. conspicua*, which he had formerly called *T. immersa*, and *vice versâ*. Since no valid grounds were adduced by him, nor do they exist for this procedure, one is not justified, as some authors have done, in paying attention to this alteration of name."

4. *Z. cyanosporum*, Cleve.

[The author mentions that he found a form with immature spores, possibly referable to *Z. leiospermum*, de By.]

Family III.—*Desmidieæ* (Kütz.), de By.

Genus I.—*Desmidium* (Ag.).

D. Swartzii, Ag.

Genus II.—*Sphærozosma*, Corda.

S. excavatum, Ralfs.

Genus III.—*Hyalotheca* (Ehr.), Kütz.

H. dissiliens (Smith), Bréb.

Genus IV.—*Micrasterias* (Ag.), *non* Ehr.

M. Crux-melitensis (Ehr.).

Genus V.—*Euastrum* (Ehr.), Ralfs.

1. *E. verrucosum* (Ehr.)
2. *E. pectinatum*, Bréb.

   β. *brachylobum*, n. v., Wittr. (Descr. on p. 48).
3. *E. elegans* (Bréb.).
4. *E. binale*, Ralfs.

   β. *insulare*, n. v., Wittr. (Descr. on p. 49).

   γ. *angustatum*, n. v., Wittr. (Descr. on p. 50).

Genus VI.—*Staurastrum* (Meyen).

1. *St. furcigerum*, Bréb.
2. *St. pseudofurcigerum*, Reinsch.
3. *St. senarium* (Ehr.).
4. *St. cyrtocerum*, Bréb.
5. *St. polymorphum*, Bréb.

   β. *subgracile*, n. v., Wittr. (Descr. on p. 51).
6. *St. hexacerum* (Ehr.).

   β. Ralfs.

   γ. *semicirculare*, n. v., Wittr. (Descr. on p. 52).
7. *St. alternans*, Bréb.
8. *St. striolatum* (Näg.).

   β. *ælandicum*, n. v., Wittr. (Descr. on p. 52).
9. *St. tetracerum* (Kütz.).
10. *St. Brébissonii*, Archer.
11. *St. pygmæum*, Bréb.
12. *St. punctulatum*, Bréb.
13. *St. læve*, Ralfs.
14. *St. cuspidatum*, Bréb.
15. *St. dejectum*, Bréb.

    β. *apiculatum* (Bréb.), Lundell.
16. *St. orbiculare* (Ehr.).

Genus VII.—*Arthrodesmus* (Ehr.).

A.? *glaucescens*, Wittr.

[Doubtless, if Dr. Wittrock had had the opportunity of withdrawing this form as relegated to this genus, he would have done so, as he now would acquiesce in its being more correctly referable to *Tetrapedia* (Reinsch.), char. mut., Arch. It is identical with *Tetrapedia Reinschiana*, Arch. ('Quart. Journ. Mic. Sci.,' vol. XII, N. S., pp. 362 and 364, Pl. XXI, figs. 11, 12, and 13).]

Genus VIII.—*Cosmarium* (Corda).

1. *C. biretum*, Bréb.
2. *C. Turpinii*, Bréb.
3. *C. ornatum*, Ralfs.
4. *C. conspersum*, Ralfs.
   β. *rotundatum*, n. s., Wittr.
5. *C. tetraophthalmum* (Kütz.), Bréb.
   β. *Lundellii*, n. v., Wittr. (Descr., p. 56).
   γ. *Denotarisii*, n. v., Wittr.
6. *C. Botrytis* (Bory), Menegh.
   β. *subtumidum*, Aresch.
7. *C. margaritiferum* (Turp.), Menegh.
8. *C. Portianum*, Arch.
   β. *nephroideum*, n. v., Wittr. (Descr. on p. 57).
9. *C. punctulatum*, Bréb.
10. *C. speciosum*, Lundell.
11. *C. Wittrockii*, Lundell.
    β. *angulare*, n. v., Wittr.
12. *C. calcareum*, n. s., Wittr. (Descr. on p. 58).
13. *C. undulatum*, Corda.
    β. *crenulatum* (Näg.), Wittr.
    γ. *minutum*, Wittr.
14. *C. Meneghinii*, Bréb.
    β. *angulosum* (Bréb.), Rabenh.
15. *C. connatum*, Bréb.
16. *C. moniliforme* (Turp.), Ralfs.
    β. Ralfs.
17. *C. Holmiense*, Lundell.
    β. *integrum*, Lund.
18. *C. pyramidatum*, Bréb.

19. *C. pseudopyramidatum*, Lund.
20. *C. Gotlandicum*, n. s., Wittr. (Descr. on p. 60).
21. *C. granatum*, Bréb.
22. *C. pygmæum*, Archer.
23. *C. turgidum*, Bréb.
24. *C. De Baryii*, Arch.
25. *C. cucumis*, Corda.

Genus IX.—*Pleurotænium*, Näg.

1. *P. nodulosum* (Bréb.), de By.
2. *P. truncatum* (Bréb.), Näg.
3. *P. trabecula* (Ehr.), Näg.
   β. *crassum*, n. v., Wittr. (Descr. on p. 62).
4. *P. maximum* (Reinsch), Lund.
   β. *subclavatum*, n. v. (Descr. on p. 63).

Genus X.—*Gonatozygon*, de By.

G. monotænium, de By. (= *G. Ralfsii*, de By., *Docidium asperum*, Ralfs).

Genus XI.—*Closterium*, Nitzsch.

1. *C. Ehrenbergii*, Menegh.
2. *C. moniliferum* (Bory), Ehr.
3. *C. Leibleinii*, Kütz.
4. *C. rufipes*, Ehr.
5. *C. acuminatum*, Kütz.
6. *C. Venus*, Kütz.
7. *C. incurvum*, Bréb.
   β *majus*, n. v., Wittr.
8. *C. attenuatum*, Ehr.
9. *C. Pritchardianum*, Arch.
10. *C. strigosum*, Bréb.
11. *C. rostratum*, Ehr.
12. *C. pronum*, Bréb.
13. *C. linea*, Perty.
14. *C. acutum*, Bréb.
15. *C. subtile*, Bréb.

Genus XII.—*Cylindrocystis*, Menegh.

1. *C. crassa*, de Bary.

Genus XII.—*Penium* (Bréb.), de By.

1. *P. conspersum*, n. s., Wittr. (Descr., p. 66).
2. *P. lamellosum*, Bréb.
3. *P. Jenneri*, Ralfs.

[The enumeration of the Family Desmidieæ is interspersed by some critical observations which are not translated, as I hope to take up not only this author's but also Cleve's and Lundell's communications on some future occasion.]

Order V.—Phycochromophyceæ (Kütz.), Rabenh.

Family I.—*Scytonemeæ* (Kütz.).

Genus I.—*Scytonema*, Ag.

S. turicense, Näg.

Genus II.—*Tolypothrix*, Kütz.

T. tenuis, Kütz.

Family II.—*Rivularieæ*, Harv.

Genus I.—*Zonotrichia*, J. Ag.

1. *Z. alpina* (Kutz.).
2. *Z. calcarea*, Rabenh.

Family III.—*Nostoceæ* (Menegh.), Kütz.

Genus I.—*Nostoc*, Vauch.

1. *N. margaritaceum*, Kütz.
   β *moniliforme*, n. v., Wittr. (Descr., p. 68).
2. *N. papyraceum*, Ag.
3. *N. commune*, Vauch.
4. *N. rupestre*, Kütz.
5. *N. sphæroides*, Kütz.

Family IV.—*Oscillarieæ* (Ag.).

Genus I.—*Lyngbya*, Ag.

L. cincinnata, Kütz.

Genus II.—*Phormidium*, Kütz.

Ph. subfuscum (Ag.), Kütz.

Genus III.—*Oscillaria*, Bory.

1. *O. major*, Vauch.
   β *margaritifera*, Kütz.
2. *O. æruginco-cærulea*, Kütz.

Genus IV.—*Hypheothrix* (Kütz.), Rabenh.

1. *H. vulpina*, Kütz.
   β *tumida*, n. s., Wittr. (Descr. on p. 69).

Family IV.—*Chroococcaceæ*, Näg.

Genus I.—*Merismopœdia*, Meyen.
M. glauca, Näg.

Genus II.—*Gomphosphæria*, Kütz.
G. aponina, Kütz.

Genus III.—*Chroococcus*, Näg.
C. turgidus, Näg.

The total number of freshwater algæ observed in Gotland and Oland amounts thus to only 192 species.

From the preceding enumeration it will be seen that of the highest groups of the freshwater algæ the Families Hildenbrandtieæ, Lemaneæ, Batrachospermeæ, and Chantransieæ, there here occur no representatives. The same is the case with the Oozoosporaceous family, Sphæropleeæ, as well as the Chlorozoosporaceous families, Ulveæ (*Prasiola*, *Monostroma*), and Chroolepidæ. Further, the Coleochæteæ are very scantily represented. Œdogonieæ occur more copiously, yet not at all in great abundance; twenty-six species being found. The total number known for the whole of Sweden is, according to Dr. Wittrock, seventy-seven (most of which probably occur in suitable situations in other countries — a point, it is hoped, capable of gradual elucidation by observers when Dr. Wittrock's forthcoming monograph is at their command). The most general, or perhaps more correctly, least rare species of the genus *Œdogonium*, Link, the author mentions to be *Œ. rostellatum*, Prings., and of *Bulbochæte*, Ag., *B. mirabilis*, Wittr. Of the last-named genus the otherwise general species, *B. setigera*, Ag.. and *B. intermedia*, de By., seem to be wanting. Of the species furnished with oval oospores, only *B. rectangularis*, Wittr., was met with. Vaucherieæ are very rare. Of Chætophoreæ are met with the ordinary *Chætophora* species, Draparnaldieæ more rare, and Stigeoclonia are wanting altogether. The author mentions that amongst Conferveæ *Cladophora fracta*, Vahl., is the only general one, that Tetrasporeæ are all rare, and Pediastreæ seem to flourish better than the other Chlorozoosporaceæ, *Pediastrum Boryanum*, Turp., *Scenedesmus obtusus*, Meyen, and *Coelastrum sphæricum*, Näg., being sufficiently general. Characieæ and Protococceæ are few; Volvocineæ comparatively numerous; of Characieæ, *Ophiocytium cochleare*, Eichwald, is pretty general.

The Mesocarpeæ evidently do not belong to the Algæ upon which water strongly imbued with lime acts inju-

riously. The conditions rather appear to be the reverse. They occur, says the author, especially in Gotland, almost everywhere and in great abundance, but being seldom met with spores, the species most frequently could not be determined. Still, the number of forms fully identified are sufficiently considerable, justifying the opinion that the Mesocarpeæ are not only more general and also more rich in forms than in the rest of Sweden. They may be regarded, indeed, as the most characteristic feature of the algal-flora of these islands. Cleve's 'Monograph' contains for the whole of Sweden an equal number of species as have been observed in Gotland alone.

The Zygnemeæ are not nearly so general, and the number of species proportionately less. The largest species of *Spirogyra*, *S. setiformis*, Roth., and *S. princeps*, Vauch., which elsewhere are not rare, have not been observed here.

Notwithstanding that the Desmidieæ discovered reach the number of seventy-five, this family must be regarded as the most scantily represented. Lundell's 'Monograph' makes the total number of known Swedish species as many as 325. Doubtless, three or four times as many species would have been found in any favorable locality of the same extent on the mainland if searched with the same degree of care. Some of the genera are altogether absent; such are *Bambusina*, Kütz. (=*Didymoprium*, Kütz., in part), *Xanthidium*, Ehr., *Tetmemorus*, Ralfs, and *Spirotænia*, Bréb. Some are apparently but scantily represented, as *Micrasterias*, Ag. (with a single species only, and that in but a single locality), also *Euastrum*, Ehr. Others are somewhat richer in forms, and in this regard the genera *Cosmarium*, Corda, *Staurastrum*, Meyen, and *Closterium*, Nitzsch, stand in the foremost place. The following species may be considered as general:— *Euastrum binale*, Turp., *β insulare*, Wittr.; *Hyalotheca dissiliens*, Sm.; *Staurastrum hexacerum*, Ehr.; *St. pygmæum*, Bréb.; *Cosmarium conspersum*, Ralfs; *C. tetraophthalmum*, Bréb.; *C. Botrytis*, Bory; *C. margaritiferum*, Turp.; *C. calcareum* (n.s.), Wittrock; *C. undulatum*, Corda, *β crenulatum*, Näg.; *C. granatum*, Bréb.; *C. pygmæum*, Archer; *C. Cucumis*, Corda; *Closterium Leibleinii* and *C. rufipes*, Ehr.; *C. Pritchardianum*, Archer; and *Penium lamellosum*, Bréb. The large and conspicuous forms are here rare. The following occur, but mostly sparingly: — *Desmidium Swartzii*, Ag.; *Micrasterias crux-melitensis*, Ehr.; *Euastrum verrucosum*, Ehr.; *Staurastrum furcigerum*, Bréb.; *Cosmarium biretum*, Bréb.; *C. Turpinii*, Bréb.; *C. tetraophthalmum*, Bréb., and *C conspersum*, Ralfs (the two latter, as mentioned, gene-

rally); *Pleurotænium maximum*, Reinsch; *Closterium Ehrenbergii*, Menegh.; and *C. attenuatum*, Ehr.; as well as *Penium lamellosum*, Bréb. *Closterium lunula*, Nitzsch, appears to be altogether wanting.

Of Phycochromophyceæ only a small number was noticed. This, the author thinks, is doubtless, to some extent, dependent on the circumstance that, during the journey, less attention was bestowed on these Algæ than on the others, but the principal cause, however, lies in the fact itself that they are in reality very scanty. A peculiar poverty in these Algæ may, therefore, be regarded as characteristic of the Flora of these islands.

The following algal-forms have hitherto been found only in these islands (most are, therefore, new); — of Oozosporaceæ, *Bulbochæte valida*, *B. sessilis*, *B. quadrata*, and *Œdogonium nodulosum*, Wittr., *Œ. calcareum*, Cleve, and *Œ. hystrix*, Wittr., var. β *subglobosum*. Of Chlorozoosporaceæ, *Chætophora pachyderma* and *Polyedrium gigas*, Wittr. Of Conjugatæ, *Mougeotia calcarea*, *M. Gotlandica*, *M. megaspora*, *M. elegantula*, Wittr., and *Zygnema cyanosporum*, Cleve; as well as of Desmidieæ the following:—*Euastrum binale* (Turp.), Ralfs, β *insulare*, and γ *angustatum*, Wittr.; *Staurastrum hexacerum* (Ehr.), β *semilunare*; *Cosmarium botrytis* (Bory), β *semitumidum*; *C. calcareum C. Gotlandicum*, *Closterium incurvum*, β *majus*, Wittr. (To these Dr. Wittrock adds, in manuscript, a few other new forms, about which more information is to be hoped for on another occasion.) Of Phycochromophyceæ, *Nostoc margaritaceum* (Kütz.), β *submoniliforme*, and *Hypheothrix vulpina*, Kütz., β *tumida*.

Illustrating his valuable contribution to algological literature the author appends four graphic, but uncoloured, plates.

Blue Stentorin.—*The* Colouring Matter *of* Stentor cæruleus. By E. Ray Lankester, M.A.

In the spring of 1871, when I was staying at Jena, a small aquarium tank belonging to Professor Haeckel was swarming with *Stentor cæruleus*. I had often thought that the peculiar blue tint of this infusorian was likely to give a detached absorption band or bands, and was, therefore, glad of the opportunity of examining its effect on the spectrum by means of the Sorby-Browning micro-spectroscope. I need hardly point out here that it is only by means of the application of the spectroscope

to the microscope that the colouring matter of so minute an organism as a Stentor could be examined. I succeeded in bringing the instrument to bear on single specimens of the Stentor placed on a glass slip and covered in the usual way with a thin glass cover. But my observations were greatly

facilitated by the presence in the tank which contained the Stentors of a number of *Chætogaster diaphanus*. Many of these worms had gorged themselves with *Stentor cæruleus*, and their stomachs were distended with the blueish-green undigested bodies of these Infusoria. In fact the Chætogasters had acted as most excellent collectors of the material I wished to examine, and on account of their perfect transparency I was able to make use of the Stentors packed in their stomachs just as they were, in order to obtain a greater mass of the colouring matter through which to pass the light before submitting it to spectrum analysis.

The blue colouring matter of *Stentor cæruleus* exists scattered in the form of minute granules through the cortical substance of the animalcule. It gives two strong absorption bands, bands of remarkable intensity when we consider how exceedingly small a quantity of the colouring matter is sufficient to produce the effect. Light which has passed through the thickness of only a single Stentor (which cannot be more than a few thousandths of an inch) is yet sufficiently affected to show the bands quite sharply. One band—the darker—is in the red, extending a little to the red side of the solar line C; its centre is nearer the blue than that of the reddest band of fresh chlorophyll, though the two bands overlap as seen in the cut. The second band in the green lies somewhat to the blue side of the redder oxyhæmoglobin band. Besides comparing the bands of stentorin, as we may term this blue pigment, with those of chlorophyll and of hæmoglobin, I compared them with those of phycocyan—the blue colouring matter of the Oscillarieæ. The tint of phycocyan is not unlike that of stentorin, and it has two absorption bands very similarly placed. I found, however, by superposition of

the spectra that the bands of stentorin and of phycocyan are by no means identical in position.

Dilute acetic, hydrochloric, and sulphuric acids do not appear to effect any change in stentorin. Dilute potassium hydrate had the effect of intensifying the blue colour and causing the disappearance of the bluer band, whilst the redder band was rendered more dark and moved slightly towards the red end. The extremely minute quantity of the material prevented me from making a fuller examination of the effect of chemical reagents. I must also express my regret that we have at present no uniform system of recording absorption spectra. Such indications as I have given in the woodcut and in words are only sufficient to give a general idea of the position of the bands, and would not enable any one with certainty to identify this colouring matter supposing that it should be found in other organisms. It would have been useless to give the position of the bands as fixed by Mr. Sorby's quartz interference scale: for nobody can get one exactly like his original plate, and if it were possible there could only be a limited number of them. Nitric peroxide unfortunately gives no lines in that part of the spectrum where the chief band of stentorin lines. The only plan, which is generally useful with so short a spectrum as that given by the Sorby-Browning instrument, is to refer to the chief solar lines and to a variety of substances the bands of which are known, *e.g.*, oxyhæmoglobin, chlorophyll, potassium permanganate. stentorin is interesting as being an addition to the very short list of animal substances which give banded and therefore characterisable absorption spectra. These are hæmoglobin and it derivatives, chlorocruorin, bile pigments and derivatives, chlorophylloid substances, turacin, aphidein and some other insect products of a partly vegetable origin. My friend Mr. Moseley has also obtained a banded absorption spectrum from certain Actiniæ. Before leaving the subject of Stentors let me mention that with the *Stentor cæruleus* there were present a few specimens of *Stentor Mülleri*, and these having a brilliant green colour like that of hydra gave with the spectroscope no trace of stentorin, but an absorption band far in the red indicating a chlorophylloid body like that of Hydra and Spongilla.

Let no one suppose that animal colouring matters have not been very largely submitted to spectroscopic analysis. For several years I have seldom missed an opportunity of examining a coloured animal substance which I had not previously observed, and I know that several persons have been equally busy. I published two years ago ('Journal of Anatomy and

Physiology,' November, 1870) a list of animal substances examined and found to give continuous not banded spectra. To this list I can now add two important colouring matters which I took the opportunity of studying when at Naples during the past winter (1871-72). The first of these is the blue pigment of *Velella*, which is in all probability identical with that of other oceanic Hydrozoa. A continuous spectrum with cutting off of the red from its end up to the green, and a similar cutting off of the violet was the result of spectroscopic analysis of the colour: no detached band or bands of absorption. The *Vellela* may be preserved with its natural colour in a saturated solution of acetate of potash, alcohol changes it to a salmon pink and dissolves it, ether effects a similar change, but no absorption bands are produced. The second colouring matter of some interest is that which impregnates the cheese-shaped corpuscles which float in the perivisceral fluid of *Sipunculus nudus*, and give to that fluid a madder-red colour. This colouring matter is soluble in fresh water. It gives merely a cutting off of the blue end of the spectrum and of the extreme red; no detached band or bands of absorption. Alcohol, ether, acids, alkalis, all have immediately the effect of destroying this colour. Alexander Brandt, in a recently published memoir on *Sipunculus nudus* ('Mem. de l'Acad. Imp. St. Petersbourg,' 1870), looks upon the pink corpuscles of Sipunculus as comparable to the red corpuscles of Vertebrata, in which I quite agree with him, but it is necessary to state that the colour in the one case is due to hæmoglobin, in the other to a body which is quite distinct from that compound and which has not been shown to have any properties in common with it except that of solubility in water. The pink colour from *Sipunculus* has not even an absorption-spectrum resembling that of hæmoglobin, which the green representative of hæmoglobin in *Sabella* and the Chloræmians *does* possess.

I may add to these, as further cases of interesting pigments examined by me with a negative result, the red colour from the 'nucleus' of many salpæ, the blue colour from other species, the brilliant red-pigment from the foot of species of cardium, mactra, pectunculus, &c., and lastly, a colouring matter from which I had rather confidently hoped for a positive result, the greenish-gray pigment of *Penicillium glaucum* extracted with ether.

On Actiniochrome, *a* Colouring Matter *of* Actinias, *which gives an* Absorption Spectrum. By H. N. Moseley, M.A., Naturalist in H.M.S. Challenger.

In the estuary of the Severn, in the neighbourhood of Aust and the New Passage, there are to be found, thriving in the muddy water, large numbers of Actinias, principally of two common species, *Actinia mesembryanthemum* and *Bunodes crassicornis;* and farther down the channel, at Weston-super-Mare, *Bunodes crassicornis* is to be found in great abundance. As might be expected from the non-transparency of the medium in which they live the colours of these anemonies are not by any means so bright as are those of individuals which live in pure sea-water. The red colouring seems to be especially affected. *Actinia mesembryanthemum* in this muddy water assumes a pale olive, or often a mere dirty-white colour, and *Bunodes crassicornis* a transparent green tint, which is especially remarkable in Weston specimens. About Aust Cliffs I found in one especial locality, however, some specimens of *Bunodes crassicornis,* which retained a vivid red colouring, and were almost as bright as marine specimens. The reason appeared to be that they were attached to the bottom of a rocky channel, through which, as the tide fell, there drained off in a continuous stream water with which a large natural rock basin, standing at a slightly higher level, had been filled at high water. The dense charge of mud held in suspension by the Severn water when it is in motion was precipitated rapidly when this water came to rest in the still rock pool. Thus, a short time after the stream had begun to flow from the pool, the water of the stream became perfectly clear and transparent, and the specimens of Crassicornis living in it, by the help of the greater quantity and different quality of the light they received, were able to maintain their red colouring, though their congeners close by were almost colourless or merely greenish. On examining the red colouring matter of one of these red specimens with the spectroscope, I found that that which I scraped off from the general body-walls and tentacles gave no absorption-band, whilst that from the disc surrounding the mouth did give a single well-marked one, nearly coinciding with the less refrangible band of hæmoglobin. I determined the position of the band accurately at the time I was working at the subject, and showed it to my friend Mr. E. R. Lankester, but unfortu-

nately the drawing was mislaid, and in the hurry consequent on preparing for the Circumnavigation expedition, I have been unable to make a re-examination. I think, however, that there is sufficient interest in the observation, though imperfect, to warrant its publication. Mr. Lankester suggested to me the term Actiniochrome for the red colouring matter giving this single absorption-band. I was unable to obtain the matter in solution, though I tried all the ordinary solvents of animal colouring matters.

Most of the Weston specimens of Crassicornis are entirely green, but in some few, perhaps one in ten, the tips of the gonidial tubercles retain a bright-red colouring. In many specimens these tubercles have a slight tinge of red, but in two specimens which I obtained they were of a beautiful rose colour. The colouring of these tubercles was, on examination, found to be due to Actiniochrome, though this colouring matter was absent from the entire remainder of the body. This is a remarkable fact, and seems paralleled by that discovered by Mr. Lankester, that in marine gasteropods hæmoglobin exists in the mouth-muscles, and there only. It is hoped that this observation may lead to further investigation of the colouring matter of Actinias, and it must especially be noted that colouring matter from various parts of the body should be examined, even although the colouring of those different parts appears to the unassisted eye to be identical. No difference could be detected at sight between the red colouring of the body-wall or tentacles of *Bunodes crassicornis* and that of the disc, yet one gave no absorption-band and the other did. It is further of extreme interest to observe the changes undergone by marine animals in colour when they inhabit estuaries. The question of the origin of the colouring of such forms is one of the most difficult problems of biology, and we may hope that some light may be thrown on it in this way. It is possible that the Actiniochrome may be differently distributed over the body of marine specimens of Crassicornis. I could find no similar colouring matter on *A. mesembryanthemum* nor *Actinea rosea*. It is possibly confined to this one species. Why it should last disappear from the tips of the gonidial tubercles it is impossible to conjecture.

The Formation *of* Auxospores *in* Cocconema Cistula. Ehr. By Fr. Schmitz. Translated by the Rev. E. O'Meara, M.A.[1]

Pfitzer's investigations upon the structure and development of the Bacillariaceæ have shown that this minute family of monocellular organisms possesses a mode of development which has not been hitherto observed in any other group of either the animal or vegetable kingdom. By means of a continuous process of fission into two the size of the single cells diminishes, till at length it reaches its minimum. Whereupon there interposes a formation of spores—auxospores, as Pfitzer calls them—which checks the regular process of division, and produces cells possessing the maximum size of the species, and in all other respects precisely similar to the mother-cells. These primary cells then commence anew the same process of division, which continuously gives birth to generations of cells, each more diminutive than the preceding. So the life of each single species undergoes a progressive diminution until, from time to time, a fresh act of rejuvenescence brings back the organism to its original dimensions.

This process of renewal by the formation of auxospores exhibits very different features in the different groups of the family. In some cases the auxospores are produced by actual copulation (*Surirayæ*), like the zygospores of the Conjugatæ; in others by a simple reproductive effort of individual cells (Melosireæ), like the swarmspores of the Oedogoniæ. But further most distinct transitions are found between the purely unsexual mode of reproduction by swarmspores, and the simplest form of sexual reproduction by the copulation of two cells. Such transitions, according to Pfitzer's observations, the Naviculeæ and Gomphonemeæ exhibit. But, unhappily, the number of species in which the formation of auxospores has been accurately noticed is, up the present, very small. Many circumstances contribute to the fact that in very many species the formation of auxospores has been seldom or never observed, but the misfortune of observers in this respect can scarcely be attributed altogether to the rare occurrence of this mode of reproduction. However, that the opportunity of observing the formation of spores occurs but seldom is sustained by the fact that in this paper I can only undertake

[1] From the 'Botanische Zeitung,' 1872, pp. 117—126.

to give a detailed account of the process in the case of a single species, namely, *Cocconema Cistula*, Ehr.

The formation of auxospores in *Cocconema* has already been frequently noticed. The earliest contributions upon the copulation of the Bacillariaceæ, by Thwaites,[1] describes the procedure in the case of *Cocconema Cistula* and *C. lanceolatum*. The two cells enveloped by mucus always produce two auxospores—sporangia—which lie parallel to the mother-cells, and are similar in all respects except as regards size. When the sporangial cells have reached their maturity the mucous covering is dissipated, but Thwaites gives no information as to the circumstances under which the two sporangia proceed from the two mother-cells, whether copulation occurs or not.

Carter afterwards observed[2] the same process in the genus most nearly related to the other, viz. in *Cymbella Pediculus*, Kütz. In this instance, also, two mother-cells produce two auxospores, which lie parallel in all respects, as in the case of Cocconema. But in the case of *Cymbella* Carter did not observe an actual copulation, though he assumes it as an undoubted fact. Isolated observations carry him so far as to suppose that the mother-cells copulate, and that afterwards the product of the copulation divides into two parts, which develop themselves into spores. Later still, W. Smith, in his 'Synopsis of the British Diatomaceæ' (vol. ii, 1856), pl. C, published a series of figures representing the formation of auxospores of *Cocconema cistula* and *C. lanceolatum*, which is in perfect agreement with the opinions and descriptions of Thwaites and Carter. In pl. E he adds a description of the like process of development in *Encyonema*, which presents nothing that differs from that of the closely related *Cocconema*. No figure of Smith's exhibits an actual copulation, but still the author, in his description, presumed that such a copulation had been actually observed by him in *Cocconema* or *Encyonema*. He also assumes that the two spores originate through the junction of the two mother-cells, and subsequent division into two parts.

Actual copulation of the mother-cells has not been seen, therefore, by any of these observers, though all three maintained such a copulation in the formation of the spores to be a self-evident fact. What had been noticed in some few Bacillariaceæ was considered, without further observation, as equally applicable to all the rest.

[1] 'Ann. and Mag. of Nat. Hist.,' 1847.
[2] 'Ann. and Mag. of Nat. Hist.,' 1856.

Far different, however, from the above-stated representations is the description of the same process in the case of *Cocconema Cistula,* published by Lüders in the 'Bot. Zeitg.,' 1862. According to this author, two cells invariably come together to produce spores, and place themselves parallel, with the smaller sides approximating each other. Then, surrounded with a common gelatinous envelope, each frustule parts asunder its two valves, and divides itself into two halves, standing one above the other. Of the four masses of the cell-contents thus formed, the two which lie opposite each other unite, enlarge, and become a single spore. During the process of growth there arises, as an external covering of the hitherto naked cell, a mucous skin, an outward layer of which subsequently develops itself into the cell-membrane.

The growth always proceeds in such a direction that the spores come to lie parallel to the mother-cells and between their two cast-off valves. According to this representation, in the case of *Cocconema,* the spore originates through means of actual copulation from one half of the two mother-cells. It is remarkable, however, that the figures afford no appearance of this copulation. Fig. 4 *b* discloses only the separation of the mother-cells into two halves. So that even Lüders' description fails to indicate how the two products of copulation move so as eventually to lie between the two valves of the mother-cell. The process of forming auxospores in the next related family of Naviculæ is, according to the investigations of Pfitzer, wholly different.

In September of last year I had the good fortune of finding in great abundance *Cocconema Cistula* in the act of forming auxospores.

In a full, rapidly-flowing brook, in the neighbourhood of Saarbrück, I found an abundance of large and small pebbles covered with a thick, flaky mass of mucus, the gold-brown colour of which marked it as bacillariaceous. Microscopic examination revealed *Cocconema Cistula* in extraordinary exuberance. The branched stipites of this species formed a tufted stratum on the stones, by reason of their extraordinary abundance and thick texture. In this dense forest of fixed stipites free frustules of *Cocconema* swam hither and thither. Amongst these appeared very numerous copulating pairs. The formation of auxospores proceeded in the following manner:

Two free-swimming cells laid themselves parallel nearly under one another, the girdle-bands being opposed to one another at the smaller side. Stipitate cells were never observed taking part in the process. The cells were sometimes

of the same size, but sometimes one was somewhat larger than the other; by no means, however, were they two daughter-cells of the same mother-cell. The first intimation of spore-formation is the secretion of a mucous envelope, which surrounds the two frustules until the auxospores are quite mature; by this means the spore-forming pairs could always be readily detected. Then the plasm in both frustules commences to retire a little from the ends and to accumulate in the middle; the two halves of the membrane were pushed asunder, and the plasm set free from its shell; by this the smaller girdle-bands on the concave side of the frustule were naturally separated from one another and set free sooner than were the broader girdle-bands of the convex side. The two valves were widely parted from one another on the side of the small girdle-band, while on the broader side they still remained attached. Between the two thrown-off valves the naked cell now lies, as an ellipsoidal plasm-mass, shorter than the mother-cell, but parallel to it. The single endochrome-plate, which is normal with the Cocconema-cell, and the middle of which lies on the broader girdle-band, by the contraction of the plasm-mass, has its upper and lower edges folded inwards, without having its position otherwise altered; all the remaining parts of the plasm-mass are also brought together into a small space. The two cells now lie inactive for a considerable time, one close to the other without touching. Then each plasm-mass begins to stretch out in a longitudinal direction; the turned-in edges of the endochrome-plate unfold themselves and expand; the whole plasm-mass extends and grows to a considerable length. At the same time a distinct membrane is secreted upon the upper surface of the cell, which at first is scarcely distinguished as a simple dark line, but gradually becomes observably thicker. Then each individual cell extends itself into a long cylindrical body, with short rounded ends, and twice the length of the mother-cell. Frequently the two individuals of the same pair were equally advanced in their development, but not unfrequently the one was considerably in advance of the other, but the two always remained perfectly distinct. Gradually the young auxospores changed their hitherto perfectly cylindrical form, became smaller towards the ends, and slightly bent into a crescentic shape; at the same time the two sides flattened themselves more and more. The membrane, hitherto smooth, exhibits numerous very fine, ring-like, transverse striæ (zones), which are traceable to a wavy structure, and resemble the so-called zone-covering of the auxospores of *Navicula*. The crescent-shaped spores

were usually turned towards each other with their concave edges, but sometimes with their flattened surfaces.

Within the zone-covering the two valves of the primary cell are now developed, and in this, as well as in the case of all the well-investigated Bacillariaceæ, one after the other. First the plasm of one flattened surface of the "perizonium" (Pfitzer) draws back and secretes a siliceous valve, and, after this is completed, the same process takes place at the opposite surface. In this instance, also, the edges of the older valve embrace the younger valve; both valves exhibit perfectly the usual structure of the valves of *Cocconema*.

So the formation of auxospores has attained its completion. Unhappily, however, I was unable to follow out the further development of the primary cell, the origination of the girdle-bands, and the first cell division; it, has, however, been fully established that the mucous envelope was gradually dissipated, and the two primary cells left free.

So much for the normal process of developing the auxospores. I have had the opportunity, moreover, of noticing many eccentricities. Several pairs were found, of which one frustule was dead, and that in all stages of the development of the auxospores. I have seen many pairs, of which the one frustule was dead, even before they had commenced to push asunder and throw off the old valves, and this remained upon the second frustule without the slightest injurious effect, which latter, in the regular process, developed itself into an auxospore. Neither is the number of frustules which, under the common mucous covering, contribute to form the spores, always two. I have frequently seen in the same mucous envelope three frustules develop themselves into auxospores, in the normal manner above described. On one occasion I had the opportunity of noticing in a similar envelope only one individual in the act of spore-formation; quite in the usual manner it threw off the old valves and formed an ellipsoidal plasm-mass. Unhappily, however, the specimen was lost to view, so that I could not ascertain whether or not it proceeded to the formation of a perfect auxospore. I am not able to confirm the conjecture of Lüders; I am rather compelled to deny the whole representation of the separation of each of the mother-cells into two parts. Lüders' supposition may be easily elucidated by what actually occurs, as was proved before. The plasm, subsequently to the throwing off of the old valves, forms an ellipsoid mass, the upper and lower ends of the single endochrome-plate being folded inwards, consequently four folds of the endochrome-plate lie over one another in the upper and lower

extremities of the ellipsoid, while in the middle there are only two, and these very thin. Thus, the upper and lower ends of the naked cell are of a deep brown-golden colour, while the middle zone is bright yellow and pale. Hence with a low magnifying power and imperfect observation it may appear as if the cell had divided itself into two masses of plasm, at the same time that there is no appearance of any line of demarcation, either by a mucous covering or by a simple dark line. More exact observation exhibits clearly a simple ellipsoidal naked cell, always sharply defined against the mucous covering by a simple dark line; but of the separation of this cell into two parts there is not a glimpse.

That of the four plasm bodies, therefore, the two which lie opposite each other coalesce, and develop into a single monocellular spore, was not actually observed by Lüders—in fact, not the least copulation occurs in the whole process of auxospore-formation, as has already been illustrated. On the contrary, the two mother-cells remain always apart, and separated from one another by a distinct layer of mucus. The mutual approximation of the frustules, and the flattening off of the two plasm-bodies, as Pfitzer has observed in *Frustulia saxonica*, I have never been able to discover. Any material influence of the two cells upon one another cannot occur otherwise than by diffusion through the mucous dissepiment, if such an influence does actually occur. Direct observations, at least, afford no evidence of it; but at the same time that some influence is exerted by the two cells upon one another is beyond all doubt, the invariable union of two cells for formation of auxospores is sufficient proof. But what the nature of that influence may be is a subject upon which I venture to express no opinion.

However that may be, the formation of auxospores by *Cocconema* exhibits an interesting gradation between sexual and asexual reproduction. The fact that two cells always co-operate in the formation of spores (to judge by the above-mentioned incompletely observed cases) is one which plainly approaches, by the co-operation of two cells, the simplest form of sexual reproduction; on the other hand, the utter absence of any actual copulation in the process of spore-formation removes it from the sexual reproduction and allies it to the asexual form of reproduction, namely, the simple propagation by swarm-spores.

Of all the hitherto known forms of auxospore-formation, the process in the case of *Cocconema* stands next to the analogous process in the Naviculeæ, as Pfitzer has described

it. Only, in this latter-named group, the mutual co-operation of the two frustules of a pair is far more apparent than in *Cocconema*, and approaches nearer to sexual reproduction. It is to be hoped that a more complete investigation of the auxospore-formation of numerous forms may soon render the transition from the asexual to the sexual reproduction more complete, and fill up many gaps which still remain open from the want of adequate material for observation.

Into the development of auxospores Smith has, moreover, in his representation of the reproduction of Bacillariaceæ, introduced the so-called cysts. He found, especially in *Cocconema Cistula*, and also in *Synedra*, numerous mucous masses, which contained more or less numerous small cells. These latter originate, as he supposes, through the continuous division of the large auxospores within the mucous covering. Against this supposition of Smith De Bary has expressed himself, and imagined that these formations were due to Amœbæ. Lüders further observed that, inasmuch as such cysts are formed by Amœbæ, they should be wholly excluded from the development-history of the Bacillariaceæ. Pfitzer has adopted this proposal of Lüders. I will not deny that the greater number of these so-called cysts are traceable to Amœbæ, but all have not this origin. There are cysts of of *Synedra*, such as Smith has described, and which I have myself frequently noticed. I have certainly found, in the case of *Cocconema Cistula*, cysts in abundance, which, undoubtedly, did not emanate from Amœbæ. I have found in considerable abundance individual cells, of the same size of the mother-cells of the auxospores, surrounded by a mucous investment, just in the same manner as in the formation of auxospores. On one occasion, as already related, such an individual commenced the regular course of developing auxospores, but, after a short period of rest, recommenced the regular division of the cell; the two daughter-cells divided in the same manner, and so on. Thus was formed a group of cells, which closed together by the small girdle-bands, and were held together by the ellipsoid mucous investment outside, just as Smith's Fig. 221, iv, pl. C, shows. I have seen as many as ten individuals arranged in such an inclosure. The ultimate result of this process I did not ascertain. In all probability the mucous investment ultimately broke up and set free the cells, for, besides numerous cysts containing cells, I found a very few only with numerous individuals, but none in which the cells were dead. The cysts would seem, then, an abortive attempt at auxospore-

formation. The first step towards the formation of auxospores was taken by the single frustule, but the usual process of development by simple division supervened. This view is sustained by the fact that the size of the cells within the cysts was precisely the same as that of the mother-cells of the auxospores.

On the STEM-STRUCTURE *of the* CARBONIFEROUS LYCOPODIACEÆ. By W. T. THISELTON DYER, B.A., B.Sc., F.L.S.

PROFESSOR WILLIAMSON has recently described, in the 'Transactions of the Royal Society,'[1] the various types of stem-structure possessed by the plants of the Coal-measures which are now, by general consent, referred to the Lycopodiaceæ. The great difference which exists amongst these types is remarkable, and renders them far from easy to correlate with one another. Professor Williamson's descriptions and drawings leave little or nothing to be desired. But it is altogether doubtful whether the terminology he has employed will find general acceptance. Looking at the relationship of a Lycopod to a flowering plant, it is difficult to see how more than a homoplastic resemblance can be asserted to exist between the stems of the arborescent representatives of either. Professor Williamson, however, is satisfied that the correspondence is homogenetic, and unhesitatingly employs for the fossil Lycopodiaceæ the terms used in describing a dicotyledonous axis. It seems far better, as Mr. Carruthers has urged, to avoid prejudging in this way what is, at the best, hardly an open question. The opposite course, no doubt, has the sanction of Brongniart, but at the time that that botanist wrote such material as he had the opportunity of studying led him to the conclusion that these structures actually belonged to Exogens. Professor Williamson, claiming to have given the *coup de grâce* to Brongniart's theory, still uses the terms which were appropriate to it.

The general structure of one of these fossil stems presents three regions — 1st, a central axis; 2nd, a cylinder surrounding the central axis, and exhibiting in a transverse section an arrangement of its tissues in radiating lines; 3rd, tissues external to the "radiated cylinder," which, without any implication of homology, may be briefly spoken of as cortical.

[1] 1872, pp. 197—240.

It is the consideration of the first two regions which seems to present the greatest difficulty, and Professor Williamson has described as many as seven types of stem-structure, which he thinks may be distinguished by characters taken principally from these regions, but which seems to be reducible to four. They certainly appear to support his view that there is a gradual passage from the Lepidodendroid to the Sigillarioid type. Hitherto the researches, more especially of Binney and Carruthers, have pointed to the view that the central axis of Lepidodendroid stems was vascular, while that of the Sigillarioid was cellular,[1] and I have myself urged that this was true with respect to the former more decidedly than, with Professor Williamson's present memoir before me, I am now disposed to do.[2]

The type in which the central axis is most vascular is *Lepidodendron selaginoides*, which has been figured and described by Binney, Carruthers, and Professor Williamson. Even here, however, the vascular tissue does not compose the whole axis, though it is a predominant constituent, but cellular tissue is interspersed with it. The radiated cylinder is thin.

In a type figured by Mr. Binney under the name of *Sigillaria* the two constituents of the central axis are, as it were, segregated; the external portion only is vascular, the most central being composed of vertical rows of short parenchymatous cells; the radiated cylinder is well developed. The structure of Corda's *Diploxylon*, which is the same as Witham's *Anabathra*, is substantially identical. *Lepidodendron Harcourtii* constitutes another type, for although, according to Professor Williamson, it does not essentially differ in respect to its central axis from Mr. Binney's plant, yet the radiated cylinder is altogether wanting. *Ulodendron*, also, quite agrees in internal anatomy with *Lepidodendron Harcourtii*, though it has, of course, other important differences. Lastly, *Favularia*, which belongs to the fourth or Sigillarioid type—*Sigillaria* itself being still little known—is shown by Professor Williamson to have a central axis almost entirely cellular, or, with probably, besides the radiated cylinder, a merely peripheral coating of vascular tissue. In illustrating all these forms, which he has done in great detail, Professor Williamson has employed to the greatest advantage his skill, both as a lapidary and a draughtsman. But he has not attempted, what would have been exceedingly interesting, any correla-

[1] 'Nature,' 1871, p. 445.

[2] Carruthers, 'Proc. Geol. Soc.,' 1869, p. 251. 'Monthly Mic. Journ.,' 1869, p. 179.

tion of the extinct with existing Lycopodiaceous forms.[1] Speaking with all respect of so distinguished a naturalist, and of Dr. Carpenter, whose support Professor Williamson claims for his views, it may be doubted whether anything is gained by throwing vascular Cryptogams into two groups having "affinities, as far as their stems are concerned," with Monocotyledonous and Dicotyledonous Phanerogams respectively. In so far as correlation implies homoplasy it is, as regards Ferns and Palms, certainly improbable as to details of structure; in so far as it implies homogeny it is quite certainly erroneous.

We are not at present completely in a position to trace the relations between the structure of extinct and recent Lycopodiaceæ. The latter can hardly be said to be in any case more than herbaceous; the former are well known to have formed trees fifty feet at least in height, with branching crowns. It is quite clear that, if such trees were to exist at all, as their crowns enlarged the stems that supported them must have enlarged also. We may, therefore, reasonably expect to find that one point of difference between extinct arborescent and recent herbaceous Lycopodiaceæ will consist in the appearance in the cross section of the stem of the former of provisions for continued growth. We see the same *kind* of provision, disregarding the details of its accomplishment, distinguishing arborescent from herbaceous Docotyledons. We also see it in some arborescent Monocotyledons, and, if one may use the expression, even in arborescent Algæ. We might apply the term exogenous growth to each and all of these, but it must be strictly kept in view, which Professor Williamson does not seem sufficiently to have done, that the agreement, such as it is, is homoplastic; the exogenous growth deals in each case with very different structural plans.

In a *Selaginella* the stem contains usually more than one vascular bundle. These bundles are more or less elliptical in section, and are developed independently of the leaves, though having connections with the bundles which proceed to them. The vessels in these bundles are developed from without inwards, and each bundle is surrounded with more or less delicate and usually but slightly elongated cells, constituting the "Phloëm."[2] In a *Lycopodium* these flattened bundles are usually more numerous and more closely approxi-

[1] But Professor Williamson informs me that he has done this in a memoir which is now in the press.

[2] "Phloëm" and "Xylem," are the outer and inner portions of a fibro-vascular bundle; they are separated in Dicotyledons by the cambium. These delicate investing cells, therefore, strictly speaking, belong to the bundles.

mated, forming a distinct central axis, the Phloëm-investment of each coalescing with those of the rest, and surrounding the whole axis, as well as being interposed between the different bundles. In some instances, *e. g. L. clavatum*, the bundles at the top and bottom of the parallel series, seen in a cross section, are curved irregularly, and partly fused with those next to them. This may be regarded as exhibiting a tendency to a condensation of the vascular bundles, which may be what actually has taken place in *Psilotum triquetrum*, where there is a central pentangular vascular axis, the centre of which is occupied with cellular tissue, which, on that hypothesis, would be composed in part of the cut-off Phloëm-investment of the bundles. The whole of the complicated vascular axis of *Lycopodium* is, at any rate, homologous with the more simple one of *Psilotum*, and it is therefore evident that in the structure of the central axis there is as much, or even more, difference in the recent Lycopodiaceæ than in the extinct.[1] *Psilotum* amongst existing plants appears to possess a vascular axis which approaches most nearly to that of the Lepidodendroid and Sigillarioid types, but it has nothing corresponding to the radiated cylinder. It is this region of the stem which exhibits most conspicuously "exogenous" growths, and it should be borne in mind that portions of the same plant of varying ages would have, in consequence of this growth, very different appearances, so much so, indeed, that they may quite probably be described as belonging to distinct species. Professor McNab considered, if I understand him rightly, that the radiated cylinder corresponds to the thickened prosenchymatous cells which are found in recent Lycopodiums surrounding the Phloëm zone of the central axis, and forming part of what I have called above for convenience the cortical region, but which is, of course, principally composed of the primitive tissues of the stem.[2] Formerly I was disposed to agree with Mr. Carruthers that this radiated cylinder belonged rather to the vascular axis, and for the following reasons:—Firstly, it is closely applied to the vascular axis; secondly, it consists of scalariform vessels smaller in diameter than those of the axis proper, and the most external vessels of the vascular axis of recent Lycopodiaceæ exhibit the same difference in size; thirdly,

[1] I might add that such differences exist even within the genus *Lycopodium* itself. Of two species figured by Brongniant ('Hist. des véget. foss.,' t. ii, pl. 10), one *L. Phlegmaria* (fig. 1) has the vascular bundles of the central axis quite irregularly disposed in cellular tissue, the other *L. curvatum* (fig. 3) has them condensed into a central rod.

[2] 'Nature,' 1871, p. 427.

it is vascular, and not prosenchymatous. On the other hand, it must be admitted that the vascular tissue in the bundles of existing Lycopodiaceæ is developed from without inwards, and there appears to be no tendency for them to receive additions in the opposite direction. *Isoetes*, indeed, seems to be a case to the contrary, but the structure of its central axis, which, according to Hofmeister, receives external additions, is many respects so anomalous that it cannot be very far relied upon. Supposing these difficulties removed, the continuous increase of the radiated cylinder by the multiplication of cells in a "cambium" region, formed by the adjacent portions of the primitive stem tissues, would be a simple and satisfactory explanation.

[Since what is above stated has been in type Hegelmeier's memoir "On the Morphology of Lycopodium" ('Bot. Zeit.,' 1872, Nos. 44—48) has reached me. He differs in many respects from previous writers and describes an investment to the central axis of delicate and narrow cells external to the Phloëm, which he terms the Phloëm-sheath (l. c., p. 776). Professor M'Nab has suggested ('Nature,' Feb. 6, 1873, p. 267) that it was by the multiplication of the cells of this sheath that the "exogenous" growth took place in fossil representatives. I think there is good reason for believing that this was the case.]

Researches *on* Bacteria.[1] By Dr. Ferdinand Cohn.

(With Plate V.)

Twenty years ago Cohn published his first researches on Bacteria. In the interim these organs have attained special importance in Pasteur's theory of fermentations and Hallier's theory of contagious disease; further, they have been the objective point of certain supposed demonstrations of spontaneous generation. Leuwenhoek and O. F. Müller already knew Bacteria, but Ehrenberg first distinguished the genera *Bacterium*, *Vibrio*, *Spirochæta*, and *Spirillum*. Dujardin added somewhat to the systematic knowledge of these forms; but since his time great confusion has been introduced by the vague use of names and introduction of new terms. Especially Pasteur, whose researches are greatly lessened in value by want of knowledge, on his part, of the terminology and methods of biology, has made confusion.

[1] 'Beiträge zur Biologie der Pflanzen,' 2tes Heft, Breslau, 1872.

He speaks with "sovereign will" of "végétaux cryptogames microscopiques," "Animacules," "Champignons," "Infusoires," "Torulacées, Bacteries, Vibrioniens, Monades," occasionally introducing the varied and undefined names, "Mycoderma," "Mucor," "Mucedinées," and "levure." Then we have from other writers the terms Microzyma, Bacteridium, Micrococcus, Leptothrix, Mycothrix, Microsporon, Mikrobacterien, Meso-mikrobacterien, Zooglæa, Microsphæra, and Amylobacter.

Cohn defines Bacteria as "chlorophyll-free cells of spherical, oblong, or cylindrical form, sometimes twisted or bent, which multiply themselves exclusively by transverse division, and occur either isolated or in cell-families." Bacteria make fluids milky; but if the fluid is of nearly equal refractive index with them—such as serum, lymph, &c.—they remain invisible, except with the microscope. They are not destroyed by potash, ammonia, nor acids, which is due to their *dense* cell-membrane, as is also their permanence when dead. They divide, by elongation, to double the normal length and subsequent pinching in, so as to form equal parts. Division only occurs longitudinally; branching is quite foreign to their nature. The cells produced by division separate at once (unicellular Bacteria), or remain attached as strings or threads (filamentous Bacteria). In this last state they resemble the alga genus (*Leptothrix*); but the bright green forms of true *Leptothrix* must not be confused with these Bacteria. In those distinguished as spherical and staff-shaped Bacteria, the cells resulting from division, as a rule, separate at once; but by the swelling up of their cell-membranes they may form a jelly-like mass or colony, which Cohn distinguished, in 1853, as the *Zooglæa* form. The filamentous and screw Bacteria never form jelly masses. Bacteria frequently form an oily stratum near the surface of a liquid (attracted by oxygen); this is Pasteur's "mucor." They occur in a third condition (a modification of the *Zooglæa*), as a toughish pellicle, in which the Bacteria are closely packed in rows; this is Pasteur's *Mycoderma*. A fourth condition is that of the pulverulent precipitate, which they form when they have exhausted the nutriment in a fluid. They are then to be regarded (as in the parallel case of yeast) as in a "resting phase."

Most Bacteria present a motile and a motionless condition. The movement is connected with the presence of oxygen. In certain filamentous Bacteria (Bacteridia, auct.) movement has never been observed. Cohn considers it at present undesirable to discuss the limits and transitions of natural species,

form-species, physiological varieties, and races, but indicates a variety of forms which appear to have a certain independence. He distinguishes four tribes, viz. 1, Sphærobacteria; 2, Microbacteria; 3, Desmobacteria; 4, Spirobacteria. In Sphærobacteria we have one genus, *Micrococcus*, with species grouped as Zymogenous, Chromogenous, and Pathogenous. Zymogenous (fermentative) are the common *M. crepusculum* (*Monas crepusculum*, Ehr.); *M. candidus*, Cohn; and *M. ureæ*, Cohn (the ferment of ammoniacal putrescence). Chromogenous are a very interesting series of colour-producing ferments, viz. *Micrococcus prodigiosus* (Monas prodigiosa, Ehr., the cause of blood-stained bread, &c.); *M. luteus*, Schrœter (in 'Beiträge zur Biologie der Pflanzen,' Breslau, 1872); *M. aurantiacus*, Schr.; *M. chlorinus*, Schr.; *M. cyaneus*, Schr.; *M. violaceus*, Schr. Pathogenous are—*M. vaccinæ*, Cohn; *M. diphthericus*, Dertel; *M. septicus*, Klebs; *M. bombycis*, Béchamp. This latter group is of the very highest importance, and excessively difficult to study. Other forms are supposed to exist, but have not yet been examined. The second tribe, Microbacteria, also includes but one genus, viz. *Bacterium*, with the species *B. termo* (the common Bacterium of putrefaction), *Ehr.*, 1830, Duj.; *B. lineola*, Ehr., a larger species, common in brooks and open ponds. Then we have also *B. xanthinum*, Schröt.; and *B. syncyanum*, Schröt., as chromogenous forms, and the interesting *B. œruginosum*, Schröt., the ferment of blue-green pus. The filamentous Bacteria, Desmobacteria, include *Bacillus* and *Vibrio* (newly defined); in the former the filament is straight, in the latter undulated. *Bacillus* has three species—*B. subtilis*, Ehr.; *B. ulna*, Kohn; and *B. anthracis*, Cohn. The first is the butyric ferment; the second is like it, but larger and coarser; the third is the pathogenous ferment of the diseases known as "the blood" and "malignant pustule." *Vibrio* has two species, one larger than the other, distinguished by Cohn as *V. rugula* and *V. serpens*. The Spirobacteria include two genera—*Spirochæte* and *Spirillum*—with species, *Sp. plicatilis* of the former, *Sp. tenue*, Ehr.; *Sp. undula*, Cohn; and *Sp. volutans*, Ehr., of the latter genus. Cohn remarks that *Sp. volutans* is the giant among Bacteria; he has detected a fine flagellum at each end of the screw. Ehrenberg appears to have seen this in his *Ophidomonas*. Frau Lüders asserted the occurrence of a flagellum at one extremity of the common *B. termo*, but no one has confirmed her. As regards this and other details, Cohn considers that our present microscopic appliances are unsatisfactory; he looks for further knowledge with an increased

magnifying power. The chief forms above mentioned are figured in a well-executed plate, which must furnish the basis for all future nomenclature in treating of these highly important and paradoxical organisms, and which we have accordingly reproduced (see Pl. V, and explanation.)

The Bacteria seem all to belong to the vegetable kingdom, since they exhibit direct and near affinities with undoubted Algæ. The have nothing to do with the fungi with which they often make their appearance. Cohn quotes and confirms Burdon Sanderson's important discovery of the method of separating Bacteria from Torulæ, due to the fact that fungi-spores are air-carried, while Bacteria require a surface or water to transport them. They are classed in the Phycochromaceæ, near the Oscillariæ and Nostocaceæ.

Though the term putrefaction may be considered as applying to a process occurring in nitrogenous organic matters, similar to the alcoholic fermentation of nitrogen free organic bodies such as sugar, and though Bacteria may be looked on as the active agents in the one just as Torulæ are in the other case yet that Bacteria are dependent for their life on highly organized or albuminoid nitrogenous matters is not found to be a fact. Sanderson found that Bacteria multiply in Pasteur's fluid, and used it as a "test-liquid," to ascertain the presence of living Bacteria or their germs in other liquids and substances. Cohn finds that Pasteur's fluid is better in this case when the sugar is left out from it. His researches on the nutrition of Bacteria relate only to *B. termo*. Although the sugar is superfluous, the ash-salts in Pasteur's fluid exert a very striking effect. A 1 per cent. solution of ammonium tartrate alone fails to support Bacterium life; but to the same solution add the proportion of yeast-ash salts, and at once the growth proceeds. Cohn shows, from his experiments, that the ammonia is the source of the nitrogen, whilst the tartaric acid is the source of the carbon for the multiplying Bacteria. Succinic, acetic, and lactic acid, sugar, glycerine, cellulose, but not carbonic acid, may become the source of carbon. Urea and probably nitric acid may replace the ammonia as sources of nitrogen. The Bacteria then (*B. termo*) resemble green plants in taking up their nitrogen from ammonia compounds, which animals are unable to do. They differ from green plants in not being able to take their carbon from carbonic acid, but requiring carbo-hydrates and their derivatives.

Putrescence is not a spontaneous re-arrangement of the molecules of a substance that has lived, following upon the removal of life, nor is putrescence the result of a spontaneous

combination of these molecules with the oxygen of the atmosphere. Putrescence is rather a chemical process excited by the growth of *Bacterium termo*. Putrescence never arises when the access of Bacteria to a putrescible body is prevented, and when those already present have been destroyed. In fact, putrescence is a correlative, not of death, as one sometimes thinks, but of life. The relation of Bacteria to the putrescence of albuminoid matters may be one of these four:— 1. The Bacteria may assimilate these substances, as animals do, and break them up into waste products in their own substance. 2. The Bacteria may shed out a peculiar substance, comparable to diastase or pancreatin, which acts so as to break up albuminoids. 3. Or the Bacteria may act as oxygen carriers, and so break up the albuminoids (oxydising ferments. 4. Or as oxygen-stealers, seizing the oxygen of the albuminoids, and so pulling down their complex fabric (reducing ferments). There is *no doubt* that, as with *all* protoplasm, whether vegetable or animal, so with that of Bacteria, repair and waste only go on in the presence of oxygen and with excretion of carbonic acid. In the presence of atmospheric oxygen these processes go on rapidly in Bacteria, but by no means does it follow that their putrefactive action is greater; in fact, just as the yeast-fungus reproduces itself less abundantly, but acts more powerfully as a ferment, *i. e.* converts more sugar into alcohol when growing in the absence of atmospheric air, so may it be with Bacteria. The oxygen has to be got elsewhere than from the atmosphere, and accordingly from the breaking up of organic molecules. Pasteur has already shown that the organisms of the butyric fermentation (Bacillus) multiply in the absence of atmospheric oxygen, and are checked by its presence.

The peculiar products of the fermentative action of Bacteria are, however, by no means to be regarded as due to oxydizing or deoxydizing actions effected on albuminoid bodies attacked by them, for this good reason, that these peculiar products make their appearance when Bacteria are nourished only with tartrate of ammonium and ash-salts. Cohn shows that the peculiar *smells* of putrescence, indicating the special products of saprogenous Bacteria, are produced under these circumstances. Further, the peculiar colours of the chromogenous Bacteria are abundantly produced in culture experiments of the same kind, *i. e.* with ammonium tartrate or acetate and yeast ash-salts, and he would infer the same for the peculiar products of pathogenous Bacteria. Hence the internal chemical phenomena of nutrition are the same, whether albuminoid or ammoniacal

food is supplied to the Bacteria; and since in the cases of green plants and of animals we have no analogy for supposing the Bacteria to be capable of assimilating at one time ammoniacal salts and at another time organic albumen, we must suppose that the Bacteria *always* take their nitrogen in an ammoniacal form. We have the preliminary reduction of the albuminoid to an equivalency with the ammoniacal salt to explain. In this consists the essential feature of putrescence—in a splitting of the albuminoid molecule into ammonia and other fluid and gaseous by-products—just as alcoholic fermentation (we want an English equivalent for Gährung) consists in the splitting of sugar into alcohol and carbonic acid. Dr. Cohn, in this part of his paper, is a little inconsistent, for he shows that in certain pigment Bacteria the pigment is insoluble, and is contained in the Bacterian cells. This leads to the inference that the peculiar saprogenous, chromogenous, and pathogenous products of all Bacterian growths are evolved thus internally. But in speaking of the splitting of the albuminoid molecule into ammonia and by-products as a necessary external work of the Bacteria preceding their assimilation of the nitrogen of the ammonia, he says that the by-products of the splitting may be the very stinking, coloured, and poisonous products which he has before given proof are formed as a result of the assimilation of simple ammonia salts and of subsequent changes in the protoplasm itself of the Bacteria. The by-products of the preliminary breaking-down of the albuminoid molecule cannot, it seems, be identical with the special products of the Bacterium's life, but may be less peculiar gaseous and other bodies, and consequently this external part of the action of the *Bacterium* may be compared to the breaking up of sugar by *Saccharomyces,* whilst the Bacterium's internal work (specific products) has no recognised parallel in the case of yeast.

How can we, then, figure to ourselves the power of the Bacteria to split up albuminoid molecules? Is it a direct function of their vegetation-processes, comparable to the breaking up of carbonic acid in the leaves of green plants under the influence of light? Do the Bacteria act thus "catalytically" as mere excitors or communicators of molecular vibrations, quite innocent of any chemical transference of matter? Or do they secrete a fluid capable of acting on albuminoids, just as the cells of the alimentary tract secrete digestive juices? Or is it by the abstraction of oxygen that they upset the albuminoid molecule, as the analogy of yeast suggests? Or, again, is it by bringing condensed oxygen to bear against the complex atom-fabric? These questions are not yet answered.

But Pasteur's recent generalisation as to the fermentative function of all living cells tends to the third hypothesis.

It is exceedingly interesting to note that the disease, "the blood," caused by *Bacillus anthracis*, is accompanied by all the symptoms of oxygen-starvation and CO_2 superloading, and resembles in this way entirely prussic acid poisoning. Pathogenous Bacteria may further, in some cases, produce disease and death by merely diverting nutriment to themselves which should build up the diseased organism. Thirdly, they may produce specific products (comparable to the colour-products of chromogenous forms), which act as diffusible poisons; such is the supposed poison "septicin."

The question of the real distinctness and convertibility of the forms and physiologically peculiar species of Bacteria is wisely not yet discussed by Dr. Cohn. But he makes the very important remark that it is only in consequence of the recognition of the complete distinctness of pathogenous Bacteria from the commoner Bacteria of putrescence, &c., that the question of contagion has made any advance. So far from being convertible forms or conditions, pathogenous Bacteria are destroyed by the activity of the regular putrefactive Bacteria (*B. termo*).

Cohn, in a large series of experiments, has proved that, practically, a temperature of 80° C. destroys the life of Bacteria, and prevents their development in an organic infusion. Between 65° C. and 80° C. he obtained varying results, which he attributes to the spluttering of the contents of the heated vessels and to the protective influence of solid lumps. In such fluids as Pasteur's a careful heating to 62° C. for one hour was sufficient to prevent development of Bacteria and putrescence. Some doubt seems, however, still to exist as to the butyric ferment—*Bacillus subtilis*—which in decoctions made with a pea in a tube with ten cub. cent. of water made its appearance sometimes after exposure to temperatures from 60—100° C., the tubes having been closed. Cohn considers that the solid mass of the pea here had effect as a protective, and proves this by a differential experiment; also that *Bacillus* can withstand higher temperature than can *Bacterium*. Further, he found that after long boiling no *Bacillus* nor any Bacteria at all make their appearance. It is this *Bacillus* which Pasteur believes he obtained in milk after exposure to 105° C.

Experiments on the effect of low temperatures have given Cohn the result that Bacteria are not killed by long exposure to cold below 0° C., but they become torpid, cease movement, growth, and fermentative action, recovering,

however, these activities with the return of a higher temperature. Dr. Cohn by a mistake cites Professor Frankland, together with Dr. Bastian, as entering the lists on behalf of the "generatio equivoca," and observes that so talented and exact a reasoner as Pasteur has no easy task in dealing with the French heterogenists. This is, he says, due not only to the illogical conclusions and the bad experimentation of the supporters of *generatio æquivoca*, but because there are still, in fact, certain conditions relating to Bacteria which are not fully understood, and which, though he is persuaded they do not directly affect the question at issue, yet render it possible to understand how contradictory statements arise. These conditions are those above noted as to the spluttering of the fluid[1] and the protective action of lumps in experiments where putrescible fluids are heated.

On a NEW ALGA, CRENOTHRIX POLYSPORA (Cohn), *from the* WELL-WATER *of* BRESLAU. By Dr. F. COHN. Translated in abstract by W. ARCHER, M.R.I.A.

WHILST the water-supply of many large towns, both at home and abroad, is, it is to be be feared, not everything that could be wished as regards purity, it is to be hoped that few are so badly off in that respect as, according to Cohn, the town of Breslau appears to be. The greater part of a paper in the first part of his 'Beiträge'[2] is taken up with an account of the microscopic analysis of samples from various public wells, which revealed all sorts of "abominations."

The sample in which the interesting new alga *Crenothrix* was first met with appears to have come from a part of the town which enjoyed the unenviable reputation of being a "Berüchtigte Typhusgegend," but as this water was full of many sorts of organic matter, living, dying, and dead, we may infer that the *Crenothrix* was not specially to be accounted a culpable agent in promoting the unhealthy character of the neighbourhood.

In this water, which was cloudy, owing to the quantity of Bacteria, Prof. Cohn noticed some little yellowish-brown flakes, of about 1—2 mm. in size, which soon settled to the

[1] Dr. Roberts has some remarks upon this source of error in a letter to 'Nature,' Feb. 20th, 1873, p. 302.

[2] Cohn: "Ueber den Brunnenfaden (*Crenothrix polyspora*), mit Bemerkungen über die Mikroskopische Analyse des Brunnenwassers," in 'Beiträge zur Biologie der Pflanzen,' Heft I, p. 108.

bottom, and combined there with others into larger flakes. These flakes were found to be composed of a number of colourless filaments of an alga, forming loosely contorted tufts, amongst which Bacteria, Vibriones, Amœbæ, and various Infusoria swarmed. The same alga was afterwards obtained from various wells, and Cohn supposes its habitat to be the bottom and sides of wells, whence pumping detaches and brings it up, for it has not occurred in any of the open waters, and its want of colour indicates its coming from places from which the light is excluded.

The filaments are long, of varying width, rigid and little curved, often mutually interlaced, and in structure they exhibit the characteristics of the Oscillaricæ, that is, each thread consists of a series of equivalent cells enclosed in a rigid sheath. The contents show no trace of phycochrome or of any colouring substance; in the larger filaments the cells appear as if hollow in the middle, from the contents forming a thickened parietal stratum, the interior being filled with a pellucid cell-sap. The sheath in the more slender filaments is extremely thin, but in the larger ones it offers a double contour and pergamentaceous consistence; the contents eventually become removed, leaving the sheath empty. Cohn holds that, doubtless, this sheath originates from the outermost cylindrical lamellæ of all the cells, whose individual portions, as in the formation of cuticle, become mutually fused; the sheath appears as if it sometimes became swollen up. At first the sheath is colourless; it may subsequently become yellow or brown, which is attributed by Cohn to the deposition of hydrated iron oxide.

On account of the great differences in the diameter of the threads, one might at first imagine it possible that many different species were mingled. But on closer examination it is seen that the width varies in one and the same filament; the filaments are, in fact, not cylindrical, but wider at one end than at the other. They often appear to be attached by the thinner end, and gradually to expand thence to the other or upper one. Equally variable is the length of the cells compared with their width, which is dependent, of course, on the transverse division of the cells having more or less recently taken place.

In many filaments the end-cell becomes by far longer than any of the others, and at the same time broader, and of an elongate ellipsoidal figure, calling to mind the spores of *Cylindrospermum*. Such an elongate end-cell, when it occurs, finally arrests the growth of the filament in the direction of its axis; the cell below it thereupon divides obliquely, grows out

sideways, subdivides, and forms a lateral branch (suggesting the mode of growth in Scytonemeæ). The enlarged end-cells are filled by a finely granular protoplasm (recalling the manubria of the Rivularieæ). The author had not been able directly to follow out any further development of these great spore-like bodies. He had seen, however, these end-joints empty, as if the contents had emerged through the perforated apex, and he thinks that certain colourless short filaments, with very delicate membrane, without sheaths, and with a peculiar slow gliding movement, which he met with in the material, may have originated from the large spore-like bodies and that these may, therefore, possibly have some place in the cycle of development of this plant.

But if the formation of these spore-like end-joints was a comparatively rare occurrence, another form of development was very general. This begins by the colourless contents of certain of the cells of both thick and thin filaments becoming retracted from the wall and condensed into round plasma-masses, and the filaments appear jointed in a moniliform manner. The further progress seems to follow two modifications, which the author distinguishes as macro- and microgonidia-formation.

The "microgonidia" are the most frequent, and seem to occur constantly in the thicker filaments. The cells of these increase first in width, and simultaneously become divided transversely, so that they form a series of discs, their length being scarcely more than a fourth of their width; these then divide vertically into four cuneate segments, and the latter, again, in a manner not readily followed out, repeatedly into a number of minute portions, designated by the author as microgonidia. This process begins first at the top of the filament, and gradually proceeds downwards. The individual gonidia are apparently primordial cells, which in arrangement originally corresponded to the joints of the filaments, but soon get disturbed, whilst the original septa are broken or resorbed. The sheath takes no share in this process, but forms a clavate "sporangium." The gonidia shove themselves forward, "somewhat like the zoospores of *Achlya*," but with a slowly gliding motion, to the apex, where they congregate in thousands, the hinder finally pushing out through the apex those in advance, the formation of more new gonidia going on all the while in the next deeper portion of the filament. The emerged gonidia have round or somewhat elongate figures; they show a central vacuole, but no membrane; their shorter diameter varies from about 1 to 3 m.m.m.; they are often twice as long, and

then constricted at the middle; many are transversely divided; they do not apparently move beyond a very slow rolling motion.

The formation of the "macrogonidia" seems almost never to occur in the same filament. It is distinguished by the fact that the cells subdivide into only two, or at most four portions (in the smallest threads they are formed in a single series), and the macrogonidia, accordingly, are larger (being from 3 to 5 m.m.m. in diam.); in other respects they behave alike, that is, they emerge slowly from the apex of the investing sheath. They are seen to be short cylindrical cells, with parietal protoplasm and watery cell-sap, and soon exhibit a transverse division.

In this manner, in the course of time, a mass of the *Crenothrix*-threads may emit the whole of their contents as gonidia into the surrounding water, where they lie in clusters, and, like the cells of *Palmella*, combine in masses by a common mucous matrix.

The further development of these gonidia Cohn was unable to follow, but he has no doubt that both forms of gonidia germinate into new *Crenothrix*-threads; he frequently found minute *Crenothrix*-threads which manifestly proceeded from the germinated gonidia; these occurred sometimes in bundles, and even though so minute began to show the characteristic widening towards the upper ends. There does not appear to be any difference between the "micro-" and "macrogonidia," beyond the circumstance that the former give rise to more slender filaments.

As to the systematic position of *Crenothrix*, Cohn considers it an alga, because of its close affinity to certain undoubted Algæ, though the want of colouring matter might seem to point to the Fungi, but on that ground the flowering plants *Monotropa* and *Lathræa* would be relegated to Fungi. The structure of the filaments in *Crenothrix* closely approaches Oscillarieæ, which, as a rule, contain phycochrome, but amongst them are found also colourless genera, especially *Beggiatoa* and *Spirochæte*; so far as *Hygrocrocis* represents a proper genus, the author would refer it also here. The author holds that the colourless Oscillarieæ live not only on inorganic but also on organic compounds (Hedwigia, 1865). In their mode of nutriment the colourless genera, *Beggiatoa*, *Spirochæte*, *Hygrocrocis*, *Crenothrix*, &c., seem to accord with the water-fungi, whilst their organization agrees with the phycochrome-containing Oscillarieæ. From these *Crenothrix* is distinguished by the unequal filaments, thickened towards the apices, by the subdivision of the cells in the direction of

the longitudinal axis, and, above all, by the numerous free gonidia densely filling the clavate "sporangia."

There is, however, another genus of the Oscillarieæ *Chamæsiphon*, Braun and Grunow, to which the author regards *Crenothrix* to be more nearly related; that genus forms short, erect, shortly jointed filaments, attached by one end to other Algæ, solitary or aggregated, each enclosed in a hyaline sheath of a narrow pyriform or clavate figure. The cells are produced by successive division, become rounded off, and emerge through the apex of the sheath as "resting spores," which germinate without "fecundation." From this *Crenothrix* is distinguished by the long filaments forming interwoven masses and by the numerous macro- and microgonidia, and it would appear to stand, as it were, between this genus and *Lyngbya*. In the latter Cohn thinks, from certain previous observations, "gonidia" may occur.

In a former memoir[1] Cohn had expressed the view that the phycochromaceous Algæ might be regarded as approximating to the Florideæ in a negative character evinced by both, that is, in the propagative cells being destitute of motile flagella, in contradistinction to the zoospores of Chlorosporeæ and Phaeosporeæ. *Crenothrix* affords a new connecting link between the two Classes, as it shows a considerable affinity to *Bangia*. In a marine species, *B. subæqualis*, Kütz., the sheath becomes transformed into a clavate sporangium, whilst the contents became transformed into round or ovate gonidia, which emerged from the sheath, and grew into new filaments, sometimes remaining attached by one end to the primary one. Solier and Derbes the author mentions as attributing to certain marine species of *Bangia* flagellate cells regarded as antherozoids,[2] but he thinks it probable these certain motions may have been confounded with monads or *Chytridium*-zoospores, and that they really show a great analogy with the microgonidia of *Crenothrix*. He regards, then, the genera *Oscillaria*, *Lyngbya*, *Crenothrix*, and *Bangia*, as forming a natural series connecting Oscillarieæ with Florideæ. But Cohn draws attention to the resemblance of the *Crenothrix*-gonidia to certain conditions of *Bacteria*. As is well known *Bacterium termo* occurs in certain stages as colourless mucous masses (*Zooglæa*, Cohn), from which the individual Bacteria emerge, by solution of the matrix, as free cells. Now, the author observed, in the water in which the *Crenothrix*

[1] Cohn: "Beiträge zur Physiologie der Phycochromaceen und Florideen," in 'Archiv für mikr. Anat.,' Bd. iii, t. ii.

[2] Solier and Derbes: "Mémoire sur la Phys. d. Algues" ('Supplément aux Comptes rendus de l'Acad. de Sci.,' Tome I, p. 64).

occurred, colourless mucous masses, containing minute bacillar cells (5·25 m.m.m. long and about one fourth that width), single or in pairs, with darkish granular contents. During observation he was surprised to see these cells begin to move with a revolving kind of motion; suddenly some swam out away from the mucus, and then returned; in a word, these little free cells showed all the characters of *Vibrio lineola*, Ehr., showing that that form passes through a *Zooglœa*-state, that is, a still, so to say, *Palmella*-like condition. Now, the isolated microgonidia of *Crenothrix* resemble certain of the larger *Bacterium*-cells, occurring in the same water, the more so as both were mostly met with constricted at the middle, that is, in various stages of transverse division, and combined together (in millions) by a common mucous matrix. Still, while the resemblance is surprising and the confounding of the one with the other easy, the author is disposed to think that there is really no genetic connection between the two forms.

On XANTHINE *and some of its* CRYSTALLINE COMPOUNDS. By B. WILLS RICHARDSON, F.R.C.S.I., Surgeon to the Adelaide Hospital, Dublin. (With Plate VI.)

FOR the opportunity of examining microscopically some of the salts of xanthine I am indebted to the kindness of Dr. Neubauer, of Wiesbaden, who sent me a specimen of xanthine from urine, as well as one prepared artificially from guanine. As urine, according to Dr. Neubauer, contains only about one gramme of xanthine in 600 pounds; my obligation to him is therefore not an inconsiderable one.

Xanthine itself, being amorphous, is not an attractive microscopic object. Its colour, when dry, is a light buff, but when mounted in either Canada balsam or gum dammar the semitransparent particles have a tint which I have imitated by darkening King's yellow with a little lamp-black. The larger particles do not transmit light.

The crystals of the hydrochlorate of xanthine represented in Pl. VI were prepared in the following way:—A few particles of the amorphous xanthine were put upon a glass slide placed upon a warmed iron plate; hot strong hydrochloric acid was then dropped upon them, and they were stirred together with a fine-pointed glass rod. They were allowed to cool slowly; and when, after the expiration of several days, evaporation was completed, and the crystals were perfectly

dry, they were mounted in dammar solution. An attempt to hasten the evaporation by heat ended in the disappearance of the crystals from another slide. When there is but little material to act upon, I advise that the above directions should be carefully followed.

The hydrochlorate, when successfully prepared, forms as Dr. Neubauer truly observes, "beautiful microscopic crystals," and it may be added, beautiful polariscopic objects also. Dr. Neubauer describes the crystals as "six-sided plates, lying together in groups and glandular masses," but it occasionally happens that only "round and egg-shaped forms"[1] are met with.

That they may further vary, however, in characters is evident by the drawings represented in Pl. VI, which have been carefully drawn to scale with Nachet's camera.

In some of the published illustrations of xanthine it is apparently the hydrochlorate, and not pure xanthine, that is represented, xanthine, as I have stated, being amorphous, and not crystalline.

Researches *on* Bone *and* Cartilage. By C. Heitzmann. (With Plates VII and VIII.)[2]

The Cells of Normal Bone.

If the lower extremity of the femur of a freshly killed young rabbit be freed from its attached muscles, then cut through with bone-nippers about one inch above the knee-joint, the joint opened and the condyles of the femur separated, we have a good object for the study of the histological relations both of fresh articular cartilage and of the spongy bone of the epiphysis. The bone here is easy to cut, so that vertical or surface sections may be at once prepared of considerable size, and thin enough to bear the highest magnifying power.

If a bony lamella, on such a section, mounted in $\frac{1}{2}$ p. c. salt solution, or, better, in diluted Müller's Fluid, be observed with Hartnack's No. 10 immersion lens, the bone-cells may be seen as dull-grey, roundish or oblong bodies in the shining matrix, which is traversed in many directions by clear branched canals. In the interior of the indistinctly

[1] 'A Guide to the Qualitative and Quantitative Analysis of the Urine.' By C. Neubauer and J. Vogel. The New Sydenham Society. London, 1863, p. 23.

[2] Translated from 'Medizinische Jahrbücher,' 1872, part 4.

spotted cell-body there may be often recognised a structure resembling a nucleus, bounded by a slightly indented outline. In contact with the outline of the cell itself there is a narrow, clearer zone, into which numerous conical processes project from the shell and have precisely the same aspect.

In many places we can follow the extremely delicate branched processes for a short distance into the matrix and see them freely communicating with the process of a neighbouring cell. When the processes cannot be followed far from the cell the above-mentioned clear branched canals of the matrix are seen in immediate continuity with them.

At the border of the bony lamella we may frequently notice a number of projecting short serrations of the same aspect as the cell-processes. In the same position there are commonly protoplasmic masses, with pale oblong nuclei, into which the processes from the lamella penetrate.

These processes, with their communications, may be very clearly seen in gold preparations, in which the dark violet bone-cells stand out sharply in the pale violet matrix. The processes may be equally well seen in preparations of bone from which the calcareous salts have been extracted by lactic acid. By this method the cells appear not to shrink, at least the space between them and the matrix is not broader than in fresh preparations.

In chromic acid preparations the cell appears somewhat shrunken, and the cavity enclosed by the matrix is seen to be richly provided with canalicular processes (Pl. VII, fig. 1). Most of the conical, bristle-like cell processes end in sharp points at the commencement of the canal, and it is only in a few of these that we can follow an interrupted granular substance of the same aspect as the granular cell-body.

In all cases, then, we may convince ourselves that in normal bone the cells have processes which partly extend into the canalicular system and partly communicate with each other, but the arrangement of the processes is only made clear when the bone is artificially inflamed.

Alterations of Bone in Inflammation.

Considerable changes are produced in bone as a consequence of inflammation, and, as we know from Virchow,[1] these changes take place both in the bone-cells and in the matrix.

I will first describe the alterations of the matrix as seen in a preparation taken from the scapula of a young adult dog

[1] "Ueber parenchymatöse Entzündung," 'V. u. R. Arch.,' iv, 1852.

four days after a piece had been broken out from the centre and inflammation thus set up. The injured scapula was softened in diluted chromic acid.

The section was made horizontally in the immediate neighbourhood of the wound, in the layer next to the periosteum, and deep enough to include bony lamellæ. On examining a portion of this preparation we see two kinds of formation in the same place, one in the form of passages with roundish or oblong dilatations recognisable by the abundance of large cells of variable shape, while the other is characterised by a compact structure and regular distribution of the cells. The former is osteoid tissue, the latter proper bone-substance. The boundary between the two tissues is only sharply defined here and there, in other parts so obscurely that there is a gradual transition from parts rich in cells to others in which they are comparatively scanty. The parts rich in cells are also characterised by a somewhat bright and clear matrix. In the proper bone-substance we also find dilated portions, which are sharply defined from the unaltered matrix. It has been long known that these portions are formed during life by solution and removal of the calcareous salts.

In the part figured we may recognise that the solution of the earthy salts takes place in some parts uniformly for a considerable extent, in other parts in irregular areas. These latter correspond partly to the boundaries of the cell-territories and are partly independent of them.

This poverty in earthy salts may be recognised in chromic acid preparations, even with the highest powers, by the clearness of the matrix, the blurred appearance of the boundaries of the lamellæ, and the indistinct demarcation of the lacunæ and canaliculi.

From all my preparations we must conclude that more important alterations in the cell-body are not demonstrable until the earthy salts have been, at least partially, removed from the matrix.

It is difficult to give a universally applicable description of the alterations in form of the bone-cells in an inflamed part. In bones that have been inflamed for several days we meet with portions in which all the cells of a lamellar system are intensely and uniformly altered in shape; but close by we find other parts in which no alteration can be noticed, or in a certain part some cells greatly altered, others slightly, and others not at all. One of the earliest demonstrable alterations is *swelling* of the cell and great distinctness of the processes (Pl. VII, fig. 2).

Further we find cells whose nuclei are hour-glass shaped, others with a mark of division, then others divided or broken up into several portions. These forms of division are particularly well seen in oblong cells which have a nucleus at each pole, and where each nucleus has again divided as above described. *At the same time we notice in the protoplasm homogeneous, peculiar, dimly lustrous corpuscles.* The cell-body itself is always enlarged when division of the nucleus has commenced, and its outline more sharply defined. It is common for a mark of division to pass through the middle of a cell; sometimes a nuclear structure is present in each half, or we see only a dark spot when the protoplasm is peculiarly bright. One half of a cell may now and again show another mark of division (Pl. VII, fig. 3),[1] or, finally, the enlarged cell-body is split into a number of small lumps, the lacunæ becoming increased in size.

I have given only a short description of these alterations, because a full and accurate one has already been given by E. Lang.[2] They may be followed in every inflamed bone for several millimètres from the focus of inflammation, whether long bones or scapulæ be used, and whether the inflammation be brought about by subcutaneous fracture (I have injured tibiæ and fibulæ of dogs in this way), or by cutting out or breaking out a piece of the scapula, or, finally, by rubbing or perforating a bone with the actual cautery—in my case tibia or scapula.

Before I go further in the description of the forms of inflamed bone-cells, I will define the appearances of inflammation in the cells of the osteoid tissue of the scapula, first, because the alterations here are already considerable in the first days of inflammation, and, secondly, because these cells, so far as size is concerned, show the changes in a more striking manner than those of bone.

On the fourth day of inflammation we find, in the uniformly clear matrix, which has lost its lime-salts, large districts with cells of the appearance here described. The lacunæ are enlarged, and occupied partly by roundish and angular finely granular lumps, with roundish nuclear bodies, partly by round, oval, or irregularly nodular bodies, which are conspicuous by their lustre and homogeneous aspect. Most of the cell-processes are invisible, there being only a few broader passages, which connect the lacunæ with each other and contain processes.

[1] I may mention that in inflamed cells treated with chloride of gold, the network of communicating processes is shown of a deep violet colour.

[2] 'Mediz. Jahrbücher,' 1871.

In other places large lumps are found immediately over long lamellæ, which, owing to their position, must be regarded as derivatives of osteoid cells.

In preparations which I obtained from deeper layers, from the "true" bone of the same scapula, very large dilated spaces can be seen filled with cellular structures, some of which are sharply defined from the lamellar systems; while in other parts bone, which is still distinctly to be recognised as such by its matrix, passes gradually with visible increase of its cells into spaces which contain only cells, but no matrix. Close to the wound only a few small remains of bony lamellæ can be found; the whole tissue consists only of round or spindle-shaped cells, or of such cells forming a network. The matrix of the bone has here plainly been entirely lost and replaced by cells.

How have these new spaces arisen?

Certainly, in part, from Haversian canals. At a little distance from the margin of the wound dilated Haversian canals may be seen, which indicate the commencement of lacunar spaces. The nearer we approach to the margin of the wound the wider and more irregular are the Haversian canals, which here and there still show, in their centre, blood-vessels filled with blood.

From the dilated Haversian canals tapering passages proceed filled with cellular elements between the separate lamellar systems. Then come what may rather be called islands of bone, and finally these also disappear.

But there is also a second process, which leads to the formation of cavities by increase in size of the bone-cells. I possess most beautiful and convincing specimens from the compact bone of the tibia of an adult dog, burnt with the actual cautery eight days before death. There may be seen here, in a rather thick part of the preparation, three bone-cells, which, having become enlarged in all dimensions, have thus approached nearer to each other, and the matrix between them is lost. We have here, therefore, a closed space arising in bone-tissue filled with three cells; above and below this space I can see, by focussing, slightly enlarged cells lying imbedded in the matrix. I can, therefore, confirm what Rokitansky[1] has already stated, that spaces arise in bone independently of Haversian canals, and filled with structures, which can only be derivatives of bone-cells. The result of the above processes may be shortly represented by saying that compact bone has become converted into spongy bone.

Both the isolated spaces and the marginal layers of the

[1] 'Lehrb. d. Pathol. Anat.,' ii, 1856.

bony lamellæ, which are in course of deliquescence, yield the most excellent objects for the study of that peculiar alteration of the bone-cells, with which we have already become acquainted in osteoid tissue, and on which I especially insist, because these changes, as will be shown later, are preliminary to the formation of blood. While the cells are still imbedded in deliquescing bone we find that they have increased in size, that their aspect is in part lustrous and homogeneous, and partly coarsely granular, that they have become converted into roundish or spindle-shaped elements, which, immediately after the matrix between them has disappeared, become fused together in long tracts, or form a network in whose meshes there lie large, pale, finely-granular bodies, of irregular outline, occasionally furnished with a nuclear structure, or sometimes roundish, lustrous, and homogeneous bodies.

In these parts, therefore, we can very early distinguish a separation between two kinds of substances, which are both to be regarded as derivatives of bone-cells, as their development can be followed, step by step, from these latter in closed spaces and in all stages of transition.

The greater part of the inflamed bones which I have examined have shown these structures, and always in a more striking manner the nearer the part examined was to the wound. At some distance from this, and in the first stages of inflammation, there are other appearances, which have no relation to the formation of blood, but, as I shall show later on, to ossification.

Close to the wound in the femoral condyle of a rabbit, which was perforated with the actual cautery twenty-six hours before death, the medullary spaces appear greatly enlarged, and the bony trabeculæ encroached upon and narrowed. In these latter I can find, close to the margin of the medullary space, territories which, if they originally possessed only a single cell, show in their centre a cell surrounded by a finely granular mass in place of bone, *i. e.* matrix without earthy salts, in which the processes of the cell may be recognised as finely granular radiating markings. Let us call the contents of such a territory the osteoblast zone. Territories which originally contained several cells show a corresponding number of osteoblast zones. In the immediate neighbourhood of these territories there are others, in whose osteoblast zones the difference between the central cell and the finely granular mass which surrounds it, is not so sharply defined. Again, in other territories the contents of an osteoblast zone are seen to be changed into a lump provided with a nuclear structure,

or sometimes several such lumps in a territory are fused together into a multinuclear mass.

Such portions which have lost their matrix become united to form groups of territories; finally, by the confluence of several such groups, large protoplastic layers arise, some with numerous oval nuclei, and others in which no nuclei can be seen. These layers of protoplasm frequently appear as if split up into smaller portions. In many places I have found them in the form of broad passages extending from one space to another, and presenting no traces of nuclei. At the same time we find in the pale, finely or coarsely granular protoplasm, yellow, strongly refracting granules of various size, or groups of such granules, or homogeneous lustrous lumps have arisen, which have a demonstrable connection with the formation of blood.

Wherever deliquescence of bone-substance takes place as a consequence of inflammation, a differentiation between the above described two kinds of substance may be observed, only sometimes the finely granular, pale (bone-forming) protoplasm predominates, and at others the homogeneous (blood-forming) protoplasm is in excess.

In recapitulating what we have described as taking place in inflamed bone, we may make the following propositions:

The process of solution and absorption of the earthy salts starts chiefly from the Haversian canals. The portions which have lost their salts have sharply-defined boundaries, which commonly correspond to those of the territories of the bone-cells, but are also often independent from them.

As a consequence of enlargement of the cell-body and division of its nuclei a complete deliquescence of the matrix takes place, both at the margins of the Haversian canals and in the midst of bone, independently of them. This process leads to the formation of spaces.

In the enlarged and now free bone-cells a differentiation takes place into a yellow, homogeneous, lustrous substance and a colourless finely granular substance.

On the Formation of Blood in Inflamed Bone.

In preparations taken from a dog's tibia, tangentially burnt with the actual cautery so as to leave the medullary cavity unopened eight days before death, I found, in the middle of the uninjured bone, large numbers of lacunæ, which, in addition to pale, finely granular lumps of protoplasm, contained a variable quantity of completely formed coloured blood-corpuscles (Pl. VII, figs. 4 and 5). This gives

cause for suspecting that the blood-corpuscles have been formed from the bone-cells.

I next tried to find transition forms which might show that, so far as can be concluded from the occurrence of the forms together, there was really a development of such structures. There was no lack of convincing appearances in these preparations; one of them is shown in Pl. VII, fig. 6. We see here in the lacunæ the variable forms of a substance characterised by its homogeneity, bright lustre, and yellow colour, which sometimes occur as minute bands at the margin of the pale granular protoplasm, sometimes as lumps, commonly furnished with minute sharp processes, and possessing a very dark but single outline; then we may find grey lustrous discs without a sharply defined outline, and finally bodies having the aspect of coloured corpuscles, with a distinct double outline,[1] and a cup-shaped depression on each surface, giving to these bodies, when standing on edge, the well-known hour-glass shape. This substance often occurs in the form of masses, which appear to be composed by the aggregation of coarse granules, and consequently have a cluster-like appearance; we also find it in the absoption spaces as a network with swellings at the nodular points. I must also add to the above described forms thin plates, pierced by small vacuoles, which I have very commonly found in chromic acid preparations.

Now, it can be shown that the same structures occur in blood-vessels, and in so convincing a manner that there can be no doubt of their identity with those of the lacunæ.

The fact that we must without hesitation refer those elements which are found inside blood-vessels to blood-corpuscles, and the further fact that we now have to do with blood-vessels in an early stage of development, and that in them bodies are found, which, indeed, are not yet complete coloured blood-corpuscles, but because of their yellow colour, on the one hand, and because of their protoplasmic appearance on the other, may be viewed as intermediate stages between colourless and coloured corpuscles; these considerations, I say, justify us in assuming that we see before us in the young vessel developmental stages of coloured blood-corpuscles; but we are also justified in saying that the analogous bodies found in the lacunæ are structures of precisely the same kind.

We have evidently before us a conversion of protoplasm

[1] I may remark that the bone from which the preparations were made was softened in chromic acid.

such as was found by W. H. Carmalt and S. Stricker,[1] in elements of the inflamed cornea of the frog and rabbit.

As I shall still frequently have to speak of the above described structures, I will, in order to avoid repetition, give them the name of hæmatoblasts.

It is not easy to decide from what point of the cell-body the conversion into hæmatoblastic substance takes place. I sometimes find a central part with a yellow lustre and serrated; at other times there are yellow lustrous lumps at an excentric part of the cell-body; and, again, the cortical layer of the cell may be converted into this substance. Then it happens that, together with this substance, we find only unimportant remains of the pale finely-granular protoplasm. Finally, sometimes the whole cell-body has taken on the peculiar homogeneous aspect.

A transformation of this kind may occur, not only in the body, but also in its processes. We may find round lumps of the above described appearance lying in a dilated canaliculus.

How early does the development of this substance begin? The question is difficult to answer. I have clearly recognised it in bone-cells that have become fused together after an inflammation that has lasted twenty-six hours, and I shall show later that it is regularly formed in certain parts of normal bone.

Let us now compare young blood, both fresh, and after treatment, with various colouring and hardening reagents.

In the fresh, living state—I have given instructions for making such preparations at the beginning of this paper—we see hæmatoblasts of all forms, resembling those in preparations hardened in chromic acid and wood vinegar. I can, therefore, say that these two reagents do not markedly alter the hæmatoblastic substance. Solution of nitrate of silver leaves them uncoloured. I have tried $\frac{1}{2}$ per cent. solution of chloride of gold, both with fresh and with chromic acid preparations, the latter being first thoroughly dehydrated, and in both cases I got the same result. The grey, dimly lustrous discs, after treatment with the solution for ten minutes, and examination some days later, were of a deep violet colour. Thicker masses were coloured violet, but the yellow lustre was also preserved.

The hæmatoblasts are not soluble in oil of turpentine. Thick masses, after treatment with alcohol and turpentine, remain perfectly recognisable; the large vacuolated discs

[1] 'Med. Jahrb.,' 1871.

are so cleared up that they can no longer be made out, only the pale thickened marginal layer remaining distinct.

I have arrived at essentially the same result as Rokitansky,[1] who has shown that in certain pathological processes blood is formed anew in mother-cells which are becoming joined by branching to capillaries.

Structure of Hyaline Cartilage.

Fresh articular cartilage is well known to be an object well adapted to the study of its histological relations. If a fine section mounted in $\frac{1}{2}$ per cent. salt-solution be examined with No. 10 immersion lens, a number of details may be recognised which have hitherto remained unnoticed. The following description is based upon a horizontal section from the femoral condyle of a young adult dog; the relations are precisely the same in the corresponding cartilage of the cat and the rabbit.

The cell-bodies appear finely and dimly granular, bounded by a rather denser cortical layer. If the outline of the cartilage-cell be accurately focussed, there comes into view, between it and the matrix, a clear, very narrow band, traversed by a great number of extremely fine, radially disposed, grey processes or stripes. These are all conical, the broader basis of the process proceeding from the cell-body and its fine extremity being directed to the matrix. Where two cells lie near each other, the clear band between them is traversed by delicate, grey streaks.

In fresh cartilage-cells the nucleus may be sometimes distinctly made out, and we can then see that its form corresponds to that of the cell-body. In its finely granular interior we find the brightly refracting nucleolus. Surrounding the nucleus is a clear, narrow band, again traversed by radiating, conical processes, whose bases are connected with the nucleus, their extremities being lost in the protoplasm of the cell. These delicate, conical processes are only distinct when the clear band which surrounds the nucleus is plainly seen.

If we examine the matrix we see a very faint, apparently granular marking, dark parts alternating with clear, and here and there the clearer parts appear branched, and even as if they formed a delicate network.

Having learnt that processes are given off from cartilage-cells, and that the matrix has an indistinct reticular marking; bearing in mind, also, the appearances presented

[1] 'Handbuch. d. allgem. Pathol. Anat,' Wien., 1846.

by inflammation, of which I shall speak later, I proceeded to colour the cartilage with silver.

I prepared the articular extremity of the femur of a freshly killed young dog, as described at the beginning of the paper, and after the synovia was lightly washed off I rubbed the condyles with a stick of lunar caustic for some minutes, then placed the object in spring water and exposed it to the light. After a few days the parts treated with nitrate of silver were coloured of a deep blackish-brown tint. The upper layer was rejected as useless and the one next to it examined. I may remark, by the way, that sections may be prepared from deeper layers which are apparently less coloured by silver, but in a short time become deeply coloured if exposed to the light, so that new preparations may be made for a certain thickness from the same cartilage.

The following description refers to condyle preparations from a young and from an adult, dog; both agree perfectly with each other.

In those parts of the anterior or inferior portions of the condyle which were coloured I obtained no appearances leading to distinct ideas concerning the network. But when I examined the projecting edge separating the inferior from the lateral surface matters were altered. Fig. 7, Plate VIII, shows one of the appearances which I obtained at the point of transition from the anterior to the lateral surface.

The matrix is deeply coloured at the margins of the cartilage cavities, and of a lighter reddish-brown in the other parts. From the cartilage cavities clear processes are given off in various directions, which may be grouped into about three orders, according to their calibre. The broadest processes (1st order) unite two cartilage-cells which are near, or even at some distance from each other, or they simply pass into the matrix. The narrower processes (2nd order) are given off both from the cartilage cavities and from the broad processes, and they branch very richly to form very narrow ones (3rd order). These last are given off from the whole circumference of the cartilage-cavity from the sides of processes of the 1st and 2nd order, and from the extremities of broader processes and traverse the whole matrix. They form a rich, very delicate, network of clear irregular canals, richly furnished with varicose dilatations, and in whose meshes we see the brown matrix. In the cartilage-corpuscles we find the dull, uncoloured, cell-bodies, with the radiating serrations of the enlarged nuclei, and the processes of the cell-body itself, which may be followed into the canalicular process of the 1st order.

While at the anterior and inferior surfaces of the condyles we find chiefly only processes of the 3rd order, and here and there a few of the 2nd order, which are most easily recognisable when the margins of the cartilage capsules appear traversed by radiating, clear, delicate streaks, the lateral surfaces constantly show cartilage-cells with processes of the 1st order, and these are more numerous the further we remove from the edge of the articular surface. If we examine that part in which the matrix of the cartilage has a striated appearance, we find the most beautiful silver pictures; the larger processes united by finer ones are extremely numerous and remain so in the fibro-cartilaginous, periosteal, and tendinous tissues, to which we pass at various points from the region of hyaline cartilage.

The next thing to be done was to colour the cartilage with chloride of gold. I treated the condyles with ½ per cent. solution of the salt, and followed its operation for periods extending from ten minutes to twelve hours in the same object, the most superficial sections being always rejected.

After fifteen minutes the coloration of the cartilage-cells by gold was already distinct; in the violet cell-body the nucleus could be clearly distinguished with a sharp outline; the outline of the cell-body was also sharply defined. The conical processes coming off from the margin were to be seen in great numbers in many cells of a violet colour, but it was not possible to follow them further in the matrix than when they were uncoloured; in preparations from the lateral surfaces there were numerous large processes with a serrated outline and of the same violet tint as the cell-body; the matrix remained uncoloured, or was of a pale, bluish-red.

After treatment for one hour with the gold solution the large processes and the cells were coloured a deep violet, the processes of the 2nd order were distinctly visible, and a few of those of the 3rd order were seen extending far into the matrix, and uniting cells that lie near each other.

The appearances after treatment for twelve hours are not trustworthy because they present the appearance of granular and crumbled precipitations. The cartilage-cells are of a blackish-violet colour, the nucleus indistinguishable, or only to be made out as a lighter portion in the cell-body. From the circumference of the latter fine processes are given off, of a finely granular aspect for the most part, which, in many places, form a granular network. This network is most strongly developed in the immediate neighbourhood of the cell, or at the boundaries of the cell territories—it unites neighbouring cells, and is, in parts, so confused that it

cannot be clearly be made out even with No. 10 immersion lens. Places where the marking is incomplete, as in the part shown in Pl. VIII, Fig. 8, lead us to recognise a network with numerous granular and varicose swellings, concerning whose connection with the cell-body there can be no doubt.

At the line of transition between cartilage and bone there exists, as is well known, a layer of cartilage-cells whose matrix is calcified. A deposition of lime-salts may also be very rapidly produced experimentally, as I shall show later, by injuring the cartilage in certain ways. In horizontal sections, both of fresh ossifying cartilage, and in others from which the lime-salts have been extracted by chromic acid, we see that the matrix is traversed by canals, in which there lie processes given off radially from the capsules and forming network. The appearances presented by the canalicular processes remain precisely identical from the time at which we can distinguish the earliest traces of calcareous deposit, which may take place either in the wall of the capsule or close to the boundary of a cell-territory, to that in which the matrix is completely calcified. We may convince ourselves that the only change consists in the deposition of earthy salts in the matrix, all that concerns the structure of the cell and its process remaining quite unaltered. In old animals, where bone is in immediate contiguity with calcified cartilage, we cannot, for a moment, hesitate to admit that there is junction by means of cell-processes between the osteoid-cells and true bone-cells.

The accompanying corollaries follow from my researches:

The bodies of the cartilage-cells are provided with radiating processes. These processes form a delicate, varicose network in the matrix, where hyaline cartilage passes into striated fibro-cartilage and into periosteum; the processes are very large and broad; they unite neighbouring cells either immediately or by means of five processes.

Formation of Blood and Bone from Cartilage.

If we examine a horizontal section taken some distance from the surface of the femoral condyles of a freshly killed puppy, a few weeks old, with a rather high power in $\frac{1}{2}$ per cent. salt-solution, we may distinguish in the matrix cartilage-cells of two kinds. In the first place larger cells, with pale granules and a distinct nucleus; secondly, smaller cells, which are bright, yellowish, indistinctly granular, and apparently not nucleated. Transitional forms occur between the two kinds; we find cells, whose bodies are partly pale

and finely granular, and partly, especially at some marginal point, lustrous. Both kinds of cells may lie close together, separated by a narrow bridge of matrix, and both kinds have a few coarse granules which look like fat-globules.

This relation is still more striking in vertical sections from the completely cartilaginous epiphysis of the same puppy, which is traversed by blood-vessels. Close to the articular surface may be seen tolerably uniform cells closely superimposed upon each other; these become larger the further we go from the surface towards the diaphysis, and the difference between the two kinds of cartilage cells becomes more strongly marked. At the same time fresh distinctive marks make their appearance. While the pale, finely granular cells fill their capsules almost completely, this is not the case, as a rule, with the lustrous, indistinctly granular, yellowish cells. The shining substance is deposited in the cortical layer, and surrounds the pale substance like a hollow hemisphere (giving a semi-lunar appearance in profile), or it encloses a vacuole, or it lies in the centre of the cartilage-capsule as a round or angular, irregularly dentated body, surrounded by a structureless zone.

Deeper in the epiphysal cartilage we find the cells again more flattened, or still deeper they appear as discs lying close above each other, and arranged in the well-known rows. The cartilage cavities now continue to enlarge, the matrix becomes narrower, and calcified matrix suddenly appears in a continuous line. The cartilage cavities are here surrounded by highly refractive substance impregnated with lime, which appears in profile in the longitudinal columns as an almost homogeneous mass, but seen from above as a coarsely granular, highly refracting plate.

The cartilage cells situated close to the calcified part show the difference between the two kinds above described very clearly. The large cavities arranged in rows are, in some of these rows, almost filled by the pale protoplasm of the cell. Close by these are seen others not completely filled by the cell, a pale, homogeneous border remaining. Finally, we may see, in the middle of the cavity, the lustrous, coarsely granular substance which is here specially defined by notched processes; round these, again, a finely granular zone, between which and the matrix is a structureless zone.

If we examine a vertical section from the condyle of the femur of a dog about six months old, where spongy bone is already present in the epiphysis, we find in the boundary between epiphyses and articular cartilage structures similar to those already described, with this dif-

ference that the cartilage cavities are arranged in rounded groups. Each cavity contains a pale, finely granular, or homogeneous substance, and a highly refractive body either at the centre or at the periphery; the latter being coloured dark violet, the former pale violet by chloride of gold.

The older the animal examined the thinner do we find the layer of articular cartilage, and the more scanty also is on its surface the refractive substance in the cartilage cells.

What has just been said of the dog is true also of cats and rabbits.

Let us now consider the spaces which arise in the calcified portion of cartilage, as is well known, by fusion of the calcigerous matrix of the cartilage. In the first place we find the wall of the cavity lined with colourless, finely granular, protoplasmic masses; in the centre are roundish bodies (or spaces) with an apex to one side and surrounded by spindle-cells, which converge towards the apex. In the middle of those bodies we see groups of highly refractive structures which appear as if produced by the splitting up of a lump. In young dogs I have seen several such groups in the centre of one of the spaces produced by fusion, each surrounded by the spindle cells just described.

Comparing what occurs in inflamed bone with the appearances seen in cartilage-cells I am convinced of the identity of the refractive or lustrous substance in the two, as it agrees all characters and reactions. Moreover, in the cartilage also we can follow all phases of development up to the formation of nearly perfect red corpuscles, by going through a large number of preparations. A portion of the cartilage-cells, or a part of their protoplasma, has accordingly been transformed into hæmatoblasts.

We have already seen that the cartilage-cell contains in variable quantity a pale or apparently structureless constituent. In some spaces this substance predominates, but there occur others which are filled only with large, pale, protoplasmic masses. Such masses may spread beyond the calcareous columns, and unite with neighbouring masses of the same kind. Calcareous salts become deposited in these masses, starting from the periphery; and it is easy to trace a differentiation which takes place in the protoplasmic masses simultaneously with the calcareous infiltration; and we see also that granular bodies furnished with processes, *i.e.* bone-cells, arise in them. It is not difficult to shew that the protoplasmic masses in question furnish the basis for bone lamellæ in the widest sense; and more particularly as seen in profile, when narrow layers of protoplasm, connected by

processes with one another and with the cells enclosed in the cartilage cavities, come into view.

I agree, accordingly, on this point with Waldeyer[1] so far, that bone-cell and bone-matrix are both to be regarded as formations from the protoplasma; one portion of it becoming the stellate cell, another portion the matrix, which latter becomes at once impregnated with lime. Only I say that before the calcareous impregnation layers of protoplasm are formed from a number of osteoblasts, in which the outlines of the latter are no longer visible, and the differentiation into bone-cells and matrix is only effected when calcareous infiltration begins.

Finally, it may be mentioned that roundish or flat protoplasmic masses, with distinct and brilliant round nuclei, are met with in the fusion spaces both of the normal calcified cartilage and of bone. They are the structures already described by Rollett,[2] and generally spoken of as "medullary cells."

Researches on Inflamed Cartilage.

In order to secure uniformity of results, the articular cartilages of the condyles of the femur were always selected for experiment, and were injured with a heated conical piece of iron, which was superficially applied to one condyle, and bored into the other. Rabbits, dogs, and cats were thus treated.

Superficial lesions.—Twenty-six hours after the injury the wound is found partly covered with a slough, and fissures start from its margin. The matrix has round these fissures a yellowish colour; some cartilage cavities are enlarged, and are either empty or contain a pale finely granular substance, sometimes provided with vacuolæ. The cells close to the fissure are spindle-shaped, otherwise apparently unaltered.

After five days some cartilage cavities near the wound are found enlarged to five times their normal size, and partly opening to the surface. The fundamental substance is reduced to narrow trabeculæ.

The pale finely granular substance is sometimes so divided that the cavity appears filled with several nucleated cells; but it is difficult to decide whether these have arisen by division of one large cell, or by diminution of the matrix between the individual enlarged cells. Some facts appeared to point to the latter explanation. Quite close to the altered portions are found unaltered cavities and cells.

[1] "Ueber den Ossifications-Process," 'Archiv. für Mikr. Anat.,' i, 1865

[2] Stricker's 'Manual of Histology,' article "Bone."

The appearances are essentially the same on the eighth day of inflammation; and the general result of the experiments on eight rabbits was, that the changes produced in their articular cartilages, even by intense irritation, are but slight. Formation of pus from cartilage cells could not be demonstrated.

Simultaneous lesion of the Articular Cartilage and the Epiphyses of the Femur.

The injury was effected either with the pointed hot iron or with the knife, but the former gave more striking results. The first remarkable phenomenon was one already observed by Redfern,[1] namely, deposition of lime in the cartilage close to the margin of the wound. It could be seen in sections after twenty-six hours, and on the third day with the naked eye. The lime salts were found on examination to be deposited in the matrix of the cartilage, while the cartilage cells often had a serrated appearance, produced by a delicate network of processes. In the neighbouring parts some of the cartilage-cells were embedded in normal matrix, others surrounded by a ring of bone, or with their entire "domain" calcified.

On decalcifying such portions it was found that the cartilage-cells near the calcified margin were converted into yellow, brilliant, and nodular bodies, such as have been already often described; they shewed, in fact, a striking transformation into hœmatoblastic substance. On the third and following days the spaces enlarge by absorption of the calcified matrix, but the cells are, except for the abundance of hæmatoblasts, little altered; and only where the matrix is entirely wanting are they found converted into spindle forms, or else apparently fused into masses of nucleated protoplasm, while blood and vessels appear in the midst of the fused tissue. Similar changes are seen in the bone-cells of the adjacent epiphysis.

Lesions (*deep* and *superficial*) *of the angle of the articulating cartilages* were distinguished by the intensity of the changes in the fibro-cartilage, periosteum, and tendons there inserted. Cell proliferation was seen in the latter parts, and also in the portions of cartilage nearest the bone. Simultaneously with the cell proliferation occurs a partial transformation of the cells into hæmatoblastic substance, and transitional forms leading to red corpuscles. The formation of the latter is observed during the inflammatory process not

[1] 'Anormal Nutrition in Cartilage,' London, 1850.

only in the cartilage cells, but in those of the synovial membrane, periosteum and tendon.

In one case abundant black granules were found in the cartilage cells and matrix of the articular cartilage of a dog which had been deeply cauterized with the hot iron. These granules were found to be carbon, and were first thought to have been carried from the wound; but the phenomenon was not produced in other experimental lesions.

REVIEW.

Stricker's Manual of Histology.[1]

We must always be grateful to those scientific investigators who undertake the modest but useful task of writing a text-book, and this recognition is emphatically due to Professor Stricker and the band of eminent histologists who have produced the work before us. We have here a series of articles by more than thirty writers on the different departments of histology, each giving the experience of a specially skilled observer writing on that subject with which he is presumed to be most familiar. The result is a book which could certainly have been written in no other country than in Germany, and which contains a larger mass of information on histology than has ever before been brought together. If a fault is to be found with the execution, it is that the writers have not always clearly distinguished the aims of a text-book from those of an original memoir. They appear to have been anxious rather to state some new or original view than to remain within the limits of what is firmly established in science. Sometimes, indeed, ordinary facts are passed over as if too familiar to need mention, a practice which not only renders the book unfitted for students, but even for other persons who may want to consult it as a work of reference. There is also much want of uniformity in such matters as the giving of references, and the same may be said with regard to descriptions of technical methods. In both these respects some of the contributors are very deficient. These are of course the drawbacks which naturally result from the generally excellent practice of getting a specialist to write on his speciality, but we must not be blind to the greatly preponderating merits of this plan. We have no retailing of second-hand knowledge, no mere *réchauffé* of traditional descriptions, but every statement is founded on fresh original observation. It is this which so favorably distinguishes Stricker's work from the general run of text-books, and makes us feel that,

[1] 'A Manual of Human and Comparative Histology,' edited by S. Stricker, assisted by numerous contributors. Translated by Henry Power, M.B., F.R.C.S. Vols. I and II. London: The New Sydenham Society.

after all, perhaps our main quarrel ought to be with the title; were it described as a collection of monographs, rather than as a handbook, our objections would many of them disappear. We may add that the title-page is misleading in another respect also, that is, so far as it claims to treat of the "Histology of men and *animals*" (in the German original). The work certainly does not treat of human histology only, the writers having for the most part followed the common practice of describing an organ from that animal in which the structure happens to be best displayed, and thus, like all similar works, it treats very largely of the frog and the rabbit; but only a few articles discuss the variations presented throughout even the main divisions of the animal kingdom, that is, give us anything like *comparative* histology. The use of the latter expression by Mr. Power as a translation of the German title makes the discrepancy more glaring than it need be.

If we now proceed to notice a few of the special articles we must not be understood as making any pretension adequately to review them. To do so would, indeed, require a staff of reviewers almost as numerous and competent as the staff of writers.

Professor Stricker's introduction on technical matters is interesting but a little disappointing. Certain recent methods of examination are described very well, but the account of the general routine of histological work is somewhat meagre, and with regard to some topics we feel that if more space could not be spared for the details which alone make practical directions valuable they might as well have been altogether omitted.

The next chapter, on the general characters of cells, contains a more comprehensive account of this subject than is to be found in any English work, and is well worthy of attention. We must, however, take some exception to the historical references, which, though they are freely sprinkled with names and dates, are sometimes both inadequate and misleading. Thus, in the account of the doctrine of cell genesis much credit is justly ascribed to Remak for having clearly demonstrated by his embryological researches the occurrence of cell formation by division, and for having thus overthrown the doctrine of free cell development. But to ascribe to him without qualification the merit of having established the same law in respect of pathological development also is hardly just even to Virchow (whose services receive some acknowledgment in the next sentence), and still more unjust to Goodsir, whose observations, as well a those of Redfern, were independent of and apparently an-

terior to those of Remak, while they exercised a most powerful influence on the scientific tendencies of Virchow. This was, indeed, very fully acknowledged by the latter in his original memoirs, but those who have never gone further back than the volume of lectures on the Cellular Pathology are, perhaps, not aware how large a part was contributed by the great Scotch anatomist to the foundations at least of the now prevailing doctrine.

Rollett's article on the connective tissues is an instance of how impossible it is for a text-book writer to be always on a level with the most recent views. The figures 2 and 3 and the descriptions of connective-tissue-cells would doubtless have been much modified had the important memoirs of Ranvier been previously published; for though the results of the French histologist may not be accepted in their integrity, there can be no doubt that they have quite revolutionised the prevailing views on this subject. A very interesting part of Rollett's paper is that which deals with the development of connective tissue; the author expresses himself with studied caution on the difficult question of the genesis of the connective-tissue-fibres. When he wrote he inclined to the view that the fibres are not formed directly from prolonged cell processes, as first maintained by Schwann, his belief being apparently that the fibrillæ are formed by a species of "coining" (*prägung*) out of a continuous homogeneous mass, which itself results from a metamorphosis and fusion of the substance of cells. In the translation, however, a paragraph (communicated by the editor and, we presume, expressing the views of the writer) is inserted which pronounces unreservedly for the original theory of Schwann. We must confess that this dictum appears to us (at least thus barely stated) far less satisfactory than the carefully worked-out statement of Rollett.

Little need be said of so admirable and classical an article as that of Max Schultze on the structure of the nervous system, which it is almost as difficult to praise as to criticise without presumption.

Then follow two very interesting though extremely special articles by Kühne on the termination of nerve in muscle, and Brücke on the examination of muscle in polarized light, the former recalling a controversy into which we do not care to enter. At this point occurs, perhaps, the most singular hiatus ever known in a work on histology. There is actually no general account of voluntary muscle, which seems still more extraordinary considering what a lavish allowance of space is given to the special points just spoken of. The

omission was explained on the cover of the second part as a consequence of the "commotion which had arisen in the domain of myology during the past few months," and which made it desirable to gain as much time as possible for the critical discussion of the subject. Accordingly the subject of striated muscle was transferred to the section of histogenesis, to appear in the last part. The implied promise has been, we must say, even now but imperfectly redeemed, for the very few pages given to the subject of muscle by Professor Stricker in the section referred to are mainly critical, and are as far from giving a clear elementary account of the structure of striated muscle as from doing justice to researches of Hensen and Krause which were the occasion of delay. The whole episode is curiously characteristic of the unstable and controversial character of much of the modern literature of histology. Another curious episode is represented by the account given by Pflüger in his generally admirable article on the salivary glands, of the distribution of nerve-fibres to the gland-cells. The relations there represented have been traced by no other anatomist, and it is not too much to say that they have fallen into general discredit almost before the completion of the work of which the article is a part. In this case, at all events, novelty had too much attraction for the editor and the contributor.

A very laborious histologist, the late Dr. Schweigger Seidel, of Leipzig, wrote the article on the heart, in which he made a most ingenious attempt to trace a cellular arrangement in the ramified muscle-fibres. The subject is not one which can be properly discussed here, though well worth investigation, but we do not know that the author's results have yet been confirmed.

The section on the lymphatic system by von Recklinghausen is particularly deserving of the attention of English histologists, since the details of structure there admirably described and illustrated, have been entirely worked out in Germany, and are as yet very inadequately known in this country.

The same observation would not be true of that important subject, the blood, treated by Rollett in his second paper. Here there are several points on which most valuable observations have been made by English observers, the absence of all reference to which makes us fear that English books penetrate with difficulty as far as the University of Graz. In speaking of measurements of blood-corpuscles Rollett justly observes that they are very numerous, often discrepant, and not to be adopted without reserve. But this was

hardly a reason for leaving unnoticed the life-long labours of Mr. Gulliver, and to pass over his results entirely seriously impairs the completeness of the treatment of the subject. Roberts's discovery of the macula brought out by magenta in the red blood-corpuscle was published some years ago, though Brinton's researches on the chemical composition of the corpuscle may possibly have appeared too late to be noticed.

The account of the lung given by F. E. Schulze contains more "comparative" histology than most of the articles. It also enters very fully into one of the most enduring of histological controversies, the presence or absence of epithelium in the alveoli of the lung. Schulze holds a view to some extent different from that of either of the opposing schools. The alveolar epithelium is, according to him, present as a continuous layer in all mammalia, but in the adult not homogeneous, some of the epithelial cells becoming converted into mere structureless plates. Equal attention is paid to comparative histology in the excellent memoirs of La Valette St. George on the testes, and of Waldeyer on the ovary.

Hering treats of the important subject of the liver, and explains very clearly the relations of the gall-capillaries to the liver-cells, his views on this subject being the same as he has expressed in several memoirs, and shown by preparations, some of which have been examined by many histologists in this country. Differing *in toto* from Dr. Lionel Beale, he speaks with all deserved respect of his important researches. Hering's researches appear to us more in agreement with those of Dr. Handfield Jones than with those of any other English observer, though of course by no means identical with them.

We need only direct attention to the paper of Ludwig on the kidney as an admirable monograph, giving in a very short space all essential details of the structure of this organ in mammalia; it is a model to which we could wish other writers had conformed. Equally useful and practical are the contributions made by Dr. Klein.

Doubtless the most original of all the memoirs contained in these volumes is that of Meynert on the brain, which, however, is hardly adapted to be generally instructive, and we see with pain immense labour and knowledge rendered comparatively useless by defects in the exposition.

If English readers find the translation obscure they may be told that the original is not less so to the German public. The main object, however, is clear, and is certainly one of the most important tasks that yet remains to be done in anatomy,

being no less than to trace the material connections of all parts of the brain with one another and with the paths of sensation and motion. We cannot doubt that these relations are all important and that a good part of what is now known as psychology must rest upon them. When they are known the names we now give to parts of the brain will (as Huschke says) be not more important than are in astronomy the names of the constellations. But we must ask, is this in place here? It is magnificent, but it is not histology, and, grateful as we are for the little histology which Meynert gives us we would gladly exchange for more of it some of these elaborate descriptions and figures, which would more strictly find their place in a work on the anatomy of the nerve centres in general.

While acknowledging that the translation is in general accurate and that the German syntax is rendered into fairly readable English idiom, we must point out that some cause or other has led to certain serious mistranslations. It would not be respectful to the translator to make such a statement without alleging instances, and we will therefore point out the following:

Vol. i, p. 33, in speaking of Schleiden's views on the formation of cells, there are two sentences which by the total omission of the meaning conveyed by the German word "sollte" are made to state the opinion of the writer, not of Schleiden, the real meaning being "the formation of cells took place *according to him*," &c. On the next page the phrase "Virchow's well-grounded statement made in 1855" should really be "the proposition *maintained* by Virchow in 1855," which might or might not be well grounded. The next paragraph supplies another instance. Stricker says, speaking of spontaneous generation, "dass wir in consequenz unseres Denkens die Annahme nicht von uns weisen können, dass einmal eine freie extracellulare Zellenbildung stattgefunden hat," an awkward sentence, the meaning of which, however, clearly is that the logical following out of our conceptions makes it impossible to reject the supposition that free extra-cellular formation of cells must at one time have taken place. The translation is completely misleading —"the general tendency of these facts is to disprove that a free extra-cellular formation of cells ever takes place."

On the next page we have the following quite unintelligible sentence:—"Development of nuclei proceeds in a manner essentially similar to that of cells when they undergo complete division," the real meaning of the German original being that "formation of nuclei *precedes* endogenous cell

formation in the same way as it precedes the total splitting up and division of the cell."

We are, moreover, unable to see why "Dammar firniss" should be translated Canada balsam, a confusion as perplexing to the practical microscopist as it would be shocking to the botanist or chemist. Instances might be multiplied, but enough has been said to show that some parts of the translation stand in much need of careful revision.

On the whole we must congratulate the New Sydenham Society on their enterprise and thank them for making so important a work accessible to the English reader.

NOTES AND MEMORANDA.

Draw-tubes *versus* Deep Eye-pieces.—In reference to the note by M. Prazmowski, printed in our last number, Mr. Stodder, of Boston, U.S.A., writes in the November number of the 'Lens,' that Mr. Prazmowski ought to define what he means by a short tube or a long tube. If he means to call a common German or French five-inch tube "short," then he undoubtedly means by a long tube the ten-inch tubes of English and American instruments. "Then he gives no specification of the objectives he refers to, forgetting or ignoring the fact that very different effects may be obtained from different instruments; ignoring also the fact that accomplished makers make their objectives to give the best results with a certain definite length of tube. From this fact an objective intended to give its conjugate focus at five inches ought not to be expected to do so well if the cone of rays were extended to ten inches. One made to do its *best* with a ten-inch tube ought not to be expected to do *best* with a tube of five inches or with one of seven feet."

The perfection of the image, Mr. Stodder urges, must depend upon the skill of the maker in correcting "the epherical and chromatic aberration," and not on the length of tube or power of eye-piece entirely. He has by him photographs of *Amphipleura pellucida*, taken by Col. Dr. Woodward, of Washington, with a Tolles' objective (about $\frac{1}{5}$) at forty-eight inches' distance from the objective; this is equal to a pretty long tube. Now, this photograph bears enlarging twenty diameters, and is still clear and bright. He has also other photographs by Dr. Woodward of the same object, taken with Powell and Lealand's $\frac{1}{10}$, and with Tolles' $\frac{1}{18}$, at seven feet six inches' distance, still giving good definition and bearing a low power eye-piece well. It is to be supposed that these are, at least, not "short tubes," and are good evidence that the definition is obtained by quality of the objective.

A true comparison, as it seems to us, can only be obtained by comparing the images produced by the same objective with a low, the other with a high, eye-piece, brought to the same magnifying power by altering the length of tube. The results of experience thus gained might, as Mr. Stodder suggests, be different with different instruments.

The Number of the Red Blood-Corpuscles in Mammals, Birds, and Fishes.—M. Malassez ('Comptes Rendus,' lxxv, 1528-1531) describes a method by which the red as well as the white blood corpuscles can be readily counted. According to the method recommended by M. Potam, a drop of blood is mixed in exact proportion with some preservative liquid, and introduced into an *artificial capillary*, which consists of a flattened glass tube, in which the volume is calculated for each unit of length. By means of a microscope, the eyepiece of which is divided into squares, the number of corpuscles comprised within a certain number of squares can be counted. Knowing the length of tube corresponding to the squares and the corresponding volume, one can easily calculate the number of corpuscles in the cubic millimètre.

Among the mammals the number varies from 3,500,000 to 18,000,000 in the cubic millimètre. The average number in man is 4,000,000. Camels possess from 10,000,000 to 10,400,000. In the goat the number amounts to 18,000,000. The porpoise has 3,600,000—a number exceeding that found in fishes. Birds have fewer than mammals. The maximum is 4,000,000, the minimum 1,600,000, the mean being about 3,000,000. Fishes have still fewer, and there is a difference between osseous and cartilaginous fishes. Osseous fishes possess 700,000 to 2,000,000. Cartilaginous fishes have 140,000 to 230,000.

Thus the number of corpuscles diminishes as one descends the animal series. But the richness of the blood depends not on the mere number, but also on the surface, volume, and weight of the globule in the cubic millimètre, and also on the amount of hæmoglobin in each corpuscle. The author has not been able to resolve these questions, but compares the number of the corpuscles with their dimensions. The corpuscles increase in size as we descend the animal scale, so that there is an inverse proportion between the size and number of the corpuscles. This proportion is not, however, altogether constant, for man has fewer than the dromedary or llama, and at the same time smaller corpuscles.

The consequence of this inverse proportion is, that the diminution of the number is compensated by the increase in volume. This is not always exact, however, as birds gain more by the augmentation in volume than they lose by the diminution in number, the weight of the bird's being greater than that of the mammalian corpuscle.—D. Ferrier, M.D. —*Medical Record.*

QUARTERLY CHRONICLE OF MICROSCOPICAL SCIENCE.

MINUTE ORGANISMS AND DISEASE.

Ulcerative Endocarditis with Fungoid Growth in the Heart.—Dr. Heiberg describes, in Virchow's 'Archiv,' vol. lvi, p. 407, two cases of ulcerative endocarditis in which the vegetations or deposits upon the cardiac valves consisted of masses of fungus growth, or at least of bacteria. The first had been previously published by Winge. A man, aged 44, in consequence of the suppuration of a corn, fell into a chronic febrile state, accompanied by rigors, swelling of the joints, and high fever. At the necropsy were found hæmorrhagic blocks in the lungs, kidneys, and spleen, with thrombosis of the corresponding arterial branches. On the aortic and tricuspid valves were adherent masses looking like coagula, some of the size of a pea or a bean, which were easily removed, leaving the endocardium ulcerated; also considerable ulcers on the endocardium of the right heart, covered with similar masses. These masses consisted of interwoven threads, resembling fibrine under a moderate power of the microscope, but when more highly magnified were resolved into strings of short, rod-like bodies, or round granules, precisely similar to Leptothrix filaments. There were also round and elongated granules like bacteria. All these structures were unaltered by caustic potash and acetic acid. Similar bodies composed the plugs filling the arterial branches leading to the blocks in the kidneys; but none were found in the original wound. The veins connected with the wound were natural.

The second case was that of a young woman who, after a normal childbirth, passed into a febrile condition accompanied by rigors and swelling of the joints, followed by sloughing sores on the sacrum. At the necropsy were found subserous abscesses in the neighbourhood of the uterus, containing foul pus; the state of the uterine veins not being given. The heart showed ulceration of the mitral valve, and polypoid masses adherent both to it and to the chordæ tendineæ. These, on microscopical examination, were found to be made up of chains of granules and rod-like bodies (which, as in the first case, are described as Leptothrix filaments), together with numerous bacteria-like bodies and still more

minute granules, most of which were insoluble in potash. There were also blocks in the spleen and kidneys, those in the latter having suppurated; and in these, as well as in the arteries leading to them, 'granules,' though no jointed filaments, were detected. The same were seen in the subserous abscesses near the uterus. The endocardial masses in both cases are regarded by Heiberg as 'fungus' growths which must have existed during life, and when washed off the valves would have been distributed by the circulation as emboli. In the first case he supposes the parasite to have entered the body by the open wound; in the second case, the mode of entrance was less evident, but may have been by the inner surface of the uterus.

Specimens of both parasitic masses were sent to Virchow, who hesitates to call them *Leptothrix*, while admitting that they are certainly parasitic, but rather regards them as consisting of rows of vibriones and extremely minute granules.

Reference is made to similar cases of endocarditis, some puerperal, some not, reported by Virchow himself partly in 'Gesammelte Abhandlungen' (pp. 708 and 713) and partly in a note appended to a memoir on chlorosis.[1] In these cases also masses were found attached to the cardiac valves which were described either as 'composed of granules' or as diphtheritic. Similar masses imbedded in clots also occurred in puerperal cases without endocarditis. In one case similar bodies occurred within the cerebral vessels. Virchow, in recording the cases, spoke of these granules as 'foreign particles,' or 'very minute organisms,' which must have existed in the circulating blood during life, though they may have increased after death, without committing himself to calling them bacteria or vibriones. While expressing himself thus cautiously, he had nevertheless then, as he has now, no doubt that the morbid processes in question were parasitic, and that their relations to diphtheritis and blood-poisoning were to be explained in this sense. Not indeed that every ulcerative endocarditis is diphtheritic (or parasitic), but that such a form of disease exists, which may hereafter be recognised by clearer characters.

Effects of Injecting Putrid Fluid.——MM. Greveler and Hueter have given an account ('Centralblatt,' Nov. 16, 1872) of experimental researches on the changes produced by injecting into the muscles or under the skin of the frog fluid containing monads (micrococci or microspores)[2]—that is,

[1] 'Die Chlorose,' u. s. w. Von Rudolf Virchow. Berlin. 1872.

[2] The nomenclature of these organisms is becoming very perplexing, the words monad, bacterium, vibrio, micrococcus, microsporon, microzyme, and others, being applied by different authors to the same object.—Reporter.

normal blood or normal pus in a state of putrefaction. The frogs were after some hours rendered motionless by curare, and their mesentery then examined by Cohnheim's method. The mesentery appeared to the naked eye red and vascular, while with the microscope the phenomena at once seen much resembled those of inflammation produced after some time in Cohnheim's experiment. The white corpuscles were seen clinging to the walls of the smaller veins and capillaries, as well as later to those of the arteries, but few or no corpuscles were seen which had passed out of the vessels or were in the act of passing through the walls; these phenomena of emigration being not seen till one or two days after the injection was made. The most noticeable peculiarity was that numerous capillaries seemed shut off from the circulation, perhaps one half these vessels being in this condition twenty-four hours after the experiment. They were either filled with red corpuscles or only with plasma. The obstruction was in many cases caused by the adhesion of one or two white corpuscles to the walls at the orifice of the capillary, or else in some cases by a large *monad* or several smaller monadic granules occupying the same position, which, though not actually closing the mouth of the vessel, served to turn aside the red corpuscles. The same phenomena were seen in the tongue and the web of the foot, so far as the greater thickness and opacity of these structures permitted. The authors think that these phenomena of obstructed circulation will explain many of the symptoms of the febrile state.—J. F. Payne.—*Medical Record.*

Erysipelas.—Dr. Salisbury states in the last part of Hallier's 'Zeitschrift für Parasitenkunde,' 1873, Bd. iv, pp. 1—5, that his examination of the blood and secretions with the microscope was commenced in 1862. On the fourth day of the attack, in a lady suffering from the disease, a small quantity of blood was drawn from the temple, which clotted firmly; serum exuded in large quantity, but presented nothing abnormal; portions of the clot, however, on being carefully teased out and washed, exhibited a jointed mycelium, which branched in all directions, and here and there gave off fertile heads. These last divided at the apex into four equal branches, which ran up close together for a distance equal to about four times the diameter of the filament, and were there each intercepted by a joint. At this point they all began to diverge, forming a kind of bell, and subdivided into four branchlets, each of which terminated in a long moniliform chain of highly transparent, refractive, spherical spores. He calls this fungus *Penicillium quadrifidum.*

Hay asthma or catarrhal fever.—Dr. Salisbury, loc. cit., pp. 6--11, regards this as a purely parasitic disease, arising from a peculiar animalcular organism, to which he has applied the name *Asthmatos ciliaris.* The shape of these animalculæ is either spherical or oval, the body being armed on one side with cilia. They frequently send out a long proboscis, at the end of which is a dilated and elongated cilium. One or more large nuclei and many smaller granules of various sizes are contained in the interior of the sac-like body of the parasite. The young are developed within the parent cell, and when mature are discharged at the end of the organism opposite the cilia. The active movements of the latter produce the aggravating irritation of the mucous surfaces they infest, and when they have once gained a foothold upon the membrane they multiply rapidly. Dr. Salisbury has found the inhalation of a solution of carbolic acid, and the internal administration of the perchloride of iron and sulphate of quinine, the most effectual means of curing the disease when present, or of preventing an attack at that season of the year when it is most likely to occur in those predisposed to it.

Syphilis.—Dr. Salisbury states, loc. cit., pp. 33—34, that he discovered a peculiar filament running in all directions, singly and in bundles, through the diseased connective-tissue elements. The same vegetation shows itself in the blood as soon as the disease becomes constitutional, and he believes its presence or absence is a sure guide for continuing or discontinuing treatment. In the earlier state the algoid organism, named *Crypta syphilitica*, appears under the form of minute, transparent, highly refractile spores, which gradually elongate into straight, curvilinear, or coiled filaments.—*Lancet.*

Discomycetous fungus in the "wax" of the human ear.—S. Garovaglio ('Rendiconti del R. Instit. Lombardo,' vol. v). The examination of a concretion taken from the ear of a woman affected with otitis resulted in the detection of some examples of a fungus belonging apparently to the genus *Peziza.* The examination of other cases it was thought might throw light on any relation which might exist between the malady and the parasite.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

December 4th, 1872.

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

Mr. Gayer read a paper "On a new form of Micro-spectroscope."

Dr. Royston Pigott read a paper "On a new method of using a Micrometer."

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

On December 11th, a numerous company of Fellows met at a "Scientific Evening," in the great hall of King's College, when many objects of interest were exhibited. Among other objectives were a $\frac{1}{50}$ and a $\frac{1}{80}$, by Messrs. Powell and Lealand. The latter gave a magnifying power of 4000 diameters with the "A" eye-piece. A new immersion No. 8 objective, by M. Prazmowski, of the firm of Hartnack et Cie., Paris. This glass has the great advantage of giving a focal length of about $\frac{1}{6}$ of an inch, with the power of a $\frac{1}{5}$.

January 1st, 1873.

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

The Secretary announced that the Society had received a valuable present from Mrs. Farrant, intended to commemorate her late husband's long connection with the Society. It consisted of a cabinet of about 1000 slides, and other objects.

Mr. Stewart read a "Note on the Scalp of a Negro." Beside the dark colour of the skin of the negro, which is due to extra pigmentation of the deep layer of the epidermis, there are some other peculiarities which are best seen in vertical sections of the scalp.

In such a section of a European scalp the hairs, surrounded by their follicles, may be seen to pursue a straight but oblique course through the substance of the derma. In the negro's scalp, on the other hand, not only is the imbedded portion of the hair and follicle much longer, but is remarkably curved, so as commonly to describe a half circle. The papilla at the base of the follicle consequently either lies horizontally, or even comes to point obliquely inwards.

Mr. Stewart also read a "Note on the Calcareous Parts of the Sucking Feet of an Echinus (*Podophora atrata*)." The "sucking feet" of all known Echini are strengthened by a calcareous framework, two parts of which the "rosette" and the "ring" appear to be constructed to keep the sucking extremity expanded. Certain

peculiarities in the structure of these parts formed the subject of the note.

Annual Meeting, February 5th, 1873.

W. K. Parker, Esq., F.R.S., President, in the chair.

It was announced that Mr. Jabez Hogg, one of the Secretaries, had found it necessary to resign his office, in consequence of his being appointed President of the Medical Microscopical Society. The Council had appointed Mr. Charles Stewart to replace Mr. Hogg.

A ballot took place for the Officers and Council for the ensuing year, when the following gentlemen were elected:[1]—

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

Vice-Presidents.—W. B. Carpenter, M.D., F.R.S.; Sir John Lubbock, Bart., M.P., F.R.S.; *F. H. Wenham, C.E.

Treasurer.—J. W. Stephenson, F.R.A.S.

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

Council.—*James Bell, F.C.S.; John Barney, Esq.; Robert Braithwaite, M.D., F.L.S.; W. J. Gray, M.D.; Henry Lawson, M.D.; *B. T. Lowne, F.L.S.; S. J. McIntire; *John Millar, F.L.S.; Henry Perigal, F.R.A.S.; Alfred Sanders, M.R.C.S.; *Charles Tyler, F.L.S.; Thomas C. White, M.R.C.S.

The Secretary then read the Annual Report of the Council, from which it appeared that twenty new members had been elected, and six members had died during the year. The Treasurer's statement of account was also read, which showed that the financial position of the Society was satisfactory.

The President then delivered the anniversary address.

Disclaiming the task of giving an account of other men's labours, or even of being an annalist in his own department, the President proceeded to make some remarks upon his own line of research, which was, he said, confined to the growth of vertebrated animal forms; and even here, was limited to one part—the *head.* Indeed, the head itself was too large a territory for one worker, he had selected merely its framework; the box that contains the brain; the basket-work of the face, and the wondrous passages of the ear and nose.

His special object of research was the embryo of the common pig, of which about seventy specimens had been put into his hands by Mr. Charles Stewart. These specimens were of six or seven different stages, varying from the size of a bee's grub to that of a new-born kitten.

The first formation of the cranium is not an easy process to observe. In its simplest part, at the roof, it is merely the innermost part of the skin, subdivided again into a dense membrane (the *dura mater*), and the cranial roof-bones are external to this; but the floor and side walls are preformed in cartilage.

[1] Those names to which an asterisk is placed are new.

All the specimens from which his sections are made are preserved in alcohol. They are then dried on blotting-paper, and imbedded in paraffin, a sharp razor being used for making sections from them as they lie in the cheesy mass. The slices are, one by one, transferred to alcohol again, by the use of a small bent slip of tinfoil; they are then stained with an ammoniacal solution of carmine, and are mounted in glycerine, to which a small quantity of muriatic acid has been added. For making solid sections, and for the dissection of the early embryoes, he prefers to put them, for a while, into a weak solution of chromic acid; they can then be divided vertically or horizontally, being held between the finger and thumb. Dissection must be done in water, on a black substance; paraffin and lamp-black make the best cake of this kind and the object can be fastened on with pins. The dissection is made by fine needles, mounted in small holders; it is anxious work, and is done with the help of a pocket-glass. Of the smallest embryoes portraits had to be taken from very perfect specimens which had not become shrunken in the spirit. These are of great value, as they show the form and relations of the principal masses of the skull and face. After the embryoes have grown somewhat, and bone begins to appear, they can still be treated like the smallest, for the first traces of bone do not turn the razor. These early traces of bone are of a rich crimson colour in specimens that have been coloured with carmine. In the case of larger specimens, the heads must be placed in a weak solution of chromic and nitric acids; this acid must be much stronger, and used for a longer time, in consequence of the solidity of the bone. These larger specimens make very valuable thick sections to be used as opaque objects, and with a low power. Perfect vertical sections of the heads of embryoes, taken from snout to occiput, are also of great value; a very fine saw has to be used for the purpose on the older ones. The section should be made a little to the left, so as not to injure the septum of the nose. Bird's-eye views of the skull-floor are taken from unroofed preparations, and are also very valuable.

March 5th.

C. Brooke, Esq., President, in the chair.

Mr. E. J. Gayer contributed some further notes on the Microspectroscope and Microscope, in continuation of his paper on the same subject, read at the December meeting of the Society.

A paper by Dr. Maddox "On a minute Plant found in an Incrustation of Carbonate of Lime" was also read to the meeting, and was illustrated by carefully executed drawings and prepared specimens exhibited under the microscope. The Secretary stated, with reference to some crystals shown at the previous meeting, obtained from the condensed vapour of coke, that they had been examined by Mr. Bell, and found to consist chiefly of protosulphate of iron.

A new metallic chimney for microscope lamps was introduced by Mr. Wenham, its merits being explained by the Secretary, and discussed by the meeting.

Medical Microscopical Society.

At the first Ordinary Meeting of this Society, held at the Royal Westminster Ophthalmic Hospital, King William Street, Strand, on Friday, January 17th, at 8 p.m., Jabez Hogg, Esq., President, in the chair, the minutes of the previous meeting were read and confirmed. The certificates of thirty-three gentlemen proposed for membership were read, amongst whom were Drs. Rutherford, Southey, G. C. Wallich, W. Mackenzie, C.B., C.S.I., T. Tebay Duplex, Messrs. T. Smith, T. Harvey Hill, &c., after which the President read the following introductory address:

The President after thanking the members for placing him in the chair of the Society, went on to say, that the doctrine of the elementary structures, whether in plants or animals, first took its root in men's minds about the latter part of the seventeenth century, when Malphighi and his contemporaries introduced into their anatomical investigations the use of the simple microscope.

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

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

The master mind of Newton was not fully alive to the importance of the instrument; for speaking of the extreme tenuity of the ultimate molecules of bodies, he seems to have had but an inadequate idea of their minuteness, and supposes that they might be seen through microscopes magnifying some three or four thousand times (linear), and in speculating upon the possible resolution of the colouring matter of bodies—a speculation, as Herschel observes "in the highest tone of a refined philosophy, irrespective of its theoretic bearings"—he goes on to say:—"In these descriptions I have been the more particular, because it is not impossible but that microscopes may at length be improved to the discovery of the particles

of bodies on which their colours depend, if they are not already in some measure arrived at that degree of perfection. For if these instruments are, or can be, so far improved as with sufficient distinctness to represent objects five or six hundred times bigger than at a foot distance they appear to our unaided vision, I should hope that we might be able to discover some of the greatest of these corpuscles, and by one that would magnify three or four thousand times, perhaps they might be all discovered except those which produce blackness. . . . However, it will add much to our satisfaction if those corpuscles can be discovered with microscopes which, if we shall at length attain to, I fear it will be the utmost improvement of this sense. For it seems impossible to see the more secret and noble works of nature within the corpuscles by reason of their transparency."

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

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

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

Many men, eminent in physical science, now engaged in a race after greater perfection, Tully, Goring, Brewster, Brown, Herschel, &c., in this country; and on the Continent, Selligues, Chevalier,

Amici, Fraünhofer, &c., eagerly set to work, grinding and constructing lenses for the microscope, and workers were not long wanting who understood how to apply them.

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

Professor Owen, then a rising young general practitioner, fresh from St. Bartholomew's Hospital, became President of the Society. During the first year of its existence 177 members joined the little band ; a number fully large enough to justify the anticipations of its founders, that such a society was wanted and would prove a success. May the rise and progress of the Royal Microscopical Society foreshadow the future of the bark we launch on the ocean of time to-night. May the Medical Microscopical Society fulfil in every way the wishes of its founders, and become a pillar of strength in the promotion of "Practical Histology" among students, young and old, in our profession. From the history of the Royal Microscopical Society we learn that members of the medical profession were more eager and zealous in the promotion of its objects than any other class of men ; and that the earliest and most frequent contributors to its transactions were chiefly Owen, the brothers Edwin and John Quekett, Arthur Farre, Dalrymple, Lindley, Busk, and others. Dr. Lindley strongly advocated the formation of committees to conduct particular branches of inquiry, because as he said, "The application of the powers and advantages of an associated body of observers to gain an intimacy with nature, is more important in regard to the microscope than to any other instrument of philosophical research, to conceive clearly the aim of our

researches and to give a right direction to our exertions." From this night henceforth, we feel that we are knit together by the kindliest bonds of brotherhood for the attainment of a common object, each and all striving to obtain a broader and firmer objective basis for histology than it has heretofore enjoyed. To make our labour one of more solid worth, we join heart and hand in the careful investigation of every phase of the intimate morphological condition of the animal organism ; starting from the earliest germ of existence, to development or growth, and proceeding onward to the more permanent forms in all created beings.

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

In comparative histology, the greater part of the work has yet to be done, and here we shall find a mass of material requiring years for its perfect elucidation. And whoever will perform something useful in this department of nature, must first acquaint himself with all that has been done by others. He must then prepare to discard authority, abandon old methods of research, and adopt new ones. He must also employ better and hitherto untried reagents ; otherwise he will fall into errors of interpretation conspicuous enough in the writings of many of those who have preceded him. Again, in pathological microscopical work, an immense field remains unexplored, waiting the hand of the diligent to become rich in gems of priceless worth. And let me here observe by way of caution that students commencing the work of the microscope must bear in mind the fact that, under high powers, the natural appearance of almost every object is in some way influenced or altered by the refractive nature of the fluid medium in which it has been immersed or is examined. The remarkable changes Graham's law of diffusion discloses when colloid substances enter into a preparation at once illustrates the necessity for caution in the use of preservative fluids. The alterations brought about by glycerine in substances containing alkaline salts is another familiar instance.

There are, indeed, many sources of error, to which, however, I need not more particularly allude in addressing the members of this Society, most of whom have already acquired skill in histo-

logical science. Such work as I have endeavoured to sketch out, is necessarily laborious and requires time and patience for its execution; but he who is prepared to undertake it will ultimately find his reward in having extracted some secret from nature of inestimable value.

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

But there is one point about a good working instrument of even more importance than a perfect stand, and that is a first-rate objective. In selecting a magnifying power for scientific work of any kind, it must be our endeavour to secure one giving the very best definition and penetration. These are two of the most essential qualities in every good objective; as on the first depends the truth of the optical image, and on the second the proper appreciation of its histological characters or structure. The defining power of an

objective, as I daresay most of you know, chiefly depends on the perfection of the corrections for spherical and chromatic aberration. A fourth of 120° and an eighth of 150° angular aperture may be looked upon as standard objectives. Greater angular aperture in *dry objectives* is not in my experience beneficial for medical microscopy. Increased angle of aperture frequently means impaired definition; the explanation of this is, that the manufacture of glasses with the utmost angle of aperture is attended with increased difficulties, and requires the most skilled workmanship. It is, therefore, a somewhat rare thing to meet with an objective of great angular aperture that *approaches* to *freedom from spherical aberration*. The *absolute* correction of chromatic aberration in an objective is of far less importance than the correction of spherical aberration, and just so is it less important to the histologist to have a colourless image than one with perfectly sharp definition. By this test will the student be inclined to estimate the value of any object glass. Increased angular aperture enables us to bring to our aid, when needed for the resolution of an object, a more oblique pencil of light than we otherwise could; and we should be prepared to employ every kind of illumination in our work. Indeed, we should not in any case pass judgment upon a structure until an exhaustive series of trials has been made upon it by every method of illumination. The cover-glass, as Amici long ago pointed out, exerts considerable influence on the perfection of the image. An object or preparation without a cover-glass gives a sharper image than one covered. A thick glass cover increases spherical aberration. In the immersion objective, the film of water removes or lessens many of the evils inherent to the *dry objective*. In the *immersion system* the stratum of water becomes, as it were, an adjustable film between the objective and the object, and greatly assists in the correction of spherical and chromatic aberration. As water is a stronger refracting medium than air, the reflection of the rays of light is much diminished at the upper surface of the cover-glass, and on its incidence on the objective; here, indeed, it is almost entirely neutralised, and hence a greater number of rays do actually contribute to the formation of the image. The thin film of water produces very nearly the same effect as enlarged angle of aperture. It also collects the peripheral rays from the object, and sends them on to assist in the formation of a more perfect image in the eye-piece. In short, the water becomes an integral part of, and a new optical element in, the combination, and doubtless assists in the removal of residuary secondary aberration in the lens. In awarding praise to the immersion system, I by no means intend to disparage the dry objective; I am convinced, however, it is to the immersion objective we must look for increased power and usefulness in histological work. I am glad to acknowledge that hitherto I have been quite content to employ what may now be called our old half-inch and a quarter, made by Andrew Ross more than twenty years ago; neither having a greater angle than 75°. Lately, I have used a $\frac{1}{4}$ by Dallmeyer (Andrew Ross's son-in-law), the

angular aperture of which is 120°. The excellent workmanship of this optician is well known and recognised, both on the Continent and in this country, and therefore needs no praise from me. I must, however, in justice to Mr. Dallmeyer, say that his ¼ objective works through almost any thickness of cover-glass, and its aberrations are equally well balanced for uncovered objects. It bears the highest power eye-piece, and gives a magnification of 1000 diameters in every way satisfactory; this perhaps, after all, is one of the best tests of a good objective, and proves beyond a doubt whether the angular aperture of the objective is brought to the maximum of utility, thereby increasing its value in the eyes of the pathological microscopist.

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

A vote of thanks to the President was passed for his very interesting address, and the meeting was resolved into a *conversazione*, at which many most valuable and interesting specimens were exhibited by Mr. Jabez Hogg, Dr. Pritchard, Mr. J. Needham, Messrs. White, Ackland, Atkinson, Baber, and Groves. Several of the makers, amongst whom were numbered Messrs. Baker, Horne and Thornthwaite, How, Pillischer, Ross, Swift, &c., very kindly assisted by the loan of microscopes, specimens, and lamps. The next meeting will take place on Friday, February 21st, when Mr. Schäfer and Dr. Pritchard have kindly promised to read papers.

At the second General Meeting of this Society, held at the Royal Westminster Ophthalmic Hospital, on Friday, February 21st, Jabez Hogg, Esq., President, in the chair. The minutes of the previous meeting were read and confirmed, after which the Secretary announced that six lamps had been purchased, as well as a cabinet, for the use of the Exchange and Cabinet Committee, and the President stated that the Committee had decided to provide "tea and coffee" in future at the meetings.

The President then called upon Dr. Pritchard to read his paper "On the Cochlea."

Dr. Pritchard gave a brief description of the general anatomy of the ear, and then proceeded to describe the rods of Corti, about the

exact form of which scarcely any two authors agreed. Deiters describes the form differently in the results of two investigations, Kölliker, Henle, and others agreeing with the later view, while Böttcher, Waldeyer, &c., give varying drawings, some of which approach more or less nearly to the true form. Dr. Pritchard described the rods as resembling the *hammers* rather than the *keys* of a pianoforte. In a lateral view they present two rods sloping towards each other; they consist of a shaft and two enlarged extremities, but differ in form in the two rows. The rods differ in their relative length in the two rows according to their position in the cochlea; at the base the rods of both rows are nearly equal, but they increase in length towards the apex, the outer increasing more rapidly than the inner, so that at the apex they are nearly twice as long as the inner. The rods vary in length from the $\frac{1}{500}$ to the $\frac{1}{120}$ of an inch. The proportion of rods in the two rows is about two in the outer to three in the inner, and altogether there are from 5200 to 3500 rods in the whole cochlea. Most authors, with the exception of Deiters, describe nuclei in the rods themselves, but on close examination they are found to be the nuclei of cells surrounding them. Dr. Pritchard considered that the older authors were correct in supposing that the rods of Corti were the means of distinguishing different notes, and that later writers were wrong because they had not noticed the graduation of the rods in length.

The President then stated that he thought Dr. Pritchard had established his points, and asked if he had tried staining the nerves with chloride of gold, and whether he had succeeded in setting up inflammatory action in the cochlea before the death of the animal experimented upon.

Mr. Crétin asked whether the animals examined by Dr. Pritchard were similar to those of which use was made by previous authors.

Mr. Schäfer asked why Dr. Pritchard had not mentioned the striation of the rods as it was to be seen in specimens teased up in bichromate of potash, and also in osmic acid preparations. He also stated that he believed the fact of the rods increasing in length to the apex of the cochlea was mentioned before by some German author. He considered that teased preparations were more satisfactory for examination than sections, and said he doubted with Helmholtz whether the rods vibrated, since they were firmly fixed the one to the other. Mr. Schäfer also asked whether cells existed between the rods, and said that he had found it easy to demonstrate cilia on the rods, and thought it probable that the cause of Dr. Pritchard not having seen them was that he used chromic acid in the preparation of his specimens; he also stated that he believed the cells described by Dr. Pritchard as nerve-cells were simply epithelium.

Dr. Mitchell Bruce asked how Dr. Pritchard prepared his specimens.

Dr. Pritchard, in reply, said he had tried the chloride of gold for staining the nerves of the cochlea, but not with any great success, and he had not succeeded in setting up inflammatory action. The

animals he had used were dogs, cats, rabbits, kittens, guinea-pigs, and lastly, a kangaroo, but he had found little difference in the form of the rods in any of them. He believed he had stated that the rods were to be split up into fibres. He had discovered the differences in the lengths of the rods in 1871. He said that he thought the rods might vibrate although fixed, and that it was hardly fair to compare them with a mechanical instrument. The cilia mentioned by Mr. Schäfer he considered to be the fibrillæ of the rods torn from the membrana section, and the cells regarded by him as epithelial he still considered to be nerve cells, and Dr. Beale had also expressed his opinion in favour of their being nerve-cells. Those interested in his methods of preparation he referred to a short article by himself in the 'Quarterly Microscopical Journal' for October, 1872.

A cordial vote of thanks were accorded to Dr. Pritchard for his interesting and valuable paper, illustrated as it was by numerous excellent models, diagrams, and specimens.

Thirty-three gentlemen proposed for membership at the last meeting were duly elected, and twenty-eight others were proposed for election at the next meeting.

The following presents were announced:

An Italian Medical Journal, from Signor A. Tigri.

Nine slides, from Mr. J. W. Groves.

The next meeting was announced to take place on Friday, March 31st, when Mr. E. A. Schäfer will read a paper "On the Structure of Voluntary Muscle," and Dr. F. Payne "Some Notes on Points in the Structure of the Omentum."

The meeting then resolved itself into a *conversazione*.

Dublin Microscopical Club.

October 17th, 1872.

The Rev. E. O'Meara showed *Navicula littoralis* (Donkin), as well as a number of diatomaceous forms obtained from a sample of Coorongite, or so-called Australian caoutchouc, forwarded by Professor Thiselton Dyer, who had given an account of it in the 'Journal of Botany,' 1872, pp. 103–106. The diatoms were *Denticula tenuis, Cymbella maculata* and *C. helvetica, Epithemia gibba* and *E. rupestris, Mastogloia Smithii, Nitzschia palea*, in abundance; also *Amphora minutissima, Pinnularia oblonga, P. gracilis, Cyclotella Meneghiniana, Pleurosigma Spencerii, Meridion circulare, Navicula gibberula, Cocconeis pediculus;* some fragments of *Stauroneis gracilis*, and probably *Campylodiscus clypeus*.

Dr. Macalister exhibited an uncommon variety of the rook-parasite, *Colpocephalum subæquale*.

Dr. Moore showed a preparation of the hairs from a plant from Richmond River, New South Wales, *Tyarettia argyrodendron*. They were stellate, the radii closely set like a rosette and forming an extremely pretty object.

The Rev. Maxwell Close exhibited a collection of mounted slides of Polyzoa from Western Australia, including species of the genera *Crisia*, *Salicornaria*, *Membranipora*, *Cellularia*, and *Catenicella*, mounted as opaque objects.

Mr. Archer drew attention to the shell or case of a seemingly novel form of *Acineta*, taken in North Wales (near Barmouth). It was hyaline, globosely vase-shaped, tapering below into a rather long and slender stalk (by which it had been attached to foreign objects), and contracted above into a long and rather narrow curved "mouth," with a slightly raised rim, through which had projected into the water the long and slender tentacula of the *Acineta*, seated within, and occupying only a small proportion of the cavity of its elegant *residence*. Round the base of the inflated portion of the case occurred externally two nearly parallel and closely set, thin, and rather broad annular projections, not, however, precisely straight, but somewhat curved or taking a slightly undulate sweep; there was a single similar one higher up, about halfway between these and the "mouth." It formed an extremely pretty object, but none similar seem to be alluded to in 'Pritchard.' Is this a known form?

Mr. Crowe exhibited the form, rare in Ireland (the present examples taken near Llanberis, North Wales; it also occurred near Dolgelley), *Tetrachastrum mucronatum* (Dixon) = *Micrasterias oscitans* (Ralfs), in part. This was clearly the form described by Rev. R. V. Dixon.[1] Mr. Crowe drew the attention of the Club to the characters as given by Ralfs. Now, as the examples lately taken were brought from Ralfs' "own ground," it is probable the plant that author had in view was the present. It is possible, therefore, that the distinction between Dr. Dixon's plant and Mr. Ralfs' may be not real, and the error, if error there be, would be due to the figure in 'Ralfs' not being quite true to nature; for, *à priori*, they were, no doubt, to be regarded as distinct things, and even yet, therefore, it would seem the question must remain an open one.

Dr. Reynolds showed his micro-spectroscope in use, the arrangement being that of Sorby; he exhibited the spectra of blood and of didymium, to show their curious resemblances, pointing out, however, at the same time, their distinctions.

Mr. Archer showed several rare Desmidieæ, some new to Britain, taken, in company with Mr. Crowe, on a recent brief excursion, and under the most adverse circumstances as regarded weather, in Westmoreland and in North Wales. Amongst these was *Micrasterias furcata* (Ag.), seen on this occasion by him for the first time, as it has not yet turned up in Ireland; it was taken near Ambleside (the only locality given by Ralfs is Llyn Gwernan, near Dolgelley). This

[1] 'Proc. Nat. Hist. Soc. Dubl.,' vol. ii, p. 202.

very elegant form, *before he had seen it*, Mr. Archer had been inclined to suppose a possible variety of *M. crux-melitensis* (Ehr.), Ralfs, but he must relinquish that supposition. Now that he had seen veritable examples, these two species appeared abundantly distinct; both occurred in the same locality; *M. crux-melitensis* (rare as Irish) turned up here and there in these gatherings, but still extremely sparingly. Mr. Archer showed simultaneously that rare and very beautiful British form *Micrasterias radiosa* (Ag.), taken, indeed, from the only recorded (Welsh) locality for it, that of Ralfs, at Llyn Gwernan, near Dolgelley; this species Mr. Archer had obtained from Connemara on one occasion (though omitted to be chronicled), but a subsequent search, on a more recent visit to the same site, did not yield it.—Mr. Archer further showed a fine Closterium named by Cleve, in compliment to himself, *Closterium Archerianum*, specimens of which were on the table, both from Galway and North Wales. When he first saw Lundell's description of this form he was a little dubious as to its undoubted distinctness from *Closterium lagoense* (Nordstedt) on the one hand and from *Closterium Cynthia* (de Notaris) on the other, but he could not now retain any doubt but that the handsome form with which his name had been associated was truly very distinct. Fortunately he had taken what was almost undoubtedly *Closterium lagoense* (Nordstedt) in his recent visit to Connemara, and was able to exhibit an example on this occasion, along with the figures given of both these species by Nordstedt and Lundell. *C. Cynthia* he had not by him to exhibit, but it appears a quite distinct thing—at least the form which Mr. Archer would at all events provisionally hold "in his mind's eye" as equivalent to it could not be mistaken for either of the foregoing. *C. lagoense* is far thicker in proportion in the middle, it tapers more rapidly than *C. Archerianum*, and the curvature is quite distinct. Still more does the curvature of *C. Cynthia* and that of *C. Archerianum* differ. There is a certain amount of outward resemblance of the latter to *C. Dianæ* (Ehr.), a common species in the British Islands, but it would be a waste of time to dwell on their distinctness, which can be made out even under a one-inch object-glass. Mr. Archer would defer the exhibition of certain other rare forms taken on the excursion, but could not refrain from showing conjugated examples of *Penium didymocarpum* (Lundell) which had been met with (he had also previously obtained examples from Connemara), as these were fugitive, and he might not for a long period have the opportunity again. Lundell's description is correct in every particular; and simple as this form may seem to be, it is one really (at least when conjugated) most marked and recognisable. —Amongst interesting minute algal forms taken at Llyn Gwernan (Dolgelley) were *Cosmocladium saxonicum* (de Bary), also *Dimorphococcus lunatus* (Al. Braun), as well as *Cœlastrum cambricum* (Arch.).

East Kent Natural History Society.

President.—The Reverend John Mitchinson, D.C.L., &c.

Honorary Secretary.—George Gulliver, F.R.S., &c.

January 2nd, 1873.

Mildness of the Weather at the close of the year 1872.—Colonel Cox read a paper recording the extraordinary weather, and its effects on organic and inorganic nature. In his garden, near Canterbury, on New Year's day was heard the song of the blackbird, song thrush, mistle thrush, robin, and starling. Of insects, the large and small tortoiseshell butterflies were sporting about, gnats playing in clouds in the garden, and many within doors; bees were flying about, lured from their hives by the warmth, in search of scanty food. He gathered at the same time in his garden a bouquet of roses, consisting of several varieties.

Beach Pebbles at Dover.—Though this place has a bad repute for good pebbles, Colonel Cox, after much experience there, found many fine specimens, among which were excellent Choanites, various Spongiadæ, and landscape stones; and of these and many others he displayed beautiful examples. Thus the microscopist at Dover should be in no want of splendid materials of this kind.

Stings of the Queen-Bee and Worker-Bee.—A paper by Major Munn was read, illustrated by drawings and specimens, on the honey-bee. The leading points he intended to prove are that, though the queen-bee has a sting and ejects venom therewith, she neither can nor does inflict a wound by penetration of the sting, but merely smears the poison on the stigmata of a rival queen during combats. Hence he insists on the importance of the facts to the apiarian and experimental physiologist, as the queen may be handled, even by the most delicate fingers, without the least fear of hurt by stinging. From specimens examined, at Major Munn's request, by Mr. G. Gulliver, of Pembroke College, Oxford, the queen's sting was found to be curved, larger, and slightly blunter, than that of the worker, and this last quite straight and very sharp; the queen's sting has three blunt barbs, and is about $\frac{1}{360}$th of an inch in diameter; the worker's sting has from eight to ten sharp barbs, and is about $\frac{1}{500}$th of an inch in diameter.

Pollen of Petasites fragrans.—This plant, which was imported from Italy in 1806, is becoming quite naturalised in many English and Irish places, and is abundant on a weedy waste on the north side of Canterbury Cathedral. The pollen is now extremely abundant, and its grains were shown to be beautiful objects for the microscope; their shape oval, each $\frac{1}{500}$th of an inch long and $\frac{1}{760}$th broad, and muricated, like the pollen of other *Compositæ*, on the surface, but becoming more or less spherical, with three scars apparent, when soaked in water or dilute sulphuric acid.

January 16th, 1873.

Manufacture of Agates at Oberstein.—Colonel Cox exhibited a magnificent collection of agates through all their phases, from their natural state up to their final polishing by the art of the lapidary. He had collected them at or near Oberstein, a primitive town in the Grand Duchy of Oldenburgh, and read an interesting and elaborate paper descriptive of the methods by which the stones are procured and prepared, until they appear in the well-known ornaments.

Combats of the Queens of the Honey-Bee.—A paper by Major Munn was read. Referring to his observations at the last meeting on the stings of the worker-bee and queen-bee, he now gave the result of his experimental observations on the deadly fights between the queens. When two of them were put together into a bottle they fought at once, and the conquered one soon gave the death-cry, a sort of *pip, pip*, and the conqueror, having let the conquered go, proceeded to settle her own wings and to clean her antennæ. In upwards of a dozen such combats the poison was fatally introduced into the spiracles under the wings, by a sort of smearing process, and produced death in about twenty minutes, though when the poison was only applied by the victor to the abdominal spiracles of the vanquished the latter languished for some hours. Sometimes a single queen, like a game cock, would be victorious in two fights, one immediately after the other.

Hermaphroditism and excellence as Bee-provender of Petasites fragrans.—Mr. Gulliver produced numerous specimens now in full flower in order to demonstrate the true sexual character of this species, and that it is, contrary to the current descriptions of the genus in the floras, truly hermaphrodite, and not "dicœcious or subdicœcious." Such is the early flowering of this plant, its multitudinous flowers, fragrance, and perennial luxuriant growth, as to be well worth the attention of bee masters. The pollen is so fully exposed on the exserted stamens and styles as to invite insects; and bees tempted out by a genial day in December, January, or February, might find a rich table when other food was scarce or absent. Hence *P. fragrans* would be pre-eminently valuable as the earliest provender for bees.

The Annual Meeting, January 28th, 1873.

The Rev. Canon Mitchinson, D.C.L., President, in the chair, supported by Lord Fitzwalter and Colonel Horsley, Vice-Presidents, and other members.

The usual formal business was transacted, and the president and other officers re-elected. The Society had never been in a more prosperous state, both as to the members and funds, than at present. A microscope, at a cost of not more than ten guineas, was voted for the use of the members, and five pounds additional to the annual sum for the purchase of books. The reports in the 'Quarterly Journal of Microscopical Science' were approved and

adopted for the Annual Report of the Society. The Rev. President delivered the address, which will appear in the Annual Report of the Society.

February 6th, 1873.

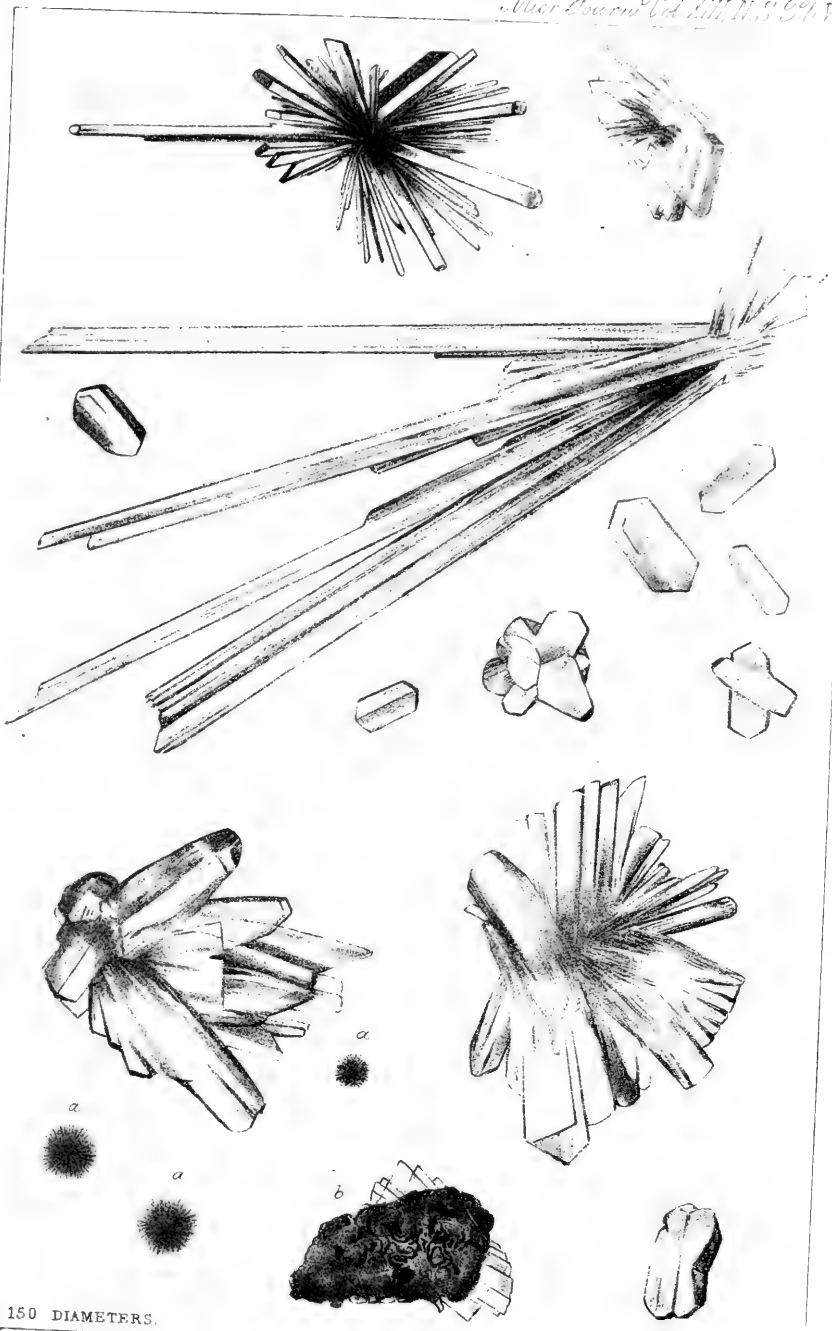
Polycystina from the Indian Ocean.—Colonel Horsley showed some beautiful specimens, all more or less perforated, and some prolonged into spires. They were mostly fossil, and some of them from the rocks of Bermuda, the tripoli of Richmond, Virginia, and the Marls of Sicily.

Hydra vulgaris.—Mr. Fullagar showed a number of specimens of this species from his aquarium, some with two or three buds, a few of the young with tentacles expanded and about to leave the parent stem, and others just commencing to bud; also several very minute hydras which had lately made their appearance in the water, and which he concluded were produced from ova deposited last autumn.

March 6th, 1873.

Crystal Prisms.—These are prismatic plant-crystals, quite different from though often confounded with raphides. Mr. Fullagar exhibited specimens of the prisms in the bulb-scale of the onion, in order to show how a very beautiful microscopic object is always at hand, and Colonel Horsley showed further that its beauty was much increased by polarised light. The crystals occur singly, very variable in size, lying across the tissue-cells, and in pretty crosses, soldered together at intersecting parts. According to Mr. Gulliver ('Annals of Nat. Hist.,' April, 1864) these prisms occur regularly in the bulb-scales of *Allium ascalonicum*, *A. Cepa*, *A. Porrum*, and *A. sativum*, but not in *A. schœnoprasum*, *A. angulosum*, *A. Moly*, *A. magicum*, and *A. ursinum*. Crystal prisms, not in crosses, may be well examined at any time in such officinal things as guaiacum—bark, quillaja, and the sweet-scented orris, and sometimes in company with raphides, in various fresh Iridacæ, such as *Iris germanica*, a very common plant in cottage gardens. Measurements and other details are given in the 'Popular Science Review,' vol. iv, p. 578.

Paramœcium feeding on Desmids.—To show how freely Paramœcium feeds at this season, Mr. Fullagar brought specimens from his aquarium, which were seen greedily injesting three species of *Closterium*.



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EXPLANATION OF PLATE VI,

Illustrating Mr. B. Wills Richardson's Observations on Xanthine.

Fig.

a, *a*, *a*. Bodies resembling spherules of urate of soda in appearance. Similar-looking bodies have been obtained by the writer by acting upon amorphous xanthine with nitric and with sulphuric acids.

b. Dark mass resembling amorphous xanthine, having crystals protruding from a portion of its surface.

With the exception of *a*, *a*, *a*, and *b*, the crystals represented in Plate VI are very transparent.

All the figures are magnified 150 diameters.

EXPLANATION OF PLATE V,

Illustrating the abstract of Dr. Cohn's "Memoir on Bacteria."

Fig.

1.—*Micrococcus prodigiosus* (*Monas prodigiosa*, Ehr.). Spherical Bacteria of the red pigment, aggregated in pairs and in fours; the other pigment Bacteria are not distinguishable with the microscope from this one.

2.—*Micrococcus vaccinæ*. Spherical Bacteria, from pock-lymph in a state of growth, aggregated in short 4—8-jointed straight or bent chains, and forming also irregular cell-masses.

3.—Zooglœa-form of Micrococcus, pellicles or mucous strata characterised by granule-like closely set spherules.

4.—Rosary-chain (Torula-form) of *Micrococcus ureæ*, from the urine.

5.—Rosary-chain and yeast-like cell-masses from the white deposit of a solution of sugar of milk which had become sour.

6.—*Saccharomyces glutinis* (*Cryptococcus glutinis*, Fresen.), a pullulating yeast which forms beautiful rose-coloured patches on cooked potatoes.

7.—Sarcina spec, *from the blood of a healthy man, **from the surface of a hen's egg grown over with *Micrococcus luteus*, forming yellow patches.

8.—*Bacterium termo*, free motile form.

9.—Zooglœa-form of *Bacterium termo*.

10.—Bacterium-pellicle, formed by rod-shaped Bacteria arranged one against the other in a linear fashion, from the surface of sour beer.

11.—*Bacterium lineola*, free motile form.

12.—Zooglœa-form of *B. lineola*.

13.—Motile filamentous Bacteria with a spherical or elliptical highly refringent 'head,' perhaps developed from Gonidia.

14.—*Bacillus subtilis*, short cylinders and longer, very flexible motile filaments, some of which are in process of division.

15.—*Bacillus ulna*, single segments and longer threads, some breaking up into segments.

16.—*Vibrio rugula*, single or in process of division.

17.—*Vibrio serpens*, longer or shorter threads, some dividing into bits, at * two threads entwined.

18.—'Swarm' of *V. serpens*, the threads felted.

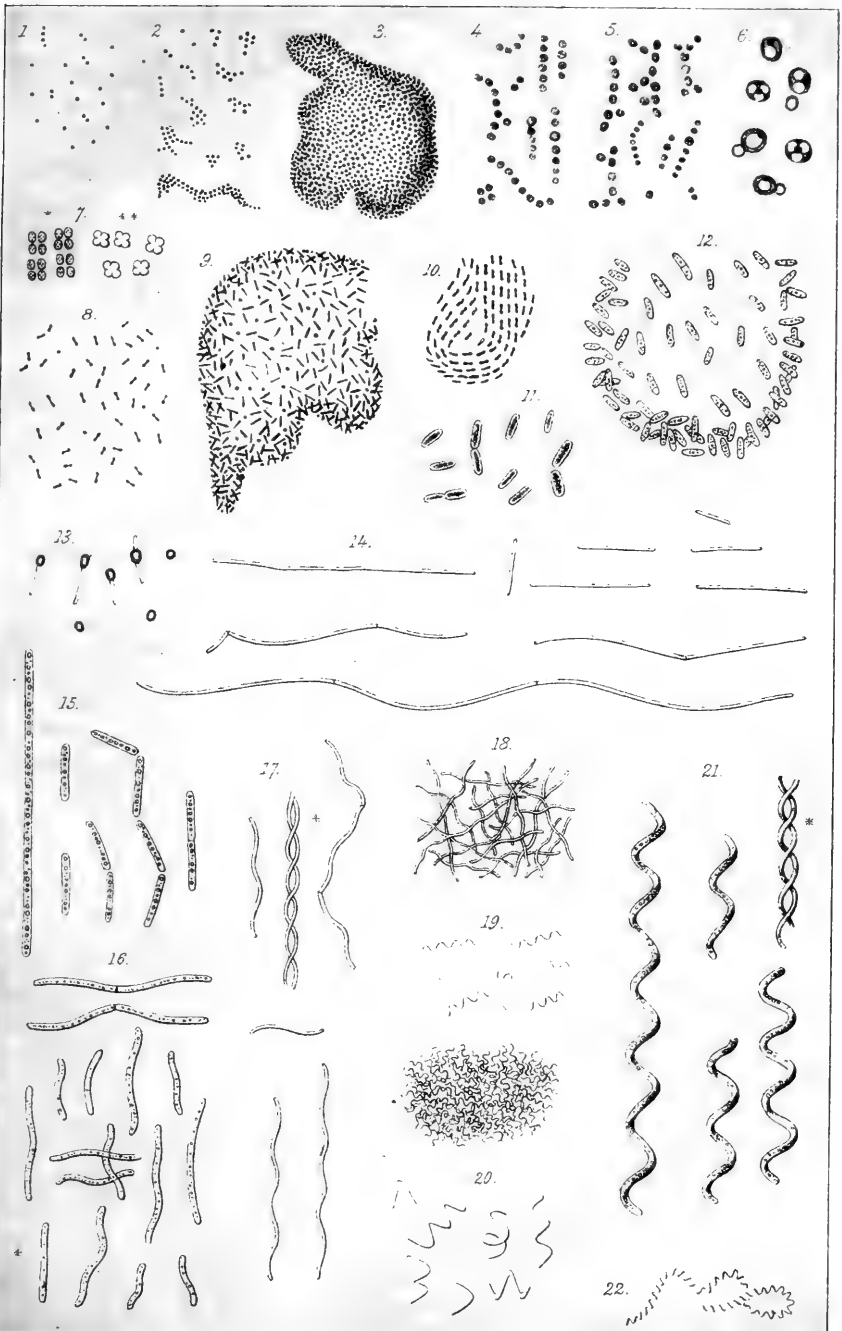
19.—*Spirillum tenue*, single and felted into 'swarms.'

20.—*Spirillum undula*.

21.—*Spirillum volutans*; * two spirals twisted around one another.

22.—*Spirochæte plicatilis*.

All the figures were drawn by Dr. Ferdinand Cohn with the immersion lens No. IX of Hartnack, Ocular III, representing a magnifying power of 650 diameters.



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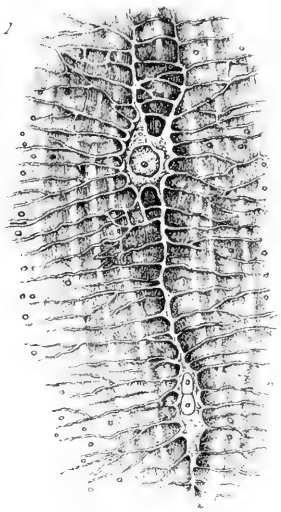

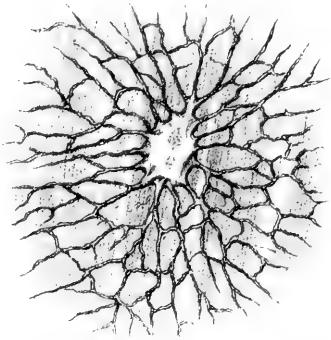

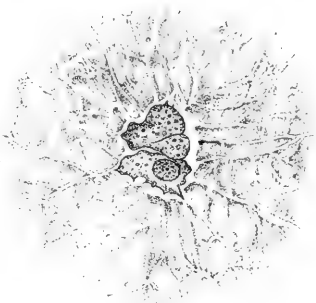

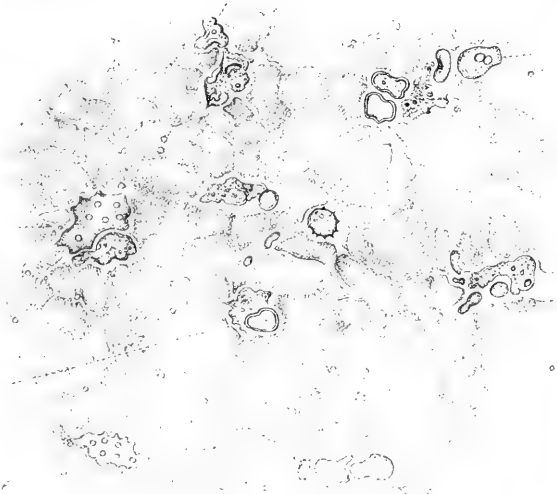

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DESCRIPTION OF PLATES VII & VIII,

Illustrating Dr. Heitzmann's Memoir on Bone and Cartilage.

PLATE VII.

FIG.

1.—Two bone-cells from the uninjured scapula of a cat. Prepared with chromic acid. Magnified 800 diam.

2.—Bone-cell from a cat's scapula, crushed with the bone-forceps, after three days' inflammation. Chromic acid. 800 diam.

3.—Bone-cell, with marks of division, from a dog's scapula, in the neighbourhood of the wound, produced by breaking out a piece. Chromic acid. 800 diam.

4 and 5.—Bone lacunæ, containing blood-corpuscles, from the compact substance of a dog's tibia injured by the actual cautery, after eight days' inflammation. Chromic acid. 800 diam.

6.—Bone-cells from the same object as figs. 4 and 5, in two planes, drawn with topographical accuracy. Forms of the hæmatoblastic substance:—*a*, parietal border; *b*, darkly outlined lumps; *c*, pale grey discs; *d*, completely formed blood-corpuscle; *e*, lamella perforated with small vacuoles. 800 diam.

PLATE VIII.

7.—Section of cartilage, coloured with nitrate of silver, from the edge of the external condyle of the femur of a young dog. 800 diam.

8.—Section of cartilage, coloured with chloride of gold, from the lateral surface of the external femoral condyle of an old dog. 800 diam.

9 and 10.—Forms of cartilage-cells from close to the calcified portion of the articular cartilage of the femoral condyle of young rabbits, wholly from vertical sections. The cells show transitional forms of hæmatoblastic substance up to the formation of almost perfect blood-corpuscles. From preparations partly fresh, partly decalcified with hydrochloric acid. 800 diam.

MEMOIRS.

A Résumé *of* Recent Views *respecting the* Nature *of* Lichens. By W. Archer, M.R.I.A. (With Plates IX and X.)

A novel hypothesis respecting the nature of Lichens, which was first propounded by Prof. Schwendener of Basel, and has since to a greater or less extent been adopted by other experienced observers, has for some time past been a subject of controversy between them and botanists of the older school. The "gonidia question," as it is called, has been discussed in several of the continental journals, and a *résumé* of some of the papers on both sides will probably possess some interest for English readers. Briefly Schwendener's view is that the growths which we know as "Lichens" are not independent plants, but ascomycetous fungi, to which certain unicellular or filamentous algæ serve as host-plants, and, therefore, that these latter have hitherto been erroneously regarded, under the name of "gonidia," as special organs of "Lichens." Hence each representative of the class is a compound organism made up of two primarily distinct elements.

Lichens, according to Schwendener's view, are, in fact, colonies of multitudes of algal individuals, involved in a more or less closely interwoven filamentous network of hyphæ belonging to the fungal parasite, and, which is very remarkable, incited by it to a more vigorous growth. Sometimes, indeed, these imprisoned algæ may, in the course of generations, though "growing," vigorously become altered in form or diminished in size, so as even to lose, to a great extent, their former identity.

This somewhat startling view was announced by the author at the conclusion of his memoir, 'On the Lichen-thallus,' published in 1868.[1] He there urges the following considerations in support of it:—

[1] Schwendener: "Untersuchungen über den Flechtenthallus," in Nägeli's 'Beiträge zur wiss. Botanik,' heft 4 (1868), p. 195. He had already begun to express similar views at the Congress of Naturalists at Rheinfelden in 1867.

1. That as yet the genetic relationship of the gonidia to the hyphæ has nowhere been directly proved, but only assumed on anatomical grounds, whilst the anatomical connection may possibly depend on "copulation."

2. That the membranes of the gonidia, in respect to their chemical behaviour, are quite distinct from the hypha-membrane; whilst the former has an algal, the latter has a fungal reaction.

3. That the different gonidia-forms, in respect to structure and mode of increase, correspond to parallel types of unicellular and filamentous algæ.

4. That the germination of the spore, not having been observed further than the foundation of a protothallus, may probably be due to the want of the cooperation of the requisite alga; in Tulasne's experiments, which lasted for months, the green cells may have arrived from without.

5. That between Lichens and *Pyrenomycetes* there is the most striking agreement in regard to the development of the fructification and formation of spermogonia.

The author subsequently urges that the arguments against his view reduce themselves to but two:—

1. In many cases where still undivided gonidia occur in tissues thickly interwoven, or even without interstices, it is difficult to comprehend how they could get there. It would seem to presuppose that a progression of the gonidia through a more or less dense fibrous mass must take place, and this often in a direction contrary to the action of gravity which seems scarcely possible.

2. The assumption that the development of the thallus and of the apothecia in the majority of the shrubby lichens, or rather of the fungi in question, should be dependent on the *same* host-plant (*Cystococcus humicola*), whilst parasitic fungi usually occur on very *different* host-plants, must *à priori* be regarded as improbable (but is, he adds, in no way impossible). Moreover, the complete covering up of the host-plant, which continues to vegetate vigorously, and even more vigorously than before, would further be exceedingly remarkable.

With regard to the statement in paragraph 1, that as yet the genetic relationship between the gonidia and the hypha has nowhere been directly proved, it is well known that Schwendener's earlier view was quite the other way, and was supported by figures,[1] and established, as he himself

[1] Schwendener: loc. cit., Heft. ii, p. 125, t. i, fig. 18, t. v, fig. 6.

thought, by the previous observations of Bayerhoffer and Speerschneider. He then pointed out, indeed, that the gonidia may be frequently found in connection with the hypha, and he held that the gonidia were produced only from the intermediate joints of the hypha, never from the terminal cells; so long as they merely appear as small expansions of the joints, they could be with difficulty distinguished from young branches. Under such circumstances it is explicable that gonidia which are not yet coloured green, but still distinctly recognisable as gonidia, are only rarely met with.

Nor are numerous similar opinions and statements wanting from other observers. It is plainly laid down by de Bary[1] that the green gonidium originates, as first shown by Bayerhoffer, by the expansion of the end of a short lateral branch of the hypha which becomes shut-off as a globular cell and acquires a green colour. Once formed it increases independently by division, and a number of the gonidia eventually lie without stipites in the interstices of the lichen-tissue. Again, Caruel is stated to have found that in *Collema pulposum* certain cells of the germ-tubes produced by the spores sown on a moist sponge, became divided into a series of globular joints which assumed the bluish-green colour of the gonidia. Koerber, Sachs, and likewise Caruel, are further quoted on the same side, and as stating that the joints of the gonidia-chains became on their part branched, putting forth colourless hyphæ, thus showing the reverse process to that of the heteromerous lichens. But de Bary himself could not confirm this. However, in *Plectopsora* a connection between gonidia and hyphæ was easy to find; rather large cells were seated upon the end of a lateral hypha-branch, and were, in fact, primary gonidia. Sachs[2] gives some observations on *Collema*, and holds to its genetic relationship with *Nostoc*, the latter being but a young or undeveloped condition of the former. He states that he had observed the growing off of undoubted hyphæ from the heterocysts, and the gradual conversion of the *Nostoc* into *Collema;* also he asserts the interstitial formation of a moniliform series of cells from the ordinary hypha, and, on the other hand, speaks of minute examples of *Nostoc* on the surface of the *Collema*-thallus, which he holds could not originate but in some way from the gonidia of the *Collema*.

[1] De Bary: 'Morphologie und Physiologie der Pilze, Flechten und Myxomyceten,' pp. 258, 263, 264, 265.

[2] See Sachs: "Zur Entwickelungsgeschichte des *Collema bulbosum*, Ach.," in 'Bot. Zeitung,' 1855, p. 1.

Itzigsohn[1] holds that young hyphæ are put forth, not from heterocysts, but from ordinary gonidia of the moniliform chains.

The consideration adduced by Schwendener, under paragraph 3, to show that the different gonidia-forms, as respects structure and mode of increase, correspond to parallel types of unicellular and filamentous algæ, had been long ago drawn attention to by Thwaites.[2] That author seems, however, unlike Schwendener, to have recognised in this only a parallelism, as it were, not an identity of the "gonidia" with the "algæ"—he saw in it, in fact, only "a typical character of essential structure binding together numerous species of various forms, and enabling us to distinguish at once in other species resemblances of analogy from those of affinity."

Itzigsohn[3] and Hicks[4] have also pointed out the similarity of certain lichen-gonidia to free algal forms; the latter identifies a number of unicellular algæ with the gonidia set free from *Cladonia;* he does not appear to have instituted experiments in the direct culture of the gonidia, and has, it is hardly to be doubted, confounded as different developmental states of one and the same thing several distinct forms that often live together.

Almost concurrently with Schwendener's novel theories there appeared papers by Famintzin and Baranetsky on the independent life of certain lichen-gonidia.[5] These observers succeeded in cultivating the chlorophyllaceous gonidia obtained from *Physcia, Evernia,* and *Cladonia,* independently of these lichens; some produced zoospores, whilst others continued to increase by vegetative growth, and they inferred that these free-living cells were identical with Nägeli's algal genus *Cystococcus.* The conclusion drawn by these

[1] Itzigsohn, 'Bot. Zeit.,' 1855. See, however, Schwendener on this point (*infra*).

[2] Thwaites: "On the Gonidia of Lichens," in 'Ann. of Nat. Hist.," 2nd ser., vol. iii, p. 219 (1849).

[3] 'Bot. Zeit.,' 1854, p. 521.

[4] 'Quart. Journ. Micr. Sci.,' N. S., vol. i, pp. 15, 90.

[5] A. Famintzin and J. Baranetsky: "Beitrag zur Entwickelungsgeschichte der Gonidien und Zoosporen-Bildung bei *Physcia parietina* D.N.," in 'Bot. Zeit.,' 1867 (translated into French in the 'Ann. des Sc. Nat.,' ser. 5, 1867, pp. 137—144); also same authors: "Zur Entwickelungsgeschichte der Gonidien und Zoosporen-Bildung der Flechten," in 'Mémoires de l'Acad. imp. d. Sciences de St. Petersbourg," ser. vii, tome xi, No. 9, 1867; 'Ann. d. Sc. Nat.,' viii, pp. 137—144, t. xvi; also Baranetsky, "Beitrag zur Kenntniss des selbstständigen Lebens der Flechtengonidiem," in 'Mélanges Biol., Bulletin de l'Acad. de St. Petersbourg,' t. v, 1868, p. 473, published also in Pringsheim's 'Jahrbücher f. wiss. Bot.,' Bd. vii, 1869.

authors was that the formation of zoospores is a character of the lichens with chlorophyllaceous gonidia in common with algæ, and that Nägeli's *Cystococcus* was no independent alga, but, in fact, a phase of the lichens in question. They also tried experiments with the cultivation of phycochromaceous gonidia obtained from lichens of a different nature, *Peltigera* and *Collema*, and announce that they found these capable of continuing an independent life. They regard the free gonidia of *Petligera* as identical with the alga *Polycoccus punctiformis*, Kütz. Itzigsohn also experimented on *Peltigera*-gonidia,[1] and states that he obtained during cultivation forms not distinguishable from *Glœocapsa monococca*, Kütz.

In a short paper on the gonimic development of *Collemaceæ*,[2] Nylander arrived at the conclusion, after his researches on this matter, that the genus *Nostoc* is truly in part, if not altogether, a beginning or developmental state of *Collema*. This view long held, or at least suggested by many, is quite opposed to that of Schwendener and Reess, who hold, as is seen, that *Nostoc* represents an independent alga, at one time growing normally as such, at another made the home of a parasite, and modified by it into a *Collema*.

Again, in 1868, shortly after the previous paper, Schwendener resumes the subject in a short communication,[3] in which he refers to Famintzin's, Baranetsky's, and Itzigsohn's observations as complete evidence that the "gonidia" of the lichens, which were the subject of their observations, are identical with the unicellular and filamentous algæ indicated. But, whilst Famintzin and Baranetsky would strike out from the list of algæ the freely vegetating gonidia, the author, on the other hand, once more asserts his view, which he now thinks beyond doubt, that these *are* in truth independent algæ—that the lichens themselves are not independent plants, but ascomycetous fungi, to which the former perform the part of host-plants. In this communication he relies upon the following observations as proving his views: (1.) The fungal filaments which he had found penetrating into *Nostoc*-colonies, and branched in their interior; amongst these colonies there were some larger somewhat lobed masses, not to be distinguished from *Collema* without a pellicle, as well as

[1] Itzigsohn: "Kultur der Glaucogonidien von *Pelligera canina*," in 'Bot. Zeit.,' 1868, p. 185.

[2] Nylander: "Circa Evolutionem gonimicam Collemaceorum Notula," in 'Flora,' 1868, p. 353.

[3] Schwendener: "Ueber die Beziehung zwischen Algen und Flechtengonidien," in 'Bot. Zeit.,' p. 290 (1868).

others of greater or less size, but without fungal filaments, though manifestly of one and the same species. (2.) Colonies of *Glœocapsa*, which he regards as transitions to the *Omphalaria*-thallus, with numerous "copulations" between fungal-threads and green cells. (3.) Cases in which fungal-threads penetrated into the sheaths of *Rivulariæ*, the filaments of which, in consequence, present zig-zag bendings; the gelatinous envelope becomes thicker, the intercalary division of the green cells still proceeds, until the whole passes over into an irregularly-lobed mass forming a thallus; the author thinks that these agree anatomically with *Racoblennaceæ*. (4.) The many-jointed, sometimes straight, sometimes zig-zag chains in the thallus of undoubted *Racoblennaceæ* (with fruit) which agree in all essential points with the foregoing, showing here and there the (defunct and somewhat altered) heterocysts. Lastly (5), his observations on individuals of *Chroolepus* and *Cystococcus*, embraced and involved by fungal-threads. (See, for examples, accompanying Pls. IX and X.)

In the following year (1869) there appeared from Schwendener's pen a further treatise on this question,[1] in which he continues to maintain the position previously taken up by him—that the lichen-gonidia are really independent organisms—that is to say, algæ, which vegetate as host-plants for the parasitic hyphæ which build up the lichen-thallus. In this work the author has enumerated the various algal-types he regards as constituting the "gonidia," and describes their occurrence in the different lichen genera. Of these types he makes eight in all, falling under two sections, according to their chlorophyllaceous or phycochromaceous cell-contents. These, owing to the length of the treatise, must be only briefly enumerated.

First Series.—Algæ, with bluish-green contents (*Phycochromaceæ, Nostochinæ*).

Type I. *Sirosiphoneæ*.—Owing to their higher organization regarded as independent plants, Schwendener thinks that these retain, as lichen-gonidia, a larger share of their individuality than do the algal-types of other groups; this is shown by the retention of their longitudinal growth, of their apical increase, of their heterocysts (though as gonidia these become diminished in number, and even disappear), and of their normal ramification. The author argues that, in these forms, a genetic union of the gonidia and hyphæ is impossible. New gonidia are formed at the apices and before the hypha reaches them, so that any possible connection must at least

[1] Schwendener: 'Die Algentypen der Flechten-gonidien.' Basel, 1869.

date back to the germination of a spore, and even at that period it has not been observed. Here of course belong *Ephebe, Spilonema, Gonionema, Polychidium.* (See Pl. X, fig. 9.)

Type II. *Rivularieæ.*—Algæ of this type are regarded as less able to maintain their individuality in the capacity of gonidia. In favorable cases they retain their forms, but in others they suffer modification. Thus the apical growth is arrested, and the heterocysts become more scanty or altogether wanting. Schwendener gives at some length observations on *Thamnidium Willeyi,* Tuck., which he regards as not generically distinct from *Lichina.* Its whole gonidial system consists, he says, of nearly unaltered *Rivulariæ* imbedded in the outward region of a shrubby thallus, some having distinct sheaths and long whip-like filaments. (See Pl. X, figs. 16, 17.) The gonidia of *Lichina* and *Racoblenna* are of this type. (See Pl. X, fig. 18.)

Type III. *Scytonemeæ.*—Here the algal-type becomes in the gonidial state much less recognisable, apical growth ceases, and heterocysts disappear. An exception to this, however, is shown by *Ephebella Hegetschweileri,* also by the *Cephalodia* of *Stereocaulon;* it is in the highest degree probable that *Scytonema* is a gonidia-former for *Heppia* and *Porocyphus.* (See Pl. IX, figs. 1—8; Pl. X, figs. 12, 13.)

Type IV. *Nostochaceæ.*—Here the algæ concerned retain all their peculiarities of form and growth as gonidia-formers, excepting those, of course, which relate to the nature of the aggregate colony. Only terrestrial and not aquatic forms can manifestly play such a part; those which live submerged are protected from the attack of the parasite. *Nostoc* itself is most frequently the gonidia-former appertaining here. Schwendener asserts that he has seen and proved the transformation of a *Nostoc* into a *Collema* by the penetration of certain fungal-threads; the origin of the latter in his instances he does not state. (See Pl. X, figs. 19—21.) *Polycoccus punctiformis,* erroneously as he holds referred to *Chroococcaceæ,* is a true *Nostoc,* and forms the gonidia of *Leptogium subtile, Pannaria brunnea, Peltigera canina.* Certain species of *Stereocaulon-Cephalodia* are formed from *Nostochaceæ.*

Type V. *Chroococcaceæ.*—Species of *Glœocapsa* are the gonidia-formers for *Omphalaria* and *Enchylium;* the gonidia of *Phylliscum* are made up of colonies simply unaltered of *Chroococcus turgidus,* Näg. *Anacystis, Polycystis, Cœlosphærium,* which live in water, do not come into the question, being inaccessible to the fungus. *Synechococcus, Glœothece,* &c., in which the cell-division takes place in *one* dimension of

space only, have not become known as gonidia-forming representatives. "As to the behaviour of the gonidia-formers referred to within the lichen-thallus there is little to say; they retain essentially their form and mode of growth. It is striking that almost every cell becomes copulated with a hypha-branch or with a bifurcation of the primary stipes-cell." In *Omphalaria* and *Enchylium* the author states that he has repeatedly seen the penetration of the fungal-threads into the colonies of *Glœocapsa*, certain branches of which "copulate" early with the cells of that alga.

Second Series.—Algæ with chlorophyll-green contents:

Type VI. *Confervaceæ*.—The greater number of these being aquatic can only exceptionally occur as gonidia-formers; as, for instance, *Cœnogonium* and *Cystocoleus*, which the author holds to represent nothing else than algæ involved by the fungus, retaining, however, their typical forms and mode of growth.

Type VII. *Chroolepideæ*.—These take part in the formation of gonidia in only a small proportion of lichens (*Graphideæ*, *Verrucarieæ*), retaining in such condition their form and mode of growth. According to the author *Roccella*-gonidia are simply *Chroolepus*-forms. (See Pl. X, fig. 14.)

Type VIII. *Palmellaceæ*.—If very few of these algal-forms seem adapted to serve as lichen-gonidia, still those few play a very large part in the gonidia-question, *Cystococcus* offering itself as the gonidia-former for a large number of the shrubby and foliaceous lichens, as well as *Pleurococcus vulgaris* and *Protococcus*. Manifestly only such as live unsubmerged are capable of becoming gonidia-formers; many, however, of these have not yet been observed in the gonidial state.

Schwendener concludes this remarkable memoir by observing that the algal nature of the lichen-gonidia is thus established for a series of cases, is extremely probable in others, and is in no case improbable. Lichens are accordingly ascomycetous fungi parasitic upon algæ, whose assimilation and asexual increase are accomplished by means of the gonidia. The only fact, he considers, which can be at all advanced in favour of the genetic connection of the hyphæ and the gonidia is the existence of the stipites or stalk-cells. This, he holds, is completely deprived of force when it is known that "similar stipites are formed by copulation, a mode of formation which has been demonstrated with absolute certainty. On the other hand, the development of the gonidia from the end-cells of short hypha-branches has as yet been observed by no one, but only inferred from the existence of the stipes-cells." He

maintains, as a consequence, that the view that the gonidium is a self-developed organ of the lichen is destitute of all foundation in fact.

He goes on to state that the setting free of the gonidia, as a result of soredia-formation, has been just as little established by observation. The soredia themselves, as is well known, are not free, but involved by the hyphæ. If, now, these latter should perish in an atmosphere which is too moist, that does not prove that *Cystococcus* individuals, for example, vegetating on the barks of trees have become free in this manner. That must still indeed be observed. At any rate (urges the author) the setting free of the gonidia is a question of subordinate nature, the solution of which would, as regards the main point, decide neither for nor against it.

Of more importance, on the other hand (he admits), is the other point lately put forward by Baranetsky, namely, the outgrowth, stated to have been observed, of the gonidia into hyphæ. He regards these statements to have been controverted by his own previous researches. He insists too that the perfect agreement of the gonidia-forms with parallel algal types is a fact that, by the interpretation of Wallroth (including, of course, Hicks, Famintzin, and Baranetsky, and others), remains absolutely inexplicable.

Although the gonidia are not to be regarded as *organs* of the Lichens, Schwendener holds that they in no way lose their systematic significance. We know how fastidious fungi, as a whole, are in their choice of a host-plant; some will only accomplish their development on one particular species, whilst on another they may at most only enter the first condition of germination. So with the Lichens. It is indubitable that the greater number of these are dependent on quite definite algal species, and that a substitution of different algal genera does not occur. Hence it happens that systematically related lichens very frequently make choice of systematically related host-plants (e. g. *Ephebe, Collemaceæ, Omphalariaceæ, Racoblennaceæ*, and the series of shrubby and foliaceous forms involving *Cystococcus*). In this lies the significance of the gonidia for systematic purposes. The attention which they deserved as supposed organs they still deserve as host-plants.

Physiologically considered, the gonidia remain now as before instruments of assimilation and of asexual increase. They are not, indeed, brood-cells in the ordinary sense, because they have not the power in themselves to form a thallus; they are, however, an essential constituent of the "brood-clusters." Every "brood-cluster" is a little daughter

colony, to which the gonidial layer contributes at least *one* green cell; it is the fungus, on the other hand, which furnishes the hypha-covering which surrounds it.

The gonidia are incontrovertibly the most important, but not the only, ministers of nutriment for the lichens; for many lichens flourish, as is known, only on the bark of pines, others on forest trees, others again upon decaying or dead wood, &c., and this must depend on chemico-physiological conditions with which we are not at present exactly acquainted. One may say, to employ a trivial illustration, that the gonidia furnish the ordinary food; the substratum, on the other hand, the condiments.

The lichens are, so far as they vegetate on trees, wood, and the products of their decay, double parasites. These may be regarded, with respect to the gonidia-formers, on the one hand, as *Algophytes*, or with respect to the substratum, on the other, as (according to circumstances) *Epiphytes*, *Endophytes*, or *Saprophytes*.

Finally, the author adds a word as to the name "Lichens." He thinks there is no valid reason to reject it for the future. Lichenology has its special history and literature: why should not the objects of which it treats continue to bear their customary name?

Following Schwendener, Dr. Nylander[1] expressed himself (in 1870) upon this question, in a brief communication, as very decidedly opposed to the Schwendenerian view, dwelling on the considerations that such an unnatural existence as the lichen-gonidia must pass is not at all consonant with their algal nature, that it has no parallel in nature, and that anything physiologically analogous to such a "parasitism" occurs nowhere else. He also inquired as to what stands in the way of the acceptance of the view that there may be certain really independent algæ or states of algæ similar or nearly similar in form and structure to the lichen-gonidia? He puts himself rather on the side of Hicks, Itzigsohn, Famintzin and Baranetsky, in supposing that the unicellular algæ which are assumed as, or appear to be, identical with some lichen-gonidia, may be in reality, at least in part derived from Lichen and continuing abnormally to vegetate.

The interesting paper by Professor Reess[2] has been already mentioned in this Journal, in a communication on "A Minute

[1] Nylander: "Animadversio de Theoria gonidiorum Algologica," in 'Flora,' 1870, p. 52.

[2] Reess, "Ueber die Entstehung der Flechte *Collema glaucescens*, Hoffm., durch Aussaat der Sporen derselben auf *Nostoc lichinoides*, Vauch," in 'Monatsb. der k. Akad. d. Wiss. zu Berlin,' Oct., 1871, p. 523,

Nostoc with Spores,"[1] &c. It is, therefore, unnecessary to recapitulate the views deduced by him from certain experiments in "sowing" *Collema* spores upon the substance of *Nostoc;* these germinated, produced a hypha penetrating into and spreading within the *Nostoc*-jelly, and the author concluded that he had thus witnessed the production of a *Collema* by this artificial combination of the two elements—a process which is a matter of every-day occurrence in nature, and, in point of fact, is simply the mode of origin of every "*Collema*." If no hypha comes to the *Nostoc*, it remains a "*Nostoc;*" if it should be so visited (and the inoculation successful), it becomes by-and-by transformed into a *Collema*. Whilst, then, the author claims to have thus established Schwendener's views as regards this particular type, he calls in question the figures (see Pl. X, figs. 19—21) given by Schwendener himself from his own specimens in evidence of his views, as being truly *ad rem*, as he strongly doubts, indeed distinctly denies, that the fungal filaments depicted as attacking a *Nostoc* by Schwendener are *Collema*-hyphæ, or even lichen-hyphæ at all, but takes them rather to be those of a true mould. In a subsequent communication Schwendener takes up the whole matter as brought forward by Reess, and discusses his objections and defends his own figures, and the conclusions he had drawn from them. He further touches upon the objections of other authors. That communication it is desirable to give the readers of this Journal the opportunity of perusing in full. It is not probable that Schwendener would consider a solitary instance of a *Nostoc* with "spores" as in itself of any particular bearing on this question, as certain other algæ which produce similar spores are, in his opinion, not exempt from being compelled to do duty as gonidia.

To Schwendener's hypothesis von Krempelhuber[2] declares himself as quite opposed, regarding it as unnatural and forced; he holds indeed that the evidence proves, not the lichen parasitism contended for by Schwendener, but rather the resemblance of certain lichen-gonidia to certain lower algæ, and even their identity with them, which is equally, indeed, maintained by Schwendener; but he interprets the fact differently, and the conclusion he draws is quite in accordance with that put forward by Famintzin and Baranetsky.

In reference to the considerations adduced by Schwendener in favour of his conception, enumerated above (p. 218), von

[1] W. Archer, "On a Minute Nostoc with Spores, &c.," 'Quart. Journ. Mic. Sci.,' n. s., vol. xii, N.S., p. 367.

[2] v. Krempelhuber: 'Geschichte und Litteratur der Lichenologie,' III. Bd., 1872.

Krempelhuber urges, as opposed to the first and third, that there are no conclusive reasons against the assumption that the lichen-gonidia may be self-developed organs of the lichen proper rather than algæ, nor that these gonidia can, after separation from the lichen, continue to vegetate, and so be mistaken for unicellular algæ.

Referring to Schwendener's second consideration, he urges that it is quite without importance that the membrane of the gonidia reacts differently from that of the hyphæ, inasmuch as the membranes of the asci originating from the latter exhibit a different reaction therefrom, and, indeed, the same reaction as that of the gonidia-membrane.

As to Schwendener's fourth point, he refers to the observations of Tulasne, Speerschneider, and Gibelli upon the development of the spores. They appear to have found clusters of gonidia on the first thallus-rudiments produced by the germinating spores; and although they did not succeed in directly establishing that the development of these gonidia actually took place from the hyphæ, still from their regular appearance upon the latter, the probability that this was their origin cannot be denied, or at least cannot be refuted by random assertions, such as, "In Tulasne's experiments the green cells may have arrived from without."

Upon the fifth point referred to by Schwendener, von Krempelhuber urges that the alleged agreement between certain lichens and *Pyrenomycetæ* can hold good, as regards the former, only as to one small group taking the lowest place in the lichen system, and standing near the (limits which can never be sharply drawn) between lichens and fungi. This is consequently of no essential importance. Further, he urges that the presence of the gonidia is not the only hitherto known distinguishing characteristic between lichens and such fungi as show an agreement in their fructification.

He then touches upon the observation brought forward by Schwendener of certain fungal threads penetrating from without into *Nostoc*- and *Glœocapsa*-colonies and the supposed resulting transformation of these into *Collema*, as being limited at best to but two cases, and urges these observations as too imperfect to be of value [he was not then aware of Reess's experiments], whilst there are no such observations referred to for lichens bearing chloro-gonidia. He also takes it as probable that these questionable threads may have owed their origin to true fungi (moulds), and not to lichens proper, and hence that the cases seen by Schwendener may have been really quite different from the beginnings or prolifications of *Collema*.

Von Krempelhuber then adds some considerations which seem to him of importance, as opposed to the Schwendenerian view:—

1. A number, by no means small, of the shrubby and foliaceous species of lichens are, as is known, cosmopolitan, occurring in all parts and zones of the globe, and, indeed, not rarely in great quantity, and for the most part in places little or not at all adapted for the occurrence of algæ. The gonidia of these lichens are everywhere alike. Now, if these gonidia be not developed from the lichens themselves, but are really algæ, it must be assumed that these have quite as wide and general distribution as the lichens, and that, for example, the famous *Cystococcus humicola* must occur in the extreme north and south, as well as under the tropics, on the highest mountains and the deepest vallies, on the bark of trees, on rocks, and on the surface of soil. He urges that such an extraordinary distribution of a unicellular alga, so far as knowledge goes, has never yet been observed; it may be, indeed, possible, but it certainly is in a high degree improbable.

2. Schwendener does not attempt to explain the source which supplies the (fungal) hyphæ, which, in order to originate a lichen, involve the green algal-colonies; von Krempelhuber assumes that he would understand the (mycelioid) threads produced from the germinating lichen-spore to be the hyphæ of the first thallus-rudiments; but the assumption that the fully grown lichen sends out hyphæ from any part of its thallus in search of its host alga, and that from such hyphæ a lichen-thallus with fructification and spores afterwards originates, is, indeed, inconceivable. There are, urges von Krempelhuber, as is known, species of lichens which in many regions and in whole countries never fructify, and whose propagation can, therefore, only be carried on by means of the soredia. Now, it is indeed very improbable that the colourless hyphæ of such lichens, not proceeding from spores, are able in themselves alone to form a new lichen-thallus, and that they acquire this faculty by interposition of the gonidia, if these are nothing else but algæ, and that they draw from them, in part, the necessary nutriment for their development into a new thallus. On the other hand, it is much more conformable to nature that the gonidia as self-developed organs of the lichens should, like the spores, enable the hyphæ proceeding from them to propagate the individual. The ordinary hyphæ of the lichen-thallus would be just as little able, in themselves alone, to serve for propagation as the hyphæ from the pileus or stalk of an Agaricus. It is, indeed, possible that the process of soredia-formation may be

quite different from what Schwendener has described it, and that instead of a fibre from a hypha of the thallus penetrating a soredium-forming gonidium attached to the hypha by a short stalk, ramifying between the dividing cells reaching the surface of the gonidium through its membrane and finally surrounding the latter,—the tissue surrounding the gonidia of a soredium is developed by these gonidia themselves, as has already previously been taught by Wallroth. Important considerations at least may pronounce against the assumption that each soredium-forming gonidium is first of all attached to a hypha, and that the delicate thread which the hypha sends into the gonidium penetrates with the points of its branches everywhere through the membrane of the gonidium to its outer side, and then involves this. That misconceptions may arise in researches so difficult, even with an observer of Schwendener's ability, no one will deny. Von Krempelhuber proceeds to remark that the figs. 6—8, Pl. II, of Schwendener's 'Untersuchungen über den Flechtenthallus,' do not seem to accord correctly with the account given by him upon the soredia-formation.

3. The gonidia, as is known, are completely involved by the hyphæ of the lichen-thallus. But in this condition they stand in need of nutriment for their growth and increase. This, however, in consequence of their total isolation from the outer world, they can draw only from the thallus itself, to whose nutrition they should, according to Schwendener, themselves subserve. The thallus itself further obtains its nutriment, not only from the gonidia, but, as has already been abundantly established, also from the substratum upon which it is attached. We have therefore this position:—(1.) The smaller host-plant is everywhere overgrown and enclosed by its much larger parasite; (2) the parasite and host-plant mutually afford each other the nutriment requisite for life; and (3) the parasite obtains its nutriment elsewhere besides from its host-plant. These are cases so abnormal, so destitute of analogy, that their credibility appears very small.

Von Krempelhuber subsequently takes up another point involved by the assumption, to him most improbable, that Schwendener's views as to the parasitism of lichens should be really founded in fact. In such a case even Schwendener would consider it very questionable that the Lichens should be united in a class with the Fungi. The very peculiar form of parasitism shown by these plants (if proved) would form a characteristic distinction. Von Krempelhuber puts forward the following additional considerations as antago-

nistic to their union into a single class:—(1.) The tissue of the fungi, if indeed anatomically not distinct from that of the lichens, is certainly chemically different. The cell-membrane in the fungi is never coloured blue by iodine and sulphuric acid; in the lichens, on the other hand, at least in the asci and paraphyses, constantly so, passing over into starch. (2.) The cell-contents in the fungi are very rich in nitrogen; in the lichens they are poor. (3.) The lichens are characterised by the presence of numerous peculiar acids absent in fungi. (4.) The tissue of fungi is, as a rule, very perishable, that of the lichens very persistent. (5.) The spores of most fungi become free by constriction, the spores of all lichens by simple exit from the opening asci. (6.) The asci of the lichens show distinct thickening layers; these never occur in fungi. (7.) The fungi choose their habitats only upon dead organic substances, products of putrefaction and decay, or as parasites upon living organisms, but in impending dissolution, without, however, surrounding their host as happens with the hyphæ of the lichen-thallus in respect to the gonidia; the lichens, on the other hand, avoid such substrata, or soon perish upon them. If a flourishing tree, for example, upon which lichens have established themselves, should die, all the lichens found upon it also thereupon die and make way for fungi. Finally, the whole physiognomy of lichen-vegetation, taken in totality, speaks against the union of the class of lichens with the class of fungi.

Schwendener concludes his remarks (in another part of his work) by the most just observation that the controversy can certainly not be definitely determined in the way taken as yet, by hypotheses and single one-sided observations, but, as de Bary has previously pointed out and recommended, by numerous, careful, and accurately carried-on experiments in the culture of lichen-spores, lichen-gonidia, and unicellular algæ, by which alone it can be established with certainty whether the germinating lichen-spore developes gonidia or not, and whether such free gonidia groups, which Schwendener takes for algæ, form from themselves a hypha-bearing thallus or not.

At the meeting of the botanical section of the "Schlesische Gesellschaft für Vaterländische Cultur," 18th January, 1872,[1] Professor Koerber drew attention to the recently published memoir of Reess detailing his experiments in the culture of *Collema*-spores upon *Nostoc*, and its presumed confirmation

[1] 'Botanische Zeitung,' No. 13, March, 1872, p. 215.

of Schwendener's view. Professor Koerber took the opportunity to express most decidedly that, in opposition to that view, he continued firmly to hold by the nature of lichens as independent plants. No arguments by him on the occasion are recorded.

On the same occasion Professor Cohn stated his opinion that the Schwendenerian view, at least as regards the heteromerous lichens, is untenable, since their typical independence as regards their entire morphological, physiological, and geographical conditions is hardly to be doubted; indeed, algæ from which *Usnea*, *Cladonia*, &c., could proceed are not known. But for the *Collemaceæ*, on the other hand, the facts put forward by de Bary and Reess possess importance. He had himself observed that in a gelatinous substratum parasitic mycelia are constantly developed; thus he had found the so-called "star-jelly" almost always permeated by bundles of hyphæ, which Ehrenberg and Meyen have, in fact, taken for a gelatinous fungus of a proper species (*Tremella* and *Actinomyce*). The gelatinous algæ (*Palmella*, *Glœocapsa*, &c.) are likewise regularly permeated by myeelioid threads; and the colourless gelatinous algæ (*Palmella*) living in mines, as much as 100 fathoms beneath the surface, are permeated by fungal-threads so regularly that a special genus (*Erebonema*) was made from them by Roemer. Professor Cohn expresses similar views in "Hedwigia," on giving the Conspectus of a suggested arrangement of the Cryptogamia in general.[1]

Another advocate presents himself in this discussion on Prof. Schwendener's side in M. Ed. Bornet, who communicates to the Academy of Sciences at Paris some remarks on the question.[2] After referring to Schwendener's hypothesis, and after giving a brief general description of the typal lichen-thallus, and drawing attention to the resemblance or parallelism offered by the gonidia of the different lichens to the corresponding algal types, he proceeds to argue that such cannot be regarded as merely an accidental coincidence. These gonidia multiply, following their own laws, and in complete independence of the hypha, and reproduce in quite the same manner as the corresponding algæ.

Holding the view that the better way to throw light on the question would be to ascertain the relations of the hyphæ to the gonidia and to determine, if possible, their real origin, since science possesses upon these points so few observations, and

[1] Cohn: "Conspectus Familiarum Cryptogamarum secundum Methodum naturalem dispositarum," in 'Hedwigia,' No. 2, 1872, p. 17.

[2] Bornet: "Sur les Gonidies des Lichens," in 'Comptes rendus, Acad. des Sciences,' tome lxxiv, No. 12 (18 Mar. 1872), p. 820.

these contradictory, he undertook researches upon these points beginning with a species of *Plectopsora*. This possesses gonidia in chaplets, like those of *Collema*, and he states that he has seen with precision that the short branches detach themselves from the principal filaments of the hypha, and become applied upon one of the cells of the chaplet. Upon this contact the cell becomes considerably swollen up, and surrounded by a thick membrane. Its contents become altered, finally entirely disappearing, and only an empty sac remaining adherent to the hypha. Here the parasitism is evident.

Some other neighbouring genera—*Synalissa*, *Omphalaria*, &c.—present exactly the same phenomena. However, the gonidia do not become so greatly modified; their contents become more homogeneous and more diluted, but their form remains unchanged.

In the higher lichens the attachment of the hypha is difficult to see well; but in certain species the author has been able to make out an adhesion between the two organs, of the same nature as in the preceding cases. The attachment may be directly on the side of the filaments of the hypha and the gonidium is sessile, or by a lateral branch and then it is pedicellate. In either case the filament is applied closely on the gonidium, and is in a measure moulded around it. Very often it becomes dilated at the point of contact by a kind of irregular flattening or by a little cup, which embraces all the base of the cell. All the lichens the author examined, except *Collema* and *Leptogium*, show the same disposition.

In sowing the spores of *Parmelia parietina* along with the "globules" of *Protococcus viridis*, one can in some measure comprehend the mode by which the connection is established. After some days the spores begin to germinate; they send out radicle filaments, which rapidly elongate, and whenever these meet with the *Protococcus*-cells, isolated or in groups, they become attached as mentioned above. If the presence of the spores still adherent did not show the true nature of these radicles, it would be impossible to distinguish the gonidiferous filaments from the adult lichen.

To sum up, it appears to the author (1) that the gonidia no more originate from the hypha than the hypha from the gonidia; (2) that the presence of the latter is requisite for the development of the hypha, whose growth becomes arrested if the gonidia are not forthcoming. The lichens would be thus in reality parasites on the algæ; but the parasitism would have different degrees. In some species

the hypha destroys the cells to which it becomes attached. In the majority of cases, on the contrary, the two organisms continue to live associated, and the gonidia preserve the faculty of multiplying according to the ordinary laws of their reproduction. Finally, in *Collema* and *Leptogium* there is no intermediate connection between the hypha and gonidia and these plants, whose resemblance to *Nostoc* has been so often cited as an example of the transformation of algæ into lichens, are precisely those in which the parasitism is the least characterised.

Does it not seem, from the foregoing *résumé* of Bornet's observations, that, so far as Schwendener's hypothesis is concerned, they, to a certain extent, prove too much? If in certain cases the hypha "destroys" the gonidium upon becoming attached to it, can such a true parasite be the same thing, or, indeed, the *intruder* itself be of the same nature, as that which Schwendener sees in the lichen-hypha? *His* parasite does not destroy: on the contrary, it rather stimulates. Nor does it appear explained how "copulation" of the hyphæ (that is, a kind of "conjugation" of the threads, but without any transfer of contents as in ordinary conjugation), as stated by Schwendener, accounts for the presence of gonidia at the apices of branches in direct connection with them. That a "parasite" about to live at the expense of a gonidium should become attached to or penetrate it is sufficiently intelligible. Why, again, it might be asked, do not several other aërial algal types quite as accessible to an intruding parasite as other species, play their part as gonidia-formers?

Woronin has added the last contribution to the discussion.[1] He is inclined to accept Schwendener's view, but points out that the statements of Bayerhoffer, Schwendener himself, and others, according to which the formation of the gonidia takes place at the end of short lateral ramifications of the hyphæ, must in that case be rejected. He found, by watching the development of the zoospores set free by isolated gonidia of *Parmelia parietina*, that they reproduce new colonies of young gonidia, or, which comes to the same thing, young individuals of a unicellular alga of the genus *Cystococcus*, but no filament or hypha. Woronin, however, thinks it both more reasonable and more prudent to abstain from pronouncing definitely in favour of Schwendener's opinion, at least till we possess an exact and complete account of the development of several lichens of two or three distinct forms.

[1] "Recherches sur les gonidies du lichen *Parmelia parietina*, Ach.," which will be found in 'Ann. des Sc. Nat.,' sér. v, t. xvi, p. 317.

He sowed theca-spores of *Parmelia pulverulenta* with gonidia of the same species. The germinating tubes adhered firmly to the gonidia, and even surrounded them, but nothing like a lichen-thallus was produced.

On the Nature *of the* Gonidia *of* Lichens. By Prof. S. Schwendener. Translated by W. Archer, M.R.I.A.[1] (With Plates IX and X.)

I.

Reess has lately made a very valuable contribution towards the determination of the Gonidia-question ('Sitzungsberichte der Berliner Akademie,' Oct., 1871), which, in the main, I welcome as a confirmation of my view as to the nature of lichen-gonidia. He cultivated small and large colonies of *Nostoc lichenoides*, Vauch., after he had previously sown them with the spores of *Collema glaucescens*, Hoffm., and then followed the germination of these spores up to the penetration of the germinating tubules into the *Nostoc*-jelly and the formation of copiously branched *Collema*-mycelia in the interior of the young *Nostoc*-colonies.

In the introduction to this memoir, as well as at its conclusion, the author, however places my own observations in so unfavorable a light that I regard myself as compelled to make a brief rejoinder. He says, "What Schwendener considers to be *Collema*-rudiments are certainly no such thing. For not only do *Collema*-hyphæ, penetrating into *Nostoc*, look quite differently, but a *Collema*-thallus will never grow from a manifestly dying *Nostoc* of the colour of fig. 13, or of the swollen-up state of fig. 14 ('Algentypen,' p. 29, t. ii; reproduced *uncoloured* on Pl. X, figs. 19, 20)." Not much more favorable are the remarks at the close.

I may be permitted, then, to consider separately the statements about the *matter*, which is in dispute, and the *figures* chosen for its illustration, for these are distinct things. As regards the matter, I continue most decidedly to hold by the accuracy of my observations. The *Nostoc*-colonies penetrated by fungal-hyphæ, which I observed in every stage (of which two are represented in the figures above mentioned) up to balls of 300m.m.m. in diameter, were most undoubtedly fresh and vigorous *Collema*-rudiments, and not dying off.

[1] "Erörterungen zur Gonidien frage," in 'Flora,' May, 1872. Some abridgement has been effected in unessential points.

On this point I cannot admit of any hesitation, for I not only *believe* myself to be, but I am certain. I have also observed the thallus-formation, not merely in *one* species, but in *several*, of which, indeed, two particularly, owing to their dissimilar colour and disposition of the jelly to become swollen up, as well as also on account of the striking differences in the form of their general outline, especially in an advanced state, were easily to be distinguished with certainty. For, that the thallus-rudiments of *Collema*, that is, the young *Nostoc*-colonies, are always colourless or in the same degree gelatinous, as Reess appears to assume, no observer understanding the matter would maintain. On the contrary, the investing membranes are both colourless and coloured, and more or less dense or rich in water, in the most manifold gradations.

So much as regards the matter. As to the figures I must, in the first place, dwell upon the fact that most of the preparations were boiled in water and in potash, and many were, besides, coloured by iodine solution or treated with acids before I examined them carefully. In the *Nostoc*-balls, which fig. 19, on Pl. X, is intended to illustrate, such a treatment with reagents causing a swelling-up was absolutely necessary, since without it not even the moniliform arrangement of the green cells, to say nothing of the ramifications of the penetrating fungal-threads, could be distinctly made out. In the text, moreover (p. 29), it is expressly said that the questionable fungal-threads come to view only in the swollen-up state. I might certainly, in order to obviate misconception, have repeated this statement in the further course of the dissertation; but I wished to dwell only upon the subject, not upon the preliminary treatment of the object, especially as I had not a single new reaction to describe. The treatment was exactly the same as that which I used in my earlier researches, always with a good result, and which I have repeatedly described. With respect to fig. 19 (Pl. X), it may be further remarked that the preparation had been boiled in dilute potash solution, then washed, neutralised with hydrochloric acid, and coloured with iodine solution. During the observation and turning of the object, the colouring was repeatedly moderated or, indeed, entirely removed by the current of water produced by means of blotting-paper, and then again brought to view by a counter-current of iodine solution. The drawing (fig. 19) was made from the coloured state, in which, however, I attached no importance to the exact reproduction of the exact tint of colour. The penetrating fungal-thread was, of course, somewhat altered by the

preceding treatment; however, the relative position of the individual cells is undoubtedly correctly represented. Of course the ramifications of the filament in the interior of the *Nostoc*-ball cannot be shown in a single view as distinctly as by observing in different positions the object itself, and the figure does leave, in this respect, something to be wished for.

With respect to fig. 20, it is to be remarked that the swollen-up state upon which Reess bases his suppositions is simply explained by the treatment above mentioned. I can answer for the exactitude of the drawing; the double contours of the hypha-cells, their length and breadth, the yellow colouring within a colourless peripheral zone—all this corresponds exactly to the reality, be it understood in the swollen-up condition. The figure, besides, represents only a small portion of the whole object; this was a completely closed vigorous *Nostoc*-, that is to say, *Collema*-ball, of 170m.m.m. in diameter, which I could turn on all sides at pleasure (see p. 29).

After these remarks as respects the trustworthiness of my representations, I turn to the further question, whether the observations contained in the 'Algentypen' supply grounds for the doubt expressed by Reess, when he remarks, "As to Schwendener's hyphæ interlacing round or perforating Algæ, it is unknown whether they originate from, or whether they are really starting-points for, lichens." As regards the first point—the source of the questionable hyphæ—I certainly admit that direct observation has given me no explanation. If, indeed, any one should hold it to be conformable with the present state of science to derive these hyphæ from growing pollen-grains or from pathologically degenerate cells, or the like, I leave free play to his fancy. But it is otherwise with regard to the second point—the subsequent behaviour of the hyphæ. Here I must decidedly maintain that these were and could be nothing else, in the cases observed, than starting-points for lichens. For I am certain that I can recognise a young *Collema*-thallus when 200 to 300m.m.m. in diameter, and I rely upon the same certainty for any microscopist who has occupied himself with *Collemaceæ*. Just as one may look forward to a young potato-plant, under favorable conditions, reaching its normal size and development and at last fructifying like other potato-plants, so I hold the assumption of an analogous further development for my *Collema*-rudiments to be also perfectly justified. Moreover, these indubitable *Collema*-balls were connected by every possible transition with *Nostoc*-colonies of far less diameter, only just penetrated by a

fungal-thread, and consequently they belonged to the same development-series. Hence, whatever the origin of the penetrating cellular filaments may be, in the interior of the *Nostoc*-balls they become unmistakable *Collema*-hyphæ, and their ramifications take their share subsequently in the building up of the thallus in quite a normal way characteristic of *Collema*. Conjectural conclusions are therefore out of the question; the matter concerns the direct result of my observations.

I go now, indeed, a step further and say:—If the questionable cellular-filaments *after* their penetration into the *Nostoc*-jelly become *Collema*-hyphæ, and retain this character during the further development of the thallus, they must have been *Collema*-hyphæ likewise, even *before* penetration. I think most botanists will share this view. Whether, furthermore, the *Collema*-hypha in question proceeds from a germinating spore, or from a portion of a thallus, whose fungal-threads have become set free in consequence of a partial break up of the jelly, I leave undecided. With respect to the question which I had to decide, this was a manifestly irrelevant matter. I do not aver, indeed, that this alternative is the only possible one, although to me, for the present, no other appears plausible. I repeat that I have *observed* the development of a *Collema*-thallus from the penetration of a cellular filament (come whence it may) into a *Nostoc*-colony up to the formation of an undoubted *Collema*-mycelium, by means of the growth of the intruder. On this point I am certain, and this certainty I feel also as regards the mode of development of the *Omphalariaceæ* and *Racoblennaceæ*, whose Gonidia-formers belong to the *Chroococcaceæ* and *Scytonemeæ* (or *Rivularieæ*). (See pp. 22, 26, and 34, of the 'Algentypen.') I hold it to be impossible that future observations can be otherwise than confirmatory of this.

Under these circumstances I cannot attribute to the experiments with the spores which Reess has carried on and described for *Collema glaucescens* the significance claimed for them by the author. There was, in fact, nothing else to be expected, according to my observations, than that the germinating tubelets should sooner or later penetrate into the *Nostoc*-colony. I can prophecy the same behaviour with the greatest certainty for the spores of *Omphalaria*, *Racoblenna*, &c., and in general for all the lichens whose development I have studied. For it is inconceivable that a plant should become developed at one time in one way and at another time in one completely different.

The matter stands somewhat otherwise (if we leave analogy out of view) with the great number of foliaceous

and crustaceous lichens, in the case of which facts from development-history, bearing out my theory, are not forthcoming. It is true the anatomical conditions according to my conception are, indeed, a difficulty, especially in *Cystococcus* (and likewise in *Chroolepus*), where the nature of the contents, the zoospores, &c., in short, the whole physiognomy of the cell and its parts, scarcely permits of a confounding with actually different things, and where the explanation given by me, as well as every other, encounters serious difficulties; but I readily admit, however, that the history of the development has here still a palpable lacuna to be filled up by experiments in the culture of the spores. But I only, indeed, here contend that they must be experiments with spores; I think, on the other hand, that the observations upon development-history which I have given in the case of *Collema, Omphalaria*, &c., have quite the same value, since I regard them as absolutely decisive. If I conceive, for example, that I had observed on any damp wall all transitions from an unaltered *Cystococcus* up to a young *Physcia*-thallus of 0·1 m.m.m. in diameter, for myself, the lacuna would be filled up, and all doubt vanish, for I am satisfied that I can recognise a thallus of that size with certainty. I should in the main consider the question as already solved, if I had been able to follow out the fungal-growth as far as the formation of a continuous investment around the *Cystococcus*-cells. Only I must, of course, be satisfied that no confounding of the objects, regarded as transitory states, with heterogeneous things, such as soredia, has taken place—a necessary condition in all researches in development-history.

I think these remarks will suffice to set the matter in its proper light. Manifestly the doubts given expression to by Reess have been in part due to the brevity of my exposition; but the assumption that I had allowed myself to be deceived by some mere mould—that my cellular-threads penetrating into a *Nostoc* (as in Pl. II, f. 13, *in orig.*, see Pl X, f. 19) "might be anything at all rather than a *Collema*-hypha"—goes surely entirely too far.

II.

A further argument for the old view von Krempelhuber believes he has found in the formation of the soredia. It may, indeed, be possible, he says, on p. 19, that the process of soredia-formation may be carried on in a way essentially different from that described by myself ('Untersuchungen über den Flechten-thallus'); that is, that the tissue surrounding the gonidia of a soredium may be perhaps developed from

the latter itself, as was taught by Wallroth. I should not have thought that I should still find myself in the position to be obliged to defend myself seriously against Wallroth's 'Umschleierungstheorie,' nor do I intend to add many words about it. My history of the development of the soredia rests in the main on observations so certain that I may say with confidence that such an origin as von Krempelhuber indicates is not possible, and most assuredly it does not occur in reality.

As to the objections raised against my views, I must further remark that there are manifestly some misconceptions. I do not say that *each* soredium-forming gonidium possesses a stipes, but I select this case as a starting-point in accordance with nature for the entire process of development. I can, at the same time, guarantee the correctness of the figures (1 Heft, t. ii, iii, v). If in this manner (or generally by fibrous branches which stood in contact with a gonidium, see p. 24 of the reprint) the first soredium be formed, its increase follows simply by division of the mother-gonidium, into eight cells, for example, and by ingrowth of the fibrous branches between these (l. c., t. ii, f. 6—8). This process may be often repeated, as the gonidia continue dividing, whilst simultaneously the hyphæ interweaving around them continue correspondingly to vegetate, and are constantly ready to send inwards their ramifications for the formation of special coverings. Whether actual supporting cells are formed here I leave undecided. According to my earlier view this was dependent on the conditions which govern the new formation of the gonidia in general; according to my present one it is primarily dependent on the nature and thickness of the membranes.

The foregoing digression on the formation of soredia leads me to a further question in connection, which there are still but little materials for discussing. I mean the occurrence of certain lichens in all parts and zones of the earth, their immeasurable abundance, &c. Is it probable, may be certainly asked, that the different kinds of Algæ, which have to supply the gonidia, possess likewise this very wide distribution? To this I answer, in the first place, that we know very little of the geographical conditions of these lower algæ. That little is, however, not at all unfavorable to my views. It is, moreover, a general rule that the simpler organisms have an extended area of distribution. A serious objection from this side is therefore not to be feared. As regards, then, in the second place, the "immeasurable abundance" of individuals and their habitats, which certainly are "scarcely, or not at all, adapted for the occurrence of algæ," that is quite

another matter. For manifestly amongst the lichens the increase by soredia plays a great part. When, for example, we observe how newly erected walls and monuments and bare spaces on rocks become covered within a few years with thousands of new lichen-rudiments, we must place this to the account of the soredia. At least, in the youngest little colonies which Arnold had sent me from Franconia, as "Prima initia vegetationis," I could never discover anything else than some green cells enclosed all round by an equally thick fibrous covering. Just the same in the smallest thallus-rudiments on barks of trees. Probability is thus in favour of the assumption that by far the greater number of the lichen individuals owe their origin to the soredia.

As regards the supposed difference in habitat for a considerable number of species with a crustaceous or scaly thallus, this is in any case not great, for we not rarely find smaller or larger algal colonies belonging to different groups along with the lichens alluded to upon the same substratum. Amongst many others, on the other hand, especially the shrubby and foliaceous forms, the gonidia-forming algæ may certainly in general seek out moister positions than the corresponding parasites. For these cases I conceive that the concurrence of the two elements may be brought about by accident, such as rain and wind, or any other likely agent, and in such a way that sometimes the spores (or in general the hyphæ) reach the algæ, at other times *vice versâ*. If the former, the young thallus-rudiments, or the soredia proceeding from them, would certainly afterwards have to seek out a somewhat drier habitat. The migration, however, need not be an abrupt one. At the lower part of a tree, for example, we may meet with the algæ, at the upper the corresponding lichens; this is, of course, not saying that *every* tree admits of these two *storeys* being distinguished upon it. If, on the other hand, the alga comes into contact with asci or protothalloid fibres on a substratum favorable for lichens a loose enveloping by the hypha-branches which speedily results in favorable weather, suffices to ensure the development of the gonidial state on the spot; for the fibrous coverings manifestly offer a certain compensation for the diminished moisture of the air by which the new habitat is distinguished from the former.

That these considerations are not illusory follows indeed from the behaviour of the soredia. When these vegetate near *Cystococcus*, *Pleurococcus*, &c., the lichen-fibres, although they continue vigorously to grow, have not the power to keep pace with the immense increase of the gonidia, on which account the formation of the thallus stops. But if a

single soredium reaches a less moist position, the hyphæ at once gain the upper hand and a young thallus originates. What influence the opposite alternation, namely, a higher degree of moisture, exerts, is unknown to me; the researches of Famintzin and Baranetzki, however, lead to the supposition that a longer exposure to water would again free the algæ perfectly from their parasite.

The conditions of distribution appear to me, therefore, to offer no difficulty. Little differences as to moisture are not of importance. Much more important in any case is the speciality *common* to the lichen-hyphæ and the gonidia-formers, of being able to withstand drought as well as frost, without being deprived of vitality.

I now pass on to the considerations which von Krempelhuber puts forward as regards the *affinity between lichens and fungi* upon which I have dwelt. Most of these, as every experienced observer will at once perceive, are based upon a decided want of acquaintance with the matter. It is stated, for example: "The spores of most Fungi are set free by constriction, the spores of all lichens by their simple exit from the opening asci." Now, in this controversy, only those fungi concern us in which the spores, as in the lichens, originate by free cell-formation in asci, and are set free in quite the same way; the question is as to Ascomycetes, not Basidiomycetes. There thus exists an unfortunate confusion which pervades the whole of the morphological, anatomical, and physiological statements. To analyse these in their details would be quite a superfluous labour. Nor is it much better with the chemical differences indicated, whose significance I also do not rate very high. For it surely can occur seriously to no one to distinguish the principal divisions of the vegetable kingdom by chemical reactions. It does not depend on this, but on structure and the mode of growth of the vegetative and reproductive organs, and in this respect the lichens agree with the Ascomycetes. (See de Bary, 'Morphol. und Physiol. der Pilze u. Flechten.')

The objections which bear upon the mode of nutriment of the lichens are in reality just as unfounded. The chlorophyll-bearing cells are, and continue to be, the only ones in the whole vegetable kingdom which possess the power to reconstitute from inorganic nutrient substances (carbonic acid, water, and ammonia, with certain salts), organic compounds (starch, sugar, &c.). It is a general law: Without chlorophyll, no assimilation. Whether these green cells are connected with the remaining parts of a plant *genetically* or *anatomically*, does not here come into consideration. The green leaves of

a standard rose are indeed connected only anatomically with the stem and roots, and they accordingly furnish them with the substances (albumen and sugar) necessary to their growth, and obtain through their means the requisite quantity of water, carbonic acid, salts, &c. The conception of nutrition as a chemico-physiological process is accordingly absolutely independent of questions such as the foregoing; my theory alters nothing of this. The new and unusual part of it is simply the assumed abnormal *form of the parasitism*, that is to say, the position of the tissue consisting of the parasitic hyphæ with regard to the cells of the host-plant. As, however, the occurrence of this peculiar form undoubtedly stands good for certain lichens, as has been shown above, it must, *nolens volens*, be considered as established, and admits of no further argument.

T. M. Fries ('Lichenographia Scandinavica,' p. 6) has recently brought forward another question independent of this, whether the Algæ, which I refer to as host-plants, may not rather be considered parasites of the lichen-hyphæ, since they certainly draw nutritive substances from the latter. To this is to be replied, that, as above remarked, it is the *green* cells which alone possess the power to prepare the substances necessary for the building-up of the plant-organs and only under the influence of sunlight; the green cells alone assimilate. Upon this one source the whole organic world depends, and the sun's rays are the arteries which feed the source. In consequence of this every plant, which is destitute of green cells, depends for its nutriment upon others (or the products of the decomposition of these); in other words, it can only exist as a parasite. The idea of parasitism is accordingly extremely clear and defined; it does not admit of being differently apprehended in a physiological point of view. If the gonidia (as is not to be doubted) were, according to the earlier view, the only organs of assimilation, they retain this significance as algæ; they are the nourishing plants of the lichens. My theory, I repeat, alters in all these things nothing but the name.

I have further named certain lichens which live upon trees, wood, or the products of their decomposition, *double-parasites*, against which phrase Fries likewise expresses himself (l. c., page 7, note). I did so only under the presupposition that these draw *organic* nutriment from the substratum, not merely water and inorganic compounds. Apprehended in this sense, the term is manifestly correct.

It remains still to touch upon several points of a general

kind. I come, in the first place, to the question whether these assumed algæ are really independent plants and not lichen-gonidia which have become free. This latter possibility has been repeatedly considered and recently, indeed, put forward by Nylander ('Flora,' 1870, p. 52); but only to be dismissed with queries such as these: "Quid autem prohibit, quominus gonidia lichenum formas et structuram offerant subsimiles vel quidem similes Algis aut gonidiis Algarum?"—the matter is not decided. To speak of "gonidia" of Algæ, where either the whole of the joints of a filament or certain chlorophyll-containing cells of parenchymatous tissue proceeding from cell-division are concerned, no longer deludes at this day; such antiquated views have been long since thrown overboard.

It will be well to remark, in the first place, that such a supposition cannot concern an *entire* group of the lower Algæ, such as the *Chroolepidæ*, *Scytonemeæ*, *Rivularieæ*, &c., but only a comparatively small number of the members of such groups. The species inhabiting water are self-evidently *à priori* excluded. Take, for example, the *Rivularieæ*. One not closely acquainted with these peculiar plants needs only to turn up any algological work with illustrations in order at once to arrive at the conviction that they are cellular filaments well characterised and distinguished by prominent marks. And that nature should once more reproduce such structures in quite another group of the vegetable kingdom involuntarily reminds one of the "freak of nature" by which it was formerly sought to explain the origin of ammonites and other fossils. One might as well, indeed, assume that mosses which live on the bark of trees are not independent plants, but morbid outgrowths of the bark. Why not? What can be objected? Well, In the first place, the want of genetic relationship, the agreements of these growths with the organs of vegetation of true mosses, the same mode of ramification, the faculty common to both to continue to vegetate upon a foreign substratum, &c.,—all, in fact, which, *mutatis mutandis*, I can, with perfect right, make apply to the gonidia of *Thamnidium* and *Lichina*, and in essentials to those of *Racoblenna*. If we consider that the same reasoning admits of being extended to the *Scytonemeæ*, *Chroolepidæ*, &c., in that case the presumed freaks of nature assume so dubious a comprehensiveness that at any rate a more exact research into them appears to be desirable. It might be demanded that the adherents of so surprising a doctrine should at least demonstrate for a *single* case the genetic connection of the gonidia with the

lichen-hyphæ in a convincing manner. That has not yet been done, and hence up to the present I stand by my proposition set forth in the 'Algentypen:' that the earlier view is destitute of all foundation in fact.

Th. M. Fries has, indeed, stated ('Lich. Scand.,' p. 7) his having directly observed the origin of the gonidia from the hypha-cells. His words are: "Hyphæ enim non solum in filamenta elongantur, sed ramulos breves etiam protrudunt. Qualis ramuli cellula terminalis sensim dilatatur, subglobosa evadit et materia chlorophyllo (vel materia sub-simili) tincta demum repletur. . . . Ita quidem invenimus, ideoque nobis est persuasum, totam illam theoriam, quæ lichenes phycomyco-compositos perhibet, ad irritum cadere." But these statements require some proof and illustration by figures which would perhaps present some material for opinion as to the transitions, upon the demonstration of which all, indeed, depends. What are these transitions like? Have there actually been such? How often one sees in the same preparation three, four, or more stipitate gonidia, amongst these perhaps a small one, another somewhat larger, and a third larger still. Does not the conjecture present itself that the smallest one, which perhaps appears somewhat paler, may have but just originated, whilst the others represent later stages? But still there is nothing proved by this. Just because I know these things from experience, I am not able to say of Fries' communication that it is convincing to me. My own observations upon this object—and I have busied myself repeatedly with it—I have long since recognised to be insufficient; for they show such considerable lacunæ that a perfect development-series is out of the question. This, indeed, any one who knows how to distinguish the actual observations in my previous publications from the expressions suggested by the leading considerations will at once comprehend.

The same author (Fries, l. c., p. 8) then further puts forward the peculiar behaviour of the lichen-hyphæ towards the gonidia. This formation of a stipes by "copulation," as I have laid it down, does not make itself clear to him. He says—"Non enim adeo clementia sunt aliorum myceliorum filamenta, ut membranas cellularum plantæ nutrientis non perforent vel saltem illis irregulariter se applicent." But this remark is not even founded on fact, since, for example, the haustoria of *Erysiphe* likewise are directed backwards towards the epidermis-cells of their host-plant. Still, I attach no importance to that. The principal thing is that the questionable copulations actually exist, and that they occur quite

independently of my theory. The matter is here not about conclusions, but observations, the correctness of which I can answer for. If, then, a portion of the stipes has, undoubtedly, so arisen, why should that be improbable for the remainder?

A further objection raised by Fries (l. c., p. 5) is likewise wanting in force. He thinks that, according to my theory, the gonidia, not the hyphæ, must determine the direction and form of the thallus, and lays this down in the following manner:— "Necesse est enim, plantam nutrientem primum adesse, cui dein affigantur parasitæ. In ramis igitur laciniisve tam ex hyphis quam gonidiis formatis, hæc primum illæque deinde existerent; atque ideo gonidia se invicem libera vel in sparsos glomerulos catenasve juncta hypharum directionem cet. ita determinarent, ut unaquæque lichenis species habitum eundem semper preberet." Wonderful reasons! It is certainly correct that a host-plant must exist if the development of a thallus is to be possible. Thus, for example, the germ-tubelets developed from the spore are not in themselves able to increase the quantity of organic substance which was contained in the spore by a single iota. Assimilation, that is to say, the new formation of organic substance, begins only when green cells arise in any manner. But who then says that they march up straight in front at the apex, and, as it were, serve each hypha-branch as a guide? Physiology knows nothing of such requisitions. It admits of a thallus-branch occasionally becoming built up wholly without gonidia, if only the connection with the store-magazine lying behind, from which proceed all the formative materials, remains intact. Phanerogams take similar liberties in a still higher degree. Besides this is a purely physiological question, and one which has nothing at all to do with my theory.

The last observation applies also to a passage in Krempelhuber's criticism. It is stated there (p. 19) that the colourless thallus-hyphæ have not in themselves alone the power to produce a new lichen-thallus (granted!), and also that it is improbable that they acquire this power by means of the gonidia, when these are nothing else but algæ, and that they deprive the latter of the nutriment necessary to development (why?). Thus, once more we meet with physiological considerations, which properly do not belong here at all. As a rejoinder the following may serve. Either the gonidia are self-developed organs of the lichens, and then the development of the thallus is dependent for nutrition upon these organs, because they are the only green cells. Or the gonidia are algæ, and consequently the lichens parasites, then it is

clear that the colourless hyphæ of the thallus are dependent on their host-plant, that is to say, equally on the same green cells; by their means indeed they are enabled to grow further, and eventually to form a new thallus; for in this consists the essence of parasitism. The matter remains on either alternative just the same in a physiological point of view; with our controversy it has nothing to do.

I gave up the intention of taking into consideration any further similar objections of a general nature. But I hope the foregoing disquisitions have shown that "the algal nature of the lichen-gonidia has been established in a series of cases, is extremely probable in others, and in no case improbable." ('Algentypen,' &c., p. 38.) Observations which refer only to single objects, and possibly depend on illusion (as, for example, in *Polychidium*), I have expressly left in abeyance, as requiring completion. As for the rest, I am not apprehensive. Already there exists in the beautiful researches of Reess a confirmation of my statements as regards *Collema*. Others will follow. In the mean time those lichenologists for whom the new doctrine has excited "a feeling of irritation at such a force imposed upon nature, and a sad dislike," may learn to consider the matter somewhat more soberly. The matter, indeed, is not about feelings. Indeed, in the history of cryptogamic science it is not a thing unheard of, that "the learned plant-anatomist, behind his microscope," announces to the botanical world things of which the systematists, "paying homage to a sound conception of nature," had not dreamed.

III.

The gonidia question has, further, its systematic aspect. T. M. Fries, of Upsala, has published a systematic work ('Lichenographia Scandinavica") in which the six principal subdivisions of the lichens are based upon the behaviour of the gonidia.

Fries' system makes necessary a more exact and thorough examination in each group of the conditions of the gonidia, and so leads to the establishment of hitherto unknown facts, which under all circumstances is an enrichment of science. The new system, in other words, contains a *fruitful idea*, for which any friends of science must wish that it may flourish for a time. How many of the numerous Lichen-systems which in this century have been placed before the world have not as much to recommend them. Any new system which promises in any way to become fruitful deserves attention. Hence I regard Fries' 'Lichenography,' of which, indeed, only the first

part has appeared, as a sign of advance in the department of lichenological Science.

Fries divides the whole of the Lichens into six Classes, of which, however, the two last (*Nematolichenes* and *Byssolichenes*), to judge from the short diagnoses, comprehend only the abnormal forms of Lichens falling under *Ephebe* and *Cænogonium*. Of the remaining four Classes, Nos. 1 and 2 coincide with the lichens with yellowish-green, Nos. 3 and 4 those with bluish-green, gonidia. The limitation, according to *my* terminology, would correspond to the following gonidia-types: — First class, *Palmellaceæ;* second class, *Chroolepidæ;* third class, *Nostochaceæ;* fourth class, all the rest, such as the *Chroococcaceæ, Scytonemeæ*, &c. As to the last class, it may be doubted whether its subdivision on the basis of the gonidia as well as general characters of the thallus might not be conformable to nature. According to my view, for example, the lichens, whose gonidia-formers appertain to the *Scytonemeæ* and *Rivularieæ* (with the exception of *Heppia*, which otherwise stands isolated in the system), form in every point of view a natural group (*Racoblennaceæ*), which, according to my observations, though certainly imperfect, consists of the following members:—*Lichina* (including *Thamnidium*), *Porocyphus, Collolechia* (according to Koerber), *Racoblenna, Lecothecium, Pterygium*, Nyl., *Wilmsia*, Kbr., *Micrarea* (?), *Pannaria* (in part). I may add the remark that, lately, through the kindness of Professor Tuckerman, I obtained for examination the true *Pterygium centrifugum*, Nyl., likewise *Pterygium Petersii*, Nyl., *Lecothecium adglutinatum*, Anzi, *Lecothecium asperellum* (Ach., Th. Fries, 'Lich. Arct.,' p. 286, *fide auctoris*), as well as *Pannaria flabellosa*, Tuck., and a form nearly related thereto, but still unidentified (*Pannariæ affinis*, writes Tuckerman), all which lichens undoubtedly belong to the foregoing group. The tissue of the thallus is, as a rule, without interstices, parenchymatous, mostly tinged blue on the under side, in the larger forms *Lichina*-like at the middle; in short, there prevails in an anatomical point of view, without regard to the gonidia, a striking agreement, which certainly points to a near affinity. The limitation of the genera, that is to say, the exclusion or admission of species unknown to me, which have been at any time described under *Porocyphus*, &c., I must, of course, leave to lichenologists; I rely meantime only on the thirteen or fourteen representatives of the above-named genera examined by myself.

I might further commend the jump to other types of gonidia which occurs especially in *Pannaria* as worth special

consideration. How is it as regards the other systematic affinities of these assumed Pannariæ? As already remarked, *Pannaria flabellosa*, Tuck., and its near relatives (*Lichen Pannariæ affinis*, according to Tuck.), belong decidedly to the *Scytonema*-type. Both furnished me with so beautiful preparations that I may adduce them as two further decisive evidences of the algal nature of the gonidia (see the author's figs. 1—8 of Pl. IX and their explanations). Other Pannariæ, such as *P. plumbea*, *P. rubiginosa*, &c., as well as the species obtained from Tuckerman, *P. melanophylla*, Tuck., and *P. crassophylla*, Tuck., belong, on the other hand, to another type, apparently to *Chroococcus;* others, again, as *P. brunnea*, Sw. (but not the form obtained from Arnold—"*genuina*"), have *Nostoc*-chains as in *Collema; P. hypnorum*, lastly, as is well known, is distinguished by its yellow-green gonidia. These are things which, in a system based on the gonidia, call for special explanation.

To the little group of the *Racoblennaceæ*, whose gonidia-formers in the free state are algæ with terminal growth, those of the *Omphalariaceæ* closely approximate, whose green cells belong especially to *Chroococcus* and *Glœocapsa*. To them I would add besides *Omphalaria*, *Enchylium*, *Phylliscum*, and *Synalissa;* according to later researches, also *Psorotichia murorum*, Mass., and *Thelochroa Montinii*, Mass. (specimens furnished by Arnold); according to description, also *Pyrenopsis*, Nyl., and *Paulia*, Fée. In the above-named Arnoldian lichens the gonidia-forming algæ were found sometimes on the same substratum in great numbers and in the most different stages of division, and amongst these also colonies with fungal-hyphæ penetrating from without. The question is here further to be discussed as to whether, perhaps, species of this genus belong to different gonidia-types (see, for example, *Psorotichia*, in Körber's Parerga). The thallus presents, unfortunately, points which are insufficient for a decision as to the affinity.

But how are the different foliaceous lichens with bluish-green gonidia to be subordinated? I am truly desirous to see how Fries will surmount all the difficulties which prepresent themselves as regards the subdivision of his fourth class.

Minor but still pretty considerable difficulties will affect the limitation of another class (*Sclerolichenes*, Fr.), in which the gonidia belong to the type of *Chroolepus*. It is not always easy to recognise with certainty the forms belonging here. Thus, I have lately examined a number of lichens, of which some left me in the mean time in doubt. Whilst, for ex-

ample, *Secoliga gyalectoides* Mass. (from Arnold), *Hymenelia melanocarpa*, Krph., and *H. Prevostii*, Fr. (from Arnold), show the most decided *Chroolepus*-forms, *Aspicilia calcarea*, and likewise *Lecidella immersa*, Web. (at least, according to the examples before me from Arnold), require, indeed, a more exact examination; and if Fries has taken the first-named lichen under the "*Archilichenes*" (in Part I of his 'Lichenography,' p. 274), I, for my part, doubt whether this position is really fitting. Further, there appear to occur here similar differences as in *Pannaria*. Fries expressly says (l. c., page 289) that the true *Hymenelia Prevostii*, Th. Fr., possesses minute gonidia; another very similar plant, which he obtained from Krempelhuber, on the other hand, *Chroolepus*-gonidia. And hence, indeed, these two plants—"externo habitu simillimæ, quæ hactenus ab auctoribus omnino fuere commixtæ"—figure in two distinct classes. Is this a truly natural arrangement?

There still deserves to be mentioned in this question a case of a peculiar kind, which in any case is not very favorable to the arrangement of lichens from the gonidia, namely, the occurrence of a *Secoliga* on "*Bryophagus*" (Pl. X, fig. 15). This plant properly consists only of an apothecium, whose hypothecium (at the thickest place 20 mik. thick) stands in direct contact with the jelly of *Bryophagus*, and further breaks up below into numerous individual hyphæ, which become lost in the jelly referred to. But of what, then, does this so-called *Bryophagus* consist? Of four or five different *Chroococcaceæ* with gelatinous, dissolving membranes, which just by virtue of this characteristic form the jelly in question. Some of these bluish-green algæ become divided in *one* direction only, and may belong partly to *Glœothece*, partly to *Aphanothece;* others, on the other hand, become divided in different directions of space, and possess besides membranes repeatedly enclosed; they are unmistakable *Glœocapsæ*, and the whole of these algæ are more or less laced around by the hyphæ of the lichen; the parasite draws from them its requisite nutriment. Thus we have four or five different host-plants for one parasite! Further, in the same jelly there was besides, here and there imbedded, a yellow-green alga (cells with a doubly-contoured membrane, about 15 mm. in diameter, not rarely oval); however, I never saw this laced about by hyphæ.

Upon the class of the *Archilichenes*, Fr., whose gonidia belong to the *Palmella*-type, I have—after, indeed, the greater part of this class have been treated of in the first part—no occasion to offer many words; however, I cannot suppress a brief remark. It would have been to be wished

that the author had given somewhat more attention to the differences occurring between the various yellow-green gonidia. Especially would a sharp limitation of the genera in which *Cystococcus* figures as a gonidia-former (whose gonidia thus possess a nucleus, excentric clear space, &c.) have filled up a noteworthy *lacuna*. However, these are things which, indeed, the systematist has less to do with, and hence every gonidiological subdivision of the lichens, be it for what purpose undertaken, will require the aid of the microscopist. According to my researches hitherto, I have no further doubt that, besides *Cystococcus* and *Pleurococcus*, at least two or three different representatives of the *Palmellaceæ* occur as gonidia-formers; amongst these, for example, *Stichococcus bacillaris*, which I have found hitherto, indeed, only in the hymenium of *Sphæromphale fissa* and *Polyblastia intercedens?* Hepp (example from Arnold), here, however, in perfect agreement with the freely vegetating alga. Manifestly these hymenial gonidia have found their way in only *after* the formation of the "tubule," and by the open apothecium. I conclude this also from the circumstance that these were not yet present in a young apothecium of *Sphæromphale* whose asci had as yet formed no spores. Other *Palmellaceæ*, that is to say, gonidia, which I found on the same substratum as well free as within the thallus of the lichen concerned, appear as yet to be not at all described; at least, it was not possible to me to find them in Rabenhorst's 'Flora Europæa Algarum.' Such a Palmellacean (perhaps *Protococcus fuligineus*, Lenorm.?), which I observed in the examination of the previously mentioned *Polyblastia intercedens?* Hepp, is represented in fig. 11 (Pl. X). I remark further, that the membranes in the free state are not rarely coloured brown, which certainly does not agree with Rabenhorst's diagnosis of the *Protococcaceæ*—"cytiodermate tenui hyalino."

These examples will merely show that the study of the gonidia is still far from being completed. There are still questions of many kinds to investigate, which deserve attention from lichenologists, and not only from microscopists. One may form any opinion one pleases upon a system based upon gonidia; this principle of classification can be fruitful for science only by being carried out as far as possible through special details.

On the Injecting *of* Objects *for* Microscopical Examination *by means of* Air-pressure. By David J. Hamilton, Junior House-Surgeon, Northern Hospital, Liverpool; formerly Resident Surgeon, Royal Infirmary and Chalmers Hospital, Edinburgh.

(Read before Microscopical Society, Medical Institution, Liverpool.)

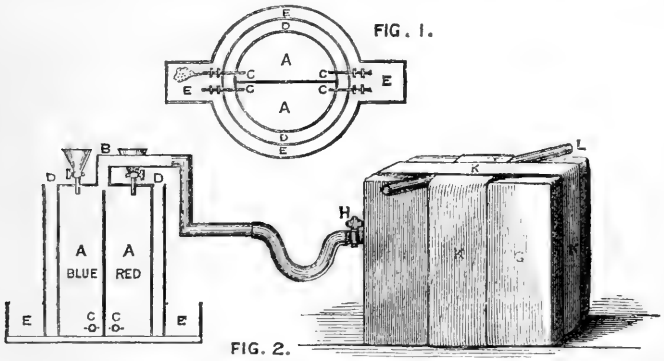
The method of substituting air-pressure for that of the piston of a syringe guided by the irregular action of the hand, in making injections of tissues for microscopic investigation, is a vast improvement. Any one who has employed the ordinary syringe in making injections, especially where the injected fluid contains gelatine, must have found the disadvantages of the method to be many, and among the chief of these extravasation and cooling of the fluid to be injected are the most common. The former is best overcome by substituting the elastic force of compressed air for the direct and inelastic compression of the ordinary syringe.

Such an apparatus as that recommended by Ludwig is based on this principle, and, seemingly, ought to be a most efficient method of injecting. When, however, one has had some practical experience of this apparatus, it is found not to be so faultless as would at first sight appear. One of its great disadvantages is that there is no means for keeping the injecting fluid warm, so that it coagulates in the tube, and hence its progress is stopped. Then, again, the water which fills the first Woulf's bottle is apt to overflow into the second, and to spoil the injecting fluid. No doubt this last objection might in part be overcome by employing mercury instead of water, but it is not always convenient to have a sufficient quantity of mercury at hand, and even then it is often difficult to get pressure sufficient to influence the injection in the second Woulf's bottle. Neither is there any ready means of keeping the tissue warm which is to be injected, and this, with injections which contain gelatine, is most important. From these causes failure is a common result when this apparatus is employed, although the principle is excellent, namely, the employment of the elasticity of the atmospheric air as a compressing agent.

The apparatus which I describe has been made with the object of overcoming these difficulties. It has been used for making a great number of injections, and works admirably. It consists of a hollow cylinder of tin (A, figs. 1 and 2), which is divided into two chambers by a partition which reaches

down to the bottom, but does not reach quite to the top, so that the two cavities communicate with one another above, but are shut off below. A tube (B, fig. 1) enters on the roof of this cylinder opposite the partition, and so communicates with both chambers. From the floor of each cavity a stop-cock comes off on each side, and runs out through a second cavity (D, figs. 1 and 2) which entirely surrounds the hollow cylinder, and is open at the top. The points of the stop-cocks are made to fit air-tight on to a number of different-sized canulæ. Each of the inner chambers is supplied with a pipe and stop-cock on the roof, which runs down to about half an inch below the upper margin of the partition, and each of these upper stop-cocks is surmounted by a funnel.

When the apparatus is going to be used, blue injection is poured through the funnel into one chamber, and red injection, in the same manner, into the other, and the space between the outer circle (D, figs. 1 and 2) and the hollow cylinder is filled with water heated to any required tempera-



HORIZONTAL AND TRANSVERSE SECTIONS OF APPARATUS.

A. Inner chambers.
B. Tube to connect inner chambers with air-bag G.
C. Stop-cocks and canulæ.
D. Chamber for hot water.
E. Trough filled with water, 100° F., in which tissue to be injected is placed.
G. Air-bag surrounded by linen bands K.
H. Stop-cock of air-bag.
L. Rod which tightens the bands K by being rotated.

ture. Thus, the chambers being surrounded with hot water, the injections which they contain can be kept at any required temperature. The only portion of the injection which is

liable to get cold is that within the lower stop-cocks and canulæ. This is prevented by placing the lower part of the apparatus in a trough (E, figs. 1 and 2), which is filled with water heated to 100° F., and in which the tissue to be injected is placed. The canula, which has been inserted into a blood-vessel or lymphatic, is then connected with the lower stop-cock. We have now the injection in its entire course, as well as the tissue to be injected, kept at the required temperature.

The inner chambers ought to be filled half with injection and half with air. Our object is now to compress this air, in order that it may press upon the injecting fluid, and force it out at the lower stop-cocks. After trying various methods, many of them more or less clumsy, I have arrived at the following as by far the most efficient. A large bag (G, fig. 1), made in the same manner as those for the oxyhydrogen light, is fitted with a stop-cock at the middle of one edge. This is joined to the pipe (B, fig. 1) which comes off from the roof of the cylinder by a piece of strong elastic tubing, and thus we have the connection established between the air in the cylinder and that in the air-bag, which can be turned off or on by the stop-cock at H (fig. 1).

It is astonishing what an immense amount of direct pressure on the air-bag is required to force out the injection from the cylinder. On first trying it, I placed on the top of the air-bag somewhere about two to three hundredweight without being able to influence the injection to any great extent. Such being the state of things, I relinquished all hope of being able to accomplish my object by means of direct pressure, and employed the following method instead.

Two bands of strong linen (K, fig. 1) are placed round the bag, so as to cross one another, and to meet over the top. A wooden rod is placed under them at this part, which can be made to revolve, so that when this is done the bands are tightened, and the air within the bag is compressed to any degree.

After the rod has revolved sufficiently to produce the necessary tension of the linen bands, it is fixed in its place by a strap and buckle. When the connection between the bag containing the compressed air and the hollow cylinders (A, A, figs. 1 and 2) is established, the injection within the cylinders is forced out at the stop-cocks (C, figs. 1 and 2), and into the blood-vessel of the part to be injected. The amount of pressure can be indicated by attaching a manometer to any of the stop-cocks of the apparatus. If the pressure is too great it can be lowered by allowing some of the

compressed air to escape. If it is too little it can be increased by tightening the bands surrounding the air-bag.

The advantages of this apparatus are—

1st. The artery and vein can be injected at the same time with the same amount of pressure, and several injections can be proceeding simultaneously.

2nd. We gain the elastic and constant pressure of a large body of air, and the amount of pressure can be clearly indicated by the use of the manometer.

3rd. The injecting fluid and tissue to be injected can be kept at any required temperature.

4th. It is not liable to get out of working order.

On Cell Theories. By John Cleland, M.D., F.R.S., Professor of Anatomy and Physiology, Queen's College, Galway.

Stricker's article on "The General Characters of Cells," in his 'Human and Comparative Histology.' Published 1868; translated by Power 1870.

Beale, in Todd, Bowman and Beale's 'Physiological Anatomy and Physiology of Man,' 1866 and 1871.

Bennett, 'Text-book of Physiology,' 1872.

In placing the titles of these books at the head of this article, I purpose less to review them than to refer to the opinions of their authors in illustration of a general survey of the conceptions at present prevalent, with regard to the vital units of living bodies.

Observation is more a means than an end. The end is to arrive at an accurate conception of the processes of nature; and very different conclusions are arrived at by different men, founding their judgment on similar phenomena. Possibly, the just zeal at the present day for accurate information tends to lead to an undervaluing of the faculty by which observations are translated. Every man is obliged to translate what he sees. No doubt he ought to distinguish carefully in his own mind the appearances seen from his translation of them; but no man ever did or ever will give a description of a complex microscopic appearance so as exactly to reflect the appearances seen, uncoloured by the element of judgment; and much less is it possible to found a statement on a variety of observations which is not largely dependent on the attitude of the observer's mind. One observation has its effect in modifying the translation of

another; and it is greatly due to this that many microscopic objects of a corpuscular nature, or what are still called cells, are capable of being interpreted very differently now as compared with the way in which they were looked at twenty years ago. Formerly, corpuscles round which no cell-wall was demonstrated were too easily supposed to have one, although it was invisible, or were regarded as exceptions to a general rule; now they are viewed with different eyes, and taken as proofs that the cell-wall is unimportant.

The history of conceptions regarding cells is in its general outlines exceedingly instructive. The notion of a cell was first derived from vegetable tissues with their easily exhibited cellulose cell-walls. The vesicular form thus caught the eye from the first. Then, in many instances in animal tissues, also a real or apparent vesicular structure was easily observed; as in adipose, epithelial, and nerve-corpuscles. The contents in vesicular structures were seen to be various, but the frequent existence of a nucleus of firmer consistence imbedded in them could be demonstrated, and within this were often seen one or more nucleoli of some sort or other. Thus it naturally happened that the cell-wall was considered a characteristic structure, and was supposed to be functionally important; and next to it, the nucleus was regarded as the seat of vital properties, because it was seen to divide preparatory to the multiplication of cells, and to be distinct in young cells, however it might dwindle out of sight in the old. The circumstance, manifest from the outset, that cell contents were various, taken in conjunction with their being the part which least caught the eye, led to their vital importance being long overlooked. But as improved microscopic methods came into use, including reagents, such as carmine, which bring masses of albuminoid matter into view not only by outline but throughout their extent, the general existence of such masses in bodies of the sort which had been known as cells came to be recognised, and, as a natural consequence it gradually dawned on the minds of independent observers that the outlines of such masses were the things which, in many instances, had been translated as cell-walls.

Two other advances aided in completing a revolution of opinion with regard to cell-walls, namely, the discovery of corpuscles undergoing amœboid changes of form and migrations, and the tracing of nerves in many instances into continuity with nucleated corpuscles. Thus a change has crept into the whole conception of the nature of those vital

units whose importance functionally had been first recognised in the case of some which had apparently a cellular form; and thus it happens that the term cell is still employed in many instances in which it would be better to use the word corpuscle. A nucleated corpuscle is as convenient an expression as a nucleated cell; a connective-tissue-corpuscle is an expression which involves no theory or description either of structure or function; and it would be an enormous advantage to the spread of accurate ideas if the word cell were never used except when it was meant to predicate the existence of a cell-wall.

For some years past I have, in teaching, been particular in this matter of nomenclature, believing that misleading names do generate confused ideas, and that no conventional compact can make it judicious to designate a solid mass by a word which indicates a hollow vesicle, or advisable to use a common word in a sense at variance with its usual meaning. It is as if we were to invest the tongs with the scientific name of poker. "There is nothing, I can assure you, gentlemen," said Goodsir, "which has more retarded science and philosophy, and the kindred subjects on which human reason has been employed, than the introduction of terms with conventional meanings." But I admit that it is difficult to escape from an accustomed groove, and that for a time one must be content, under protest, to speak occasionally of secreting cells, nerve-cells, hepatic cells, and so forth; were it for no other reason than to be in harmony with the language of text-books, in speaking to students.

The changes in the anatomical conception of the living corpuscle have not been without their influence on the physiological conception. In the days when the cell-wall was paramount, it seemed an important point to determine how far the passage of substances through that membrane was the result of mere osmosis, and how far it depended on the action of some attractive power within. To the school whose tendency was to refer everything to the laws of dead matter the cell-wall was a most important agent; to those on the other hand who considered that to account for the formative changes in living bodies the presence of another force must be assumed, the nucleus as situated in the interior seemed the source of vital actions. Moreover, in the absence of a defined knowledge of the protoplasmic element, the conception of the nucleus was obscured by extending the designation to bodies which ought not to be so named. An instance of this may be found in the case of connective-tissue-corpuscles. Many years before Virchow's researches

threw a light on these structures, a corpuscular element was recognised as present in connective tissue. It was even taught by some to be the same element as could easily be demonstrated as constituting, in a cellular form, a large part of the bulk of fœtal connective tissue, and to be of the utmost importance in its vital properties. But that element, in the adult, was known only as it may be found figured by Dr. Sharpey in 1848 (Quain's 'Anatomy,' 5th edition, pp. cxv and cxvi). No doubt Dr. Sharpey distinguished clearly the nuclei "attached to the surface of the filamentous bundles or in their interior" from the "rounded and oval corpuscles and irregular particles met with in the interstices of the tissue," which he said were "probably to be considered as belonging to the interstitial fluid;" but those who imputed importance to that latter variety of corpuscles had little help for it, at that time, but to associate them with nuclei.

In the present day the protoplasmic element has assumed an enormous importance, casting the nucleus into the shade, while the reign of cell-walls has come to an end altogether. But to speak of life, as is sometimes done, as if it were an inherent property of a particular chemical substance, is surely going too far, and is a view which has nothing true in it which is not more than thirty years old; for it has long been familiar to every one that life never exists without the presence of nitrogenous substance of an albuminoid character; and, though it has since been discovered that life in various instances exists in non-nucleated structureless masses of protoplasm, that is a very different thing from life being a property of protoplasm. Further, it may very fairly be questioned if some of the simple organisms are not rather to be compared with the nuclei in textures than with protoplasm around nuclei. If certain of them are non-nucleated protoplasmic masses, may not vibriones be regarded as mere nuclei and nothing else? Besides, in the textures, there are many nuclei which have no apparent protoplasm about them; and there are also nuclei with processes which may be regarded as bodies intermediate in character between the typical nucleus and the protoplasmic mass. The corpuscles of the deep layers of the cutis are mere nuclei, with long processes in various directions; while in the tapetum of the eye of the ox long threads extend from nuclei, like the threads at the extremities of fusiform cells in fœtal connective tissue. Also, a spermatozoon may be regarded as a simple nucleus. No doubt, as mentioned by Stricker, both Schweiger Seidel, and la Valette St. George declare that not only the nucleus, but the protoplasm of the mother cell,

enters into the construction of the spermatozoon; but if we examine the forms represented by la Valette, both in 'Stricker's Manual' and in his original paper in 'Schultze's Archiv,' we shall see that what is meant is that the protoplasm is at first adherent to the spermatozoon or nucleus, and afterwards absorbed into it or otherwise lost, but that there is no permanence in the spermatozoon of a substance preserving the characters of protoplasm and distinct from the nucleus.

One of the short-comings of Professor Stricker's article on the general character of cells, now for five years before the world, is that he exaggerated the virtue of the protoplasm. He uses the expression, "Protoplasm is termed a living substance," or, as the German (man bezeichnet) may be more strictly translated, "it is recognised as a living substance;" and he speaks in such a way as to leave the impression that it is a definite chemical compound. Now, the fact is that protoplasm when examined under the microscope is usually as thoroughly dead as anything could be well imagined to be. Living masses of protoplasm, no doubt, can be studied microscopically, and a great stride has been made in science by the examination of such masses in texture; but the composition of the protoplasm is not definitely known. It is quite unobjectionable to call the albuminoid mass of a nucleated corpuscle protoplasm, even after it has been acted on by means of chromic acid, carmine, or other reagents. In fact, protoplasm is simply a convenient name to use in speaking of the pulpy nitrogenous substance of vital corpuscles; but it is not to be forgotten that the substance referred to is variable in appearance and behaviour, as is well illustrated by Heidenhain's observations on the differences in both salivary and gastric secreting corpuscles in states of activity and rest. How, then, shall we say that in its different conditions the material which constitutes the mass of such corpuscles is one and the same chemical substance? We shall, indeed, take a very imperfect view of the living units to which an unhappy chance has given the unfortunate name of cells, if we say that because neither cell-wall nor nucleus is an essential element, therefore life is a property of protoplasm. It was recognised by observers long ago that the bond of connexion between the bodies which they described lay not in a detail of structure, but in the possession of one or more of the vital properties, irritability, growth, or reproduction; and the observations of later years do not overthrow that conception, but afford it additional support. Indeed, there are passages in Professor Stricker's article which show an appreciation of this.

That article is one which affords much food for reflection, and is a repertory of important information. But as a history it is defective, even greatly so. The author neither does justice to the work of Virchow nor of Beale; and Goodsir is a name of which he makes no mention. Yet Virchow inaugurated an era in the history of cell-conceptions; and Virchow dedicates his 'Cellular Pathology' to Goodsir, as "one of the earliest and most acute observers of cell life." And if I may again quote one neglected anatomist in support of the claims of another, it may be mentioned that in 1845, Goodsir, in his paper on "Centres of Nutrition," which never admits the possibility of cells originating otherwise than from pre-existing cells, declares that "for the first consistent account of the development of cells from a parent centre we are indebted to the researches of Martin Barry." However, we are informed by the German Professor, that Remak, in his 'Entwicklungsgeschichte,' 1852-1855, "has the merit of chiefly contributing to the abandonment of the doctrine of cell formation from free blastema," and that the same observer established the law that cells are developed by division only, in pathological processes also. Then follows the remark, naive enough, considering these statements about Remak, that Virchow's "well-grounded statement made in 1855, 'Omnis cellula e cellula,' really constitutes the basis of our present cell theory." Virchow's real claim to consideration in the history of cell theories is neither grasped nor mentioned, namely, that by displaying in their full importance the connective-tissue-corpuscles, he afforded the means of accounting for the development of the more complex elements of tissue, as well as for pathological growths.

Those omissions, however, are accounted for by a note inserted by Professor Stricker in the translation (p. 38). Misled by Cohnheim, he had believed the results of Goodsir, Redfern, and Virchow to be founded on incorrect investigation; but later observation of his own has convinced him that he was mistaken. These are not his words, but perhaps they are as clear.

No doubt the neglected discovery of Waller, again made and successfully propounded by Cohnheim, that white corpuscles pass through the walls of uninjured capillary vessels into the tissues, was one which upset previous notions, and might well create in some minds a doubt concerning the doctrine of Virchow's 'Cellular Pathology.' But looking at the subject with the advantage of the five years which have elapsed since Professor Stricker wrote his article, one

cannot doubt that the real state of matters is simply this :— that amœboid connective-tissue-corpuscles and white blood-corpuscles are all one set of bodies, though the first are in the tissues, and the others floating free in the blood; and they might well be termed common or undifferentiated corpuscles. While there need be little doubt that pus-corpuscles are derived from white blood-corpuscles, there also need be no more doubt that they likewise originate both fissiparously and endogenously in the tissues.

I have used the expression "common or undifferentiated corpuscles;" and on this subject it may be necessary to make some remark. The vital powers may be enumerated as irritability or sensibility, contractility, nutrition or elaboration of substance, and reproduction. All these properties are possessed by an amœba and by amœboid corpuscles. But every separate living organism does not possess them all; there are individuals without reproductive power. In like manner, all the vital units do not possess throughout life all vital properties; but in the process of differentiation one property becomes exalted, while another is lost. Muscular fibres, nerve-corpuscles, the corpuscular elements of peripheral nerve-terminations, and secreting corpuscles illustrate this. All of them have lost the reproductive power; muscular fibres have exalted contractility, nerve-terminations exalted sensibility, and secreting corpuscles a highly developed elaborating power.

Probably the greatest difficulty in conceiving of the origin of differentiated textural elements from common corpuscles is to settle the relation of epithelial to other corpuscles, and on that subject it is not easy to give an opinion. In particular, the phenomena of skin-grafting, including the stimulus given to the growth of skin over a whole ulcer by the presence of grafts of minute size, might even suggest the possibility of a sexual distinction between the corpuscles of the graft and those among which it is planted. At all events, the microscopy of skin grafting is worthy of study, and the utility of the practice affords evidence that all the less differentiated corpuscles are not capable of producing, at least without assistance, all other kinds of corpuscles.

It must be kept in mind that the corpuscular mass of the embryo becomes early divided into layers, of which the outer and the inner may be said to be opposed to the middle one, in respect that those become epithelial, while this becomes the source of other tissues. Further, the important observation of His, of the abundant cell proliferation at the circumference of the area vasculosa, and of the intrusion of the

elements so formed into the interior of the embryo, must be kept in mind as giving rise in early embryology to a primary division of the corpuscular masses into centrifugal and centripetal. That His is right in his view that the whole skeleton and connective tissues, as well as the blood and blood-vessels, are derived from the centripetal group I cannot believe, because it appears to me that Remak's view of the origin of the skeleton corresponds much better than that of His with what may be seen in transverse sections of embryos; but it does seem very possible that the centripetal development, being the source of the blood and blood-vessels, furnishes not only nourishment to them but corpuscles which conjugate with all those of the centrifugal mass. I throw this out as a suggestion, and it will not, perhaps, be considered a very wild one, when this remark of Stricker's is remembered:—"Were any one to maintain that the migrating cells are conjugation organisms, no stronger objection could be raised against him than against another who should maintain that the migrating cells are epithelia. Recklinghausen has advanced a theory respecting the conjugation of cells, which, however, on account of its brevity, scarcely allows us to judge of its value." The theory of Recklinghausen refers to conjugation between different elements of blood.

The part taken by Dr. Beale in the advance of the cell conception has been one of great importance, and is worthy of full consideration. The great merit of Dr. Beale appears to me to lie in pointing out that the cell-wall is in all instances an after growth, and that the vital processes of the corpuscle are independent of it. In saying this, I am not forgetful of the work of Max Schultze in 1863, quoted by Stricker; but Dr. Beale has the priority, and lays down the doctrine of the non-vital character of the cell-wall in a very clear and emphatic manner, classifying, as was not done before, cell-walls with the inter-corpuscular substance. He simplified thereby the conception of cell multiplication; for if the cell-wall be, even when present, no part of the vital corpuscle, there is no radical distinction between fissiparous and endogenous reproduction. His distinct recognition, also, that the origin of inter-communicating processes of cells is not by outgrowth of processes, but by the separation of corpuscles which gradually part, is most important; though there is grave reason to pause before denying with him that continuity of structure is ever the result of separate elements sending out processes which unite.

But, however, it may be desired to give Dr. Beale full

credit for the advances which he has made, it is easy enough to understand why that credit should be sometimes withheld, when one considers how those advances have been mixed up with a theory of "germinal and formed matter" which has made but little way. It is quite impossible to support the doctrine that all "formed matter" was once "germinal," particularly if such things as the matrix of cartilage and the fibres of tendon are to be included under the term, as they are by Dr. Beale. Certain cell-walls, as those of at least some of the fat-cells, are really altered protoplasm; but there is not the slightest reason to believe that the matrix of cartilage or the fibres of tendon are transformed portions of the vital corpuscles, or that they are undeserving of the name of "intercellular" substance. Rather would it have been well if Dr. Beale had looked on the cell-wall itself as intercellular.

His difficulty appears to be that "no well-ascertained facts have yet been adduced in favour of the view that any living structure whatever can influence matter at a distance from it, so as to alter its properties or composition, or in support of the notion that cell-wall, cell-contents, or intercellular substance possess any metabolic power whatever." And the way he gets over this difficulty in the case of hepatic cells is most ingenious, namely, by representing that the outer part of each corpuscle is no longer vital, but converted into formed material of a soft description, becoming changed into biliary constituents, albuminoid and amyloid matters. Other researches, however, come to our rescue here. There is nothing more certain than the power which Chrzonszczewsky showed hepatic corpuscles to possess of taking up sulphoindigotate of soda from the blood and passing it on into the gall-ducts. Here, then, is an indubitable instance of a living structure influencing matter external to it. And where, after all, is the unaccustomed marvel in this, when it is recollected that all the attractions of dead matter are exercised by molecules or masses, as the case may be, on others external to themselves, and that in the case of gravitation there is no limit to the distance which may be between the masses, provided that they are sufficiently large?

Looking at things from my point of view, I am also obliged to think it a pity that Dr. Beale has not recognised the true place of muscle in his theory. He calls the contractile substance of muscle "formed matter," which indeed it is in the sense of being raised to a higher state of organization than the corpuscles out of which it is formed; but the formed matter of Dr. Beale is, according to his definition, no

longer vital; and that is not the case with muscular fibre, which not only has the vital power of contractility, but the power of consuming other than its own substance in production of contraction. The real definition of a striped muscular fibre is, that it is a compound living corpuscle which has no reproductive power, but has a far more highly developed contractility than the amœboid corpuscles.

Hitherto I have assumed the importance of nucleated corpuscles as units of life; but so late as the end of last year Dr. Bennett, of Edinburgh, reiterates his molecular theory, declaring that the ultimate parts of the organization are not cells, but molecules. Although one has been familiar for twenty years with the teaching of the distinguished professor, it is difficult to arrive at his point of view, and more difficult now than it was twenty years ago, because while those changes in the cell conception which have been described in this paper have been going on, Dr. Bennett describes a cell now exactly as he would have done twenty years ago, as consisting of cell-wall, nucleus, and cell contents. The nucleus is declared to be more allied to the fatty than the albuminous compounds, although in the same page we are informed that the chyle- and blood-corpuscles of mammals and tubercle corpuscles are only nuclei. Protoplasm, cell-sap, and cell-contents are given as names for one thing, and shortly afterwards it is explained that these contents in different cells may be watery, oily, pigmentary, albuminous and schlerogenous, and mineral; from which, no doubt, the student will gather a very wide conception of the meaning of protoplasm. Many of Dr. Bennett's objections against the so-called cellular theory of organization would disappear if he adopted a more modern notion of a cell or living corpuscle. He would not then instance blood-corpuscles and striated muscular fibre as non-cellular, although vital structures.

A believer in spontaneous generation, Dr. Bennett not only alleges that molecules unite within the body to form cells, but that in infusions they run together to form bacteria, vibriones, and torulæ; of which, in the present state of the spontaneous generation controversy, nothing more need be said than that it would be exceedingly interesting if a number of observers could say that they actually had seen molecules running together before their eyes to form bacteria, like soldiers recalled from skirmishing, and falling into line. But, in the mean time, it is no disrespect to any observations made on the subject to recollect the doctrine astutely laid down by David Hume, that the more unbelievable an allega-

tion is, it requires the greater amount of evidence in its support.

However, Dr. Bennett, while attributing vital properties of some sort to molecules, admits that "each individual cell is endowed with a distinct life of its own, but of a more complex character. It is born, grows, arrives at maturity, declines, and dies." And he speaks of pus as "crowded with multitudes of compound animal existences, which are born, live, and die as man himself dies." He also admits in the fullest and most satisfactory manner the attractive and selective powers of cells. "By selection, one cell appropriates this matter, and another that, often rejecting the substance that is greedily seized upon by its neighbour." When this is admitted, one naturally feels inclined to ask if this be not enough to account for all that takes place in intercellular substance. No doubt, as Dr. Bennett argues, the first deposit of mineral matter in ossification takes place in the matrix of cartilage, and in that matrix also occur remarkable changes in ulceration, first pointed out by Dr. Redfern; but those changes do not take place without changes in the corpuscles also; and if cells have the attractive and selective powers which Dr. Bennett rightly admits, it is not easy to see why they should not elaborate the substance of the matrix between them. If one cell can secrete saliva, and another bile, a third may surely secrete cartilage matrix. In altered conditions of secreting corpuscles the secretion is changed. What marvel, then, that when cartilage corpuscles change their character in ossification and ulceration, they should exhibit avidity for new substances, causing changes in the matrix already laid down, its absorption, and the deposit of a new matrix?

But it may be asked, what reason there is to believe that the corpuscles do all these things. The answer to that is, that their presence is the proof. In cartilage, if the corpuscles are not necessary for the vital processes, they are not necessary at all, for the physical properties of that tissue depend on the matrix. Besides, when texture is young, or when growth is active, corpuscles are numerous, and show signs of rapid multiplication.

As for the textures enumerated by Dr. Bennett as destitute of cells, they have corpuscles so close to them that no argument can be founded on them. That is the case with the posterior layer of the cornea and the capsule of the lens, while the sarcolemma and vitelline membrane may be looked on as themselves cell-walls; and the anterior layer of the

cornea, the remaining example given, is not really distinct from the rest of the cornea, nor at all destitute of corpuscles.

In bringing these remarks to a close, it may be stated that at the present day there is no difficulty in believing in the uninterrupted sequence of corpuscles by reproduction through all generations. The melting of the spermatozoa and germinal vesicle within the ovum may be regarded as a variety of conjugation, resulting in the formation of a corpuscle, which by its fissiparous division constitutes the wholly corpuscular germinal membrane, from which are furnished the parents of every living corpuscle in the adult body, including the ova or the mother cells of the spermatozoa, according to the sex.

There is no need to believe, as Dr. Bennett does, in a molecular life; but there is necessity to believe, as he seems to do, in life within life. The mere tissue life in individual corpuscles will not account for the phenomena of development, without the addition of a larger life, or a formative principle common to the whole individual, and it would be of incalculable advantage in the just conception of pathological phenomena, if the central and tissue lives were more generally distinguished than they are. Further than that, I venture to conclude by reasserting the doctrine, which I understand originated with Plato, that, still on the principle of life within life, a larger life, or series of developmental changes from a simple origin to a definite goal, presides over the evolution of all animal life in the history of the globe. Such a doctrine alone is capable of explaining all the facts of morphology, and giving to the speculations of Darwin the backbone which they require. But this is not the proper occasion, nor this Journal the place to discuss that large subject.

The Development *and* Growth *of the* Layers *of the* Blastoderm. By F. M. Balfour, of Trinity College, Cambridge.

The following paper deals with the changes which take place in the cells of the blastoderm of the hen's egg during the first thirty or forty hours of incubation. The subject is one which has, as a general rule, not been much followed up by embryologists, but is nevertheless of the greatest interest, both in reference to embryology itself, and to the

growth and changes of protoplasm exhibited in simple embryonic cells. I am far from having exhausted the subject in this paper, and in some cases I shall be able merely to state facts, without being able to give any explanation of their meaning.

My method of investigation has been the examination of sections and surface views. For hardening the blastoderm I have employed, as usual, chromic acid, and also gold chloride. It is, however, difficult to make sections of blastoderms hardened by this latter reagent, and the sections when made are not in all cases satisfactory. For surface views I have chiefly used silver nitrate, which brings out the outlines of the cells in a manner which leaves nothing to be desired as to clearness. If the outlines only of the cells are to be examined, a very short immersion (half a minute) of the blastoderm in a half per cent. solution of silver nitrate is sufficient, but if the immersion lasts for a longer period the nuclei will be brought out also. For studying the latter, however, I have found it better to employ gold chloride or carmine in conjunction with the silver nitrate.

My observations begin with the blastoderm of a freshly laid egg. The appearances presented by sections of this have been accurately described by Peremeschko, "Uber die Bildung der Keimblatter im Hühnerei," 'Sitzungsberichte der K. Akadamie der Wissenchaften in Wien,' 1868. Oellacher, "Untersuchung über die Furchung und Blatterbildung im Hühnerei," 'Studien aus dem Institut für Experim. Pathologie in Wien,' 1870 (pp. 54-74), and Dr. Klein, lxiii Bande der 'Sitz. der K. Acadamie der Wiss. in Wien,' 1871.

The unincubated blastoderm (Pl. XI, fig. 1) consists of two layers. The upper layer is composed of a single row of columnar cells. Occasionally, however, the layer may be two cells thick. The cells are filled with highly refracting spherules of a very small size, and similar in appearance to the finest white yolk spherules, and each cell also contains a distinct oval nucleus. This membrane rests with its extreme edge on the white yolk, its central portion covering in the segmentation cavity. From the very first it is a distinct coherent membrane, and exhibits with silver nitrate a beautiful hexagonal mosaic of the outlines (Pl. XII, fig. 6) of the cells. The diameter of the cells when viewed from above is from $\frac{1}{2000}$—$\frac{1}{3000}$ of an inch. The under layer is very different from this: it is composed of cells which are slightly, if at all, united, and which vary in size and appearance, and in which a nucleus can rarely be seen. The cells of which

it is composed fill up irregularly the segmentation cavity, though a distinct space is even at this time occasionally to be found at the bottom of it. Later, when the blastoderm has spread and the white yolk floor has been used as food, a considerable space filled with fluid may generally be found.

The shape of the floor of the cavity varies considerably, but it is usually raised in the middle and depressed near the circumference. In this case the under layer is perhaps only two cells deep at the centre and three or four cells deep near the circumference.

The cells of which this layer is composed vary a good deal in size; the larger cells being, however, more numerous in the lower layers. In addition, there are usually a few very large cells quite at the bottom of the cavity, occasionally separated from the other cells by fluid. They were called *formative cells* (Bildungselemente) by Paremeschko (*loc. cit.*); and, according to Oellacher's observations (*loc. cit.*), some of them, at any rate, fall to the bottom of the segmentation cavity during the later stages of segmentation. They do not differ from the general lower layer cells except in size, and even pass into them by insensible gradations. All the cells of the lower layer are granular, and are filled with highly refracting spherules precisely similar to the smaller white yolk spherules which line the bottom of the segmentation cavity.

The size of the ordinary cells of the lower layer varies from $\frac{1}{2000}$—$\frac{1}{1000}$ of an inch. The largest of the formative cells come up to $\frac{1}{300}$ of an inch. It will be seen from this description that, morphologically speaking, we cannot attach much importance to the formative cells. The fact that they broke off from the blastoderm, towards the end of the segmentation—even if we accept it as a normal occurrence, rather than the result of manipulation—is not of much importance, and, except in size, it is impossible to distinguish these cells from other cells of the lower layer of the blastoderm.

Physiologically, however, as will be afterwards shown, they are of considerable importance.

The changes which the blastoderm undergoes during the first three or four hours of incubation are not very noticeable. At about the sixth or eighth hour, or in some cases considerably earlier, changes begin to take place very rapidly. These changes result in the formation of a hypoblast and mesoblast, the upper layer of cells remaining comparatively unaltered as the epiblast.

To form the hypoblast a certain number of the cells of the

lower layer begin to undergo remarkable changes. From being spherical and, as far as can be seen, non-nucleated, they become (*vide* fig. 2 *h*) flattened and nucleated, still remaining granular, but with fewer spherules.

Here, then, is a direct change, of which all the stages can be followed, of a cell of one kind into a cell of a totally different character. The new cell is not formed by a destruction of the old one, but directly from it by a process of metamorphosis. These hypoblast cells are formed first at the centre and later at the circumference, so that from the first the cells at the circumference are less flattened and more granular than the cells at the centre. A number of cells of original lower layer are enclosed between this layer and the epiblast; and, in addition to these, the formative cells (as has been shown by Paremeschko, Oellacher, and Klein, whose observations I can confirm) begin to travel towards the circumference, and to pass in between the epiblast and hypoblast.

Both the formative cells, and the lower layer cells enclosed between the hypoblast and epiblast, contribute towards mesoblast, but the mode in which the mesoblast is formed is very different from that in which the hypoblast originates.

It is in this difference of formation that the true distinction between the mesoblast and hypoblast is to be looked for, rather than in the original difference of the cells from which they are derived.

The cells of the mesoblast are formed by a process which seems to be a kind of free cell formation. The whole of the interior of each of the formative cells, and of the other cells which are enclosed between the epiblast and the hypoblast, become converted into new cells. These are the cells of the mesoblast. I have not been able perfectly to satisfy myself as to the exact manner in which this takes place, but I am inclined to think that some or all of the spherules which are contained in the original cells develop into nuclei for the new cells, the protoplasm of the new cells being formed from that of the original cells.

The stages of formation of the mesoblast cells are shown in the section (Pl. XI, fig. 2), taken from the periphery of a blastoderm of eight hours.

The first formation of the mesoblast-cells takes place in the centre of the blastoderm, and the mass of cells so formed produces the opaque line known as the primitive streak. This is shown in Pl. XII, fig. 4.

One statement I have made in the above description in reference to the origin of the mesoblast cells, viz. that they are only partly derived from the formative cells at the bottom

of the segmentation cavity, is to a certain extent opposed to the statements of the three investigators above mentioned. They state that the mesoblast is entirely derived from the formative cells. It is not a point to which I attach much importance, considering that I can detect no difference between these cells and any other cells of the original lower layer except that of size; and even this difference is probably to be explained by their proximity to the white yolk, whose spherules they absorb. But my reason for thinking it probable that these cells alone do not form the mesoblast are, 1st. That the mesoblast and hypoblast are formed nearly synchronously, and except at the centre a fairly even sprinkling of lower layer cells is from the first to be distinguished between the epiblast and hypoblast. 2nd. That if some of the lower layer cells are not converted into mesoblast, it is difficult to see what becomes of them, since they appear to be too numerous to be converted into the hypoblast alone. 3rd. That the chief formation of mesoblast at first takes place in the centre, while if the formative cells alone took part in its formation, it would be natural to expect that it would begin to be formed at the periphery.

Oellacher himself has shown ('Zeitschrift für wissenschaftiche Zoologie,' 1873, "Beitrage zur Entwick. Gesch. der Knockenfischen") that in osseous fishes the cells which break away from the blastoderm take no share in the formation of the mesoblast, so that we can derive no argument from the formation of the mesoblast in these animals, for believing that in the chick it is derived only from the formative cells.

In the later stages, however, from the twelfth to the twenty-fifth hour, the growth of the mesoblast depends almost entirely on these cells, and Paremeschko's discovery of the fact is of great value.

Waldeyer ('Henle und v. Pfeufer's Zeitschrift,' xxxiv Band, für 1869) has given a different account of the origin of the layers. There is no doubt, however, in opposition to his statements and drawings, that from the very first the hypoblast is distinct from the mesoblast, which is, indeed, most conspicuously shown in good sections; and his drawings of the derivation of the mesoblast from the epiblast are not very correct.

The changes which have been described are also clearly shown by means of silver nitrate. Whereas, at first this reagent brought out no outline markings of cells in the lower layer, by the eighth to the twelfth hour the markings (Pl. XI, fig. 3) are very plain, and show that the hypoblast is a distinct coherent membrane.

In section, the cells of hypoblast appear generally very thin and spindle shaped, but the outlines brought out by the silver nitrate show that they are much expanded horizontally, but very irregular as to size, varying even within a small area from $\frac{1}{4000}$—$\frac{1}{400}$ of an inch in the longest diameter.

At about the twelfth hour they are uniformly smaller a short way from each extremity of its longer axis than over the rest of the blastoderm.

It is, perhaps, fair to conclude from this that growth is most rapid at these parts.

At this time the hypoblast, both in sections and from a surface view after treatment with silver nitrate, appears to end abruptly against the white yolk. The surface view also shows that its cells are still filled with highly refractive globules, making it difficult to see the nucleus. In some cases I thought that I could (fig. 3, *a*) make out that it was hourglass-shaped, and some cells certainly contain two nuclei. Some of the cells (fig. 3, *b*) show re-entrant curves, which prove that they have undergone division.

The cells of the epiblast, up to the thirteenth hour, have chiefly undergone change in becoming smaller.

In surface views they are about $\frac{1}{3000}$ of an inch in diameter over the centre of the pellucid area, and increase to $\frac{1}{2000}$ of an inch over the opaque area.

In the centre of the pellucid area the form of the epiblast cells is more elongated vertically and over the opaque area more flattened than was the case with the original upper layer cells. In the centre the epiblast is two or three cells deep.

Before going on to the further changes of the blastodermic cells it will be well to say a few words in reference to the origin of the mesoblast.

From the description given above it will be clear that in the chick the mesoblast has an independent origin; it can be said neither to originate from the epiblast nor from the hypoblast. It is formed coincidentally with the latter out of apparently similar segmentation cells. The hypoblast, as has been long known, shows in the chick no trace of its primitive method of formation by involution, neither does the mesoblast show any signs of its primitive mode of formation. In so excessively highly differentiated a type as birds we could hardly expect to find, and certainly do not find, any traces of the primitive origin of the mesoblast, either from the epiblast or hypoblast, or from both. In the chick the mesoblast cells are formed directly from the ultimate products of segmentation. From having a secondary origin in most

invertebrates the mesoblast comes to have, in the chick, a primary origin from the segmentation spheres, precisely as we find to be the case with the nervous layer in osseous fishes. It is true we cannot tell which segmentation-cells will form the mesoblast, and which the hypoblast; but the mesoblast and hypoblast are formed at the same time, and both of them directly from segmentation spheres.

The process of formation of the mesoblast in Loligo, as observed by Mr. Ray Lankester ('Annals and Magazine of Natural History,' February, 1873), is still more modified. Here the mesoblast arises independently of the blastoderm, and by a process of free cell-formation in the yolk round the edge of the blastoderm. If Oellacher's observations in reference to the origin of formative cells are correct, then the modes of origin of the mesoblast in Loligo and the chick would have nothing in common; but if the formative cells are in reality derived from the white yolk, and also are alone concerned in the formation of the mesoblast, then the modes of formation of the mesoblast in the chick would be substantially the same as that observed by Mr. Ray Lankester in Loligo.

No very important changes take place in the actual forms of the cells during the next few hours. A kind of fusion takes place between the epiblast and the mesoblast along the line of the primitive streak forming the axis-string of His; but the line of junction between the layers is almost always more or less visible in sections. In any case it does not appear that there is any derivation of mesoblast cells from the epiblast; and since the fusion only takes place in the region of the primitive groove, and *not* in front, where the medullary groove arises (see succeeding paper), it cannot be considered of any importance in reference to the possible origin of the Wolffian duct, &c., from the epiblast (as mooted by Waldeyer, 'Eeierstock und Ei,' Leipzig, 1870). The primitive groove, as can be seen in sections, begins to appear very early, generally before the twelfth hour. The epiblast spreads rapidly over the white yolk, and the area pellucida also increases in size.

From the mesoblast forming at first only a small mass of cells, which lies below the primitive streak, it soon comes to be the most important layer of the blastoderm. Its growth is effected by means of the formative cells. These cells are generally not very numerous in an unincubated blastoderm, but rapidly increase in numbers, probably by division; at the same time they travel round the edge of, and in some cases through, the hypoblast, and then become converted in the manner described into mesoblast cells. They act as carriers

of food from the white yolk to the mesoblast till, after the formation of the vascular area, they are no longer necessary. The numerous cases in which two nucleoli and even two nuclei can be seen in one cell prove that the mesoblast cells also increase by division.

The growth of the hypoblast takes place in a very different way. It occurs by a direct conversion, cell for cell, of the white yolk spheres into hypoblast cells. This interpretation of the appearances, which I will describe presently, was first suggested to me by Dr. Foster, from an examination of some of my specimens of about thirty-six hours, prepared with silver nitrate. Where there is no folding at the junction between the pellucid and opaque areas, there seems to be a perfect continuity in the silver markings and a gradual transition in the cells, from what would be undoubtedly called white yolk spheres, to as undoubted hypoblast cells (*vide* Pl. XI, fig. 5). In passing from the opaque to the pellucid areas the number of white yolk spherules in each cell becomes less, but it is not till some way into the pellucid area that they quite cease to be present. I at first thought this was merely due to the hypoblast cells feeding on the white yolk sphericles, but the perfect continuity of the cells, and the perfect gradation in passing from the white yolk cells to the hypoblast, proves that the other interpretation is the correct one, viz. that the white yolk spheres become directly converted into the hypoblast cells. This is well shown in sections (*vide* Pl. XI, fig. 4) taken from embryos of all ages from the fifteenth to the thirty-sixth hour and onwards. But it is, perhaps, most easily seen in embryos of about twenty hours. In such an embryo there is a most perfect gradation: the cells of the hypoblast become, as they approach the edge of the pellucid area, broader, and are more and more filled with white yolk sphericles, till at the line of junction it is quite impossible to say whether a particular cell is a white-yolk cell (sphere) or a hypoblast cell. The white-yolk cells near the line of junction can frequently be seen to possess nuclei. At first the hypoblast appears to end abruptly against the white yolk; this state of things, however, soon ends, and there supervenes a complete and unbroken continuity between the hypoblast and the white yolk.

Of the mode of increase of the epiblast I have but little to say. The cells undoubtedly increase entirely by division, and the new material is most probably derived directly from the white yolk.

Up to the sixth hour the cells of the upper layer retain their early regular hexagonal pattern, but by the twelfth

hour they have generally entirely lost this, and are irregularly shaped and very angular. The cells over the centre of the pellucid area remain the smallest up to the twenty-fifth hour or later, while those over the rest of the pellucid area are uniformly larger.

In the hypoblast the cells under the primitive groove, and on each side as far as the fold which marks off the exterior limit of the proto-vertebræ, are at the eighteenth hour considerably smaller than any other cells of this layer.

In all the embryos between the eighteenth and twenty-third hour which I have examined for the purpose, I have found that at about two thirds of the distance from the anterior end of the pellucid area, and just external to the side fold, there is a small space on each side in which the cells are considerably larger than anywhere else in the hypoblast. These larger cells, moreover, contain a greater number of highly refractive spherules than any other cells. It is not easy to understand why growth should have been less rapid here than elsewhere, as the position does not seem to correspond to any feature in the embryo. In some specimens the hypoblast cells at the extreme edge of the pellucid area are smaller than the cells immediately internal to them. At about the twenty-third hour these cells begin rapidly to lose the refractive spherules they contained in the earlier stages of incubation, and come to consist of a nucleus surrounded simply by granular protoplasm.

At about this period of incubation the formative cells are especially numerous at the periphery of the blastoderm, and, no doubt, become converted into the mass of mesoblast which is found at about the twenty-fifth hour in the region of the vascular area. Some of them are lobate, and appear as if they were undergoing division. At this time also the greatest number of formative cells are to be found at the bottom of the now large segmentation cavity.

In embryos of from thirty to forty hours the cells of the hypoblast have, over the central portion of the pellucid area, entirely lost their highly refractive spherules, and in the fresh state are composed of the most transparent protoplasm. When treated with reagents they are found to contain an oval nucleus with one or sometimes two nucleoli, imbedded in a considerable mass of protoplasm. The protoplasm appears slightly granular and generally contains one or two small vacuoles. I have already spoken of the gradation of the hypoblast at the edge of the blastoderm into white yolk. I have, therefore, only to mention the variations in the size of its cells in different parts of the pellucid area. The

points where the cells are smallest seem generally to coincide with the points of maximum growth. Over the embryo the cells are more regular than elsewhere. They are elongated and arranged transversely to the long axis of the embryo. They are somewhat hexagonal in shape, and not unlike the longer pieces in the dental plate of a Myliobatis (Pl. XII, fig. 5). This regularity, however, is much more marked in some specimens than in others. These cells are about $\frac{1}{4000}$th of an inch in breadth, and $\frac{1}{1000}$th in length. On each side of the embryo immediately external to the proto-vertebræ the cells are frequently about the same size as those over the embryo itself. In the neck, however, and near the end of the sinus rhomboidalis, they are considerably smaller, about $\frac{1}{4000}$th inch each way. The reason of this small size is not very clear, but probably shows that the greatest growth is taking place at these two points. The cells, again, are very small at the head fold, but are very much larger in front of this—larger, in fact, than any other cells of the hypoblast. Outside the embryo they gradually increase in size towards the edge of the pellucid area. Here they are about $\frac{1}{1000}$th of an inch in diameter, irregular in shape and rather angular.

The outlines of the cells of the epiblast at this time are easily distinguished from the cells of the hypoblast by being more elongated and angular; they are further distinguished by the presence of numerous small oval cells, frequently at the meeting point of several cells, at other times at points along the lines of junction of two cells (Pl. XII, fig. 7). These small cells look very like the smaller stomata of endothelial membranes, but are shown to be cells by possessing a nucleus. There is considerable variation in size in the cells in different parts of the epiblast. Between the front lobes of the brain the cells are very small, $\frac{1}{4000}$th inch, rising to $\frac{1}{2000}$th on each side. They are about the latter size over the greater part of the embryo. But over the sinus rhomboidalis they fall again to from $\frac{1}{3000}$th to $\frac{1}{4000}$th inch. This is probably to be explained by the growth of the medullary fold at this point, which pushes back the primitive groove. At the sides of the head the cells are larger than anywhere else in the epiblast, being here about $\frac{1}{1000}$th inch in diameter. I at present see no explanation of this fact. At the periphery of the pellucid area and over the vascular area the cells are $\frac{1}{1500}$th to $\frac{1}{2000}$th inch in diameter, but at the periphery of the opaque area they are smaller again, being about the $\frac{1}{3000}$th of an inch. This smaller size at the periphery of the area opaca is remarkable, since in the

earlier stages the most peripheral epiblast cells were the largest. It, perhaps, implies that more rapid growth is at this time taking place in that part of the epiblast which is spreading over the yolk sacs.

On the Disappearance *of the* Primitive Groove *in the* Embryo Chick. By F. M. Balfour, of Trinity College, Cambridge.

The investigations of Dursy ('Der Primitivstreif des Hühnchens,' von Dr. E. Dursy. Lahr, 1866) on the primitive groove, showing that it is a temporary structure, and not connected with the development of the neural canal, have in this country either been ignored or rejected. They are, nevertheless, perfectly accurate; and had Dursy made use of sections to support his statements I do not think they would so long have been denied. In Germany, it is true, Waldeyer has accepted them with a few modifications, but I have never seen them even alluded to in any English work. The observations which I have made corroborating Dr. Dursy may, perhaps, under these circumstances be worth recording.

After about twelve hours of incubation the pellucid area of a hen's egg has become somewhat oval, with its longer axis at right angles to the long axis of the egg. Rather towards the hinder (narrower) end of this an opaque streak has appeared, with a somewhat lighter line in the centre. A section made at the time shows that the opaque streak is due partly to a thickening of the epiblast, but more especially to a large collection of the rounded mesoblast cells, which along this opaque line form a thick mass between the epiblast and the hypoblast. The mesoblast cells are in contact with both hypoblast and epiblast, and appear to be fused with the latter. The line of junction between them can, however, almost always be made out.

Soon after the formation of this primitive streak a groove is formed along its central line by a pushing inwards of the epiblast. The epiblast is not thinner where it lines the groove, but the mass of mesoblast below the groove is considerably thinner than at its two sides. This it is which produces the peculiar appearance of the primitive groove when the blastoderm is viewed by transmitted light as a transparent line in the middle of an opaque one.

This groove, as I said above, is placed at right angles to the long axis of the egg, and nearer the hind end, that is, the narrower end of the pellucid area. It was called "the primitive groove" by the early embryologists, and they supposed that the neural canal arose from the closure of its edges above. It is always easy to distinguish this groove, in transverse sections, by several well-marked characters. In the first place, the epiblast and mesoblast always appear more or less fused together underneath it; in the second place, the epiblast does not become thinner where it lines the groove; and in the third place, the mesoblast beneath it never shows any signs of being differentiated into any organ.

As Dursy has pointed out, there is frequently to be seen in fresh specimens, examined as transparent objects, a narrow opaque line running down the centre of this groove. I do not know what this line is caused by, as there does not appear to be any structural feature visible in sections to which it can correspond.

From the twelfth to the sixteenth hour the primitive groove grows rapidly, and by the sixteenth hour is both absolutely and considerably longer than it was at the twelfth hour, and also proportionately longer as compared with the length of the pellucid area.

There is a greater interval between its end and that of the pellucid area in front than behind.

At about the sixteenth hour, or a little later, a thickening of the mesoblast takes place in front of the primitive groove, forming an opaque streak, which in fresh specimens looks like a continuation from the anterior extremity of the primitive groove (*vide* Pl. XII, fig. 3). From hardened specimens, however, it is easy to see that the connection of this streak with the primitive groove is only an apparent one. Again, it is generally possible to see that in the central line of this streak there is a narrow groove. I do not feel certain that there is no period when this groove may not be present, but its very early appearance has not been recognised either by Dursy or by Waldeyer. Moreover, both these authors, as also His, seem to have mistaken the opaque streak spoken of above for the notochord. This, however, is not the case, and the notochord does not make its appearance till somewhat later. The mistake is of very minor importance, and probably arose in Dursy's case from his not sufficiently making use of sections. At about the time the streak in front of the primitive groove makes its appearance a semicircular fold begins to be formed near the

anterior extremity of the pellucid area, against which the opaque streak, or as it had, perhaps, better be called, "the medullary streak," ends abruptly.

This fold is the head fold, and the groove along the medullary streak is the medullary groove, which subsequently forms the cavity of the medullary or neural canal.

Everything which I have described above can without difficulty be made out from the examination of fresh and hardened specimens under the simple microscope; but sections bring out still more clearly these points, and also show other features which could not have been brought to light without their aid. In Pl. XII, figs. 1 and 2, two sections of an embryo of about eighteen hours are shown. The first of these passes through the medullary groove, and the second of them through the extreme anterior end of the primitive groove. The points of difference in the two sections are very obvious.

From fig. 1 it is clear that a groove has already been formed in the medullary streak, a fact which was not obvious in the fresh specimen. In the second place the mesoblast is thickened both under the groove and also more especially in the medullary folds at the sides of the groove; but shows hardly a sign of the differentiation of the notochord. So that it is clear that the medullary streak is not the notochord, as was thought to be the case by the authors above mentioned. In the third place there is no adhesion between the epiblast and the mesoblast. In all the sections I have cut through the medullary groove I have found this feature to be constant; while (for instance, as in Pl. XIII, fig. 2, and in Pl. XII, figs. 4 and 5) all sections through the primitive groove show most clearly an adhesion between the epiblast and mesoblast. This fact is both strongly confirmatory of the separate origins of the medullary and primitive grooves, and is also important in itself, as leaving no loophole for supposing that in the region of embryo there is any separation of the cells from the epiblast to form the mesoblast.

By this time the primitive groove has attained its maximum growth, and from this time begins both absolutely to become smaller, and also gradually to be pushed more and more backwards by the growth of the medullary groove.

The specimen figured in Pl. XIII, fig. 6, magnified about ten diameters, shows the appearance presented by an embryo of twenty-three hours. The medullary groove (*m c*) has become much wider and deeper than it was in the earlier stage; the medullary folds (A) are also broader and more conspicuous. The medullary groove widens very much posteriorly, and also the

medullary folds separate far apart to enclose the anterior end of the primitive groove (*p r*).

All this can easily be seen with a simple microscope, but the sections taken from the specimen figured also fully bear out the interpretations given above, and at the same time show that the notochord has at this age begun to appear. The sections marked 1—5 pass respectively through the lines with corresponding numbers in fig. 6. Section 1 passes through the middle of the medullary canal.

In it the following points are to be noted. 1. That the epiblast becomes very much thinner where it lines the medullary canal (*m c*), a feature never found in the epiblast lining the primitive groove. 2. That the mesoblast is very much thickened to form the medullary folds at A, A, while there is no adherence between it and the epiblast, below the primitive groove. 3. The notochord (*ch*) has begun to be formed, though its separation from the rest of the mesoblast is not as yet very distinct.[1]

In fig. 2 the medullary groove has become wider and the medullary folds broader, the notochord has also become more expanded: the other features are the same as in section 1. In the third section the notochord is still more expanded; the bottom of the now much expanded medullary groove has become raised to form the ridge which separates the medullary from the primitive groove. The medullary folds are also flatter and broader than in the previous section. Section 4 passes through the anterior end of the primitive groove. Here the notochord is no longer visible, and the adherence between the mesoblast and epiblast below the primitive groove comes out in marked contrast with the entire separation of the two layers in the previous sections.

The medullary folds (A) are still visible outside the raised edges of the primitive groove, and are as distinctly as possible separate and independent formations, having no connection with the folds of the primitive groove. In the last section, which is taken some way behind section 4, no trace of the medullary folds is any longer to be seen, and the primitive groove has become deeper. This series of sections, taken in conjunction with the specimen figured in Pl. XIII, fig. 6, must remove all possible doubt as to the total and entire independence of the primitive and medullary grooves. They arise in different parts of the blastoderm; the one reaches its maximum growth before the other has commenced to be formed; and finally, they are distinguished by almost every

[1] In the figure the notochord has been made too distinct.

possible feature by which two such grooves could be distinguished.

Soon after the formation of the notochord, the proto-vertebræ begin to be formed along the sides of the medullary groove (Pl. XIII, fig. 7, *p v*). Each new proto-vertebra (of those which are formed from before backwards) arises just in front of the anterior end of the primitive groove. As growth continues, the primitive groove becomes pushed further and further back, and becomes less and less conspicuous, till at about thirty-six hours only a very small and curved remnant is to be seen behind the sinus rhomboidalis; but even up to the forty-ninth Dursy has been able to distinguish it at the hinder end of the embryo.

The primitive groove in the chick is, then, a structure which appears very early, and soon disappears without entering directly into the formation of any part of the future animal, and without, so far as I can see, any function whatever. It is clear, therefore, that the primitive groove must be the rudiment of some ancestral feature; but whether it is a rudiment of some structure which is to be found in reptiles, or whether of some earlier form, I am unable to decide. It is just possible that it is the last trace of that involution of the epiblast by which the hypoblast is formed in most of the lower animals. The fact that it is formed in the hinder part of the pellucid area perhaps tells slightly in favour of this hypothesis, since the point of involution of the epiblast not unfrequently corresponds with the position of the anus.

The Development *of the* Bloodvessels *of the* Chick.

By F. M. Balfour, of Trinity College, Cambridge.

The development of the first blood-vessels of the yolk-sac of the chick has been investigated by a large number of observers, but with very discordant results. A good historical *résumé* of the subject will be found in a paper of Dr. Klein (liii Band der 'K. Akad. der Wissensch. der Wien'), its last investigator.

The subject is an important one in reference to the homologies of the blood-vascular system of the vertebrata. As I shall show in the sequel (and on this point my observations agree

with those of Dr. Klein), the blood-vessels of the chick do not arise as spaces or channels between the cells of the mesoblast; on the contrary, they arise as a network formed by the united processes of mesoblast-cells, and it is through these processes, and not in the spaces between them, that the blood flows. It is, perhaps, doubtful whether a system of vessels arising in this way can be considered homologous with any vascular system which takes its origin from channels hollowed out in between the cells of the mesoblast.

My own researches chiefly refer to the development of the blood-vessels in the pellucid area. I have worked but very slightly at their development in the vascular area; but, as far as my observations go, they tend to prove that the mode of their origin is the same, both for the pellucid and the vascular area.

The method which I have principally pursued has been to examine the blastoderm from the under surface. It is very difficult to obtain exact notions of the mode of development of the blood-vessels by means of sections, though these come in as a valuable confirmation of the other method.

For the purpose of examination I have employed (1) fresh specimens; (2) specimens treated with spirit, and then mounted in glycerine; (3) specimens treated with chloride of gold for about half a minute, and then mounted in glycerine; and (4) specimens treated with osmic acid.

All these methods bring out the same appearances with varying clearness; but the successful preparations made by means of the gold chloride are the best, and bring out the appearances with the greatest distinctness.

The first traces of the blood-vessels which I have been able to distinguish in the pellucid area are to be seen at about the thirtieth hour or slightly earlier, at about the time when there are four to five proto-vertebræ on each side.

Fig. 1 shows the appearance at this time. Immediately above the hypoblast there are certain cells whose protoplasm sends out numerous processes. These processes vary considerably in thickness and size, and quickly come in contact with similar processes from other cells, and unite with them.

I have convinced myself, by the use of the hot stage, that these processes continually undergo alteration, sometimes uniting with other processes, sometimes becoming either more elongated and narrower or broader and shorter. In this way a network of somewhat granular protoplasm is formed with nuclei at the points from which the processes start.

From the first a difference may be observed in the character of this network in different parts of the pellucid area. In the anterior part the processes are less numerous and thicker, the nuclei fewer, and the meshes larger; while in the posterior part the processes are generally very numerous, and at first thin, the meshes small, and the nuclei more frequent. As soon as this network commences to be formed the nuclei begin to divide. I have watched this take place with the hot stage. It begins by the elongation of the nucleus and division of the nucleolus, the parts of which soon come to occupy the two ends of the nucleus. The nucleus becomes still longer and then narrows in the centre and divides. By this means the nuclei become much more numerous, and are found in almost all the larger processes. Whether they are carried out into the processes by the movement of the surrounding protoplasm, or whether they move through the protoplasm, I have been unable to determine; the former view, however, seems to be the most probable.

It is possible that some nuclei arise spontaneously in the protoplasm, but I am much more inclined to think that they are all formed by the division of pre-existing nuclei—a view favoured by the number of nuclei which are seen to possess two nucleoli. Coincidently with the formation of the new nuclei the protoplasm of the processes, as well as that surrounding the nuclei at the starting-points of the processes, begins to increase in quantity.

At these points the nuclei also increase more rapidly than elsewhere, but at first the resulting nuclei seem to be all of the same kind.

In the anterior part of the pellucid area (fig. 4) the increase in the number of nuclei and in the amount of protoplasm at the starting-points of the protoplasm is not very great, but in the posterior part the increase in the amount of the protoplasm at these points is very marked, and coincidently the increase in number of the nuclei is also great. This is shown in figs. 2 and 3. These are both taken from the tail end of an embryo of about thirty-three hours, with seven or eight proto-vertebræ. Fig. 3 shows the processes beginning to increase in thickness, and also the protoplasm at the starting-points increasing in quantity; at the same time the nuclei at these points are beginning to become more numerous. Fig. 3 is taken from a slightly higher level, *i. e.* slightly nearer the epiblast. In it the protoplasm is seen to have increased still more in quantity, and to be filled with nuclei. These nuclei have begun to be slightly coloured, and one of them is seen to possess two nucleoli.

Very soon after this a change in the nuclei begins to be observed, more especially in the hinder part of the embryo. While before this time they were generally elongated, some of them now become more nearly circular. In addition to this, they begin to have a yellowish tinge, and the nuclei, when treated with gold (for in the fresh condition it is not easy to see them distinctly), have a more jagged and irregular appearance than the nucleoli of the other nuclei.

This change takes place especially at the starting-points of the processes, so that the appearance presented (fig. 5) is that of spherical masses of yellowish nuclei connected with other similar spherical masses by protoplasmic processes, in which nuclei of the original type are seen imbedded. These masses are surrounded by a thin layer of protoplasm, at the edge of which a normal nucleus may here and there be detected, as at fig. 5 (*a*) and *a'*, the latter possessing two nucleoli. Some of these processes are still very delicate, and it is exceedingly probable that they undergo further changes of position before the final capillary system is formed.

These differentiated nuclei are the first stage in the formation of the blood-corpuscles From their mode of formation it is clear that the blood-corpuscles of the Sauropsida are to be looked upon as nuclei containing nucleoli, rather than as cells containing nuclei; indeed, they seem to be merely ordinary nuclei with red colouring matter.

This would make them truly instead of only functionally homologous with the red corpuscles of the Mammalia, and would well agree with the fact that the red corpuscles of Mammalia, in their embryonic condition, possess what have previously been called nuclei, but which might perhaps more properly be called nucleoli.

In the anterior part of the blastoderm the processes, as I have stated, are longer and thinner, and the spaces enclosed between them are larger. This is clearly brought out in Pl. XIV, fig. 4. But, besides these large spaces, there are other smaller spaces, such as that at *b*. It is, on account of the transparency of the protoplasm, very difficult to decide whether these are vacuoles or simply spaces enclosed by the processes, but I am inclined to think that they are merely spaces. The difficulty of exactly determining this point is increased by the presence of numerous white-yolk spherules in the hypoblast above, which considerably obscure the view. At about the same time that the blood-corpuscles appear in the posterior end of the pellucid area, or frequently a little later, they begin to be formed in the anterior part also. The masses of them are, however, far smaller and far fewer than

in the posterior part of the embryo. It is at the tail end of the pellucid area that the chief formation of blood-corpuscles takes place.

The part of the pellucid area intermediate in position between the anterior and posterior ends of the embryo is likewise intermediate as regards the number of corpuscles formed and the size of the spaces between the processes; the spaces being here larger than at the posterior extremity, but smaller than the spaces in front. Close to the sides of the embryo the spaces are, however, smaller than in any other part of the pellucid area. It is, however, in this part that the first formation of blood-corpuscles takes place, and that the first complete capillaries are formed.

We have then somewhat round protoplasmic masses filled with blood-corpuscles and connected by means of processes, a few of which may begin to contain blood-corpuscles, but the majority of which only contain ordinary nuclei. The next changes to be noticed take place in the nuclei which were not converted into blood-corpuscles, but which were to be seen in the protoplasm surrounding the corpuscles. They become more numerous and smaller, and, uniting with the protoplasm in which they were imbedded, become converted into flat cells (spindle shaped in section), and in a short time form an entire investment for the masses of blood-corpuscles. The same change also occurs in the protoplasmic processes which connect the masses of corpuscles. In the case of those processes which contain no corpuscles the greater part of their protoplasm seems to be converted into the protoplasm of the spindle-shaped cells. The nuclei arrange themselves so as completely to surround the exterior of the protoplasmic processes. In this way each process becomes converted into a hollow tube, completely closed in by cells formed from the investment of the original nuclei by the protoplasm which previously formed the solid processes. The remainder of the protoplasm probably becomes fluid, and afterwards forms the plasma in which the corpuscles float. While these changes are taking place the formation of the blood-corpuscles does not stand still, and by the time a system of vessels, enclosed by cellular walls, is formed out of the protoplasmic network, a large number of the connecting processes in this network have become filled with blood-corpuscles. The appearances presented by the network at a slightly later stage than this is shown in Pl. XIV, fig. 6, but in this figure all the processes are seen to be filled with blood-corpuscles.

This investment of the masses of corpuscles by a cellular wall occurs much earlier in some specimens than in others,

both in relation to the time of incubation and to the completion of the network. It is generally completed in some parts by the time there are eight or nine proto-vertebræ, and is almost always formed over a great part of the pellucid area by the thirty-sixth hour. The formation of the corpuscles, as was pointed out above, occurs earliest in the central part of the hour-glass shaped pellucid area, and latest in its anterior part. In the hinder part of the pellucid area the processes, as well as their enlarged starting-points, become entirely filled with corpuscles; this, however, is by no means the case in its anterior part. Here, although the corpuscles are undoubtedly developed in parts as shown in fig. 7, yet a large number of the processes are entirely without them. Their development, moreover, is in many cases very much later. When the development has reached the stage described, very little is required to complete the capillary system. There are always, of course, a certain number of the processes which end blindly, and others are late in their development, and are not by this time opened; but, as a general rule, when the cellular investment is formed for the masses of corpuscles, there is completed an open network of tubes with cellular walls, which are more or less filled with corpuscles. These become quickly driven into the opaque area in which at that time more corpuscles may almost always be seen than in the pellucid area.

By the formation of a network of this kind it is clear that there must result spaces enclosed between the walls of the capillaries; these spaces have under the microscope somewhat the appearance of being vesicles enclosed by walls formed of spindle-shaped cells. In reality they are only spaces enclosed at the sides, and, as a general rule, not above and below. They have been mistaken by some observers for vesicles in which the corpuscles were supposed to be developed, and to escape by the rupture of the walls into the capillary spaces between. This mistake has been clearly pointed out by Klein (loc. cit.).

At the time when these spaces are formed, and especially in the hinder two thirds of the pellucid area, and in the layer of blood-vessels immediately above the hypoblast, a formation takes place which forms in appearance a secondary investment of the capillaries. Dr. Klein was the first to give a correct account of this formation. It results from the cells of the mesoblast in the meshes of the capillary system. Certain of these cells become flattened, and send out fine protoplasmic processes. They arrange themselves so as completely to enclose the spaces between the capillaries, forming in this way vesicles.

Where seen on section (*vide* fig. 6) at the edge of the vesicles these cells lining the vesicles appear spindle shaped, and look like a secondary investment of the capillaries. This investment is most noticeable in the hinder two thirds of the pellucid area; but, though less conspicuous, there is a similar formation in its anterior third, where there would seem to be only veins present. Dr. Klein (loc. cit., fig. 12) has also drawn this investment in the anterior third of the pellucid area. He has stated that the vessels in the mesoblast between the splanchnopleure and the somatopleure, and which are enclosed by prolongations from the former, do not possess this secondary investment; he has also stated that the same is true for the sinus terminalis; but I am rather doubtful whether the generalisation will hold, that veins and arteries can from the first be distinguished by the latter possessing this investment. I am also rather doubtful whether the spaces enclosed by the protoplasmic threads between the splanchnopleure and somatopleure are the centres of vessels at all, since I have never seen any blood-corpuscles in them.

It is not easy to learn from sections much about the first stages in the formation of the capillaries, and it is impossible to distinguish between a completely-formed vessel and a mere spherical space. The fine protoplasmic processes which connect the masses of corpuscles can rarely be seen in sections, except when they pass vertically, as they do occasionally (*vide* Pl. XV, fig. 2) in the opaque area, joining the somatopleure and the splanchnopleure. Dr. Klein considers these latter processes to be the walls of the vessels, but they appear rather to be the processes which will eventually become new capillaries.

From sections, however, it is easy to see that the appearances of the capillaries in the vascular area are similar to the appearances in the pellucid area, from which it is fair to conclude that their mode of formation is the same in both. It is also easy to see that the first formation of vessels occurs in the splanchnopleure, and that even up to the forty-fifth hour but few or no vessels are found in the somatopleure. The mesoblast of the somatopleure is continued into the opaque area as a single layer of spindle-shaped cells.

Sections clearly show in the case of most of the vessels that the secondary investment of Klein is present, even in the case of those vessels which lie immediately under the somatopleure.

In reference to the origin of particular vessels I have not much to say. Dr. Klein's account of the origin of the sinus

terminalis is quite correct. It arises by a number of the masses of blood-corpuscles, similar to those described above, becoming connected together by protoplasmic processes. The whole is subsequently converted into a continuous vessel in the usual way.

From the first the "sinus terminalis" possesses cellular walls, as is clear from its mode of origin. I am inclined to think that Klein is right in saying that the aortæ arise in a similar manner, but I have not worked out their mode of origin very fully.

It will be seen from the account given above that, in reference to the first stages in the development of the blood-vessels, my observations differ very considerably from those of Dr. Klein; as to the later stages, however, we are in tolerable agreement. We are in agreement, moreover, as to the fundamental fact that the blood-vessels are formed by a number of cells becoming connected, and by a series of changes converted into a network of vessels, and that they are not in the first instance merely channels between the cells of the mesoblast.

By the forty-fifth hour colourless corpuscles are to be found in the blood whose exact origin I could not determine; probably they come from the walls of the capillaries.

In the vessels themselves the coloured corpuscles undergo increase by division, as has already been shown by Remak. Corpuscles in the various stages of division may easily be found. They do not appear to show very active amœboid movements in the vessels, though their movements are sometimes very active when removed from the body.

To recapitulate—some of the cells of the mesoblast of the splanchnopleure send out processes, these processes unite with the processes from other cells, and in this way a network is formed. The nuclei of the original cells divide, and at the points from which the processes start their division is especially rapid. Some of them acquire especially at these points a red colour, and so become converted into blood-corpuscles; the others, together with part of the protoplasm in which they are imbedded, become converted into an endothelium both for the processes and the masses of corpuscles; the remaining protoplasm becomes fluid, and thus the original network of the cells becomes converted into a network of hollow vessels, filled with fluid, in which corpuscles float.

In reference to the development of the heart, my observations are not quite complete. It is, however, easy to prove from sections (*vide* figs. 3 and 4, Pl. XV) that the cavity of the

heart is produced by a splitting or absorption of central cells of the thickened mesoblast of the splanchnopleure, while its muscular walls are formed from the remaining cells of this thickened portion. It is produced in the following way:— When the hypoblast is folded in to form the alimentary canal the mesoblast of the splanchnopleure follows it closely, and where the splanchnopleure turns round to assume its normal direction (fig. 4) its mesoblast becomes thickened. This thickened mass of mesoblast is, as can easily be seen from figs. 3 and 4, Pl. XV, entirely distinct from the mesoblast which forms the outside walls of the alimentary canal. At the point where this thickening occurs an absorption takes place to form the cavity of the heart. The method in which the cavity is formed can easily be seen from figs. 3 and 4, Pl. XV. It is in fig. 4 shown as it takes place in the mesoblast on each side, the folds of the splanchnopleure not having united in the middle line; and hence a pair of cavities are formed, one on each side. It is, however, probable that, in the very first formation of the heart, the cavity is single, being formed after the two ends of the folded mesoblast have united (*vide h* 2, fig. 3, Pl. XV). In some cases the two folds of the mesoblast appear not at first to become completely joined in the middle line; in this case the cavity of the heart is still complete from side to side, but the mesoblast-cells which form its muscular walls are deficient above. By the process of absorption, as I said, a cavity is produced in the thickened part of the mesoblast of the splanchnopleure, a cavity which is single in front, but becomes divided further behind, where the folds of the mesoblast have not united, into two cavities, to form the origin of the omphalomeseraic veins. As the folding proceeds backwards the starting-point of the omphalomeseraic veins is also pushed backwards, and the cavities which were before separated become joined together. From its first formation the heart is lined internally by an endothelium; this is formed of flattened cells, spindle shaped in section. The exact manner of the origin of this lining I have not been able to determine; it is, however, probable that some of the central mesoblast-cells are directly converted into the cells of the endothelium.

I have obtained no evidence enabling me to determine whether Dr. Klein is correct in stating that the cells of the mesoblast in the interior of the heart become converted partly into blood-corpuscles and partly into a cellular lining forming the endothelium of the heart, in the same way that the blood-vessels in the rest of the blastoderm are formed. But

I should be inclined to think that it is very probable—certainly more probable than that the cavity of the heart is formed by a process of splitting taking place. Where I have used the word "absorption" in speaking of the formation of the cavity of the heart, I must be understood as implying that certain of the interior cells become converted into the endothelium, while others either form the plasma or become blood-corpuscles.

The originally double formation of the hinder part of the heart probably explains Dr. Afanassiev's statement ('Bullétin de l'Académ. Impériale de St. Petersb.,' tom. xiii, pp. 321—335), that he finds the endothelium of the heart originally dividing its interior into two halves; for when the partition of the mesoblast which separated at first the two halves of the heart became absorbed, the endothelium lining of each of the originally separate vessels would remain complete, dividing the cavity of the heart into two parts. The partition in the central line is, however, soon absorbed.

The account given above chiefly differs from that of Remak by not supposing that the mesoblast-cells which form the heart are in any way split off from the wall of the alimentary canal.

There can be no doubt that His is wrong in supposing that the heart originates from the mesoblast of the splanchnopleure and somatopleure uniting to form its walls, thus leaving a cavity between them in the centre. The heart is undoubtedly formed out of the mesoblast of the splanchnopleure only.

Afanassiev's observations are nearer to the truth, but there are some points in which he has misinterpreted his sections.

Sections Pl. XV, figs. 3 and 4, explain what I have just said about the origin of the heart. Immediately around the notochord the mesoblast is not split, but a very little way outside it is seen to be split into two parts (so) and *sp*; the former of these follows the epiblast, and together with it forms the somatopleure, which has hardly begun to be folded at the line where the sections are taken. The latter (*sp*) forms with the hypoblast (*hy*) the splanchnopleure, and thus has become folded in to form the walls of the alimentary canal (*d*). In fig. 4 the folds have not united in the central line, but in fig. 3 they have so united. In fig. 4, where the mesoblast, still following the hypoblast, turns back to assume its normal direction, it is seen to be thickened and to have become split, so that a cavity (*of*) (of the omphalomeseraic vein) is formed in it on each side, lined by endothelium.

In the section immediately behind section fig. 4 the mesoblast was thickened, but had not become split.

In fig. 3 the hypoblast folds are seen to have united in the centre, so as to form a completely closed digestive canal (*a*); the folds of the mesoblast have also united, so that there is only a single cavity in the heart (*h z*), lined, as was the case with the omphalomeseraic veins, by endothelium.

In conclusion, I have to thank Dr. Foster for his assistance and suggestions throughout the investigations which have formed the subject of these three short papers, and which were well carried on in the apartments used by him as a Physiological Laboratory.

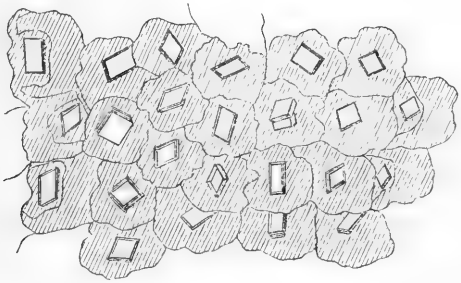
On the CRYSTALS *in the* TESTA *of the* ELM (*Ulmus suberosa*, Ehrh.), *and the* CHARACTER *of the* EPIDERMIS *of the* TWAY-BLADE. By GEORGE GULLIVER, F.R.S. (With a Woodcut.)

DURING the month of May the fruit of the elm is shed and scattered in profusion on the ground, often so as to make patches in our paths. Each fruit is a capsule, of a pale green colour, somewhat oval or rounded, very flat, and about as big as the thumb-nail. The seed is contained near the centre of this compressed and winged capsule or samara, and the outer coat of the seed is the seat of the crystals. Every cell of this part contains a short, brilliant crystal, in form cubical, lozenge-shaped, or prismatic; sometimes flattened parallelograms, rarely either single or double octahedrons, or twins like certain crystals of gypsum. Their mean diameter is about $\frac{1}{3000}$th of an inch. They are so firmly imbedded in the tissue as to be separable from it with difficulty; this may be overcome by macerating the capsule in water until the tissue becomes pulpy from putridity, when the crystals may be isolated, and made to roll over in the microscopic field of vision.

The testa, thickly studded with these minute and brilliant crystals, is a very beautiful microscopic spectacle easily prepared for observation, and in the spring season most bountifully provided in every rural district. And this is one reason for noticing this profusion of crystals; but I am chiefly led to do so because it is probable that they will prove excellent for the purpose of investigations concerning the development of such crystals, since this must be rapid in a part so quickly arriving at maturity as the fruit of the elm. They are not unlike in form to those depicted in the stem of the Screw

Pine by Professor Thiselton Dyer ('Quart. Journ. Micr. Science,' vol. xii, new ser.). I think he has somewhere noticed that he is inclined, in accordance with some observations by Pfitzer, to suppose that the crystals are developed in the cell-wall, and not within the cell.[1] If thus originally formed outside the cell, and then projected into its interior, and carrying a surrounding film of membrane thither, the crystal within the cell would be really outside the membrane, just as an animal viscus is seated without, though apparently within, the serous sac. Though unfortunately prevented in the spring from fulfilling my intention of watching the earliest formation and subsequent development of these crystals, I saw a few of them, in the mature fruit, appearing as if they had first been formed in the cell-wall, and then pushed its inner layer within the cell-cavity, as shown in some of the drawings of the crystals; but the observations on this point were by no means conclusive, though they sufficed to indicate that further inquiries concerning it are desirable, and would probably be solved by careful examination of these crystals in the young fruit of the elm.

The annexed woodcut represents a fragment of the testa of the elm, as seen under an object-glass of a quarter-inch focal length.



Testa of Elm, seen with a $\frac{1}{4}$-inch object-glass, May, 1873.

It has long been known that such crystals occur more or less abundantly, and have been commonly and erroneously called "raphides," in trees and shrubs; sometimes the crystals are scanty, seldom altogether absent, and always rather plentiful in British Amentiferæ. In the leaf-stalks and endo-

[1] [See 'Quar. Journ. Micr. Sci.,' 1872, pp. 288-9. Pfitzer considered that the crystals which he described originated free in the middle of the protoplasmic cell-contents and subsequently became imbedded in the thickened cell-wall.—W. T. D.]

phlœum and mesophlœum of the elm these crystals occur, but far less remarkably than in the testa of this species. Therein they were noticed by the brothers Edwin and John Quekett, but seem to have been forgotten and never depicted. And now it is high time, while admitting these crystals into the cabinet of microscopic curiosities, and probably as interesting for experiments with polarised light, that they should be impressed into the service of scientific phytotomy; and hence they are now so described that either the tyro or expert may avail himself of their lessons. The crystals, so far as I have been able to make out, are composed in great part of oxalate of lime.

Epidermis *of the* Tway-blade (*Listera ovata*).

Some years have passed since Dr. Lankester solicited observations in this Journal on the taxonomic value of the vegetable epidermis and its appendages; but, judicious as that solicitation was, I know not that it ever met with any adequate response, though I am quite sure that, after the subject has been fully investigated, interesting and useful results will be secured for systematic botany. The cell-characters and crystals should be recorded in the description of every species, if we mean to attain to a precise knowledge of the life-history of the plant; and I have often shown how in this manner valuable assistance may be afforded to taxonomy.

The present communication is intended to show that thy form of the epidermal cells of *Listera ovata* differs plainly from that of the corresponding part of some other Orchids.

In this species the epidermal cells on the under side of the leaf-blade have their margins so remarkably sinuous as to form a good example of that kind of tissue which botanists have named colpenchyma; while on the upper surface of the leaf of the same plant the cells have smooth margins, more or less polygonal from the mutual pressure of the oval or oblong cells; and thus, besides the stomata on the lower surface, the epidermis of the two sides of the leaf differs so plainly as to present pretty objects for microscopic examination.

But there are several Orchids in which this character does not exist. Thus, after an examination of the fresh leaves of nine other species, the epidermal cells, except the stomata, were found alike on both sides of the leaves, and resembling the corresponding cells on the upper side of the leaf of the Tway-blade. The species thus examined at the same time for comparison were *Orchis fusca*, *Orchis mascula*,

Gymnadenia conopsea, *Aceras anthropophora*, *Habennaria bifolia*, *Ophrys muscifera*, *Ophrys aranifera*, *Ophrys arachnites*, and *Cephalanthera grandiflora*.

To define the exact value of this character would require an examination of all the other British species, as well as the wilderness of exotic orchids. But this kind of inquiry into the diagnostic importance of cells and plant-crystals is just what is still wanted in descriptive botany, and the more so because regularly neglected by systematists; yet surely sufficient evidence—to which the merest fragment is added in the present paper—exists of the usefulness of such characters; for example, though *Lemna minor* and *Wolffia arrhiza* were long regarded as identical, they may now be well distinguished by the difference in the cells of the epidermis;[1] so may *Hymenophyllum Wilsoni* and *H. Tunbridgense* by the frond-cells ('Seeman's Journal of Botany,' October, 1863); certain nearly allied Juncaceæ by the pith-cells ('Ann. Nat. Hist.,' December, 1863); some Ranunculaceæ by their pollen-grains ('Popular Science Review,' 1868, vol. vii); and, as I have so often shown, many orders or genera by the presence or absence of raphides.

[1] [See 'Seeman's Journal of Botany,' 1866, pp. 375-377.]

QUARTERLY CHRONICLE OF MICROSCOPICAL SCIENCE.

BOTANY.

Development of the flower of the Papilionaceæ.—Urban finds ('Bot. Zeit.,' 1873, p. 265) that in the genus Medicago (*Sativa lupulina*, &c), that the anterior sepal appears first, then the two lateral, and lastly the two posterior sepals. As soon as these latter appear, the whole series become coherent at the base. After the formation of the sepals the carpel appears as a semilunar projection which rapidly increases in size. In its early stages the carpels exhibit a very striking resemblance to the first leaf which is produced immediately after the cotyledons. Before any indication of the petal appears, the outer whorl of stamens develops in the hollow between the sepals and the carpel. The second alternating whorl of stamens follows rapidly after the first. The petal appears last.

Crystal prisms in Leguminosæ.—Urban directs attention ('Bot. Zeit.,' 1873, p. 266), to the occurrence of numerous crystals of calcium oxalate in the bracts of *Medicago triginella* and *Pocockia*. They are well developed rhombic prisms with oblique ends and occur in the parenchymatous cells surrounding the fibro-vascular bundles. (Aus den Sitzungsberichten der Gesellschafte naturforschenden Freunde zu Bolim.)

Development of the flower of Canna.—Dr. A. W. Eichler concludes ('Bot. Zeit.,' 1173. p. 177 *et seq.*) that the flower corresponds closely with that of the typical monocotyledons having the formula $Ca_3\,Co_3\,An_0 + (0.1^2.1)\,Gn_3$.

The flower has five whorls of three parts. The first and second whorl from the penanth the next two the androcani, the last is the glynocani. The whorl of the penanth and glynocani are complete, while the outer whorl of the androcani is entirely wanting. In the inner whorl of stamens the first stamen is wanting, the third remains simple and forms a stammode forming the so-called labellum of canna. The second stamen has a half anther, with pollen, the other half

forming a petaloid appendage. This second stamen often develops stipule-like appendages; 2, as a *Canna Indica*; 3, in *C. Sellowii*.

On the General Occurrence of Multiplication of Chlorophyll Granules by Division in the Vegetable Kingdom,—First noticed by Naegeli in *Nitella*, *Bryopsis*, *Valonia*, and in the prothallus of ferns. It was considered as special to the cryptogams until Sanio discovered it in the tissue of the young internodes of *Peperomia blanda*, and in the cells of the lower leaf-epidemis of *Ranunculus Ficaria*. Knef ('Bot. Zeit.,' 1872, p. 14) considers it a common occurrence in flowering plants, and he observed it in *Ceratophyllum*, *Elodea canadensis*, *Utricularia vulgaris* and *Myriophyllum spicatum*, it is easy to observe in all but the last. Numerous other cases were observed, but the best examples can be obtained by examining the young epidermis from the lower side of the leaf of *Sambucus nigra*, *Nupulicus parviflora* and *Lactuca mucalis*.

Amyloid Corpuscles (*Somatia*) *in the Favilla of Pollen*. P. A. Saccardo ('Nuovo Giorn. Bot. Ital,' 1872, pp. 241—243).—These particles have been detected by the author in numerous plants belonging to widely different families, with a magnifying power of 800 to 1000 diameters. Their diameter varies from 1- to 3-1000ths of a millimètre, or about one third the size of the human blood-corpuscle. The usual shape is circular passing to elliptical, and slightly dumb-bell shaped. In *œnotheia* they are fusiform. Treatment with iodine colours the nuclear portion violet, but the superficial portion remains white and pellucid. The particles exhibit oscillating movements—probably Brownian Tubes existing in stem of *Sambucus nigra*, and described as *Ahizomorpha parallela*, Roberge (C. A. J. A. Oudeman's 'Archives Néerlandaises, t. vii., pp. 209—229).

On the exterior of the pith of the elder striæ exist, varying in number from fifty to sixty on a pith cylinder 12 mm. in diameter, but much less on the smaller branches. Immediately below the terminal bud the striæ are colourless, lower down they are pale pink, and finally brown. They are never branched, and suffer no interruption at the nodes. Their diameter varies from one eighth to one fortieth mm. They have been described as a parasitic fungus, but they appear to be a normal element of the pith of the elder, but can only be referred to the vague class of "tubes succifères" (*Saftslanche Sachs*). These tubes contain, in the dry state, a hyaline brittle matter, which is applied to the inner surface of their wall, and forms a more or less thick layer, which at

intervals is developed into partitions dividing the tube into distinct cavities. Treated with water or alcohol this matter swells considerably—so much so as almost to obliterate the central cavities. (It also swells with ether, glycerine, and acetic acid.) It presents, under these circumstances, exceedingly minute and faint striæ, and occasionally a somewhat spongy aspect.

In their earliest condition these tubes consist of rows of highly elongated cells, lined internally with a protoplasma-like layer which accumulates at the extremities. Eventually the partitions merge in this accumulation of lining matter; at any rate they cannot be distinguished from it. The formation of the tube appears not to be confined to the primitive meristem, but it continues on the surface of the pith. The cavities of the tubes are at first filled with a watery sap, but eventually contain nothing but air, and the incrusting matter is then not swollen, though immediately becoming so when treated with water.

Chemically the incrusting matter appears to be a modification of cellulose. It is coloured blue by solution of iodine with zinc chloride, although it is unacted upon by strong sulphuric acid, by which ordinary cellulose is dissolved. A blue reaction with iron sulphate and the brown colour which the incrusting matter finally assumes appear to indicate its containing tannin. Carmine colours it. The author has more recently observed similar tubes upon the surface of the pith in *Robinia pseud-acacia* and *hispida*, in which, however, the transverse partitions still persisted.

Vascular Diaphragms of Aquatic Plants.— Duval-Jouve ('Mém. de l'Acad. d. Sc. et. Lettr. de Montpellier,' 1873, pp. 157—175, pl. viii) describes the occurrence of vascular bundles in the transverse diaphragms which divide the internal lacuna of the leaves of many aquatic plants.

The leaves of *Luzula maxima*, D. C. (not especially aquatic), are entirely traversed by lacunæ, hollowed in the green parenchyma, and originally occupied by a very delicate stellate cellular tissue, which is soon dried up, and is interrupted at varying distances of three to six mm. by slightly oblique diaphragms composed of two layers of small scarcely stellate cells filled with chlorophyll. These diaphragms are traversed by a single fibro-vascular bundle, which connects the bundles of the longitudinal nerves by articulating laterally with them always on their inner side. These bundles are often difficult to detect from the presence of air in the tissues. After, however, soaking the tissues for some hours in alcohol, the bundles can be clearly

made out in *Juncus, Luzula, Cyperaceæ, Gramineæ,* and many other families.

Flowering Plants, with Heterogenous Stem-structure.— Lestiboudois, in the 'Comptes Rendus,' 1872, pp. 811-819, describes a number of cases in which, outside the primary cambium, there is an entirely distinct formation of one or more fibro-vascular zones. The Beet is the type of this arrangement. If, a short time after germination, a section be made just below the cotyledons a ring of fibro-vascular bundles may be detected without difficulty; each consists of spiral vessels externally, corresponding to the wood, and of delicate, elongated, transparent cells, internally corresponding to the liber. Outside this zone a second circle of bundles is formed similar to the first, and externally to this a third, and so on. Seven or more may be counted in some cases. During the formation of the external bundles those first formed increase in size. Throughout its entire existence the beet-root consists, therefore, of numerous vascular zones, separated from one another by cellular layers. Each zone has its own cortical region and its own provision for growth. The transparent or cortical portion of each zone contains the greatest amount of sugar, and as the number of these zones increases with the size of the root it is explicable that, as observed by Peligot, the amount of sugar is proportional to the volume.

In *Spinacia, Chenopodium,* and *Salsola* there is substantially the same arrangement, but not in *Camphorosma.* The *Phytolaccaceæ* have "extra-liberian" bundles; *Amaranthaceæ* are similar to *Chenopodiaceæ; Nyctagineæ* are also distinctly heterogenous. The *Plumbagineæ* have, as pointed out by Unger, vascular layers alternating with others which are evascular; they do not appear, however, to be otherwise abnormal, and are not heterogenous.

Amongst *Convolvulaceæ* several species of *Convolvulus* appear to have numerous distinct fibro-vascular zones. Various species of *Avicennia* (*Vitaceæ*) have distinct cortical layers between the ligneous zones. *Gentiana cruciata* has an anomalous stem arrangement with more or less distinct fibro-vascular bundles, but does not appear to be heterogenous.

Development of Trichomes. — Joseph Ranter describes the development of the hairs of *Lamium album, Veronica agrestis, Hippuris vulgaris, Shepherdia ferruginea, Correa virens* and *C. rufa, Ribes sanguineum, Hieracium aurantiacum* and *H. pilosella, Azalea indica, Bellis perennis, Centaurea scabiosa, Dictamnus fraxinella, Echium violaceum, Malva sylvestris, Humulus lupulus, Urtica dioica,* and *Rosa.*

The trichome (hair) is developed from a single cell of the epidermis, the mother-cell of the hair. The perfect hair may be the product of this cell alone, and be unicellular (*Ribes*, *Dictamnus*, &c.), or the mother cell may form a simple or branched row of cells (*Lamium*, *Veronica*, *Hieracium*, *Echium*, &c.), or may form a flat cell-surface (*Hippuris*, &c.), or a mass of cells.

In other cases the tissues under the epidermis of the stem and leaf take part in the formation of the hair, as in *Echium*, *Malva*, *Urtica*. Lastly, the spines and glandular hair of the rose develope from the underlying tissues. To this form, which offers a kind of transition from hair to leaf, Sachs has given the name of *Emergence* ('Lehrbuch,' ed. iii, p. 144). The spines of *Rubus* and the spring projections of the fruit of *Ricinus*, &c., are probably of the same nature. (W. McN.)[1]

Morphology of Lycopodium.—Hegelmaier ('Bot. Zeit., 1872, p. 773, *et seq.*) directs attention to the two parts seen in the young stem, the cortical portion and the central axile portion. The central procambium cylinder becomes separable into two parts, a narrow peripheral portion, and a large central part, which consist of the fibro-vascular bundles and the interfascicular tissue. The first, or peripheral portion, is not to be confounded with the inner layer of the cutical tissues, and forms a sheath, surrounding the bundles and interfascicular tissue. To this portion Hegelmaier gives the name of Phloem-sheath. Internal to this is the phloem portion of the bundles, and is not to be confounded with the interfascicular tissue, which has been described by Sachs and others as the phloem portion of the bundle. The large elements in the interfascicular tissue are described by Sachs as cribriform vessels, but Hegelmaier is unable, after repeated observation of their structure, to confirm this view. Hegelmaier also describes and figures peculiar intercellular canals, containing a mucilaginous substance, which are developed in the stems and leaves of *Lycopodium inundatum*. In structure and development they resemble the gum-canals in Cycads and in *Angiopteris*. (W. McN.)

Reproduction of Lycopodium.—De Bary discovered in 1858 that the spores of *L. inundatum* produce a body composed of a few cells, but till lately the life history of these plants has been beyond this utterly unknown in the sexual stage. J. Fankhauser ('Bot. Zeit.,' 1873, pp. 1-6) has recently discovered the prothallium, which is subterranean and destitute of chlorophyll. In September, 1872, he found it more or less preserved, and still attached to young plants

[1] The articles signed W. McN. are contributed by Prof. McNab, of Dublin.

less than three inches high, growing in moss in a damp wooded locality near Langenau, in Emmenthal.

He describes it as a yellowish-white, irregularly lobed structure furnished sparingly with small root hairs. The under side is comparatively smooth, while the upper has numerous grooves and protuberances. In these grooves the antheridia and archegonia are situated. A vertical section through the prothallium shows that the cellular structure is formed of three regions; the uppermost, in which the antheridia and archegonia are developed, consists of thin walled cells poor in cell-contents; the cells of the middle layer are rather smaller, and filled with dark granular contents rich in fatty matter. Those of the lowermost region are somewhat elongated parallel to the surface, and their contents are turbid and finely granular. Starch does not appear to be present in any part of the prothallium. The antheridia were filled with innumerable spermatozoid mother-cells; the spermatozoids are only slightly twisted, and are stout compared with those of Selaginella. The archegonia was not observed, but the position they would occupy was indicated by that of the germ plants, and it seems probable that they are not sunk completely in the tissues of the prothallium.

In general, only one germ plant is produced from each prothallium, but it appears that a second may be developed from a second archegonium when the first is abortive. The reproduction of *Lycopodium* appears, therefore, to have the greatest resemblance to that of the *Ophioglosseæ*, and would appear to demand for the genus a systematic position between *Ophioglosseæ* and *Equisetaceæ*, and, therefore, far removed from *Selaginella*, with which its vegetative resemblance would have to be regarded as merely homoplastic. A great difficulty, however, presents itself in breaking up the Lycopodiaceæ in this way. The close affinity of the carboniferous *Lepidostrobus*, *Flemingites*, and *Triplosporites* cannot be doubted; only the last of these, however, agrees with *Selaginella* in having spores of two kinds. The conditions necessary for spore-germination are still unknown in *Lycopodium*, as Fankhauser has been unable, even by carefully imitating those which he had observed naturally, to advance further than De Bary in artificially promoting it.

Comparative Researches on the Development of the Archegonium.—The development of the archegonia of the Muscineæ and vascular cryptogams is fully described by Janczewski ('Bot. Zeit., 1872, p. 869 *et seq.*). Among the Muscineæ three types of development are observed:—1. Of the Hepaticæ. 2. Of

the Mosses. 3. Of the Anthocerotæ. The archegonia of the vascular cryptogams, as far as known (*Psilotum*, *Phylloglossum*, and *Mesipteris* being still unknown), also exhibit three types of development:—1. That of the Marsileaceæ (*Marsilea* and *Pilularia*), comparable with that of the Hepaticæ. 2. That of the ferns, Equisetaceæ and Salviniaceæ. 3. That of *Isoetes* and *Selaginella*. The archegonium is most highly individualised in the Hepaticæ, Mosses, and Marsileaceæ. In the ferns Equisetaceæ and Salviniaceæ the lower portion of the archegonium enclosing the embryonal cell is fused with the tissue of the prothallus; while in the archegonium of the Anthocerotæ, Isoetæ, and Selaginellæ, although the archegonium is differentiated from the tissue of the thallus or prothallus, still it is not individualised. (W. McN.)

Porphyreæ.—Janczewski ('Mém. de la Soc. d. Sc. Nat. de Cherbourg,' vol. xvi, p. 345) describes the reproductive organs of *Porphyra leucosticta* (Thur.) and *P. laciniata* (Ag.) In the former the frond, consisting of a single layer of cells, produces octospores by the division of the contents of marginal cells. The octospores are set free by the softening and solution of the mother-cell-walls and of the partitions between them. When free they are destitute of a cellulose investment, and move by slow contractile changes of shape, only, however, very rarely putting out short pseudopodia. The octospores finally come to rest, develope a cellulose wall and germinate. The antherozoids are developed in cells like those which produce octospores; there are usually, however, sixty-four in number from each mother-cell, which are spherical when free, destitute of a cell-wall, and without any motility. Occasionally a portion, the contents of a mother-cell, is converted into octospores, and the rest into antherozoids. *Porphyra laciniata* (Ag.) differs from the preceding species in being diœcious. The segmentation of the contents of the mother-cells producing octospores is not, however, fuly carried out. The antheridial mother-cells only produce thirty-two antherozoids. The protoplasmic contents of the cells in the *Porphyreæ* is coloured violet by iodine solution (with KI), a reaction also exhibited by the paraphyses, and the epiplasma of the thecæ of ascomycetous fungi. The chromule is a mixture of chlorophyll and phycoerythrine. *Porphyreæ* appear to be connected with the *Florideæ* through the *Dictyoteæ*, all three agreeing in the immobility of their antherozoids and spores (disregarding the amœboid movements of the latter), but distinguished by their female organs, which are quite distinct in the *Dictyoteæ* from those of the *Florideæ*, and, perhaps, do not require fecundation. They are absolutely wanting in the *Porphyreæ*.

Sphacelarieæ.—Janczewski ('Mém. de la Soc. Nat. d. Sc. Nat. de Cherbourg,' vol. xvi, p. 337) states, on the authority of Thuret, that the *Sphacelarieæ* multiply by zoospores produced in unilocular or plurilocular sporangia. The antheridia described by Pringsheim as bodies contained in the terminal cells appeared to be really an entophytal *Chytridium*. The "propagula" are made the subject of a careful study. They are produced on the lateral branches, usually on the upper side, and are connected to the branch by a cell or "sterigma," which may produce two, or even three, propagula in succession. They may be compared in some respects to the conidia of Fungi. When fully developed they consist of a "pedicel" of three rays, directed upwards but always convex, and of a multicellular hair proceeding from the centre of the three rays. The rays and pedicel produce, when they come in contact with another alga, short, ramified, stellate shields, forming a sort of prothallus, of which the peripheral cells may produce as many as three new plants. Sometimes the scutellum produces a root-like filament, which spreads over other algæ and developes young plants from point to point.

Podisoma juniperi.—Duchartre ('Journ. de la Soc.-Centr. d'Hort.,' 1872, p. 700) claims for M. Blais the credit of having ascertained, as early as 1860, by actual experiment, that the spores of *Podisoma juniperi* produce on the leaves of the pear *Ræstelia cancellata*. Œrsted's experiments on the same subject did not appear till 1865.

PROCEEDINGS OF SOCIETIES.

Royal Microscopical Society.

April 2nd, 1873.

Charles Brooke, Esq., President, in the chair.

Mr. Henry Davis read a paper "On a New Callidina; with the Results of Experiments on the Desiccation of Rotifers." He described a new species of Rotifer (family, Philodineæ; genus, Callidina), to which he gave the name of *C. vaga*, with the following specific characters:—"Figure, depressed fusiform; crystalline and nearly colourless; flat frontal lobes continuous with ventral surface, and uniformly covered with short cilia, not disposed as peripheral wreaths; non-retractile proboscis with broad anterior hook; two coarse and numerous fine teeth in each jaw. Progression by crawling. Length, 1-50th in. to 1-36th in." With regard to the revival of Rotifers after desiccation, the author came to the conclusion that they do not revive if completely dried, but that when apparently quite dry they are not so really. He found that Rotifers which had been exposed for three days to the air-pump vacuum over sulphuric acid, and exposed for two hours to dry air at the temperature of boiling water, nevertheless showed, on being compressed, that they still contained fluid. He concludes that the slimy matter with which they are commonly covered forms on drying a coating which prevents further evaporation.

The Secretary read a communication from Mr. Parfitt, of Exeter, describing a presumed new animal, to which the name of *Aychisteus plumosus* (*Parfitt*) had been given. It was supposed to be nearly allied to the annelids, though resembling a Rotifer, but had been seen on one occasion only.

Mr. Stewart exhibited a preparation of the rabbit's kidney, showing the epithelium lining the capsule of the Malpighian tuft and its continuity with that of the convoluted uriniferous tube.

May 7th, 1873.

Dr. John Millar, Vice-President, in the chair.

The Secretary read a paper by Dr. Maddox on a Cestoid parasite found encysted on the lower part of the neck of a sheep, in which he described its general appearance and characteristics and the result of microscopic examination. The specimen was the encysted larva of some species of Tænia, but was remarkable for containing a distinct ovarian structure, with numerous ova;

so high a stage of development in the cystic form being very rare, if not unknown before.

A paper was then read by Mr. W. K. Parker, F.R.S., "On the Development of the Facial Arches of the Sturgeon," especially with reference to the formation of the mouth. The general characteristics of the Ganoid fishes and their relation to the Osseous fishes and mammals, especially in the embryonic state, were explained and illustrated by drawings, and the formation and development of the sturgeon's mouth were similarly described.

Medical Microscopical Society.

The third Ordinary Meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital, on March 21st, at eight p.m., Jabez Hogg, Esq., President, in the chair.

The papers promised for the present meeting having been unavoidably withheld by their authors, Mr. Schäfer described some of the "Methods of observing tissues in the living state," illustrating his remarks by means of diagrams and instruments.

Having dwelt briefly on the importance of the subject, Mr. Schäfer remarked that the investigation of a subject was not complete till it had been examined in the living state, and that such examination, at least for warm-blooded animals, should be carried on at the temperature of the body. Much was to be learnt from the investigation of tissues still attached to the living body; for thus had cell migration been discovered by Cohnheim in the frog's mesentery, and experiments on embolism had been made in that animal's tongue; while the tail of a tadpole had taught us much about connective-tissue-corpuscles, and the development of blood-vessels. Muscular tissue in the living state was best seen in the smaller Crustacea.

Living tissues removed from the body allowed of being studied in many ways: some, immediately, without any addition whatever, as red blood-corpuscles, and striated muscular fibre; while if any addition were necessary, a saline solution of 0·75 per cent. or serum would be best. For some structures a moist chamber might be necessary, such as Recklinghausen's, in which frog's blood had been preserved for days in a living condition (Schultze's Arch., 1866). Another form was Stricker's stage, which was also useful for the application of electricity to microscopical research, by means of two electrodes of tin-foil, the points of which nearly meet in the centres of the slide.

Mr. Schäfer finally described and exhibited various forms of warm stages: one kind of which, as Schultze's, was heated by means of a spirit lamp applied to metal arms, which conducted the heat to the object bearers; another kind, as Stricker's, in which a constant temperature was maintained by means of a

current of water continually flowing through it; while a very ingenious form of stage, somewhat similar to Stricker's, was so managed that a constant circulation of warm water was kept up in a closed system of tubes, the temperature of which was regulated by a mercurial gas regulator and measured by a thermometer, the bulb of which lay close to the central chamber.

The President having opened the discussion in a few words, Dr. Bruce, Dr. Heywood Smith, and Dr. Pritchard made some remarks.

The fourth Ordinary Meeting of the above Society was held at the Westminster Ophthalmic Hospital, on April 18th, at eight p.m., Jabez Hogg, Esq., President, in the chair.

The first part of the meeting was considered special, for the election of two members of committee, Dr. Greenfield and Dr. Matthews, who were balloted for in the usual manner, and unanimously elected. Several new members of the society were elected.

Mr. Needham, of the London Hospital, then read a paper on, and demonstrated the various modes of, "Cutting sections of animal tissues for Microscopical Examination." After some preliminary remarks Mr. Needham stated that all tissues might be divided into three classes, with regard to section-cutting, according to their consistence.

I. *Hard tissues, e. g.*, bone.

II. *Tissues of intermediate consistence, e. g.*, cartilage : softened bone.

III. *Soft tissues, e. g.*, kidney and gland structures : generally nerve-tissues.

A. *Mode of dealing with tissues in Class I, e. g.*, bone. After a thin slice had been sawn off with a hair saw, it should be ground down with files of varying coarseness, and then upon a bone, and finally polished on a strop, or between two glass plates with tripoli. The section should then be cleaned by brushing with a camel's-hair pencil or old tooth-brush, and mounted dry, or in Canada balsam. The former method of mounting had been employed for years by Mr. Carter.

The chief point to attend to was the previous treatment of the bone; it should be well stripped of all the soft parts covering it, and soaked in successive changes of pure water, by which means the fat contained in it was completely removed, and then subsequent exposure to the air insured a perfectly white specimen.

B. *Mode of dealing with tissues in Class II, e. g.*, decalcified bone; cartilage; chromic acid specimens, &c.

By decalcifying bone the periosteum, vessels, and nerves could be studied *in situ*. Various solutions had been suggested for softening bone; the principal were:—

I. Aqueous solution of chromic acid of 3—4 per cent.

II. (Dr. Rutherford's).—Aqueous solution of chromic acid

1 per cent., with nitric acid 2 per cent. The advantage in this was that the bone was stained a deep green colour, by the formation of the sesquioxide of chromium.

III. Very dilute solutions of nitric acid, hydrochloric acids, mixed in equal proportions.

Whichever solution was used, large quantities of it, compared to the size of the tissue acted on, ought to be employed.

Cartilage required no preparation whatever. Soft glandular structures, if previously hardened, belonged to this class. They might be hardened by any of the following solutions: alcohol; chromic acid, with or without sulphate of soda, $\frac{1}{8}$th to 2 per cent.; Picric acid, a saturated solution (Ranvier); Osmic acid, $\frac{1}{4}$ per cent. (Schultze); corrosive sublimate or chloride of palladium (Schultze); or Bichloride of platinum (Merkel).

The solutions of chromic acid and its salts, and alcohol, were to be preferred.

Tissues thus prepared may then be cut with a razor or scalpel, previously wetted with spirit, glycerine or water, to prevent the section adhering to the blade, while the movement employed was not to be that of sawing, but one direct cut through the whole thickness of the tissue.

C. *Mode of dealing with tissues in Class III.*—To obtain sections of these, some special arrangement is required, either on the part of the cutting instrument or in the mode of holding the specimen.

1. *Of the cutting instrument.*—The knife most usually employed, and of which the varieties are endless, is Valentin's. Four kinds might be specified:—

A. Valentin's original instrument, the two blades of which are triangular, and separated by means of a screw to the required distances.

B. Made by Matthews, the blades of which are convex from point to heel.

C. Dr. Maddox's knife, made by Baker. This is three-bladed, so that two sections are cut at once, of which the opposite but contiguous surfaces can be examined.

D. Hawkesly's (of Blenheim Street).—This is an improvement on Matthews', and is so constructed that exact parallelism between the blades is secured, and their distance from each other can be always known, as the screw that separates them is graduated.

There were two especial cautions in using a Valentin's knife, in order to be successful: one, that at the end of the stroke the blades should be inclined rather upwards to ensure complete separation of the section from the surrounding parts; the other, that the section, when cut, should be floated off in water.

In cutting sections of intestine, unhardened, Dr. Fenwick had suggested that it should be drawn over the thumb like a glove, and the section be then made downwards upon the nail.

In making sections either with a razor or Valentine's knife it

was convenient to cut down upon a cake of wax; there was then no chance of blunting the instrument. A mixture of olive-oil and wax was preferable to wax alone; but cork and leather might also be used for the same purpose.

2. *Methods of supporting soft tissues.*

A. *By imbedding.*—This was done by covering a small piece of the tissue with molten wax, spermaceti, or paraffin, poured into a small paper-box; the preparation should not be inserted until the wax had commenced to congeal, otherwise the heat might cause it to shrink; and it should previously have been placed in spirit, and then dried on its outer surface to promote the ultimate adherence of the wax.

For the same purpose, gum, or glycerine and gelatine, might be employed.

If wax, &c., be used, the section, when made, should be soaked in absolute alcohol; if gum or gelatine, in water only; the section would then be ready for staining.

In dealing with lung or cavernous tissues it was well to place them in the imbedding substance used, while liquid, under the air-pump; by this means internal as well as external support was obtained, the material being forced into the interstices of the tissue.

In injected lung Prof. Quekett used to inject tallow into the bronchi, and then soak the section, when made, in turpentine; by this means the air-cells were distended, and the necessary solidity required for cutting sections given.

Any razor might be used for tissues prepared as above, but one with a thin blade was preferable. Mr. Needham generally used a doubly convex blade, as this held plenty of alcohol.

B. *By freezing.*

i. Klein's method of freezing a small piece of the tissue pinned to cork, in a crucible placed in ice; but sections made thus required great rapidity and dexterity in manipulation.

ii. Freezing in M'Carthy's modification of Prof. Rutherford's microtome.

This method Mr. Needham fully illustrated, and cut some most beautiful sections of fresh lung. The principle differed from Klein's in the fact that the sections could be made while the process of freezing continued.

Mr. Needham believed that freezing was superior to all other methods of preparing soft tissues for section cutting.

A discussion followed, in which many members took part.

The fifth Ordinary Meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital, on May 16th, at 8 p.m., Jabez Hogg, Esq., President, in the chair.

Mr. Atkinson read a paper on "The Preparation of the Brain and Spinal Cord for Microscopical Examination," of which the following is an abstract:

Nerve-tissues may be examined fresh or after undergoing certain preparatory processes. In the latter case the first step is to harden them, and this is best done by first steeping the specimen in methylated spirit for twenty-four or for forty-eight hours, and then transferring it to an aqueous solution of chromic acid ($\frac{1}{4}$—$\frac{1}{2}$ per cent.), in the case of spinal cord, or a solution of chromic acid (1 part), bichromate of potassium (2 parts), and water (1200 parts), in the case of brain tissue. The process of hardening generally occupies from four to six weeks, and if the section be not then made the tissue may be for the future preserved in spirit. If left longer than stated in the hardening solution the specimen becomes too brittle to be of any use. Over-hardened tissues also are less easily coloured afterwards.

In preparing spinal cord the chromic acid solution should be changed after the first twenty-four hours, and once or twice afterwards. In the case of the brain the strength of the fluid should be doubled after the first fortnight.

The staining of sections of brain and spinal cord is best done by carmine, and a modification of Dr. Beale's solution (diluted to one seventh of its original strength) will be found best.

The sections, washed in water to free them from chromic acid, must be left, according to the degree of colour required, from one to twenty-four or forty-eight hours in the carmine solution. They are then washed and then placed in rectified spirit to fix the colour.

The sections are mounted in Canada balsam or gum dammar, for which they are prepared by alcohol and oil of cloves or turpentine in the usual way.

Nerve-tissues may be examined fresh if small portions, the size of a pea, or a section made after freezing, be placed in the diluted carmine solution, and be subsequently teased out on the slide, and mounted in a mixture of glycerine and hydrochloric acid (1 oz. of the former and 2 drops of the latter). Nerve-cells are thus well seen.

A discussion followed, in which the President, Mr. White, Dr. Pritchard, Mr. Paul, Mr. Needham, and Mr. Groves, took part.

Dr. Osler read a paper upon the "Action of certain Reagents —Atropia, Physostigma, and Curare—on the Colourless Blood-corpuscles."

The reagents made use of were a fresh solution of sulphate of atropia, a fresh solution of sulphate of physostigma (1 per cent.), and a rather stronger solution of curare; a half per cent. saline solution was used to dissolve them.

In the case of newt's or frog's blood, about four times as much reagent as blood was made use of, while for human blood the proportion of reagent to blood was 5 to 1.

The specimens were examined on a Stricker's stage, at a temperature of 39° C.

The experiments were undertaken to show, if possible, on the

corpuscles, the antagonism between the reagents, which had been already demonstrated by Dr. Fraser.

A solution of 1 part of sulphate of atropia to 2000 of water allows the normal amœboid movements of the corpuscles, while a 1—3 per cent. solution definitely alters the form and structure of their processes, for it is in these that the changes noticed lie.

Generally, in about ten minutes, the corpuscle is seen to throw out processes, bud-like, long and thin, or tuberous, the number of processes being indirectly as their size, while the outline of a corpuscle may change two or three times in a minute.

Sometimes the processes are retracted, but not always, and they may remain without any change of shape, while some corpuscles in the field never alter nor move at all; all, however, retain their spherical form. The processes are mostly hyaline, but sometimes granular, and have a sharply defined line, where they join the body of the corpuscle; a fusion of the granules they contain may restore their original transparency.

The phenomena described do not always occur upon the addition of the reagent, being sometimes more evident than at others.

A number of experiments were here narrated in detail, but of which it is impossible to give an abstract, showing the action of atropia on the corpuscles, but the result was to the effect that all motion ceased in the corpuscles, on the application of the reagent, sooner in the blood of the newt and frog than in that of man, and sooner, also, the stronger the solution used.

The blood of frogs and newts poisoned with atropia showed normal amœboid movements without any modification whatever.

The action of physostigma is somewhat different. A solution of the strength of 1 to 800 of water allows the normal movements of the white corpuscles; a solution of 1 in 1000 of water stops all motion in two hours; while one of a strength of 1—300 of water all but completely prevents the formation of processes and causes the movements to be of an undulating and heaving character. A rather stronger solution produces changes the same as atropia. As a rule, fewer corpuscles are affected by a given amount of the reagent than in the case of atropia.

The red corpuscles are changed by a 1—2 per cent. solution of these reagents; their surfaces become irregular from involutions and cuppings of the surface, but scarcely two corpuscles are affected alike.

The explanation of the changes above mentioned is difficult; that they are of a vital nature seems certain, the hyaline processes strongly reminding the observer of some of the pseudopods in the Rhizopoda. The normal prolongations of the white corpuscles are formed of its hyaline substance (protoplasm), together with the granules it contains; but those resulting from the application of atropia and physostigma are free from granules; similar processes can be seen in the yolk-granules of the batrachia. The result of these experiments would show that no

antagonism exists between atropia and physostigma, at least as far as their action on blood-corpuscles is concerned; and in proof of this, blood treated with the reagents mixed showed just the same changes as when used separately.

Experiments to show the action of curare on blood-corpuscles produced only negative results; the normal movements going on as usual, yet where a ½ per cent. solution was used these ceased in ten minutes.

Dr. Payne read a paper on "Certain points in the Histology of the Omentum."

The fenestrated portion of the human omentum consists of fibrous bands or trabeculæ, in which are imbedded connective-tissue-corpuscles, and on which is spread a continuous and mostly uniform layer of endothelial plates. It is with the latter that the present notice is concerned. The best mode of examining them, that of staining with silver, is generally inapplicable in the human subject in consequence of the time which elapses before examination is possible, but the structures can be very well seen either without any reagent or after staining with carmine. The attention of the writer was first drawn to the subject on examining the omentum in persons dying of acute tuberculosis with miliary tubercles in the peritoneum. In these cases were found around the tubercles endothelial cells in various phases of change —some with nuclei, some almost divided so as to form two cells, some with many nuclei, and some groups of cells with the evidence of shape, showing that they had been produced by cell division or multiplication; in fact, all the appearances regarded as indicating cell-proliferation. These have been described by several authors (Rindfleisch, Kundrat, &c), as showing the origin of tubercle. There were also seen large compound cells like "myeloid or giant cells," and small masses of adenoid tissue.

Similar proliferative changes are seen in acute inflammation, and the appearances in the neighbourhood of small cancerous growths are also very similar. In the one case they have been regarded as the source of pus-cells, in the other, of new cancerous growth.

The important fact, however, is, that appearances precisely like those above described may be found in the normal omentum, viz. evidences of cell proliferation, many-nucleated or giant cells, and masses of adenoid tissue. It appears, then, that the morbid changes which accompany inflammation, and which accompany the formation of tubercles, are not only essentially alike, but are also identical with processes which are always going on in the omentum, and are certainly not indicative of any special disease. The inflammatory changes, or those of specific diseases, differ from the normal chiefly in their greater abundance and activity; differences which are doubtless simply due to hyperæmia and consequent increased nutrition. It is probable that appearances which are strictly normal have sometimes been described as those of disease.

Dublin Microscopical Club.

21*st November*, 1872.

Tryblionella debilis, Arn., *in Ireland.*—Rev. E. O'Meara showed a *Tryblionella*, new to Ireland, which, he had learned from the Club's corresponding member, Mr. Kitton, had been found by Dr. Walker Arnott in some places in South Brittany, and named by him *Tryblionella debilis*.—He likewise exhibited a beautiful slide of *Glyphodesmis eximia* mounted by Mr. Kitton.

Habitat of Cosmarium curtum, Bréb.—Mr. Crowe showed examples of *Cosmarium curtum*, Bréb., taken from a roadside pool, almost a mere cart-rut, near Bray, showing the singular position in which this species, contrary to the habit of its fellows, seems to love to dwell.

Endosperm of Tupistra nutans.—Dr. M'Nab exhibited sections of endosperm of *Tupistra nutans*, one of the *Aspidistreæ* allied to the *Smilaceæ*. The cells have very large pores with no visible solid contents, the fluid not containing starch, oil, &c. One of the sections shown was coloured with Busk's solution, iodo-chloride of zinc, and showed that the walls were of pure cellulose, this being, in fact, the reserve material of the seed.

Organism associated with Vorticella.—Mr. Crowe showed a *Vorticella* remarkable for the very great relative length of the body as compared to its diameter, as well as having ordinarily entangled about its stipes examples of a monadiform creature, almost as if seeming to have some genetic relation to it.

Structure of Zygospore of Xanthidium armatum, Bréb.—Mr. Archer exhibited the conjugated state of that extremely common and widely-distributed Desmidian, *Xanthidium armatum*, (Bréb.) the zygospore of which, nevertheless, seems to be extremely rare. The figure given by Ralfs in 'Brit. Desm.,' Pl. xviii, g, was made from the only instance seen by him, and it does not appear to have been since recorded until Lundell's work ('De Desm., quæ in Suecia inventæ sunt, obs. crit.,' p. 75). That author correctly points out that the conspicuous dots on the outer wall are not, as Ralfs supposed, nascent spines, but in reality *scrobiculi*. Mr. Archer thought that Lundell was certainly in error in building upon that fact a homology between this scrobiculate outer wall and the occasionally likewise scrobiculate mesosporium of certain *Conjugatæ*. The thick and hyaline *outer* envelope of the zygospore is beautifully and deeply scrobiculate; presently the contents recede from the wall and become contracted together in a dense globular mass, leaving a rather wide interspace between it and the outer wall, thus presenting all round a clear band or region, in width about equal to one sixth or one seventh of the diameter of the zygospore itself, which, however, may be mentioned, seems rarely quite globular (as depicted by Ralfs), but notably longer in one direction, and thus the empty band or margin is not of equal width all round.

The inner globular, or nearly globular, mass now becomes

coated with a new membrane; this latter, however, is not simple, but composed of three distinct laminæ, as is readily to be demonstrated by crushing a specimen under the microscope. It is clear that this triple coat is homologous with the similar trilaminated covering in certain other Conjugatæ. Both the endo- and exosporium are hyaline, delicate, and colourless, whilst the mesosporium in *X. armatum* is of an olive-brown colour, rather opaque, and somewhat thicker in texture than the other laminæ, but destitute of puncta (or scrobiculi). The scrobiculi of the outer wall, therefore, offer a resemblance, not an homology, as Lundell suggests, with the markings on the mesosporium in some Conjugatæ. Whether the zygospore of other forms presents similar facts remains to be determined. Mr. Archer was sorry that when, for instance, the rare and somewhat similar zygospores of *Tetmemorus lævis* (Kütz.) and others were on former occasions at command, his attention was not directed, as it ought to have been, to the study of these points.

Staurastrum Ophiura, Lundell, *new to England*.—Mr. Archer likewise exhibited one or two rare Desmidieæ (almost the only organisms that satisfactorily withstood the effects of travelling), taken on his and Mr. Crowe's visit to the north of England and Wales. Amongst the rarities taken on that occasion (near Ambleside) was that splendid form *Staurastrum ophiura* (Lundell), especially interesting, as it at once sets aside the idea of the possible identity of this form and *Staurastrum verticellatum*, Archer ('Quart. Journ. Micro. Science,' vol. ix, n.s., p. 196), as yet found only in a restricted locality near Maam, Co. Galway (not obtained at all on the most recent visit to the site). The latter is even a finer form—that is, larger, more expansive, taller—whilst the former in details is more ornate; both are truly noble objects.

A Drop-measurer.—Dr. Frazer drew attention to a handy modification of a drop-measurer, which might prove useful to the microscopist. This consisted of a small bottle furnished with two slender bent glass tubes inserted into the cork, and divergent from one another. During the act of delivering a drop by one of the tubes the other is kept closed by the finger, and the drop can be graduated by slightly opening the latter to the requisite minute extent by simply lessening the pressure of the finger, and the amount of fluid discharged regulated with the greatest nicety.

December 19*th*, 1872.

Navicula bicuneata, Grunow, *new to Britain*.—Rev. E. O'Meara exhibited, new to Britain, *Navicula bicuneata*, Grunow. He compared the examples with the figures given by Grunow and by Cleve, and showed that they corresponded in all details more closely with the description given than with that of Cleve. The longitudinal sulci referred to by the former author, and not by the latter, were quite conspicuous. In one case only, however, had Mr. O'Meara seen the front view, which was symmetrical.

In connection with this, he referred to the following observation of Cleve:—"Grunow does not describe the (side view) front view, which, in the specimens examined by me, was cuneate as in the case of *Gomphonema* or *Novilla*, for which reason the species ought to be transferred to a new genus distinct from *Navicula* by its cuneate (side view) front view."—'Om Svenske och Norske Diat.,' p. 227).

Leaf of Gum Plant.—Dr. Moore showed the leaf of the "gum-plant" given to him by Dr. Richardson, remarkable for its very strong and offensive smell. This had its seat in certain dark glands, which dotted the leaf and presented little elevations upon each surface.

Quartz.—Dr. Frazer showed sections of quartz with air-globules in certain fluid-containing cavities.

Dr. Barker's Dark-ground Illuminator.—Dr. Barker had had a new "parabolic" constructed, with certain alterations, and he exhibited same, which he had found to perform very nicely, readily bringing out the fine test-marking on the most "difficult" diatomaceous frustules.

Tyloses from Bignonia.—Professor M'Nab showed an instructive example of "Tyloses" from *Bignonia*. This section showed examples of the tissue inside the duct, in which the thin-walled cells had not yet become polygonal by mutual pressure, but remained globose or balloon-shaped, with considerable inter-spaces between them, thus confirming the view that this curious growth is due to hernial protrusions into the ducts, through the pits, from the adjoining tissue.—Dr. M'Nab likewise showed sections from stem of *Welwitschia*.

Remarks on Nematophycus Logani, Carruthers.—Mr. Archer referred to sections (once before exhibited to the Club) of *Nematophycus Logani*, Carruthers (*olim*, *Prototaxites Logani*, Dawson), kindly lent by Prof. M'Nab from the collection in the Royal College of Science, Dublin. It seemed to be quite evident, from an examination of the transverse section, that the larger tubes of this growth, whatever it was, were really laminated, five or six strata being usually noticeable. This was an observation which Mr. Carruthers in his paper ('Monthly Microscopical Journal,' Vol. VIII, p. 168) was unable to confirm.

Mr. Archer took occasion to remind the meeting that when Professor Dyer and himself had been examining these sections he had then ventured to suggest that there appeared a certain amount of resemblance to the lichenous genus *Cænogonium*. The late Admiral Jones had once given him the opportunity of examining *C. Linkii*. The structure consists of a conferva-like jointed filament (a very peculiar kind of gonidia) involved by an interlacing covering of much more slender fibres (the hyphæ), the latter producing, by-and-bye, the characteristic apothecia. Doubtless a Schwendener would explain such a structure quite differently. The central conferva-like string of "gonidia" would be simply a confervoid "alga" which a foreign parasite had wound

itself round, and compelled to play an altogether new part; to others, indeed, *Cœnogonium* would remain a "lichen"—of an aberrant type, it is true, yet still *sui generis*. There can, indeed, be little doubt but that the alpine, so-called, alga, *Chroolepus ebeneum*, (Dillw.) will be found to be likewise a "lichen," and indeed a species of *Cœnogonium*. Now the two reasons which had caused Mr. Archer, at the time, to say nothing in these Minutes touching any possible affinity of this interesting fossil and *Cœnogonium*, and which likewise, he thought he might say, had impressed Professor Dyer, were (1) the laminated structure of the larger tubes, and (2), which was more important, their want of septa or "joints." If they showed septa at regular, in place of at very remote intervals, if at all, there would be much to call to mind a "mass" of a *Cœnogonium*-like character—that is, large filaments (as is seen in the preparation) running longitudinally, with an intervening hypha-like "*tela contexta*." as it were, binding them together. Of course, if the *Nematophycus* could be so construed, the fossil should be expected to show apothecia, but in a "mass" of such a growth these would probably be produced only on the exposed surface.

On Two distinct Diatoms, presenting the appearance of being on the same Stipes as bearing on Dr. Bastian's views.—Mr. Archer exhibited an example of two minute stipitate diatoms, which at first glance would momentarily seem sufficiently surprising, for here was a pretty little dendroid group of a rather common form, *Gomphonema constrictum*, presenting all the appearance as if it had growing with it *on a common stipes* another frequent, much more minute form, *Achnanthes exilis*. Professor Meade-Edwards had stated that he had met with, indeed, two diatoms recognised as distinct growing on a common stipes, and he based on this the assumption of their necessary genetic relationship.[1] His case may possibly, however, have been just a similar one to the present, in which, at least, the seeming genetic connection, when more closely examined, just meant—nothing at all. You could readily see by focussing down and looking closely along the stipes of the *Achnanthes* down to its lowest extremity the little rounded base of attachment where it was connected with the stipes of the *Gomphonema*, distinctly showing that the former grew *upon* the latter, just as it might grow attached to *any other* fulcrum. In any case the frustules at the summits of the branched stipes of a *Gomphonema* are not comparable to the blossoms of a tree, which latter must *be* before blossoms could be produced, but in the diatom the stipes seem rather to be produced by the frustule—not the latter, as it were, a *blossom* upon the former. Nor does the singular example now shown afford support to the Bastian doctrine, as exemplified by certain figures given by that author.[2] He has accepted, certainly, more startling transmutations than even the change of a *Gomphonema* into an

[1] 'Monthly Micr. Journ.,' Vol. IV, p. 36.

[2] 'The Beginnings of Life,' *e.g.*, fig. 82, p. 417 (vol. ii).

Achnanthes. Several of Dr. Bastian's illustrations merely show cases of the same thing as the example now drawn attention to, that is, minute or nascent algal forms growing upon other larger ones by sheer accident, just as they would attach themselves for support upon any other friendly foreign object. But it is surely without reason to assume, as Dr. Bastian does, that because the two organisms are found temporarily attached, the little one owes its origin to a transmutation of the substance of the larger, or to a kind of heterogenetic budding-off, as it were, from it. Just as many "seedlings" are difficult to identify at first springing up, it would be even more difficult to guess what some of the little "epiphytes" in Dr. Bastian's figures may really be; some look like young Œdogonia perhaps, or Characia, or Draparnaldieæ (*Stigeoclonium*?) of some kind, &c., &c. Most assuredly not one put forward by Dr. Bastian as such will be accepted by any one who ever saw a Desmidian as even a primitive type of that family. Such things as he depicts (l. c., fig. 82) are at least as common as possible. For the minor forms attached to those of greater size, the one, indeed, as well as the other, reproductive processes of some kind or other, are in many cases known, each to all appearance *sui generis*. Of what use, then, is the gratuitous supposition that the little ones grew off by a process of (needless) transmutation from the larger? This, to say nothing of the even greater *assumptions*, which would be startling if they were not inconceivable, such as that of a Rotatorian becoming gradually evolved from a "mass of *Chlorococcus* corpuscles,"[1] of Nematoids from spores of Vaucheria,[2] of Diatoms, Bacteria, Pediastrieæ, and other Algæ, from Euglenæ,[3] &c., &c. Very wonderful modifications occur, in fact, beyond doubt, the "amœboid" changes of the protoplasmic mass of a vegetable-cell, for instance; but perhaps after all this is no more surprising, now that we know it to occur, than the equally marvellous zoospore-condition. Many so-called "alternations of generation" occur too (perhaps more generally than may be as yet supposed), and yet such do not prove that any two in reality distinct entities, which to all appearance continue to run, though it be from stage to stage still each in its own "groove," mutually borrow from each other characteristics or capacities which are individually foreign, and which are not requisite in the place in nature which each seems to be destined to fill. In many cases where certain internal parasites occur within the cells of algæ, &c., the scrupulous and unremittingly assiduous researches of keen observers seem to tend to show that the germs of these make their way in from without, and this, it would almost even seem, as if with a certain selective power. What becomes of the law of heredity (and all that is built upon it) if such assumptions as those supported by Dr. Bastian regarding the utter unstableness of *spores* and *ova*, &c.,

[1] Op. cit, vol. ii, p. 93 (p. 516). [2] Fig. 95 (p. 531).
[3] Fig. 85 (p. 447).

generated with such a marvellous amount of apparent fixity of *purpose* and of *plan*, be correct? These "things," though their mere size be so insignificant, are, in their more limited way, in proportion as highly organised, and their parts (sometimes the *sexes*) as differentiated, and their reproductive processes as marked, as if they were a hundred or a thousand times bigger. Did they really attain dimensions so as to "take up more room in the world" they would, doubtless, be exempt from the extraordinary conjectures and hazarded assumptions as to their origin and growth, to which it would seem as if nothing but the difficulties presented by their minuteness has laid them open.

Gotland Algæ.—Mr. Archer drew the attention of the Club to a recently published memoir on the Algæ of Gotland, by Dr. Veit B. Wittrock, of the University of Upsala, referring to some points involved in the mode of classification adopted, and pointing out his discovery of so remarkable an alga as his *Mougeotia calcarea*, combining in itself the characters of the genera *Mesocarpus*, *Plagiospermum*, and *Staurospermum*.

[A *résumé* of this memoir, by Mr. Archer, is given in the April number of this Journal, pp. 117—139.]

Euastrum binale, var. *angustatum*, Wittr., and *E. binale*, var. *insulare*, Wittr., *their occurrence in Ireland.*—Mr. Archer drew attention to the fact that *Euastrum binale*, var. *angustatum*, Wittr., and the Irish form, recent specimens of which he had already exhibited at the August meeting of the Club, were quite identical, and he would have regarded them as representing quite a distinct species. Dr. Wittrock's reason for not so treating this form (op. cit., p. 53)—its great scarcity and single locality—will hardly be generally admitted as a valid one. It is a very scarce form in Ireland, and had only occurred in one gathering, but, very minute as it is, one could see at a glance, with a one-inch objective, that it was quite a characteristic form. The empty cell shows itself quite smooth and destitute of any of those little nodules or inflations characteristic of *Euastrum binale*. It has only a minute depression at the ends, and thus appears a very aberrant *Euastrum;* its general outline is that of a Euastrum, but its negative characters place it near *Cosmarium*, though such a form as *Cosmarium sublobatum* (Auct.)= *Euastrum sublobatum* (Bréb.). But not only of this, but of several other forms, pretty nearly the same remark might be made—one of which, (op. cit., p. 48, t. iv, fig. 7), *Euastrum binale*, var. *insulare* indeed, is Wittrock's own form, also Irish—as well as several others, yet every one of these appear quite distinct and constant from various part of the country, and apparently readily recognisable.

Cosmarium tetrachondrium, Lundell, *new to England.*—Mr. Archer exhibited, for the first time to the Club, one from the number of those inornate Cosmaria which to the casual observer offer not much interest, but one quite different from those referred to above; this was *Cosmarium tetrachondrum* (Lundell), taken in Co. Tipperary, the only site whence he had ever obtained it.

New locality for Cœlastrum cambricum.—Mr. Archer drew attention also to *Cœlastrum cambricum*, ejus, from the same gathering. This form had now been found in several distant localities—Co. Galway (Connemara), Co. Tipperary, and North Wales. It is a very fine and handsome form.

On Selenastrum Bibraianum, Reinsch, (?), *new to Ireland.*—Mr. Archer further showed, from the same gathering as both the foregoing, another minute algal form, which seemed to resemble, if it be not identical with, *Selanastrum Bibraianum* (Reinsch, in 'Algenflora des mittleren Theiles von Franken,' p. 64; T. iv, fig. 2, *b*).

To some extent Reinsch's plant, to judge by the figures, has much of the habit of *Sorastrum*, though the cells are not combined into a radiating group by the union, at its centre, of elongate, stalk-like processes; *Sorastrum bidentatum*, Reinsch (l. c., t. iv, fig. 1), looks uncommonly similar, almost as if congeneric with Selenastrum itself; whilst, again, *Selenastrum gracile* (Reinsch) seems to point to *Ankistrodesmus* (=*Raphidium*). Be it as it may, the present plant now exhibited had much of the habit of some such form as *Dictyosphærium reniforme* (Bulnheim), but no stipes or connecting threads were visible. Here the groups of cells, usually four in each group, stood in opposite directions, these groups of four arising from self-division. Sometimes in one or more of the groups, the self-division could be seen carried on to another generation in advance of the remaining group, the general cluster or colony thus getting more or less irregular. The much-curved lunate cells are always disposed with their convex sides towards the centre of the colony; the contents a bright chlorophyll green, on the whole homogeneous-looking, but with a few darker granules embedded therein; the cell-wall appears to be very thin. This formed a very pretty object; it was certainly new, at least to this country. But, inasmuch as no further development save self-division of the cells had been seen, or, indeed, any refounding of the typical colonies (though, of course, it is conceivable that a single cell could become the starting-point for such definite and seemingly characteristic groups), as a "species" it must be for the present left in abeyance; nay, it is not even certain that it is really one and the same thing with *S. Bibraianum* (Reinsch), but at least it seems very probable; still the figures given by Reinsch are too formal to be accepted as *portraits* of the forms, or, indeed, as more than diagrammatic.

On a Pleurococcoid Alga on Flies in Water.—Mr. Archer exhibited an interesting and singular algal form sent by Professor Alexander Dickson; it appertained to *Pleurococcus*, and its most marked outward character was the singular groups formed by the ramifications of the mucous envelope or matrix in which the cells were embedded. This presented a number of *stalks*, branching upward in a tufted manner, and a little mass removed and placed on a slide with the cover pressed down

offered to the unassisted eye an appearance somewhat like a miniature, badly-grown *cauliflower*, almost, say, a quarter of an inch or more high; at the lower portion of the tuft it was pale or nearly colourless, towards the somewhat abruptly terminated ramifications, however, presenting a kind of yellowish-green and more dense aspect. "The plant [that is, of course, this mucous, more or less ramified tufted mass in the aggregate] was apparently growing from the body of a dead fly, which had got into the water" (in a vessel in which *Hydrodictyon* was being cultivated). What relation the *plant* had to the *fly* is unknown. Placed under a "quarter-inch," it presented a very pretty sight; the embedded cells, of a bright green, were strewn through the matrix, gradually more and more crowded, however, upwards, until at the extremities of the thickish branches of the tufts formed by the aggregate mucous mass, they became very densely accumulated, thus explaining the varying degrees of comparative absence of colour and the increasing opacity towards the upper ends of the tufts. These cells were comparatively large, globular, and distinctly nucleated, and were, some of them, not only in all stages of division, but others had become enlarged and involved by a noticeably thicker cell-wall than the former, as if so many resting-cells; further, certain others of them showed a zoospore condition, biciliated, and with the usual pale spot whence the two flagella emanated; these zoospores were apparently locked in, as it were, by the mucous matrix, hence their motion was but slight, their change of place very inconsiderable, and the waving action of the cilia, though decided enough, was comparatively feeble. Nor was this all. Certain others of the cells—there could, indeed, scarcely be a doubt that they were (all of them) one and the same plant—showed a marked "amœboid" state, in a somewhat broad sense. The "pseudopodia" were not rounded (ever changing) lobes with a smooth outline, like those of an Amœba, but they were long, branched, tapering, rather slender, granular-looking, opaque, very slow and gradual in being projected, and yet requiring not a very long exposure upon the slide to expand under one's eyes to a somewhat tree-like, irregular, rather lop-sided, very rhizopodous-looking object, with the pseudopodia given off as a whole rather notably in two opposite directions. It will be thus seen that it was not like Amœba proper, but rather comparable to the so-called *Amœba porrecta* (Schultz)—surely no proper Amœba—or at least to the rare form (with us) which Mr. Archer had thought himself justified in referring thereto. It was a thing completely unlike the "Amœboid" state of the primordial cells of Stephanosphæra once witnessed by him ('Quart. Journ. Micro. Science,' Vol. V, p. 116), or of the "Amœboid" state of Volvox—very different, indeed, from the former (see Dr. Hicks, 'Quart. Journ. Micro. Science,' Vol. II, p. 96); nor did it seem like "*Amœba porrecta*," though a comparison therewith would give, probably, the best idea. Placed on the slide and kept moist for many hours,

this arborescent condition, having, as it were, attained its maximum, suffered very little change; the same slide afterwards hermetically closed, without any addition of other fluid than its own water, showed by-and-by the "Amœboid" structure with the "pseudopodia" greatly, if not entirely, drawn in, and the mass forming a rounded, more or less "shapeless" little clump.

The colour in this "amœboid" condition was alike to that of the resting-cells, darker than the vegetating-cells, by reason, apparently, of an accumulation of opaque dark granules, not evident, or at least so numerous, in the ordinary cells. Still, there could be little, indeed, no doubt but that the ordinary dividing-cells, the resting, the zoospores, and "amœboid"-cells, were all one and the same. The quantity of material at command was so minute that it soon became all "wasted" in endeavouring to observe these curious appearances. One point more in connection with the sample of this interesting growth deserves mention—the mucous matrix was everywhere between the cells of the alga permeated by Bacteria embedded therein; these were in a quiescent state; if one could eliminate every one of the algal cells one would have under view a growth which might be regarded as Zoogloœa (Cohn). It is, however, hardly conceivable that these two organisms (to call them two algæ, though not nearly "allied," would seem to be in accordance with Nature) had anything to do with one another, beyond its being possible that each may have contributed to the production of the mucous matrix; yet see the marked form assumed by the plant in the aggregate—the little *quasi* "cauliflowers"—which were offered to view.

It is a pity that no further data than the little this brief and crude record contains can be given in respect to this production. Let us hope that Prof. Dickson may hereafter succeed in rearing up some more of the same thing from his tank of *Hydrodictyon*, aided (?) by a few *flies*, and thus be able to shed a further light on a little production sufficiently curious and interesting.

23*rd January*, 1873.

On Epithemia marina (Donkin).—Rev. E. O'Meara exhibited *Epithemia marina* (Donkin). At first view this form appears to belong to the genus *Amphora*, but on closer examination it must be referred to *Epithemia*, where Donkin has properly placed it. The first view, seen in a direction slightly oblique, presents the pointed ends described by him ('J. M. S.,' Vol. VI, Pl. III, fig. 14 *a*); but observed directly the produced ends are broadly and distinctly capitate and reflexed.

Skin of Monitor.—Dr. Barker showed preparations of skin of Monitor.

Lepidosiren.—Professor Traquair exhibited section of tail of Lepidosiren.

Licmophora flabellata.—Dr. Steele showed *Licmophora flabellata* from near Kingstown, a diatom, in Mr. O'Meara's experience, very rare on the East Coast.

Gundlach's Glasses.—Dr. Richardson showed a so-called $\frac{1}{24}$th of Gundlach's, on some difficult diatoms, which seemed to perform very well.

New Localities for Closterium Archerianum (Cleve), and *C. Cynthia* (de Notaris).—Mr. Archer showed examples of *Closterium Archerianum* (Cleve), taken for the second time in Ireland, now from County Tipperary, the first collection in which he had seen this species being from County Galway. He now showed also, side by side, the form which he had for some time recognised as *Closterium Cynthia* (de Notaris), though not quite coinciding with that author's figure in his 'Elementi per lo Studio delle Desmidiace Italische' (pl. vii, fig. 71); but at least the latter form was shown to be quite a distinct thing from the former. Both are certainly rare and very pretty.

Flowers of Welwitschia.—Professor M'Nab showed preparations of the young female flower of *Welwitschia*, drawing attention to the several parts, and expressing his view that the outer was carpellary, though he had formerly regarded it rather as a part of the perianth.

Mesotænium violascens (de Bary).—Mr. Crowe showed *Mesotænium violascens* (de Bary), taken from the little "Stephanosphæra pool" (a little hollow amongst the rocks) on Bray Head. These were nice characteristic examples and formed a pretty object.

New Species of Mastogloia.—Rev. E. O'Meara referred to an undescribed form of the genus *Mastogloia*, found by him in a gathering made some time since from a moist rock at Ballyshannon, which he proposed to name *Mastogloia costata*. Valve narrow, elliptic. Length ·0013″. Breadth ·0005″. Striæ strongly costate and close, the three central convergent towards the nodule, shorter than the rest, and making a tolerably large free space around it; the remainder slightly radiate towards the apices. Loculi about 10 in number. In consequence, perhaps, of the convexity of the valve or the coarseness and closeness of the costate striæ, or both, exhibiting only a very faint outline on the side view.

A New Calycella.—Prof. E. Perceval Wright showed specimens of a new species of *Calycella* from the deep sea, an account of which he had in preparation.

Hairs from Tumours.—Dr. John Barker showed examples of hair obtained from an ovarian tumour, which upon examination appeared to be of fœtal character; there was an absence of bulbs, and several hairs appeared as if sharply cut across.—Dr. Barker likewise showed an entozoan, *Ascaris rigida*, from the œsophagus of *Lophius piscatorius*, found by Müller, Bellingham, and others.

Corresponding Members.—It was announced that, at a preceding business meeting of the Club, Judge Mouchet, Rochfort-sur-Mer, France, Colonel Woodward, America, Professor P. T. Cleve, and Dr. Veit Brecher Wittrock, of Upsala University, had been elected Corresponding Members of the Club.

February 20th, 1873.

Lynceus tenuicaudis, Sars (new to Ireland).—Mr. A. Andrews exhibited *Lynceus tenuicaudis* (Sars), taken by him, for the first time, in a small pond near Dublin. This form is evidently rare, having been but twice recorded in England. The spiny armature of the abdomen was well shown.

Flowers of Welwitschia.—Professor M'Nab exhibited preparations of the male flower of *Welwitschia mirabilis*. In its earliest stages it appears as a minute papilla, the outer leaves of the perianth forming, at the base, right and left; no stalk-like portion of the axis existing like that of the female flower. The inner parts of the perianth form anteriorly and posteriorly shortly after the formation of the outer parts. The stamens develop right and left, each at first appearing as a single primordial stamen, which branches into three. Dr. M'Nab could not confirm Strasburger's observation as to the origin of the stamens in two rows. After the formation of the stamens the two carpels form anteriorly and posteriorly, the *punctum vegetationis* occupying the centre of the flower. The carpels rapidly develop, forming a style and stigma, while the end of the axis which forms the ovule in the female flower enlarges but slightly, and never forms an integument or embryo-sac.

Hormospora plena (Kütz.).—Mr. Crowe showed an alga—probably *Hormospora plena* (Kütz.)—one generally found sparingly and isolated. The present examples showed cases of self-division of the quadrate cells taking place, not transversely or across the filament, but in the opposite direction, when the adjacent ends of the just-divided cells became elongated in a somewhat oblique manner, and passing one another by, thus become the starting-points for new portions of the filaments not in direct continuation, but side by side—but still maintaining linear arrangement of the cells—the whole involved in the common mucous envelope. This gave this filament a seemingly unusual appearance, though doubtless such a mode of growth may be of common occurrence.

Note of a new Astrodisculus (?).—Mr. Archer showed a drawing of a Rhizopodous form seemingly appertaining to Greeff's genus *Astrodisculus*. This did not agree with any of the forms figured by that author in those characters regarded by him as specific, and moreover differed from them all in the outer envelope being double or forming two strata, both of great tenuity and quite hyaline, but the inner having a greater degree of consistence than the outer. Professor Greeff speaks of the (single) envelope in his forms being possibly of a silicious nature, as it withstood the action of sulphuric acid; and he supposed, therefore, that this envelope must of necessity be minutely perforated to admit of the passage outwards of the extremely slender filiform pseudopodia. Mr. Archer had only once before met with any form seemingly belonging to *Astrodisculus*, and the brief examination he had been then able to give his examples suggested to him that the outer envelope was not silicious, but quite of a soft and plastic nature, quite hyaline, and without any striæ or

markings or indeed any apparent structure; and he could not but suppose (as in other forms) that the pseudopodia projected through it by virtue of their capacity readily to penetrate so soft an envelope. Such was also his view in the present instance; and indeed the application of sulphuric acid at once removed the outer envelope, then, but not so quickly, the sarcode body, leaving the brown "central capsule" lying with a thin stratum of granules adhering to its outer surface, so that it did not show a sharply bounded outline. The brown central capsule occupied about one third that of the body-mass, which was of an orange-brown colour, more brown towards the middle, gradually more clearly orange towards the circumference; the pseudopodia not numerous, slender, filiform, about the length of the diameter of the body, the two outer envelopes hyaline, and the inner one in depth about one half the diameter of the body, the outer less deep and of greater seeming tenuity than the inner, the line of demarcation being quite sharp. Of course the conflicting views as regards the nature of the outer envelope suggests the likelihood or possibility that, after all, not either of the two forms alluded to occurring in this country is congeneric with those forming Greeff's genus *Astrodisculus;* but at any rate the likeness is strong, and the probability seemingly great, that they are really so. Mr. Archer would rather refind this form ere saying any more about it. These forms appertaining to Astrodisculus seem to be very rare, a fact which may render a decision regarding them all the more difficult: one requires a goodly number of examples to operate upon satisfactorily; so minute objects, too, on being treated with reagents have a great tendency to *roll away*, and, despite long search, hopelessly to evade rediscovery.—Mr. Archer showed also some living examples of *Amphizonella vestita*, a form not uncommon (in suitable localities, of course) and varying as to presence or absence of chlorophyll-granules, or of the outer hair-like processes, in the manner already described by him; the present were without the specialities referred to; they showed, however, the pseudopodia well extended.

East Kent Natural History Society.

Honorary Secretary—George Gulliver, F.R.S., &c.

April 3rd, 1873.

Raphides, Sphæraphides, and Crystal Prisms.—Slides, drawings, and extemporaneous preparations of these beautiful plant-crystals were exhibited and explained by Mr. Gulliver, who read a paper thereon, and remarked that, considering their interest both as microscopic objects and botanical characters, it is surprising that they have not received more attention. They are

ignored in our Floras, and but crudely described in our treatises on the microscope. Hence, these crystals require more explicit treatment, so that experts may realise their value as taxonomic characters, and ladies and novices find an additional source of microscopic amusement and instruction for idle time, thus not idly spent. Our present knowledge of the value of raphides as natural characters in systematic botany is chiefly confined to the memoirs by the author, published piecemeal in various journals, and summarised in the 'Popular Science Review' up to October, 1865, since extended in the 'Annals of Natural History' of that year, and in several numbers of 'Seemann's Journal of Botany,' and the 'Quarterly Journal of Microscopical Science.' But independent inquirers have not yet subjected those observations to such practical criticism as would prove either their erroneousness or truthfulness. We hear only, and but seldom, of exceptions, whether correct or incorrect, as if these were not well known to be common to some of the best diagnostic characters in natural science.

The chief source of error has been in the confusion of terms, for all sorts of microscopic crystals in plants are too commonly included under the term "raphides." But this error is quite fatal to any due estimate of their taxonomic value. Crystals of one form or other are common, and often abundant, in plants that never produce any raphides at all. Hence we have had and still have endless ambiguity and confusion, which it is to be hoped that the author's drawings and detailed descriptions, reproduced in the 'Science Gossip,' May 1, 1873, will correct in future. But, besides the vagueness of the current knowledge of the subject, a prevailing cause of the difficulty in the acceptance by systematists of the characters afforded by raphides, is the difficulty and extensiveness of the inquiry as to the value of such diagnoses. The question first to be determined concerns the constancy of raphides or other crystals in several single species of our native plants, at all periods of their growth and in every soil or situation; and then come the wider researches as to the constant absence of the crystals from other species, and the still more laborious task of carrying the whole investigation throughout the Flora of the world. On this last point the author's observations have been fragmentary only, but they have been continued for many years on British plants, with occasional elucidations by parallel examinations of exotic species. Difficulties will often occur. Thus, after searching for years for a plant of the Order Onagraceæ devoid of raphides, it was, seemingly, found in *Montinia*, but only to afford one of those exceptions that best prove the rule, as this genus, though placed in the order Onagraceæ by Lindley, has since been removed from it to the Saxifragraceæ. The angular minute crystals, about $\frac{1}{400}$th of an inch long and $\frac{1}{20000}$th thick, occurring for the most part scattered here and there singly in the old leaves of *Gentiana acaulis*, and some other plants, are not true raphides.

April 17th, 1873.

Apparatus for Drawing Microscopic Objects.—Col. Horsley exhibited for this purpose a very simple contrivance, which is easily used and need not cost a shilling. It consists of a deal box, four and a half inches square and nine inches in length, with a circular aperture at one end large enough to admit the draw-tube of the microscope with the eyepiece attached, and at the other end a square of ground glass of the same size as the box, the wood having been removed for the purpose. To obtain the desired image of the object the microscope is placed horizontally, with its eyepiece end into the hole made for it in the box, when the object is focussed and illuminated on the ground glass, and then very easily drawn by hand. The whole apparatus is more fully described in 'Science Gossip,' 1868, p. 236.

Queen of the Honey-Bee—Major Munn exhibited drawings in illustration of the structure and functions of the oral apparatus of the queen as compared with the corresponding parts of the drone and worker. He also continued his observations on the power of the queen to sting the hand, and decided the question, as before, in the negative.

Starch-sticks in the Latex of Spurges.—Colonel Horsley gave extemporaneous demonstrations of these in the milky juice of *Euphorbia amygdaloides*. These rods of starch are, in our Flora, sharply diagnostic of the genus *Euphorbia*, as described at a former meeting of the Society, reported in the 'Quart. Journ. of Mic. Science,' for January, 1872.

Red Flint.—Capt. S. Gordon McDakin submitted some observations on red flint found in chalk, near Canterbury, several feet below the surface, and suggested that in them microscopic examination might detect fragments of sponges or other bodies which may be supposed to afford the iron that gives colour to the mass.

May 1st, 1873.

The meeting was fully occupied in the examination of specimens provided by Col. Horsley, Mr. Sibert Saunders, and Mr. Fullagar, of fluviatile and marine zoology, and fresh botanical specimens collected by Mrs. Dean.

May 15th, 1873.

Extirpation of Rare Plants.—Mrs. Dean brought several rare plants, and made the usual complaint that they are becoming gradually so scarce as to threaten their total extinction. Whereupon some strong observations were made on the rapacious cupidity of mere collectors, and the vain and absurd notion that a knowledge of botany consists in collecting specimens and calling them by their scientific names—an error fostered by the too common practice of societies in offering premiums for the largest collections, instead of being guided by the proper tests of the

candidates' knowledge, which would nowise cause the destruction of our rare plants.

Senecio squalidus.—This ragwort, though reported in our Floras as peculiar to Oxford and Bideford, is abundant at Canterbury. The pollen-grains were examined by the Hon. Sec., and found to be oval and muricated, $\frac{1}{300}$th of an inch long and $\frac{1}{1143}$rd broad, and showing three scars when treated with sulphuric acid.

Crystal Prisms in the Ovary of Compositæ.—Of these Mr. Gulliver showed specimens in the ovary-coat of Cyanareæ, and described their taxonomic import. They are figured in 'Science Gossip' of May, 1873.

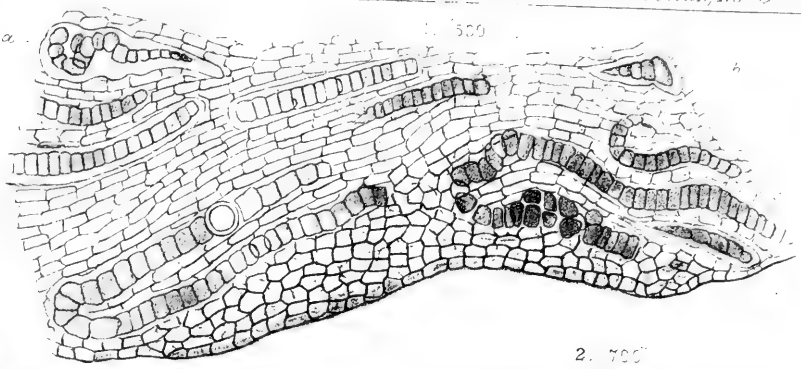
Shape of the Nucleus of the Blood-discs of Pyrenæmatous Vertebrates.—He also exhibited preparations from which it appeared that, stating the breadth of the nucleus at 1, its length is from 2 to $2\frac{1}{2}$. This is the regular form in most birds, but there are exceptions, as in the common fowl, which has the nucleus much shorter, often merely suboval, and hence, perhaps, the German error, by Rollett, in 'Stricker's Human and Comparative Histology,' that the nucleus of the pyrenæmata is "sometimes more or less circular, as in the birds, or elliptical, as in the frog."

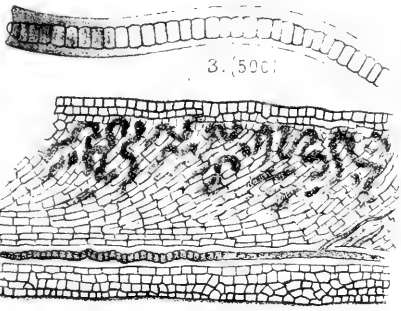
June 5th, 1873.

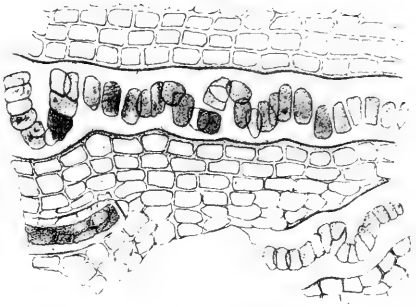
Lophius piscatorius.—Mr. Sibert Saunders exhibited and described a specimen of this fish, about nine inches long, a female, with the ovaries quite immature. It was taken at Whitstable, where it is much less common than on some other parts of our coast. The blood-discs were examined by Mr. Gulliver, and found to be regularly oval, with the long diameter $\frac{1}{1895}$th, the short diameter $\frac{1}{2666}$th, and the thickness $\frac{1}{3000}$th of an inch, these being average sizes, and larger than is common in osseous fishes, though rather smaller than in the Salmonidæ, of which these corpuscles are figured in the 'Proc. Zool. Soc.,' Nov. 19th, 1872.

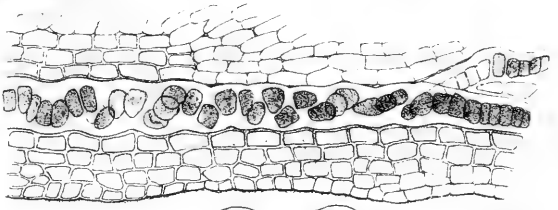
Economy of Freshwater Polyps.—On this subject Mr. Fullagar continued his observations, and illustrated them by living specimens and drawings. He described the eggs of *Hydra viridis* as dark brown in colour, somewhat tuberculated on the surface, globular in shape, and about $\frac{1}{60}$th of an inch in diameter. These were hatched in April, and while emerging from the ovum the young hydra had two short tentacles, to which a third was added about the seventh day, when the animal was free and able to adhere by its sucker to the glass. Though *H. vulgaris* regularly deposits its ova in the autumn, he has seen this species, in his aquarium, produce eggs during March, which were hatched early in May. He is preparing for publication descriptions and drawings of his observations.

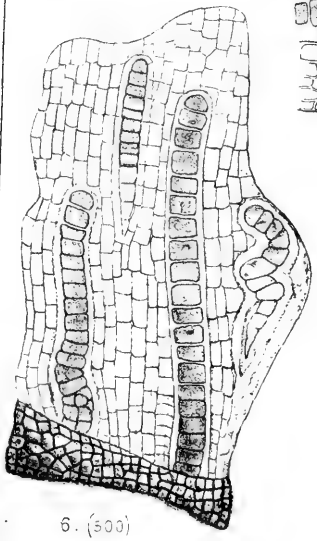
Crystals in the Seed-coat of the Elm, and Character of the Epidermis of the Tway-blade.—Preparations and drawings of these were communicated by Mr. Gulliver. The substance of his observations thereon is given in his paper at page 290 of the present number of this 'Quarterly Journal of Microscopical Science.'

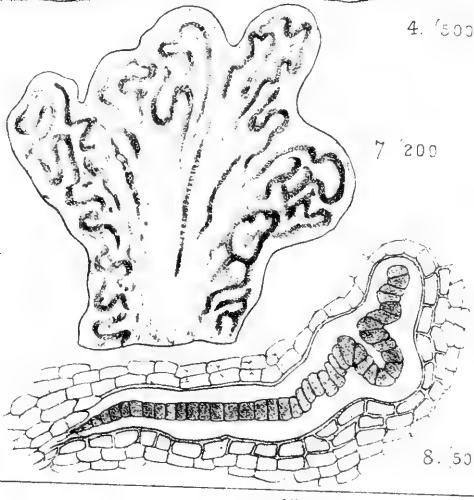












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EXPLANATION OF PLATES IX & X,

Illustrating Prof. Schwendener's Papers on the Nature of Lichens.

The amplification is indicated by the figures in brackets on the plates.

PLATE IX.

Figs. 1 & 2.—*Lichen "Pannariæ affinis,"* Tuck.—1. Portion of a section of a thallus with embedded *Scytonema*-filaments. At *a*, *b*, and *c* the sheaths assumed the well-known green colour with hydrochloric acid. The tissue is without interstices, parenchymatous in the interior with the cells elongate, towards the surface short-celled with almost isodiametral cells.—2. Portion of a section of a thallus with curved *Scytonema*-threads in colourless sheaths, which are intimately united to the neighbouring cells. To the left below, a slightly altered *Scytonema*-filament with dead heterocyst.—The tissue of this lichen is, on the whole, *Pannaria*-like, calling to mind in several points of view also *Lichina*. The embedded Gonidia-chains, which sometimes appear as unaltered *Scytonemeæ*, but sometimes also form irregular convolutes, not rarely show globular heterocysts. In the interior of the thallus-lobes the sheaths are altogether colourless; at and near the surface, a green colour presents itself on the application of hydrochloric acid. This lichen was communicated to me by Prof. Tuckerman, with the remark: "absque fructu, California."

Fig. 3.—Portion of a freely-vegetating *Scytonema*-filament from the same substratum as the lichen from Tuckerman just mentioned. The sheath towards the right hand was colourless, at the end to the left, however, yellowish (in hydrochloric acid green). Is manifestly the gonidia-former of the foregoing lichen.

Figs. 4—8.—*Pannaria flabellosa*, Tuck.—4. Radial section through the under part of the thallus with a similar sheath as in fig. 2.—5. The same radial section completely from the upper to the under cortical stratum. The gonidia-chains in the upper part of the thallus are irregularly curved, here and there conglomerated into regular clusters; the sheaths here become coloured in hydrochloric acid slightly or intensely green (the same as in *Racoblenna*). Diam. of the cortical cells in the fully-grown state 10 m.m.m. and more.—6. Portion of a thallus section with partly straight, partly more or less curved, gonidia-chains. Tissue indistinct on account of the previous warming with hydrochloric acid. Below a portion of the lower (blue-coloured) cortex in oblique superficial view, somewhat crushed and put out of position. The gonidia here do not lie at the upper surface.—7. Young thallus-lobe in superficial view in order to present its form and the arrangement of the gonidia. 8. A curved gonidia-chain with sheath (same as fig. 6 to the right). The sheath becomes coloured intensely green in hydrochloric acid. This lichen essentially agrees with the foregoing, only the globular heterocysts were here more rare. Free *Scytonema*-filaments of 8—10 m.m.m. thickness were not rare upon the same substratum, *Rivularieæ* none. On the label Tuckerman had added the remark: 'Obs. Lich. in Proceed. Acad. Amer.,' 5, p. 401. In Nova Anglia.

PLATE X.

Fig. 9.—*Cephalodia Stereocaulorum.*—*Sirosiphon*-group from the medullary portion of a *Cephalodium;* with exception of the projecting apices of the alga, closely surrounded by *Stereocaulon*-fibres.—This figure is given in completion of what has been mentioned previously.

Fig. 10.—*Sphæromphale fissa.*—Hymenial-gonidia; mostly two-celled and then 10—14 m.m.m. long and $3\frac{1}{2}$—4 m.m.m. broad; besides others also three-celled, as in the figure, which reach 17 m.m.m. in length. Membrane very delicate; contents, in comparison with the rest of the gonidia, more *bluish*-green. Agree exactly with *Stichococcus bacillaris*, which, for the sake of greater certainty, I examined from examples which Dr. Rabenhorst kindly sent me as "forma minor."

Fig. 11.—*Polyblastia intercedens ?*, Hepp.—(*a*) Alga, that is, gonidia in a free condition. Cells as much as 20 m.m.m. in diameter, ordinarily 12—16 m.m.m., often with brown, more frequently with colourless, membrane; (*b*) the same green cells on the surface of the thallus (here without cortex), partly involved by short-celled hyphæ.

Figs. 12 & 13.—*Gonionema.*—12. Young spermogonium. The development of these spermogonia manifestly took place in the interior of the *Scytonema-sheath*, that is, in the membrane-substance itself; for the yellow coloured peripheral membrane-lamella is swollen out in an inflated manner and longitudinally torn. Such occurrences speak in any case rather for than against the parasitism.—13. A portion of the thallus of the same lichen from Arnold exceptionally with curved gonidia-chains. As a rule, the thallus is more slightly surrounded and appears like an unaltered *Scytonema*-filament.

Fig. 14.—*Secoliga gyalectoides*, Mass.—Two gonidia-groups, manifestly belonging to *Chroolepus*. Contents intensely orange-coloured.

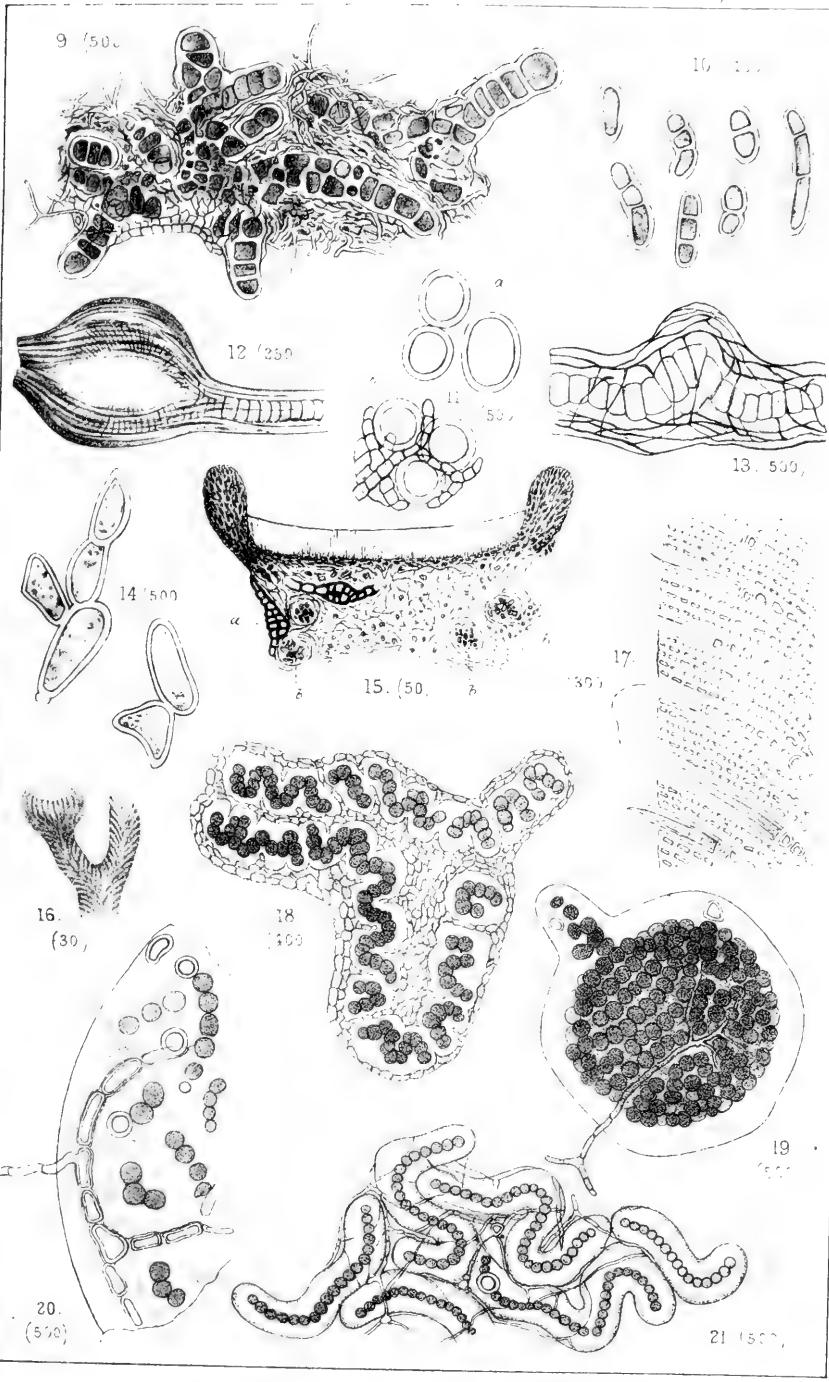
Fig. 15.—*Secoliga* upon "*Bryophagus.*"—Median section through an apothecium. The hypothecium (its maximum thickness 20 m.m.m.) is directly seated upon the jelly of *Bryophagus*, and sends downwards numerous individual hyphæ, which surround the scattered Chroococcaceous colonies. To the left (at the margin at *a*) and somewhat deeper in the interior are two obliquely intersected brown moss-leaves; below (at *b*) colonies of *Glœothece*. The spores of this *Secoliga* were linear-spindleshaped, 12 m.m.m. long and about 2 m.m.m. broad.

The following figures are copied from the Plates (coloured in the original) illustrating the 'Algentypen der Flechtengonidien:'—

Figs. 16 & 17.—"*Thamnidium Willeyi*," *Lichina*, sp.—16. Portion of the shrubby thallus of "*Thamnidium Willeyi*," *Lichina*, spec., showing a terminal apothecium (t. i, f. 4, in orig.).—17. Portion of a longitudinal section through the thallus of the same. The embedded gonidia, which here figure as gonidia, partly still possess yellowish-coloured sheaths and flagellate ends (t. i, f. 5, in orig.).

Fig. 18.—*Racoblenna.*—Two gonidial chains, bent zig-zag and with parenchymatous envelope. A portion of a larger convolute, in which here and there some algal filaments, still unaltered, projected outwards (t. ii, f. 2, in orig.).

Figs. 19—21.—*Nostoc.*—19. A *Nostoc*-colony, into which a fungal-thread has penetrated from without (t. ii, f. 13, in orig.).—20. A portion of a larger *Nostoc*-colony penetrated by a fungal thread (t. ii, f. 14, in orig.).—21. *Nostoc* threads with special sheaths (*Hormosiphon*) permeated by fungal threads. Seen in a tangental section through the thallus of *Lempholemma* (t. ii, f. 15, in orig.).



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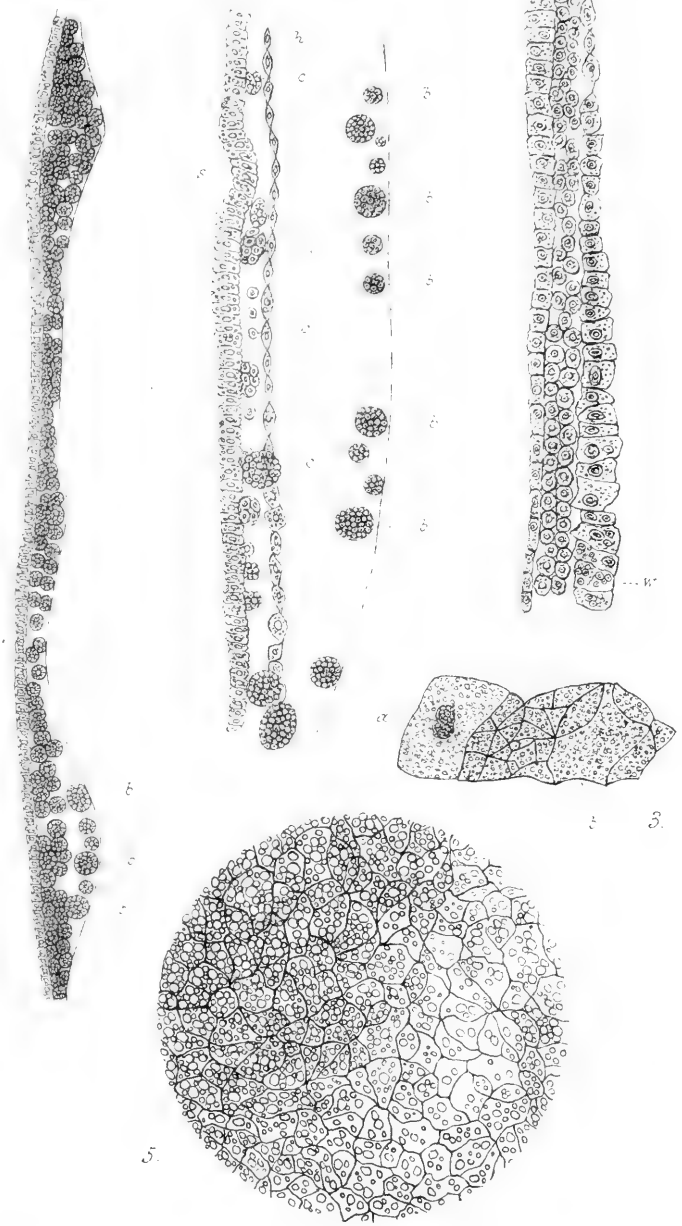
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Plate XI.—To illustrate the paper upon the Growth and Development of the Layers of the Blastoderm.

Fig 1.—Section through an unincubated blastoderm, showing the upper layer, composed of a single row of columnar cells, and the lower layer, composed of several rows of rounded cells in which no nucleus is visible. Some of the "formative cells," at the bottom of the segmentation cavity, are seen at (*b*).

Fig. 2.—Section through the periphery of an eight hours' blastoderm, showing the epiblast (*p*), the hypoblast (*h*), and the mesoblast commencing to be formed (*c*), partly by lower-layer cells enclosed between the epiblast and hypoblast, and partly by formative cells. Formative cells at the bottom of the segmentation cavity are seen at *b*. At *s* is one of the side folds parallel to the primitive groove.

Fig. 3.—Portion of the hypoblast of a thirteen hours' blastoderm, treated with silver nitrate, showing the great variation in the size of the cells at this period. An hour-glass-shaped nucleus is seen at *a*.

Fig. 4.—Periphery of a twenty-three hours' blastoderm, showing cell for cell the junction between the hypoblast (*h*) and white-yolk spheres (*w*).

Fig. 5.—Junction between the white-yolk spheres and the hypoblast cells at the passage from the area pellucida to the area opaca. The specimen was treated with silver nitrate to bring out the shape of the cells. The line of junction between the opaque and pellucid areas passes diagonally.

Plate XII, figs. 1 to 3.—To illustrate the paper upon the Primitive Groove.

Figs. 1 and 2 are sections through an embryo rather earlier than the one drawn in fig. 3. Section 1 passes through the just commencing medullary groove (*md*), which appears in fresh specimens, as in fig. 3, merely as an opaque streak coming from the end of the primitive groove. The notochord is hardly differentiated, but the *complete* separation of mesoblast and hypoblast under the primitive groove is clearly shown. Section 2 passes through the anterior end of the primitive groove (*pr*), and shows the fusion between the mesoblast and epiblast, which is always to be found under the primitive groove.

Fig. 3 is a view from above of a twenty hours' blastoderm, seen as a transparent object. Primitive groove (*pr*). Medullary streak (*m, d*), which passes off from the anterior end of the primitive groove, and is produced by the thickening of the mesoblast. Headfold (*p, f*).

Plate XII, figs. 4, 5, 6, and 7.—To illustrate the paper upon the Growth and Development of the Layers of the Blastoderm.

Fig. 4.—Section through the primitive streak of an eight hours' blastoderm. The specimen shows the mesoblast very much thickened in the immediate neighbourhood of the primitive streak, but hardly formed at all on each side of the streak. It also shows the primitive groove just beginning to be formed (*pr*), and the fusion between the epiblast and the mesoblast under the primitive groove. The hypoblast is completely formed in the central part of the blastoderm. At *f* is seen one of the side folds parallel to the primitive groove. Its depth has been increased by the action of the chromic acid.

Fig. 5.—Hypoblast cells from the hinder end of a thirty-six hours' embryo, treated with silver nitrate, showing the regularity and elongated shape of the cells over the embryo and the smaller cells on each side.

Fig. 6.—Epiblast cells from an unicubated blastoderm, treated with silver nitrate, showing the regular hexagonal shape of the cells and the small spherules they contain.

Fig. 7.—Portion of the epiblast of a thirty-six hours' embryo, treated with silver nitrate, showing the small rounded cells frequently found at the meeting points of several larger cells which are characteristic of the upper layer.

Plate XIII.—To illustrate the paper on the Primitive Groove.

Figs. 1, 2, 3, 4, 5, are sections through the blastoderm, drawn in fig. 6 through the lines 1, 2, 3, 4, 5, respectively.

The first section (fig. 1) passes through the true medullary groove (*mc*); the two medullary folds (A, A) are seen on each side with the thickened mesoblast, and the mesoblast cells are beginning to form the notochord (*nc*) under the medullary groove. There is no adherence between the mesoblast cells and the epiblast under the medullary groove.

The second (fig. 2) section passes through the medullary groove where it has become wider. Medullary folds, A, A; notochord, *ch*.

In the third section the notochord (*ch*) is broader, and the epiblast is raised in the centre, while the medullary folds are seen far apart at A.

In section fig. 4 the medullary folds (A) are still to be seen enclosing the anterior end of the primitive groove (*pr*). Where the primitive groove appears there is a fusion of the epiblast and mesoblast, and no appearance of the notochord.

In the last section, fig. 5, no trace is to be seen of the medullary folds.

Figs. 6 and 7 are magnified views of two hardened blastoderms. Fig. 6 is twenty-three hours old; fig. 7 twenty-five hours. They both show how the medullary canal arises entirely independently of the primitive groove and in front of it, and also how the primitive groove gets pushed backwards by the growth of the medullary groove. *pv*, Protovertebræ; other references as above. Fig. 6 is the blastoderm from which sections 1 to 5 were cut.

Plate XIV.—To illustrate the paper upon the Development of the Vascular System in the Chick.

Fig. 1 is taken from the anterior part of the pellucid area of a thirty hours' chick, with four proto-vertebræ. At *n* is a nucleus with two nucleoli.

Figs. 2 and 3 are taken from the posterior end of the pellucid area of a chick with eight proto-vertebræ. In fig. 3 the nuclei are seen to have considerably increased in number at the points of starting of the protoplasmic processes. At *n* is seen a nucleus with two nucleoli.

Fig. 4 is taken from the anterior part of the pellucid area of an embryo of thirty-six hours. It shows the narrow processes characteristic of the anterior part of the pellucid area, and the fewer nuclei. Small spaces, which have the appearance of vacuoles, are shown at v.

Fig. 5 is taken from the posterior part of the pellucid area of a thirty-six hours' embryo. It shows the nuclei, with somewhat irregular nucleoli, which have begun to acquire the red colour of blood-corpuscles; the protoplasmic processes containing the nuclei; the nuclei in the protoplasm surrounding the corpuscles, as shown at *a*, *a'*.

Fig. 6 shows fully formed blood-vessels, in part filled with blood-corpuscles and in part empty. The walls of the capillaries, formed of cells, spindle-shaped in section, are shown, and also the secondary investment of Klein at *k*, and at *b* is seen a narrow protoplasmic process filled with blood-corpuscles.

Fig. 7 is taken from the anterior part of the pellucid area of a thirty-six hours' embryo. It shows a collection of nuclei which are beginning to become blood-corpuscles.

Figs. 1—5 are drawn with an $\frac{1}{8}$ object-glass. Fig. 6 is on a much smaller scale. Fig. 7 is intermediate.

Plate XV.—To illustrate the paper upon the Development of the Vascular System in the Chick.

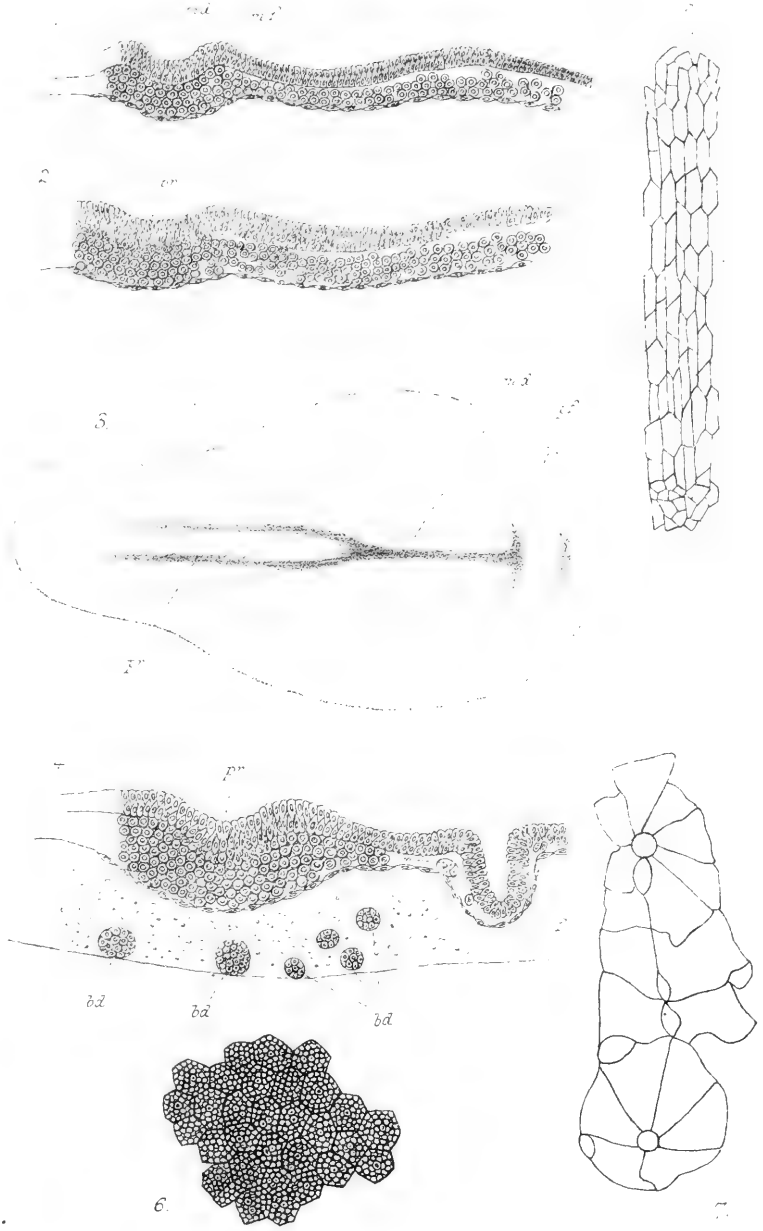
Fig. 1.—A transverse section through the dorsal region of a forty-five hours' embryo; *ao*, aorta with a few blood-corpuscles. v, Blood-vessels, all of them being formed in the splanchnopleure, and all of them provided with the secondary investment of Klein; *p*, *e*, pellucid area; *o*, *p*, opaque area.

Fig. 2.—Small portion of a section through the opaque area of a thirty-five hours' embryo, showing protoplasmic processes, with nuclei passing from the somatopleure to the splanchnopleure.

Fig. 3.—Section through the heart of a thirty-four hours' embryo. *a*, Alimentary canal; *hb*, hind brain; *nc*, notochord; *e*, epiblast; *s*, *o*, mesoblast of the somatopleure; *sp*, mesoblast of the splanchnopleure; *hy*, hypoblast; *hz*, cavity of the heart.

Fig. 4.—Section through the same embryo as fig. 3, and passing through the orifice of the omphalo-meseraic vein. *of*, Omphalo-meseraic vein; other references as above.

These two sections show that the heart is entirely formed from the mesoblast of the *splanchnopleure*, and that it is formed by the splitting of that part of the mesoblast which has turned to assume its normal direction after being folded in to form the muscular wall of the alimentary canal. In fig. 4 the cavities so formed on each side have not yet united, but in fig. 3 they have united. When the folding becomes more complete the cavities (*of*, *of*) in fig. 4 will unite, and in this way the origin of the omphalo-meseraic veins will be carried further backwards. In the section immediately behind section 4 the mesoblast had become thickened, but had not split.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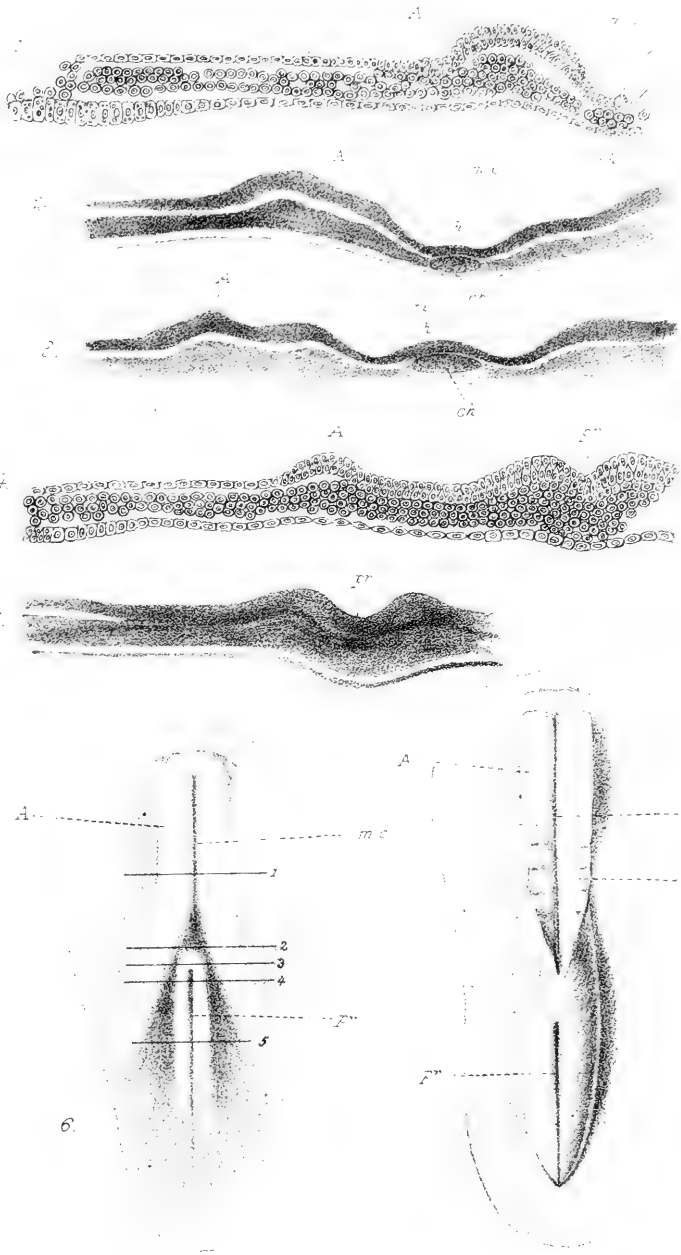
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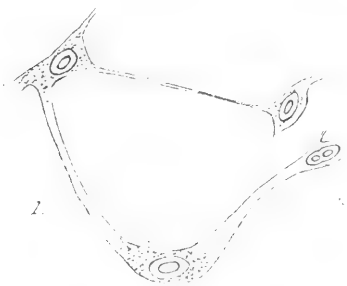

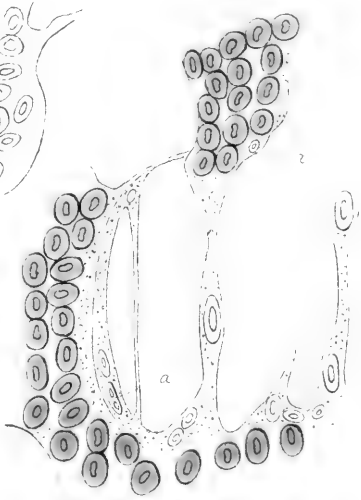

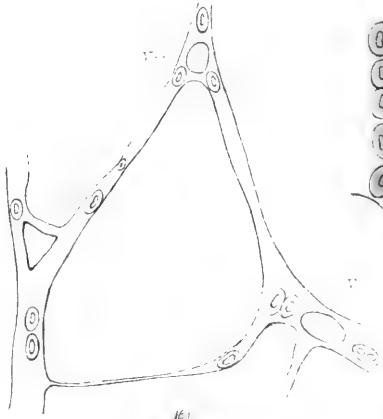

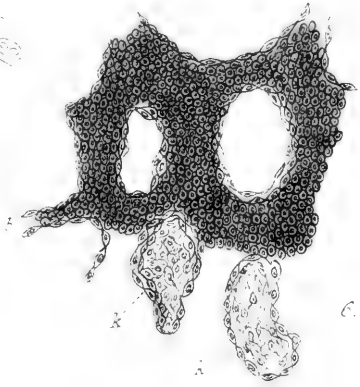

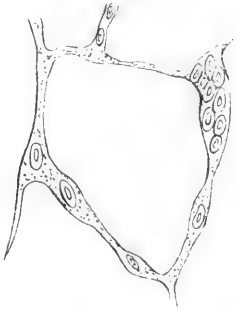

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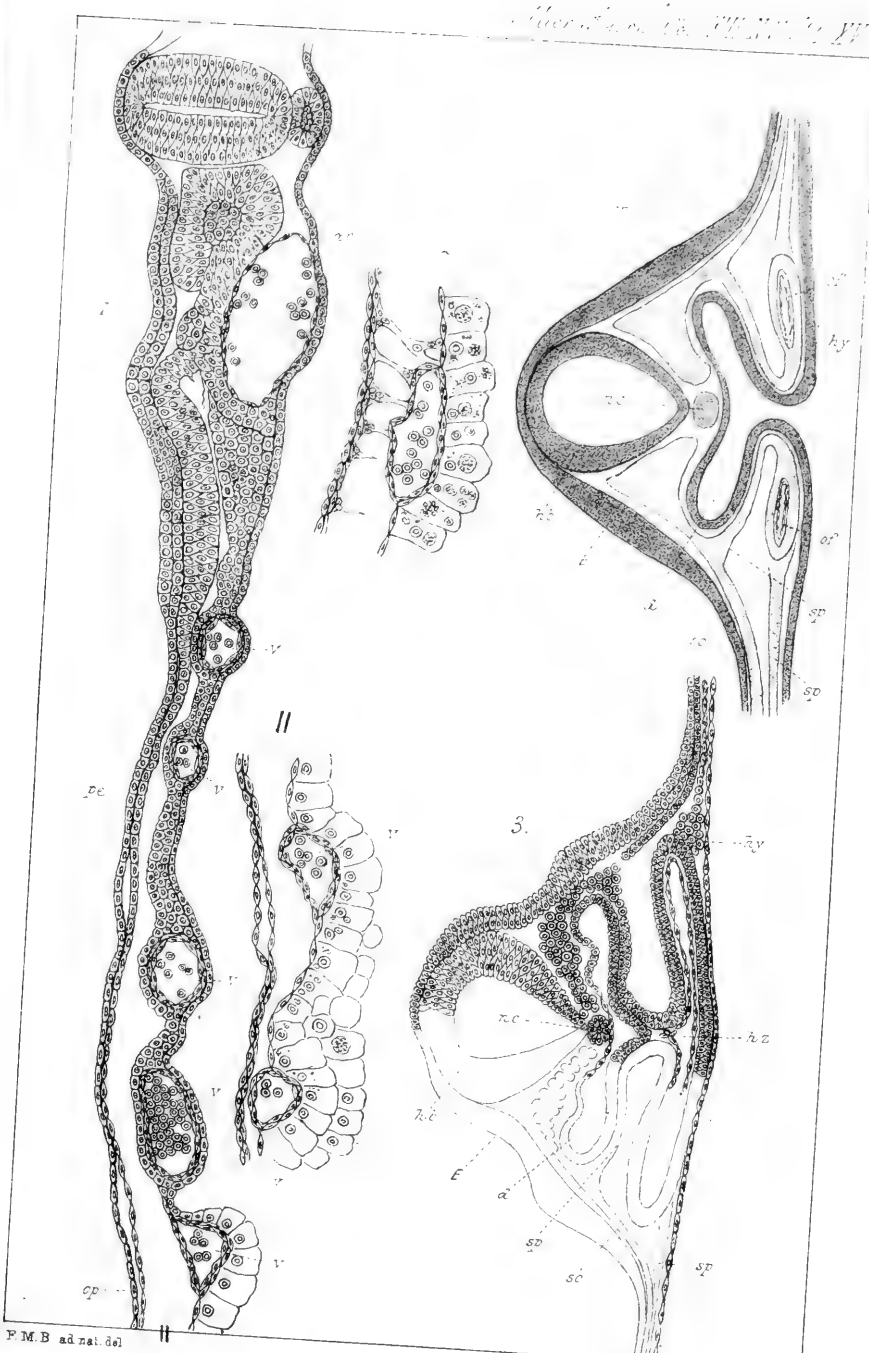
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Fig 3, × 90

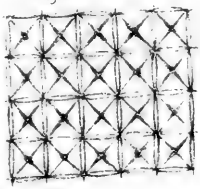

Fig 1

Fig 7, × 90

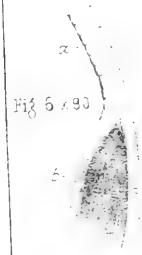

Fig 5 × 90

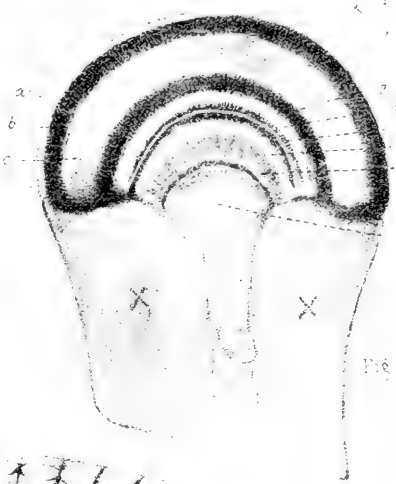

Fig 2 × 8

Fig 5, × 90

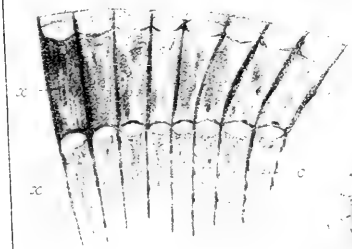

Fig 8 × 90

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Fig 13.

Fig 12, × 15

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Fig 16, × 90.

Fig 17, × 250

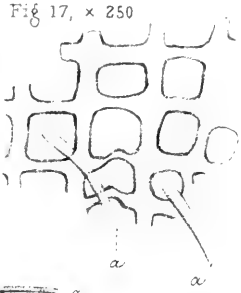


Fig 15, × 130.

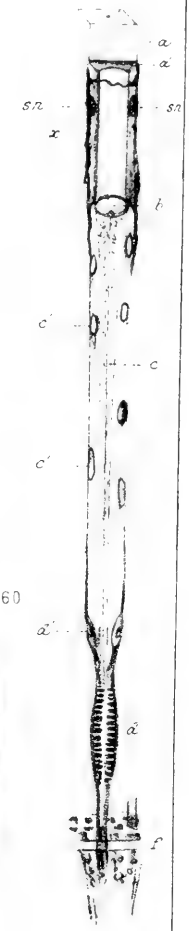


Fig. 19, × 250.

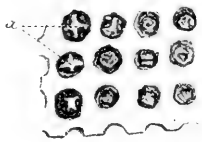


Fig 14, × 15.

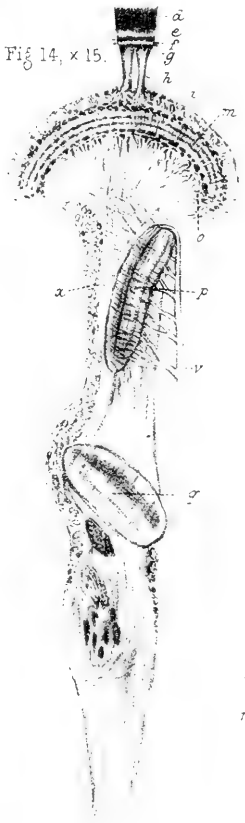
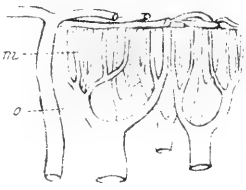


Fig 22.

Fig. 18, × 260

Fig. 20.

Fig 21. × 94



EXPLANATION OF PLATES XVI & XVII,

Illustrating Mr. Edwin T. Newton's paper on the Structure of the Eye of the Lobster.

PLATE XVI.

Fig.

1.—Front part of the head of a lobster (*Homarus vulgaris*), natural size, the upper part of carapace and rostrum being removed. (*a*) The right eye showing the semilunar form of the cornea and (*b*) its basal joint. The upper portion of the left eye has been removed to show the optic nerve which swells anteriorly to form (*c*) the so-called optic ganglion. (*d*) The kidney-shaped body seen in section in fig. 13 *r*. (*e*) The supra-œsophageal ganglion, raised somewhat out of its natural position.

2.—A longitudinal and horizontal section of a right eye seen by reflected light (× 8). (*a*) Cornea. (*b*) First band of pigment, beneath which are the crystalline cones. (*c*) A broad band of radiating fibres free from pigment. (*d*) Second black band composed of the pigmented spindle-shaped bodies; the lower ends of these bodies are covered with an opaque white pigment which forms (*e*) the first white band. (*f*) The third black band; the fine line running in the centre of this band is the fenestrated structure (fig. 19), it shows the boundary of the so-called optic ganglion. (*g*) Second white band. (*h*) Bundles of radiating nerve-fibres. (*i*) Enlarged end of the optic nerve. The muscles and connective tissue which naturally fill the cavity (×) have been omitted in this figure.

3.—Portion of cornea as seen by reflected light, showing the cross and central spot.

4.—Perpendicular section from the middle of the cornea, showing the smooth outer and slightly convex inner surfaces.

5.—Similar section near edge of cornea, showing smaller size of facets and their more convex inner surface. This section is viewed somewhat obliquely, and, therefore, three or four rows of facets are visible upon the inner surface.

6.—Similar section at junction of cornea (*a*), with the calcareous portion (*b*). (*c*, *d*, *e*) The three layers into which the shell appears to be divided.

7.—Cornea seen by transmitted light. The shaded part *a* is a portion of the substance intermediate between the cornea and crystalline cones.

8.—A group of elements showing the relation of the pigment to the cones. The cornea is not present. (*a*) Substance intermediate between cornea and cone. (*b*) Crystalline cone. (*c*) Nerve-rod. (*x*) Pigment. (*d*) Festoons of pigment.

9, 10, 11.—Crystalline cones, with portions of the nerve-rods, after treat-

PLATE XVI.—*continued.*

ment with caustic potash (eye having been hardened in chromic acid), showing the tendency to break up into separate portions.

12.—Longitudinal and perpendicular transparent section of optic nerve and ganglion. (*d*) Spindle bodies; a portion of these are represented as broken away from *f*, the perforated structure forming the surface of the so-called optic ganglion (sclerotic of Leydig). (*g*) Nerve-fibres running together to form the bundles (*h*). *h* to *o* as in fig. 20. (*p*) First lenticular body. (*q*) Second ditto. (*r*) Kidney-shaped body seen at *d*, fig. 1.

13.—Kidney-shaped body enlarged.

PLATE XVII.

14.—Longitudinal and horizontal transparent section of optic nerve and ganglion with some of the crystaline rods and cones left attached. *a* to *h* as in fig. 2, the remainder as in fig. 12.

15.—A partly diagrammatic view of one of the elements of the eye from the cornea to the optic ganglion. (*a*) cornea. (*a'*) Substance between cornea and cone. (*s n.*) Semper's ? nuclei. (*b*) Lower end of crystalline cone. (*c*) Nerve-rod, around which is seen the investing membrane with its nuclei (*c'*). (*d*) Spindle-shaped or transversely striated body. (*d'*) Nuclei in the horns of the spindle body. (*f*) Perforated membrane at surface of optic ganglion.

16.—View of surface of a number of elements after treatment with potash. (*a*) Crystalline cone. (*b*) Investing membrane much swelled.

17.—Fenestrated structure found near the lower part of the investing membrane parallel with the surface of the optic ganglion. (*a*) nerve-rods passing down to the spindles.

18.—Spindle body enlarged to show disposition of pigment.

19.—Portion of the perforate membrane covering outer surface of optic ganglion. The nerve-rods and pigment occupy the apertures, which have a diameter of $\frac{1}{2000}$ in.

20.—Enlarged view of a portion of the optic ganglion. Letters correspond with those in figs. 1, 2, and 14. (*h*) Bundle of nerve-fibres. (*i*) Horizontal layer of nerves. (*k*) Bundles of nerves leaving interspaces in which are cells and fibres. (*l*) Horizontal nerve-fibres and elongated cells ?, below which are numerous horizontal blood-vessels. (*m*) Two layers of cells. (*n*) Layer of cells and blood-vessels. (*o*) Nerve-fibres with interspaces in which are spindle-shaped cells.

21.—Blood-vessels which, for the sake of distinctness, were not represented in the last figure. The fine parallel capillaries at *m* occupy naturally a position among the cells indicated by the same letter (*m*) in fig. 20.

22.—Enlarged view of the first lenticular body (*p*, fig. 14) showing the bands and blood-vessels.

MEMOIRS.

The Structure *of the* Eye *of the* Lobster. By Edwin T. Newton, Assistant Naturalist, H.M. Geological Survey. (With Plates XVI and XVII.)

The Eyes of Arthropoda having been investigated by so many competent histologists,—among whom may be mentioned Dr. Franz Leydig, Dr. Edouard Claparède, Professor Max Schultze, Dr. Gottsche, and Professor Joh. Müller—there seems little likelihood of anything remaining to be done in this direction; nevertheless, the literature of this subject, so far as I have been able to ascertain, contains but little concerning the eyes of the Lobster (*Homarus vulgaris*). Having some months ago made several preparations of Lobsters' eyes, Professor Huxley very kindly examined them, and perceiving that they showed points of structure which he believed to be new, advised me to work the matter out more fully. This I have endeavoured to do, and the results of my observations I purpose giving in the following communication. I am the more induced to bring the subject forward, because in this country very little has been published concerning the eyes of the Crustacea, and nothing, so far as I am aware, about the eyes of the Lobster specially. This dearth of English literature upon the subject renders it perhaps advisable to give a general account of the structure of the Lobster's eye before entering upon the more detailed description.

If the eye of the Lobster be examined with regard to its external form and structure, it will be seen to consist of a slightly flattened somewhat cylindrical body, the anterior portion of which is rounded and forms the cornea (fig. 1 *a*); and of a posterior flexible portion (*b*) by which it is attached to the bottom of the socket in which it lies. This posterior basal portion (fig. 1 *b*), which is partly calcareous, is much smaller than the distal. When the eye is retracted the basal segment is drawn by its muscles into the cavity of the anterior portion. When the eye is seen from above, the

cornea appears in the form of a deeply curved crescent, extending far round upon the sides of the eye, but only to a very slight extent over the upper surface. Viewed from below the crescentic form is much less marked, although it is still distinct. From this conformation it will be obvious that the range of vision will be very great in a lateral direction, the two eyes taking in a very large arc of a circle; whilst, when the animal is in its ordinary horizontal position, little or no light from above could penetrate the eye to a sufficient distance to influence the optic nerve, as in all probability it would be absorbed by the pigment which is found within the eye near the cornea (fig. 2 *b*).

A point of some interest which is noticed by M. V. Lemoine (24, vide list of authors) with regard to the Crawfish, I have found to be true also in respect to the Lobster; viz. that the eye itself is not retracted, however close an object may come to it, unless actual contact takes place, and then it is drawn in suddenly. If for example a feint be made to strike the eye with a stick the animal appears quite unconscious of the vicinity of the stick, but if the eye be only gently touched it is suddenly drawn into its socket. I have not had an opportunity of testing whether the same would be the case if the Lobster were in its native element; but it certainly appears as though the sense of touch were more acute than that of sight.

A longitudinal section of an eye (fig. 2), which has been hardened in chromic acid, shows the cornea (*a*), and beneath this and parallel with it a series of layers. Almost in contact with the cornea is a band of dark, almost black pigment (*b*); then succeeds a wider uncoloured portion (*c*) followed by (*d*) another black band; the next band (*e*) is of opaque white pigment. The third black band (*f*) is narrower than either of the foregoing; (*g*) is the second opaque white band, from which threads of white pigment extend for some distance upon the bundles of nerve-fibres (*h*), which form the next band. The bundles of fibres are continued into and form part of the so-called optic ganglion.

The optic nerve, which presents certain structural peculiarities, connects the foregoing apparatus with the supra-œsophageal nervous ganglion.

If a transparent section be examined, it will be seen that a radiating structure passes through all these bands or layers, and that in fact the eye is composed of a great number of similar structures placed side by side, and extending from the optic ganglion (fig. 2, *i*) to the cornea. Externally, the cornea is smooth, but divided into a great number of facets

(fig. 3), which in the Lobster are mostly square, each of them corresponding to one of the radiating structures just mentioned (see fig. 15). The structure of these elements of the compound eye appears to be fundamentally the same in all Arthropoda, but there is considerable variation in detail. Each division or segment of the eye consists of a refracting portion, termed the crystalline cone (*b*, figs. 8 and 15), and of a peculiar transversely striated body (*d*, figs. 2, 14, 15), connected anteriorly with the cone by a longer or shorter rod, and posteriorly with the nerve-fibres. Probably this striated body has the function of perceiving the light.

The intimate structure of each of the parts will now be considered.

Cornea.

The Cornea in the Lobster, as in some other forms of Crustacea, is smooth externally, and therefore, unlike the cornea of many insects, in which the facets are each raised up into a minute convex lens. The Lobster, in common with most Crustacea, has the facets square (figs. 3 and 4), but towards the periphery, however, they become somewhat irregular in form. The lines dividing the facets do not run horizontally or perpendicularly, but (as in fig. 7) diagonally across the front of the eye. When the outer surface of the cornea is examined by reflected light, with an enlargement of 60 diameters or more, each facet is seen to have a central depression, and from this fine lines run to each of the four angles, and form the cross-like depression (fig. 3) mentioned by Leydig (28). Claparède (6) has shown us that this cross is due to the manner in which each of the segments of the eye is developed. The lines of junction between the facets appeared not merely as a simple depression, but the bottom of the groove seemed to be raised into a ridge or seam, as indicated in fig. 3. This, however, was probably due to internal reflection.

In sections of the cornea cut perpendicularly to the surface (fig. 4), the divisions between the facets may be seen to pass quite through from the inner to the outer surface. This figure, which is taken from near the middle of the cornea, shows the flatness of each facet externally, and its slight internal convexity. Towards the edge of the cornea the facets become smaller, and the internal face is more convex (fig. 5).

The cornea, which is entirely horny in structure, becomes thicker where it joins the calcareous portion of the eye; the junction between the two parts is shown in the figure (6),

where (*a*) the cornea is seen to be of much less thickness than (*b*) the calcareous portion; this difference of thickness is even more marked in the upper part of the eye than it is in the figure, which is taken from the side. The calcareous portion shows the usual finely laminated structure, and generally appears divided into three layers (*c, d, e*, fig. 6), the middle one exhibiting the laminations and fine canals very distinctly, and the outer and inner layers having their marking much less distinct. In the cornea the middle layer appears to be wanting, or at least it is reduced to a mere film at the junction of the calcareous portion and the cornea.

Leydig says (28) that in the Crawfish the cornea is beautifully laminated, as it is also in some insects. In the Lobster I have only succeeded in tracing the lamination for a short distance from the calcareous portion into the cornea, and then it soon disappeared.

Claparède tells us that there are, in almost all arthropod eyes, four rounded cells or nuclei, which are found directly behind each facet of the cornea (*Semper's nuclei*). These, he says, are the nuclei of the cells, which give rise to the cornea, and stand in intimate relation to the crystalline cone, and that, therefore, they probably exist in the young condition of all arthropods, although they may be lost in the adult. I have, at present, been unable to convince myself of their presence in the adult Lobster, unless certain rounded bodies (fig. 15, *sn.*) which are to be found upon the sheath, near the front part of the crystalline cone, may answer to them. There are, however, four bodies which run together, and form a layer between the cornea and the cone, which will be considered further on; but the relation which these bear to the cornea seems to preclude their being *Semper's nuclei*.

Investing Membrane (*Umhullungsschlaüche*).

From the borders of each facet a membrane extends inwards as far as the transversely striated body, if not to the optic ganglion (figs. 8, 15, 16). Leydig says (29) that this membrane in the Crawfish extends to the optic ganglion. Each segment corresponding to a facet of the cornea, is thus surrounded by its own investing membrane or sheath. In some instances in the Lobster these sheaths before reaching the transversely striated bodies run together and form a network,[1] which appears as a fenestrated membrane when

[1] I am inclined to think that this fenestrated structure is absent in some specimens.

viewed from below (that is to say, when one looks at the surface opposite to and parallel with the surface of the optic ganglion). Figure 17, which represents this structure, was drawn with the camera from a specimen mounted in balsam; two of the nerve-rods (*a*) are represented passing through the perforations, which they can be seen to do in the preparation, as they pass from the crystalline cones on one side of this structure to the transversely striated bodies upon the other.

Upon the inner side of the investing membrane a number of large nuclei are visible, which stain readily with carmine (fig. 15, *c'*). If specimens have shrunk, as mostly happens when mounted in balsam, it is not easy to say whether the nuclei belong to the investing membrane or the nerve-rod.

The space within the investing membrane, which is not occupied by the crystalline cone or nerve-rod, is filled with a material which in hardened specimens appears granular, and with a greater or less number of (albuminous ?) globules, which appear to be more especially aggregated near the inner end of the cones.

Crystalline Cone.

Leydig says, when speaking of the structure of the eye of the Lobster (28, p. 410): "In the Lobster the cylinders are at their origin from the optic ganglion only 0·006‴ wide, but swell directly into the ribbed bodies, which measure at their widest part 0·0120‴. It appears to me, that they possess more than four corners, the continuation of the swelling loses itself anteriorly in a homogeneous granular mass, which reaches to the under part of the corneal facet, without a portion having separated as crystalline cone. In the sheathlike membrane, which extends from the facets to the optic ganglion, nuclei are here and there visible, as well as globules (*Gerinsel*) which seemed very similar to drops of albumen. The pigment appears here[1] to be collected only in a girdle around that substance which replaces the crystalline cone, and around the ribbed swelling, and extends from there as faint perpendicular stripes into the optic ganglion."

In some of my first preparations of the Lobster's eye I was unable to trace the crystalline cone, but I have specimens now before me, permanently mounted, which have been prepared more recently, and these show the crystalline cone perfectly well defined, both anteriorly and posteriorly (fig. 8). If

[1] That is, in the Lobster.—E. T. N.

a thin section of an eye which has been hardened in spirit or chromic acid be treated with dilute caustic potash, the pigment is dissolved, and all the parts swell very considerably, but the crystalline cones become clearly and sharply defined. By careful dissection with needles the elements may be more or less completely separated, and cones may be obtained exhibiting posteriorly certain rounded prominences and longitudinal grooves, showing that they have a tendency to break up into segments (see figs. 9, 10, 11); in these figures the splitting of the nerve-rod is also seen. Specimens treated for a short time with osmic acid, and then teased out, show the longitudinal divisions of the cones more distinctly, and the surfaces by which these divisions join each other may frequently be seen to be deeply and irregularly notched.

Between the cones and the cornea there is a space (about $\frac{1}{1700}$th of an inch) filled with granular material. In some instances when making preparations of the cornea this intermediate substance remained attached to it, and if removed, which could be done without difficulty, the divisions corresponding to each of the facets could be seen, and when treated with caustic potash or acetic acid each division showed a tendency to break up into four segments (fig. 7 *a*), but these segments did not correspond with those seen upon the surface of each of the corneal facets; that is to say, the cross-like divisions of each facet run from corner to corner of the square, whilst in the bodies in question they run from the middle of each side. One would be inclined at first to consider these four coalesced bodies to be *Semper's nuclei;* but Claparède's developmental researches seem to show that these nuclei stand in relation to the formation of the divisions of the corneal facets; this being the case, then these four bodies are not their homologues, because they do not correspond to the divisions of the corneal facets, which they should do if they were *Semper's nuclei.*

Is it possible that each of these bodies is a part of the pigment-cell which runs up each corner of the investing membrane, and which, remaining unpigmented at this part, has insinuated itself between the cornea and the cone?

Nerve-rod.

The granular fibres, called nerve-rods by Leydig, and by Max Schultze described as the hinder ends of the crystalline cones (fig. 15 *c*), which extend from the inner ends of the crystalline cones (*b*) to the transversely striated bodies, when treated with caustic potash become grooved longitudinally, and where they are broken across have an inclination to

split along the grooves; but if fresh specimens are treated for a short time with osmic acid before teasing out, these rods split very readily throughout their entire length into four fibres. The nerve-rods, which are as wide as the cones where they join these structures, become reduced to about $\frac{1}{4500}$th of an inch or less before they reach the transversely striated body.

The manner in which the nerve-rod joins the spindle[1] in some arthropods is described by Max Schultze (45 & 46); he says that the inner end of the crystalline cone (nerve-rod of Leydig) terminates in four produced corners which rest upon a rounded swelling of the nerve-rod (the striated swelling of nerve-rod of Leydig), but without being continuous with it. This conformation I have at present been unable to trace in the Lobster.

Transversely Striated Body.

This has been termed by Leydig the swelling of the nerve-rod, by Schultze the nerve-rod, and by others the spindle-shaped body. This structure in the Lobster (fig. 15, *d*) is continuous with the fine termination of the nerve-rod, which can be traced into the midst of the four nucleated horns (fig. 15, *d*); two only of the horns are represented in the figure. The horns themselves are not, strictly speaking, a portion of the swelling of the nerve-rod, but are pigment-cells, which run up the corners of these swellings. I am inclined to think that they correspond to the second swelling of the nerve-rod described by Leydig as existing in certain of the Crustacea, for example, in *Herbstia condyliata* (vide 28, pl. 17, fig. 32); this appears the more probable, as in each there is a cell or large nucleus.

It is a matter of no great difficulty to see the transverse striations upon the spindles (the Plättchenstructur), and the arrangement of the pigment granules (figs. 15 and 18), but to see the fine striations which some of these plates exhibit is by no means so easy a matter. The transversely striated body is not in the Lobster so wide in proportion to its length as that figured by Leydig from the Crawfish. In the Lobster these structures measure about $\frac{1}{150}$th of an inch in length, from the surface of the optic ganglion to the base of the horns, and their greatest diameter is about $\frac{1}{1100}$th of an inch. The transverse striations are finer than those of the Crawfish, being only about $\frac{1}{8500}$th of an inch in width. The inner

[1] The *nerve-rod* of Leydig is termed by Schultze the *hinder part of the crystalline cone;* and the *transversely striated swelling* of the *nerve-rod* of Leydig is termed by Schultze the *nerve-rod*.

end of the spindle, where it passes into the so-called optic ganglion, has a diameter of about $\frac{1}{2000}$th of an inch.

In order to join the optic ganglion, each spindle passes through the membrane which covers the surface of that structure, and the pigment-cells which run down the corners of the spindle pass through with it. In preparations showing the surface of the perforated membrane (fig. 19), where the spindles have been broken off, I believe I have been able to trace the central bright portion (the nerve-rod) and the four pigmented corners, as seen at (*a*). It would, perhaps, be more correct to say that a bundle of nerve-fibres from the optic ganglion passes through the membrane to join, or become distributed upon, the spindle. The apertures of this membrane are about $\frac{1}{2000}$th of an inch in diameter.

Leydig (32) speaks of the perforated membrane which covers the surface of the optic ganglion in certain forms, as the sclerotic, and (31) describes it as the papillæform rising of the bottom of the sclerotic.

Gottsche mentions the papillæ, and says they are to be well seen in the Lobster.

After reading these statements, I have of course endeavoured to trace these papillæ, but have not succeeded in seeing them. In almost all my preparations of this structure portions of the lower ends of the spindles remain in the apertures, and project slightly above the surface, giving it a somewhat papillæform appearance; but I can hardly think that they are the structures seen by Leydig and Gottsche, for the ends of these do not appear rounded, but always broken irregularly. In Leydig's figure these papillæ appear to be between the apertures (31, taf. x, fig. 1).

Pigment.

The first pigment band is black or dark brown (fig. 2, *d*, fig. 8, x); it surrounds the investing membrane of each segment in the region occupied by the cone (fig. 15, *x*). When a number of segments are seen together, the pigment has the appearance of a black band, but when they are more separated the pigment is then seen to be in somewhat irregular patches upon the sides, and in dotted threads upon the corners of each segment, that upon the sides being lighter in colour. The pigment upon the corners extends both forwards to the cornea, or nearly so, and backwards for a considerable distance, sometimes meeting the threads coming from the horns of the spindles. Fine, irregularly dotted pigment threads pass in festoons across the sides from one corner to

another (fig. 8, *d*), the spaces between these festoons being more or less filled up with brown pigment, the amount of which varies in different parts of the same eye. The distance to which the pigment threads extend down the corners varies very considerably.

The second dark band is found upon examination to be formed by the pigment which occurs upon the corners of the spindle-shaped bodies. The pigment is granular, and is sometimes arranged as in figure 18, where each corner is divided in a very regular manner into alternate pigmented and unpigmented portions; but this is not always the case, for very frequently the pigment is simply in longitudinal bands. Anteriorly, the pigment bands become separated from the nerve-rod, and appear like horns, four to each spindle (two of these are shown in fig. 15), each horn having a distinct nucleus within it; beyond the nuclei the bands become reduced to fine granular threads, which run up the corners of the investing membrane, and sometimes join those coming from the region of the cone.

Passing towards the optic ganglion, the bands of dark pigment become thinner, and the spaces between them are occupied by an opaque white substance, which forms the first white pigment band. The dark pigment, however, again increases before reaching the surface of the optic ganglion, and passing with the nerve-rod through the perforated membrane, is continued for some little distance upon the nerve-fibres. The perforated membrane is thus seen to occupy the middle of this third band of dark pigment (fig. 14, *f*).

The second white band is of similar pigment to that which forms the first white band; it occurs upon the bundles of nerve-fibres (at *g*, fig. 14) for a short distance, as these pass inwards from the last black band.

The white pigment loses its brilliancy after being kept for a time in spirit or chromic acid. The dark pigment mostly occurs in the form of granules, but that which is found upon the sides of the investing membrane, in the region of the crystalline cones, is of a much more homogeneous character.

Optic Ganglion.

The termination of the optic nerve, which is generally known as the Optic Ganglion, consists of several portions, and these present, at least in the Lobster, greater complications than have hitherto been described.

Leydig, in 1851 (27), in his description of Branchipus and Artemia, speaks of the optic nerve as having two ganglionic

enlargements upon it, from the anterior of which eight bundles of nerves pass to the pigment layer. (This latter corresponds to the region of spindles in the Lobster, &c.)

Gottsche, in 1852 (18), describes as existing in the Lobster and in Lupea a certain nervous plexus and layer of granules, which form the face of the optic-bulb, and, as he thinks, represent the retina.

Again, Leydig, in 1855 (28), says it appears to him that the optic ganglion in many beetles is divided into an anterior and a posterior portion, which are connected by a nervous plexus; and, further, in 1857 he says in his 'Histologie' (29, p. 252) that the optic ganglion may be looked upon as the retina, minus the rods and cones, and that in Arthropoda it consists of "greater and smaller cells, nuclei, granules, and the fibrillated substance of the optic nerve; we recognise, also, a certain lamination and interlacing of these elements, in the Crawfish particularly, a certain radial unfolding, or radiation of the fibrillated substance of the optic nerve, but the softness and, therefore, slight individualisation of the parts allows, for the present, scarcely anything to be made out concerning the more intimate structure."

Among the beautiful drawings which this same author has given us in the plates accompanying his comparative anatomy (31) we find figures of the eyes of several insects, and the optic nerve of *Dytiscus marginalis* (tab. ix, fig. 1), with its ganglia, as there represented, somewhat resembles that of the Lobster, although the details do not show the same complexity.

The above references will serve to show what has hitherto been known, so far as I have been able to ascertain, concerning the parts now to be considered.

The surface of the so-called optic ganglion (*f*, figs. 2, 14, and 15) is covered by the perforated membrane which has already been mentioned, and through this, as it appears to me, the nerve-rods pass. Immediately below the membrane I think each rod ends in a rounded cell, but of this I am by no means certain; from this point the nerve-fibres pass off, and soon collect into bundles. There is considerable difficulty in deciding whether *one* or *more* of the nerve-fibres are connected with each *spindle*. The bundles of nerve-fibres are accompanied to a greater or less extent by the granulated stripes of dark and opaque-white pigment. The general direction of the nerve-fibres is radial and parallel to each other, but it is possible that they join to form a plexus near the perforated membrane; the appearance presented at this part may, however, be only due to the connective tissue.

Among these interlacing fibres cells occur in greater or less abundance, sometimes being only very few in number. Occasionally large vessels may be traced running among the fibres in this region.

Passing inwards, the bundles of nerve-fibres become more definitely separated from each other, until they arrive at the point (*i*, figs. 14 & 20), where they again divide. Figure 20 gives an enlarged view of the parts forming the layers between *i* and *o* in figure 14. At *h* the lower part of one of the bundles of nerves is seen, which at *i* divides; some of the fibres forming a horizontal layer, and others passing directly onwards.

The next layer (*k*) in most sections appears to be composed of irregularly placed cells and nerve-fibres, but when very thin sections—which have been carefully prevented from drying whilst being mounted—are examined, it is seen that the fibres are chiefly in bundles, leaving irregular interspaces, in which small cells are collected. Some of these cells appear to be connected with fibres which pass into the next horizontal layer.

This layer (*l*) consists of two portions, the upper consisting of horizontal fibres, derived apparently in part from the bundles and in part from the cells of the previous layer; upon these fibres elongated enlargements could be seen, but not well defined. The lower portion consists of large blood-vessels, which appear, according to the direction in which they have happened to be cut, as rounded or elongated patches: there may be also cells among these vessels.

The next layer, or rather two layers (*m*), consists of two rows of cells, from each of which fibres pass off in every direction, but chiefly from the ends and sides. Some, if not all, of the fibres from the bundles in the layer (*k*) are connected with these cells.

At *n* there is another layer of vessels, possibly accompanied by cells. The nerve-fibres passing inwards from this point leave irregular oval spaces, in which are groups of spindle-shaped cells (*o*); from the opposite poles of these cells fibres are given off, which may be traced for some distance in a horizontal direction.

All the layers from *o* to *i* are somewhat obscured by the presence of a number of vessels which are large before they reach *o*, but rapidly divide into much smaller branches, and these pass in a very regularly parallel manner through the layer of cells at *m*. Having passed through this layer, they appear to run together again, for at *l* large vessels are again found, but now running in a horizontal direction. Large

vessels occur also in the layers *k* and *h*, but do not seem to extend beyond the perforated membrane (fig. 14, *f*).

Lenticular Bodies of Optic Nerve.

If a longitudinal and horizontal section of the optic ganglion and nerve be examined (fig. 14) the fibres of the latter, it will be observed, are interrupted by two peculiar bodies of an elongated oval form. These bodies are not placed upon one side of the nerve, but are among its fibres; neither do their long axes correspond to the direction of the nerve-fibres, but they are set obliquely as indicated in the figure, the long axis of the one nearest the optic bulb being directed backwards and inwards, whilst that of the second is directed backwards and outwards. These curious bodies exhibit several longitudinal bands, which converge as they approach the ends; when stained with carmine these bands appear alternately lighter and darker, and one of the dark ones is less distinctly subdivided into two or three narrower bands.

These problematical bodies are very richly supplied with blood-vessels; several large ones (one of which is shown at *v*, figs. 14 and 22) give off numerous side branches which penetrate the optic nerve, and when they reach the surface of the body itself they divide again into much finer branches which enter it as indicated in the figure, and pass across the longitudinal bands. Some of these vessels appear to pass quite through, and to extend to the cellular layer which exists upon the inner side of the optic nerve (*x*, fig. 14). The large vessel *v* passes round the anterior end of the body, and gives off branches which enter it on this side.

The layer of cells just mentioned (fig. 14) forms, I believe, part of the nervous apparatus, for these cells closely resemble those found in the supra-œsophageal ganglion, where the nerve-fibres can be distinctly traced to them.

The direction of the fibres of the optic bulb and nerve is peculiar. As shown in the section (fig. 14), the fibres from the outer side pass over the end of the first of the above-mentioned bodies, and appear to enter the lower part of its anterior face, whilst those from the inner side pass over to the anterior part of this body. The fibres appear to leave the body by its posterior and outer face, thence they pass to the anterior and outer face of the second body.

If it should be, as indeed from appearances seems most probable, that the fibres of the optic nerve pass through these bodies, it will be necessary to consider them to be

nervous structures and not glands, which one might at first take them to be. A comparison of horizontal and perpendicular sections of the optic nerve and ganglion (figs. 14 and 12) shows that the latter, corresponding to the compressed form of the eye and to the width of the cornea, is wider from side to side than it is from above downwards; and the former is wider from above downwards than it is from side to side.

The longitudinal and perpendicular section of the optic nerve (fig. 12) shows some resemblance to Leydig's figure of the same part in *Dytiscus marginalis* (31, tab. ix, fig. 1), inasmuch as there are two areas (*p* and *q*) in which the structure appears very different from the adjoining parts. By comparing this section with the horizontal one (fig. 14) it will be obvious that these areas occupy about the same position as the bodies above described, and they are in fact the same structures cut in an opposite direction; and the appearance which they present is, as might be expected, very different in the two sections. The vessels shown in the horizonal section, when cut across, as they are in the perpendicular section, appear as small circles, which, if examined from this point of view only, might easily lead one to consider them to be aggregations of cells.

In the perpendicular section these bodies show no definite outline; this, however, appears to be due to the oblique direction in which they are cut.

The appearance presented by the fibres of the optic nerve when cut in this perpendicular direction is peculiar, and will be best understood by reference to the figure (fig. 12), where it is shown that between the two bodies which have just been considered (*p* and *q*), and from these to the layer (*o*), the fibres form a series of V patterns, an appearance produced by the crossing of the fibres. The darker portions among these fibres in this figure are the blood-vessels which supply the lenticular bodies.

The second lenticular body appears to have the same structure as the first, but is somewhat smaller.

Kidney-shaped Ganglion.

Close behind the second lenticular body there is an enlargement which differs from the two lenticular bodies, both in the manner of its connection with the optic nerve and also in its intimate structure. This body (fig. 12 *r*) is, in this perpendicular section, kidney-shaped, and is attached to the upper surface of the optic nerve by a pedicle of nerve-

fibres which in the section form two chief bundles; these spread out, leaving irregular interspaces, and finally lose themselves in the finely granular peripheral portion. The nerve-fibres both of the body and of the portion of the optic nerve lying below it, interlace in a very complex manner. This kidney-shaped body is supplied by numerous branching blood-vessels, some of which pass into it from the pedicle, and others dip down into it from the surface.

The similarity in structure between this organ and certain known ganglia has induced me to call this a ganglion.

After leaving this pedunculated body the optic nerve becomes very soon reduced to about one fourth of its previous dimensions, and passing out of the eye-stalk joins the supra-œsophageal ganglion.

Comparison of the Compound Arthropod Eye with the Eye of a Vertebrate.

The agreement in the fundamental plan of structure between the vertebrate and arthropod eye has long been recognised by Leydig, and in following out the comparison of the different parts he gives it as his decided opinion that the *crystalline cones*, with the *nerve-rods*, and their transversely striated swellings, found in the arthropod eye, correspond to the *rods* and *cones* of the vertebrate eye. He says, further, that he believes all these parts, notwithstanding their difference of structure, to be modifications of the terminal portion of the nerve-fibre.

Schultze holds the same view as regards the correspondence of the *nerve-rods* and *crystalline cones* to the *rods* and *cones*. A further resemblance between these parts, as pointed out by him, is seen in the laminated structure (Plättchenstructur), which the *outer member* of the *rods* and *cones* of the vertebrate retina is said to exhibit, and which corresponds to the transversely striated structure of the *swelling of the nerve-rod*. A certain power of refraction is also common to both the *crystalline cones* of the arthropod and the *cones* of the vertebrate retina.

The so-called optic ganglion of the Arthropoda has been considered by most investigators to be the equivalent (at least in part) of the retina of the vertebrate, and the arrangement of the nerve-fibres and cells in this part, which has been described above, tends only to confirm their opinion. The general arrangement of the cells and interlacing fibres shows a very considerable resemblance to the layered structure of

the vertebrate retina, and there can be but little doubt that their functions are similar.

Claparède does not think that we are justified in considering the parts of the vertebrate and arthropod eyes to be homologous; but whatever diversity of opinion there may be on this point, there can certainly be only one as to the great similarity in the structures, and this being the case, we seem to be brought to this conclusion: that the surface produced by the combined ends of the rods and cones in the vertebrate eye—that is, the surface upon which an image is projected—corresponds to the surface formed by the combined crystalline cones in the arthropod eye; but then we have this difficulty, in the former there is a cornea and lens by which the light is refracted and the image formed within the eye, whilst in the latter there is no lens and cornea capable of producing such an image. Experiments have shown that an image of any object in front of the compound eye, is produced behind each facet, and, therefore, it is said that each segment of the eye acts as a separate eye, and perceives the image; but from the structures as we now know them, it is difficult to understand how each segment could give more than a general impression of light or darkness; and it seems very improbable that such an elaborate apparatus, constructed so much like that of the vertebrate, should be incapable of distinct vision. It appears to me that the refraction of the light by the crystalline cones is only comparable to that which takes place in the vertebrate rods and cones, and which we know by experience does not interfere with distinct vision. We are, however, still left with the difficulty before mentioned. How can an image of an object in front of a compound eye be projected upon the surface, formed by the external ends of the crystalline cones? This may, perhaps, be brought about in some such way as Joh. Müller supposed in his Mosaic theory, but there are certain objections to this idea. Notwithstanding all that has been written up to the present time concerning the mode of action of the compound arthropod eye, we are still unable satisfactorily to solve this difficult physiological problem.

Mode of Preparation.

The plan which I have found most successful in making preparations of the lobster's eye has been to take an eye from an animal just killed, and to make an incision in the cornea at a point which would not interfere with the section intended to be cut, and then to place it in $\frac{1}{4}$ to $\frac{1}{2}$ per cent. chromic

acid solution, or to put it in spirits of wine for some hours and then into the chromic acid. In the course of three or four days the shell was sufficiently soft to be able to be cut with a razor. The interior was then examined to see if it was sufficiently hardened; when this was the case, the eye was imbedded in wax, and sections cut in the ordinary way. Some of the sections thus prepared were stained with carmine. The more perfect specimens were mounted in Canada balsam (by the cold method), and the rest preserved in spirit for more minute investigation. For this latter purpose portions of the sections were teased out with needles upon a glass slide, and treated with dilute acetic acid or caustic potash. When potash was used the parts swelled to a much greater extent than they did with acetic acid, but upon the whole the results were more satisfactory. The pigment, which was only slightly affected by the acetic acid, was dissolved by the potash. The effect of these reagents depended very much upon the extent to which the specimen had been hardened.

Hyperosmic acid was of great service in working out many of the structures; the method of using it was the following: a perfectly fresh eye was cut in half longitudinally, and placed in a one per cent. solution for one or two hours; small portions were then cut off with a razor and teased out in water.

The structure of the optic ganglion and nerve was best studied in sections; but it was only in those specimens which were extremely thin that the parts figured (fig. 20) could be made out.

Measurements.

The following measurements are given in fractions of an inch, and must be taken as the average size of each of the parts, as these vary, more especially as regards their length, in the different parts of the eye.

	Inch.	
Corneal facets near centre of eye	$\frac{1}{425}$	in width.
„ towards the sides	$\frac{1}{650}$	„
Substance intermediate between the cornea and cones	$\frac{1}{2000}$	in thickness.
Crystalline cones	$\frac{1}{100}$	in length.
„	$\frac{1}{700}$	in diameter.
Nerve-rods near junction with cones	$\frac{1}{1200}$	„
„ „ „ spindles	$\frac{1}{4500}$	„
Spindle-shaped bodies	$\frac{1}{150}$	in length.
„ „ at thickest part	$\frac{1}{1100}$	in diameter.
„ transverse striations (fig. 18)	$\frac{1}{8000}$	in thickness.

Lenticular body (first)	$\frac{1}{18}$	in length.
„	$\frac{1}{80}$	in thickness.
Kidney-shaped ganglion	$\frac{1}{25}$	in length.
Optic nerve where it leaves the eye . .	$\frac{1}{80}$	in diameter.
„ widest part within the eye . .	$\frac{1}{20}$	„
Oval nuclei of investing membrane . .	$\frac{1}{1700}$ to $\frac{1}{2800}$	longest diam.
Semper's ? nuclei	$\frac{1}{2100}$	in diameter.
Nuclei in the horns of spindles . . .	$\frac{1}{2100}$	longest diam.
The perforations of the membrane covering the surface of optic ganglion	$\frac{1}{2000}$	in diameter.
Apertures in fenestrated membrane (fig. 17) .	$\frac{1}{1000}$	in width.
Cells of optic ganglion, fig. 20, at *k* . .	$\frac{1}{4200}$ to $\frac{1}{2800}$	in diameter.
„ „ „ at *m* . .	$\frac{1}{4200}$ to $\frac{1}{2000}$	„
„ „ „ at *o* . .	$\frac{1}{1700}$	in length.

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Methods *of* investigating *the* Central Nervous System *in* Man. By Professor W. Betz, of Kieff.[1]

1. *Methods of Hardening.*

To give the preparations a certain degree of firmness I place them first in a solution of iodine in alcohol, and afterwards in aqueous solution of bichromate of potassium. The different parts of the nerve-centres, viz. the spinal cord, the medulla oblongata, the pons Varolii, the cerebellum, and the cerebrum, require various times and various degrees of concentration of both these solutions, as well as some mechanical preparation.

The spinal cord with the medulla oblongata and the pons are treated as follows:—These parts freed from dura mater are suspended in a high cylinder containing alcohol of 70 or 80 per cent. with enough iodine to give it a light brown colour. After one to three days the preparation becomes superficially harder; it is then taken out and the pia mater and arachnoid are removed. If any difficulty arises in removing the pia mater from any particular part, it is left for some days longer in the fluid and then prepared. The membrane is removed with most difficulty from the thoracic portion of the cord. After removing the membranes, the preparation is

[1] Translated from Max Schultze's 'Archiv für Micr. Anat.,' vol. ix, p. 101.

returned to the same fluid, which has in the mean time become colourless owing to the absorption of iodine by the organs. For this reason a concentrated solution of iodine is from time to time dropped into the fluid till no further absorption takes place. Meanwhile the preparation has become uniformly soaked with iodine, as is shown by the uniform yellow colour of transverse sections. This complete soaking in iodine is only necessary for preparations which are not quite fresh, since they do not otherwise become sufficiently hard on treatment with bichromate of potassium; for quite fresh specimens a less complete infiltration suffices. If the membranes are carefully removed the complete infiltration will be accomplished in six days at the furthest, and generally sooner. The removal of the membrane has a great influence not only on this process but also on the uniformity of the hardening in bichromate of potassium.

After this preliminary hardening the preparation is placed in a three per cent. solution of bichromate. Being partially dehydrated by the alcohol, the preparation is lighter than the solution and swims on the top. To correct this, a small piece of lead may be fastened by threads to two or three of the lower nerve roots, and the whole thus sunk beneath the fluid. After a day or two the whole will sink to the bottom and the lower end become bent and thus unsuitable for section. This may, however, be remedied by removing the preparation and sinking the other end, either by a weight attached to the roots of some of the cervical nerves (which have been left for the purpose), or else by a loop tied round the pons Varolii, and also connected with the weight. The roots of the cervical and lumbar nerves may be conveniently left attached to the cord, since they serve to determine the precise situation of a section, but this is not the case with the thoracic nerves, which must be carefully removed, as otherwise the corresponding portions of the cord cannot be brought to a right consistence, but either remain for a long time soft or else become brittle and crumbling at the points where the nerves enter. The time necessary for hardening is different for different parts of the cord, and depends also upon the freshness of the preparation and the temperature of the solutions, not upon the thickness of the preparation. Hardening occurs soonest in the cervical portion, especially in the cervical enlargement; the thoracic portion, on the other hand, requires the longest time. The conus medullaris, especially its thinnest part, hardens very slowly, the pons Varolii and medulla oblongata comparatively rapidly and very uniformly. These parts give the best and thinnest sections.

When the preparations have been first placed in iodine and alcohol, it is best to let them remain in a cool place (except in cold winter weather) and not to keep them at the ordinary temperature till after two or three days. While the preparations are in the bichromate solution the temperature must not be so high as it commonly is in summer in buildings used for anatomical teaching, since they become covered at a high temperature with a brown coating which hinders proper imbibition. The appearance of turbidity in the fluid and a brown precipitate on the objects if occurring after some time, show that the hardening process is complete, and the preparations should then be washed, and placed in a half per cent. or one per cent. solution of bichromate, otherwise they become too hard and brittle. In a solution of this strength they can be preserved any length of time without detriment.

The cerebellum only gives good preparations when it is taken from a perfectly fresh body; before placing it in iodine and alcohol it must be carefully freed from vessels and membranes. If it be difficult to remove the pia mater, the preparation must be placed first in a weaker solution than that finally made use of; and when the membranes are removed, it is placed on a layer of cotton wool in a strong iodine solution, iodine being also added more frequently as it is rapidly absorbed. After two or three days the preparation is taken out, and the removal of the pia mater completed. This is repeated several times in order to remove the membranes entirely from the convolutions, and the degree of firmness estimated. When the organ is hard enough to be supported rigidly without bending when poised centrally by the vermiform process on the finger, which may be after a week or two, it is placed for twenty-four hours longer in spirit, and then in a five per cent. solution of bichromate of potatsium, where it remains till sufficiently hardened.

The cerebral hemispheres are treated somewhat differently. The brain is divided longitudinally through the corpus callosum into two halves, and placed in weak spirit containing enough iodine to give it a light brown colour. After some hours the pia mater is removed from the Sylvian fissure, as well as, if possible, the choroid plexus. The preparation is then placed in a cool place, and more iodine added. After one or two days it is taken out and the whole of the pia mater, if it comes off easily, may be removed. If otherwise, it is taken off at once along the larger furrows, and the remainder subsequently. A knife and forceps may be used to separate the pia mater from the convolutions; but if it is difficult in

this way to avoid injuring the grey matter of the brain, a small pair of curved scissors may be employed to divide the attachments of the membrane as deep in the convolutions as possible.

When the pia mater is removed the preparation is returned to the fluid (to which half of its bulk of fresh solution is added), and remains there one to three days, till it has attained such a degree of hardness that the sides of the fissures are equally hard with the external aspect of the convolutions. Small pads of cotton wool are advantageously introduced into some parts, as into the Sylvian fissure, the region of the descending cornu, &c.

After all these manipulations the preparation is placed in stronger (70 per cent.) spirit containing iodine, and remains therein till it is hard enough to be supported without bending on two fingers. It is then placed in a 4 per cent. solution of bichromate, in which it remains till it is adequately hardened. If a brown precipitate forms on the mass before it is properly fit for section, it must be washed and placed in a fresh bichromate solution; but in general this appearance may be taken as evidence that the brain is sufficiently hardened.

The preparation is fit for making fine sections when a section through the whole of the hemisphere shows a nearly uniform yellowish-brown colour. In the spinal cord and medulla oblongata the grey substance will, however, be paler than the white.

By this process I have succeeded in hardening complete cerebral hemispheres, even when removed from the body three or four days after death; but in such cases longer time and more concentrated spirit are required. A perfectly hardened brain permits very large thin sections to be made. I have preparations of the whole pons Varolii with the corpora quadrigemina, which are thin enough to be examined with immersion lenses, and I have lately obtained complete transverse sections through the whole of the hemisphere. Very thin sections are obtained from the cord and medulla oblongata, *e. g.* of one twelfth or one twentieth of a millimètre in thickness.

2. *Cutting Sections.*

When the hardening is complete the preparation is placed in water, in order to remove the potassium bichromate, and, according to its size, this process may occupy one or several days. It is impossible to remove it entirely from large masses, which should, however, be washed until the water shows, in two or three hours, only a trace of yellow colour.

The construction of the knife used is a matter of great importance. The chief evils to be guarded against are *friction* in drawing the knife along and *adherence* of the knife to the mass from which the section is made, or of the section itself, to the upper surface of the blade. Want of consideration of these points has led to the idea being entertained that the preparation of large and thin sections is wholly a matter of individual skill and dexterity.

By my plan any person will be able, with a little practice, to cut large sections, and always in the same plane. This end is attained by using a razor, the blade of which is made *convex* on its *upper* surface, and *concave* on the *lower*. The curvature of the under surface is also more abrupt than that of the upper. The blade must be one and a half times or twice as long as it is broad, but not more than one third as thick as broad, and must be firmly fastened to the handle. For preparing very large sections large knives are used, made on the same principle, but not proportionally thick, the thickness not being greater than in the smaller blades; for instance, to make sections of a complete hemisphere, blades about eight inches long and four broad. With knives of this shape it is possible to keep a layer of fluid both above the place of section and under the section as it is made, by which all friction is avoided. In order to keep an excess of fluid on the surface, which is especially necessary in making large sections, water must be kept continually dropping. This object is attained by using a chemical wash-bottle, the mouthpiece of which is connected with an india-rubber tube, by which air can be gradually blown in while the section is making. For very large sections a wash-bottle with three jets may be used.

In order to know the arrangements of the nervous elements, series of successive sections must be studied without any interval. Such a series cannot be obtained by mere free-hand cutting, since the surface of a section always becomes, after a time, concave, and a screw section machine must be used.[1]

A mixture of wax and oil is used to imbed the preparation in the section machine, and alcohol used to wet the knife.

3. *Tinting the Preparations.*

Every section, as it is cut, is placed directly from the knife in a vessel of pure water; but, in order not to mistake them,

[1] We omit the description of the section-machine, since it resembles Stirling's section-cutter, commonly used in this country, except in being less complete.—Ed.

it is best to put each into a separate glass; in order to avoid mistaking the upper and under surfaces, I mark the original preparation with a knife before it is cut, by which means the right hand may be known from the left.

The preparations are left in water for from twenty-four to seventy-two hours according to their degree of hardness. The water must be frequently changed, especially in summer, and a small fragment of camphor placed in each glass (as proposed by Max Schultze) to prevent the growth of infusoria. From water they are placed in a carmine solution. Various colouring matters, which have been of late years recommended for tinting microscopical preparations—such as indigo, aniline colours, and various vegetable dye-stuffs—are not suitable for preparations of the nervous centres. Some of them are imperfectly absorbed, and others are washed out by the process of depriving the preparations of water. Carmine, or, as some call it, the carminate of ammonia, introduced by Gerlach, still remains the best, and perhaps the only colouring material for these preparations. I think I can confirm Deiters's statement, that if it is necessary to colour nerve preparations at all, the use of carmine for this purpose leaves nothing to be desired. That carmine acts differently upon different elements of the preparation is shown by the circumstance that some nerve-cells are coloured (as Mauthner showed) differently from others. Deiters, who will not admit this, still allows that some groups of nerve-cells absorb more carmine than others. This circumstance I can confirm, and will add that some groups of nerve-cells will only absorb a certain quantity of carmine and undergo no further change, although colouring solutions of various degrees of concentration may be used and allowed to act for any length of time upon the preparation.

The carmine solution is prepared in the following manner:—Commercial carmine is rubbed in a mortar with a small quantity of water till it forms a thick syrupy mass; on this is poured, with continual agitation, solution of ammonia. The solution thus obtained is diluted with a large quantity of water and filtered in order to separate the substances, such as powdered glass, which are mixed with the best carmine.

This filtered solution is exposed in an uncorked bottle (of *green* glass) to the sun till a flocculent precipitate appears, and then filtered though a fresh filter-paper. The filtrate is again left to stand in the same conditions, and again filtered from the precipitate which forms. Generally speaking no third precipitate appears. Should this, how-

ever, be the case, the liquid must be again filtered, and afterwards preserved in a closed vessel. The solution thus prepared may be kept for months, or even for a whole year, without further change. It tints all microscopical preparations very rapidly, but especially those of the nerve-centres. Half an hour, or an hour at most, is enough to impart the most perfect and intense colouring to preparations of any size. If the sections are very thin and well soaked in water the most perfect colour appears in ten or fifteen minutes, and after this the colouring matter is absorbed very slowly.

In preparing the carmine solution, especially during summer, it sometimes happens that it becomes covered with a white granular crust, and has an offensive smell. This does not, however, at all interfere with the preparation of the solution, but, on the contrary, accelerates it. After once filtering the odour need no longer be regarded, as it goes away of itself. The properties and reactions of the precipitate lead to the following conclusions :—(1) The ammoniacal solution of commercial carmine contains two distinct albuminoid substances, which, under the influence of light and heat, are partly decomposed, partly precipitated, from the solution. (2) The colouring properties of carmine, especially with respect to animal tissues, depend on the presence or absence of small traces of free ammonia. (3) The formation of minute granules in a filtered carmine solution, which, as has often been remarked, always occurs suddenly, spoiling the clearness of the preparation, is explained by a separation of the albuminoid bodies from the solution, and not of hydrate of alumina, as has been supposed by some.

This solution colours, in the first place, the grey matter of the nerve-centres, and especially its granular matrix, then the different groups of nerve-cells, the epithelium, and then the other constituents. With a more dilute solution some parts can be coloured, others left uncoloured; and in this way beautiful and instructive preparations showing the grouping of the grey matter are obtained.

4. *Putting up the Preparations.*

The tinted preparations, after remaining a sufficiently long time in water, are transferred to alcohol of gradually increasing strength. For this purpose I use a series of glasses, ten in number, containing ordinary spirit. Each preparation is first placed in the first glass, then in the second, third, and so forth; so that the first preparation is in the tenth glass, while the tenth is in the first.

After passing through ten glasses of spirit the preparation

is placed in absolute alcohol, where it can be completely dehydrated and afterwards rendered transparent. This mode of treatment has the following advantages:—that the complete dehydration of the preparation occurs in a definite time and sooner than by the ordinary method; that the contraction is more uniform and the preparations do not become so brittle. The latter fault also occurs when the sections are kept too long in absolute alcohol.

For clarifying the preparations, turpentine, somewhat resinous but not too thick, is the best medium, and is preferable even to the oil of cloves mostly used for this purpose. Preparations placed in it are decidedly softer and more flexible than those treated with other clarifying substances.

For the preservation of the sections, the most suitable material is a solution of Gum Dammar in turpentine, known in commerce as dammar varnish. Preparations put up in this solution have all the properties of those which are clarified with turpentine. The outside of the varnish at the edges of the cover glass also dries more rapidly than Canada balsam or mastic varnish. To fix the cover glass its edges may be covered with a thick layer of alcoholic solution of shellac coloured with aniline blue, which hardens very rapidly.

5. *The Polarizing Apparatus.*

The examination of tissues in polarized light is involved in many difficulties, and has, so far as I know, never been applied to the anatomical analysis of the nerve-centres. Since the nerves of the brain appear in polarized light coloured with the different colours of the colour spectrum, and may thus be distinguished from nerves not belonging to the brain and from other tissues of the nerve-centres, I have made use of polarized light to trace their course and arrangement. For this purpose it is best to use a thin Selenite plate, which with crossed Nicol's prisms gives a carmine red field, but with parallel prisms a green one. The Selenite disk is fastened by means of slips of paper to a pasteboard cover, which is laid on the stage of the microscope and has an opening corresponding to the opening in the stage. The cover may thus be taken off and replaced without altering the previous position of the selenite disk.

The polarizing apparatus is especially useful in tracing the fibres of the cerebral hemispheres which cannot be coloured with carmine, and which are, therefore, difficult to recognise in large sections under the microscope.

New Contributions *to the* Theory of Fermentations.

By L. Pasteur.[1]

For a long time past I have been led to regard fermentations, properly so called, as chemical phenomena correlative to physiological actions of a particular kind. Not only have I shown that ferments are not dead albuminoid matter, but living organisms; I have further produced the fermentation of sugar, of lactic acid, of tartaric acid, of glycerine, and generally of all fermentible substances in media exclusively mineral, an incontestable proof that the decomposition of fermentible matter is correlative to the life of the ferment, and that it is an essential part of its nutrition. For example, under the conditions to which I refer it is impossible that a single atom of the carbon entering into the composition of the ferments which originated could have been derived except from the fermentible matter.

That which separates the chemical phenomena of fermentations from a crowd of other, and particularly from ordinary vital phenomena, is the fact of the decomposition of a weight of fermentible matter far superior to that of the ferment taking part in it. I have long suspected that this particular character must be connected with the carrying on of nutrition apart from contact with free oxygen.

Ferments are living things of an exceptional nature in so far as they possess the property of accomplishing all the acts of their life, including that of multiplication, without the necessity of making use of atmospheric oxygen. We may call to mind the remarkable infusoria which produce butyric or tartaric fermentations or certain putrefactions, and which not only live and multiply without contact with oxygen, but which perish and cease to bring about fermentation if we dissolve that gas in the medium from which they derive their nutriment. But this is not all. I have shown by precise experiments made with yeast that if the life of this ferment is partially carried on under the influence of free oxygen, this small cellular plant loses in proportion to the intensity of this influence a part of its character as a ferment; that is to say, the weight of yeast which originates under these conditions during the decomposition of the sugar progressively approaches the weight of sugar decomposed in proportion as its life is carried on in the presence of increasing quantities of free oxygen.

[1] Translated from the 'Comptes Rendus,' 1872, pp. 784—790.

Guided by these facts, I have been gradually led to regard fermentation as a necessary consequence of the manifestation of life, when it is carried on without the direct combustions due to free oxygen. It follows as a consequence of this theory that every organism, every organ, every cell which lives or which continues its life without making use of atmospheric oxygen, or which makes use of it to an extent insufficient for the whole of its phenomena of nutrition, must possess the character of a ferment for the substance which is a complete or partial source of heat to it. This substance must necessarily contain oxygen and carbon, because, as I have just stated, it serves the ferment as food. All fermentible substances include, in fact, these two elements amongst their constituents.

I have now brought to the support of this new theory, which I have already enunciated on different occasions since 1861, although with hesitation, some new facts which will, I hope, carry conviction with them.

Place a saccharine liquid suitable for the nutrition of ferments in a vessel so arranged that the liquid can be inoculated with any particular organized product without any chance of other organisms becoming associated with it afterwards without the knowledge of the experimenter by spontaneous introduction, that is to say, by germs in suspension in the atmosphere. On the surface of the medium so prepared introduce a trace of pure *Mycoderma vini*. During the following days the mould will cover, little by little, the whole liquid in the form of a continuous coat. Granting this, it is easy to show that the development of the *Mycoderma* under these conditions gives rise to an absorption of atmospheric oxygen, which is replaced by a nearly equivalent volume of carbonic acid gas, and that at the same time there is no formation of alcohol.[1]

Let us now repeat this experiment under exactly the same conditions, with the difference, that when the coating is continuous we agitate the vessel so as to dislodge and submerge it as much as possible, for the greasy matters which accom-

[1] I have stated that the *Mycoderma vini* has two modes of existence: that it is a monad or a ferment, according to circumstances, and that yeast called *levûre basse* is nothing more than the ferment into which the *Mycoderma* is transformed when it is deprived of contact with atmospheric oxygen. These assertions are not in all respects concordant with the truth, or, rather, the phenomena of which they are characteristic have a complication which has escaped me. I shall soon be in a position to explain them in all their detail. This observation is necessary, because I now speak of *Mycoderma vini* in terms which do not exactly accord with the statement which I now withdraw.

pany it prevent it being completely moistened. On the next day, or even after some hours, when the experiment is made at a temperature of from 25° to 30°, small bubbles of gas rise without intermission from the bottom of the vessel, indicating that the fermentation of the saccharine liquid has commenced. It continues on the following days, although always feeble, and it is easy to ascertain in the liquid the presence of sensible quantities of alcohol. A careful microscopic examination of the cells or joints of the submerged *Mycoderma* shows that these joints do not reproduce themselves, but that they swell up for the most part, and that the internal structure of their plasma is profoundly modified. If the fermentation stops, it can be made to recommence by dislodging again the coating which reforms.

The interpretation of these facts does not appear to me to be doubtful. In the two comparative experiments we have under our eyes cells which acquire or lose the character of a ferment at the will of the operator. But what is in the two cases the difference of the conditions of existence of the cells of *Mycoderma vini?* There is but one which can be maintained. In the first case, the life of the plant takes place at the surface of the liquid in the presence of atmospheric air, or rather of oxygen, whilst in the second it maintains itself apart from its influence, or, at least, in contact with quantities extremely feeble, because what tends to be dissolved in the liquid is retained by the cells which remain at the surface. Life is not extinguished in the submerged cells, as is demonstrated by microscopic examination, but this life takes place or, rather, is maintained in the absence of air, and, under these circumstances, the cells produce fermentation.

I do not speak of the cases where the spores which are sown produce true yeast. I shall return to that on another occasion. We see, in a word, in this double experiment on one side the life and multiplication of cells, with the absorption and employment of free oxygen; on the other hand, the continuation of the life of a part of these same submerged cells without the intervention of oxygen, but with the concomitant appearance of alcoholic fermentation, that is to say, a continuous disengagement of bubbles of carbonic acid gas and a production of alcohol. It is a curious fact, and certainly remarkable, that these experiments succeed equally with true moulds. *Penicillium glaucum*, which lives in the presence of free oxygen, and which uses this gas for the accomplishment of all its nutritive actions, does not

produce alcohol. But if when it is in full vigour it is submerged, or if we impede the access of atmospheric air, the changes which take place in the plasma of its mycelium are accompanied by the formation of alcohol and of bubbles of carbonic acid gas. This is the result of the performance of the acts of nutrition of the mould under the new conditions.

Yeast, which is the type, as well as the other organized ferments which I have discovered, assume henceforth the character of plants or animalcules, differing in no respect from other inferior organisms except in possessing the faculty of living and multiplying in a regular and continuous manner apart from contact with atmospheric air.

I am led to the belief that the mystery of fermentation finds itself unveiled by these unexpected results. What we call organized ferments are organisms which for a time can continue their life, and can even reproduce themselves, without the intervention of free oxygen being necessary to burn and make use of the material of their food; they are organisms, in fact, which can directly assimilate oxidized matters (sugar, for example), capable of furnishing heat by their decomposition.[1] Regarded from this point of view, fermentation appears as a particular case of an extremely general phenomenon, and one may say that all living things are ferments in certain conditions of their life, since there are none in which one cannot instantaneously suspend the action of the oxygen. If one kills by asphyxia, by division of a nerve, &c., any living thing whatever, or one of its organs, or even a mass of cells in an organ, the individual cells will supply themselves with the heat which they require for new acts of nutrition, or of change in their tissues from substances which surround them; they will forthwith decompose them, and the characteristic property of fermentation will make itself evident if the quantity of heat developed corresponds to the decomposition of a weight of fermentible matter sensibly greater than the weight of the materials employed constantly by the living thing, the organ or the cell.

The following facts appear to me to be the logical deduction from these principles:

M. Bérard in his memoirs, which is a model of acuteness

[1] The splitting up of a carbo-hydrate into alcohol and carbon dioxide is accompanied by a loss of chemical force. The alcohol formed has a much smaller thermic equivalent than the sugar from which it was formed. The case is one of internal combustion (innere Verbrennung). Fermentation is therefore a source of heat, that is, of force, for the maintenance of the vital activity of the ferment. See Adolph Mayer in 'Pogg. Ann.,' cxlii, 293. 'Journ. Chem. Soc.,' 1871, pp. 426-8.—(Eds.)

and experimental procedure, states that when fruits are placed in air or in oxygen, a certain volume of the gas disappears at the same time that there is a formation of a nearly equal volume of carbonic acid gas. If these fruits are left, on the contrary, in carbonic acid, or in any inert gas, there is still a formation of carbonic acid gas, as if by a kind of fermentation. In my view this is the true explanation. When a fruit, or in general any organ, is separated from the plant or the animal to which it belongs, life is not at once extinguished in the cells which compose it. The ripening of fruits after separation from the tree which bears them is an incontestable proof. If air be present, oxygen interposes, and takes part in the changes which take place in the interior of the fruit. The heat is furnished by the combustion which results, a combustion in which the sugar without doubt takes a large part; but the nutrition is of the same kind as the nutrition of the fruit while on the tree, as that which takes place ordinarily in living things, and which is characterised by the circumstance that the weight of the materials transformed or employed is comparable with that of the materials which serve for food. Under these conditions alcohol and carbonic acid cannot appear except in an accidental manner, any more than in the life of *Mycoderma vini* in contact with air. For a volume of carbonic acid produced, a volume about equal of oxygen is consumed. This is ordinary respiratory combustion. When a fruit, on the contrary, is placed in an atmosphere of carbonic acid, life continues by borrowing from the decomposition of sugar the heat it needs for its manifestation; the cells are then in the condition of cells which live although deprived of free oxygen. The case is the same as that of *Mycoderma vini* which has been submerged. In fact, scarcely is the fruit placed in carbonic acid gas before carbonic acid is produced as well as alcohol. It is true it is produced in small quantity, but one sufficiently large to allow me to obtain after some days, in one of my experiments, 6·50 grms. of absolute alcohol from twenty-four *Prunes de Monsieur* placed in carbonic acid. The plums remained firm, hard, and sound in appearance, even if some of these characters were not sensibly increased; a corresponding quantity of sugar was destroyed; twenty-four similar plums left in contact with the air became soft, watery, and very sugary. Grapes, all acid fruits, melons, &c., behave in the same manner. A leaf of rhubarb placed in an atmosphere of carbonic acid gas, without apparent alteration, exhales at the end of forty-eight hours a slightly vinous odour, and yields small quantities of alcohol on dis-

tillation. I am satisfied that under these circumstances, when the experiment is properly conducted, neither yeast nor any other ferment is produced. It is in exceptional and rare cases that the cells of yeast can penetrate and pass from the exterior to the interior of a fruit.

Grapes offer in these experiments a peculiarity especially worthy of attention. Every one has remarked that the vendange, that is to say, the juice of the crushed berries and the berries themselves in the vat, have a taste and odour entirely different from that of grapes eaten when fresh. Grapes which have been kept in CO_2 have exactly the taste and smell of the vendange. In the vendange the grapes are almost suddenly enveloped in an atmosphere of carbonic acid gas. I do not doubt that the study of the phenomena of which I have spoken, regarded in their relation with the practical gathering of the grape crop, will become useful in the art of wine making, and I should not be surprised if, by the preservation of grapes in an atmosphere of carbonic acid, we may succeed in producing wines and spirits with peculiar properties.

I have not yet pursued these new views with regard to the organs of animals. It is probable that the phenomena will differ from those which vegetable cells present. Probably also the equations of all these fermentations of a new kind will not only differ according as the cells are vegetable or animal, but will differ also according to the characteristic nature of the cells in each class. The few experiments which I have made with the animal kingdom are too incomplete to be mentioned, but I already foresee from the results which I have obtained that a new path is opened to physiology and medical pathology. I hope that a clear light will be thrown on the phenomena of putrefaction and gangrene. The production of putrid sores apart from the action of organized ferments, will receive, without doubt, an explanation as natural as the formation of alcohol and carbonic acid apart from the presence of the cells of yeast.

Archebiosis *and* Heterogenesis. By Prof. H. L. Smith of New York.[1]

In investigating a subject so difficult as "The Beginnings of Life," or "Archebiosis," as Dr. Bastian calls it, we cannot be too careful in experimenting, or too cautious in interpreting results. Dr. Bastian very properly insists upon this when criticising the views of the "Panspermatists," and has devised certain experiments, which as they appear to him are crucial. At present I feel disposed to admit the conclusions to which he has arrived from these experiments, viz., that life can originate *de novo*, so far as *Bacteria, Torulæ*, and certain minute algoid filaments are concerned, of all of which, however, we have only the slightest knowledge—very little, in fact, except that certain minute things, apparently living, and called by these names, do really exist. Granting, then, for the present, that Dr. Bastian has made out a clear case as regards the *de novo* origin of these organisms, one must pause here, and affirm that he has not proved anything beyond this All the wonderful changes and transformations, or at least the majority of them, detailed in Part III of the second volume of his 'Beginnings of Life,' under the name of "Heterogenesis," are simple conjectures, as he will find when he subjects these statements to such critical tests as he has applied to those of Pasteur, and others of the "air germ" school. This is the more unfortunate, because the perusal of the first volume of Dr. Bastian's so far really excellent book, led me, as doubtless it has others, to believe that the same care and conscientious search for truth would characterize the whole. But now, when I find so much of manifest error in ground which, for fifteen years past, I have carefully and patiently worked over, I cannot help feeling somewhat shaken as to the truth of the results which Dr. Bastian has arrived at in those parts of his book in which he treats of subjects with which I am less familiar? If Dr. Bastian, or any one competent, "will but devote two or three months to the careful study of the changes which" (he supposes) "are apt to take place in the substance of many of the fresh water Algæ, or in those beautiful green animalized organisms

[1] Prof. H. L. Smith has enabled us to reprint this paper with some corrections from the 'Lens,' for January, 1873. In such a controversy as that in which Dr. Bastian has taken so prominent a position, it is very desirable to limit the number of points in dispute. The opinion of an observer of such acknowledged competence as Prof. Smith, will probably be held conclusive in respect to appearances, in which Dr. Bastian's necessarily smaller experience, may easily have led him into error.—(Eds.)

known by the name of *Euglenæ*, some of whose marvellous transformations" (as he asserts) "were faithfully described more than twenty years ago in the highly valuable, but much neglected memoir of Dr. Gros," then he, or any other competent observer, would find that there is no proof for the majority (if not for all) of these "marvellous transformations;" and Dr. Bastian himself would learn that he would have produced a better book if he (as well as others) had neglected the memoir of Dr. Gros.

That there may be no doubt as to what is Dr. Bastian's real belief, for he confesses that he has not witnessed actually these wonderful transformations, though he does not doubt them, "rashly trusting" to his "own theoretical convictions" —a freedom for which he rightly blames others[1]—I quote verbatim from the Index to the two volumes: "Desmids, convertibility of, into Diatoms or Algæ, ii, 455. Diatoms, origin of, ii, 412, 416, 418, 441, 444, 453; terminal forms of a divergent series, ii, 455; Euglenæ, transformation of, into Diatoms, ii, 441, 444, into Desmids and Pediastræ, ii, 446." I propose to examine somewhat critically the passages indicated above, as well as others, principally from Dr. Bastian's second volume, in which at p. 455 we find the following passage: "It seems, however, to be quite certain that a community of nature exists between Algæ, Pediastreæ, Desmids, and Diatoms, since similar vegetable cells *may*, on the same or on different occasions, grow into forms belonging to either one of these groups; and, moreover, the forms *are strictly convertible with one another* until they *chance to assume the forms of Diatoms.* * * Diatoms constitute the terminal forms of a divergent series. The middle terms of the series, however, viz., Pediastreæ and Desmids, are convertible in both directions, either back into Convervæ or onwards into the *less vitalized Diatoms.*" The italics are mine. Now, here is a distinct assertion; but, as we shall see, it is simply an assertion, supported by no real proof. Dr. Bastian knows very little about Diatoms or Desmids, and deals with them altogether at second hand, and from very doubtful authorities. As to the less vitalized character of Diatoms, and their *chancing* from Pediastreæ and Desmids, no one at all familiar with them in the living condition can for a moment believe it. They have a far more complicated internal structure than the more highly vitalized (!) Pediastreæ and Desmids, from which, according to Dr. Bastian, they may chance to derive their forms. I have observed the growth and

[1] Preface, vol. i, p. xii.

reproduction of Diatomaceæ to little purpose, if we are to believe Dr. Gros and Dr. Bastian.

I have myself probably witnessed more of the phenomena of conjugation and growth than any other person, and can affirm, without fear of its being disproved, that such chance, or indeed any kind of transformation of Pediastreæ or Desmids into Diatoms, never has happened, nay more, never will happen. Dr. Bastian has never seen it, and as for Dr. Gros—well, twenty years ago men might be pardoned for believing many things which we smile at now. When Dr. Bastian, or any competent observer, watches the transformation *through every stage*, and no link of the chain is missing or defective, then, and not till then, can we believe it. It will not do to take for the same things in different phases of development, certain microscopic appearances agreeing in size, form, or place. What I insist upon is the positive proof; and that Dr. Bastian has been misled by appearances (and by Dr. Gros), or to nearly use his own words, that his "presumptions have stolen a march upon established facts," will, I think, be tolerably evident when I explain the real significance of some of the appearances actually observed by Drs. Bastian and Gros.

Passing by Dr. Gros' own words, quoted by Dr. Bastian, we come, on pp. 414, 415, 416, and 417, to the actual observations of the latter. I will not question now that part which relates to the production of "unmistakeable filamentous Desmids" (though there is no proof of their Desmid character other than a remote resemblance), I look more particularly to the account of evolution of Diatoms, fully convinced, however, that the errors in misinterpreting what he saw are quite as great with the Desmids as with the Diatoms. The woodcut, p. 417, fig. 82, entitled "Modes of Origin of Desmids and Diatoms," has, by way of explanation, "*e e'*, * * * * * "Pediculated Diatoms were also seen budding from the same *Cladophora* filament," Poor as the cut is, we easily recognise these "pediculated diatoms" as *Achnanthes exilis in its normal condition;* and, if Dr. Bastian wishes, I can show him thousands of this well-known form, pretty much as he figures it, growing on a pedicel, the *result of its own secretions*, not only on *Cladophora*, but quite as frequently on *Mougeotia, Vaucheria*, or some other fresh water Alga. The marine forms, *Achnanthes longipes, A. brevipes, A. subsessilis*, &c., all attach themselves by a similar stipes, to marine Confervæ. *E. g.*, we have *A. brevipes* by us now, abundantly, on *Ectocarpus siliculosis*.[1]

[1] Mr. Archer brought a similar instance before the Dublin Microscopical Club, see *supra*, p. 313, and *infra*, p. 367.—[Eds.]

What is represented by Dr. Bastian, then, is no process of *budding* at all. The little diatom in question, *A. exilis*, I have found conjugating, and it differs in no wise, in its life history, from the larger well known forms. Any one who will observe the large and living Diatoms with care will notice the nucleus and ramifying nerve-like threads, and the beautiful distribution of endochrome with reference to these. These have been partly figured by Prof. Max Schultze, in Müller's 'Archiv,' 1858, Taf. xiii, and copied (not equal to the original) in the "Microscopical Journal,' vol. vii, pl. 2.

In addition to the nucleus and ramifying threads, many Diatoms exhibit a germinal dot, with reference to which the endochrome is arranged, rather than to the nucleus; particularly is this the case with *Surirella*. I may add, that the coloured figures in Smith's *British Diatomaceæ*, almost without exception, are caricatures; indeed, the late Tuffen West admitted to me that some of those representing conjugations were manufactured to order. I assert, then, that the little "budding" Diatom figured by Dr. Bastian, fig. 82, is growing quietly, after the fashion of Diatoms, a direct result of self division of some former *A. exilis*, and so back, to a sporangial frustule; and that, if it had been allowed to live, it would have continued the process of self-division, until finally, at the proper season, and under proper influences, a new sporangium would have been formed, the commencement of a new series, in all respects, however, like the normal form; and that no transformation of *Euglena*, *Pediastrum*, Desmids, *Vaucheria*, or *Cladophora*, is ever in any way connected with it. I have by me now a gathering of this Diatom with conjugating forms, and the process is entirely similar to what we have witnessed in the marine forms belonging to the same genus, as well as to Diatoms in general.

With regard to the marine forms, which are far more numerous than those of fresh water, one might ask, where did *they* originate, or rather how become terminals of a series, with Pediastreæ and Desmids for middle terms? since, if I mistake not, these middle terms are seldom, if ever, found except in fresh water! Perhaps this might not appear to be much of an objection, inasmuch as some species affect equally fresh and salt water, but if we get the gist of Dr. Bastian's argument, he would not only have us believe that *Bacteria*, &c., originate *de novo*, which at present I may grant, but that somehow (the way not yet proved) fungus spores, *Euglena*, *Astasia*, *Actinophrys*, or something else, come from these "beginnings;" and next, that somehow, not yet shown how, Pediastreæ and Desmids, and finally,

Diatoms, come from the previous existing organisms, a series of transformations, not effected once for all, but all *continually going on;* so that these things are being manufactured, as it were, every day.

Doubtless *Bacteria* were developed at a very early period of the earth's history (Dr. Bastian informs us, and we have no desire to question it, that they soon make their appearance after a prolonged boiling of the infusion), but it is remarkable that these primæval "beginnings" appear to have been very chary of evolution, as neither Diatoms or Desmids appear earlier than the Cretaceous, or what is far more probable, the Tertiary. Somehow, through all this long period, they behave just as we have always found them to do now, viz. not long after their appearance, die, or at least become quiescent; and if other organisms appear where they were, or among them, it is by no means proved that these are transformed *Bacteria* or *Torulæ*, or anything similar. And while upon this subject of the first appearance of *Bacteria*, I may be permitted to ask, why, in watching their development in thin films of fluid, beneath a covering glass, after it had been cemented to the glass slip, it is necessary, as explained in the foot note, vol. i, p. 294, "to leave a minute aperture at the circumference of the glass uncovered by the cement?" Is this for the admission of air-germs?

We resume our consideration of figure 82 and the explanation. Certain algoid vesicles, budded off (probably like *A. exilis*) from *Vaucheria*, "gradually become converted into different kinds of Diatoms! (*l l'*, *m m'*)." With reference to these algoid vesicles, Dr. Bastian states, vol. ii, p. 416, that "These bodies increased in size, and it soon became obvious that they were young *Naviculæ* (*l l'*). The exact pattern assumed in the early stages is subject to much variation, and several different Diatoms *seemed* (italics mine) to be produced corresponding to these different initial forms (*m m'*)." This would be wonderful, *if true;* but, not only is there no evidence that actual Diatoms *did* come from the vesicles of *Vaucheria*, but any one familiar with the observations of living Diatoms can tell *where* they did come from. I venture to assert, that not a Diatom observed by Dr. Bastian came from the vesicles in question, but that they, or their immediate progenitors, were in the gathering which contained the *Vaucheria*, and made their appearance out of the *débris* and general mass after a little period of quiet, as we know they will do under the influence of light (of which something more presently). But besides, Diatoms *do not grow by increase of size,* there are no such things as broods of

young frustules, as the late W. Smith and others have supposed; they generally diminish by continual self-division, or at least continue of the same size, as I have abundantly proved. The late Dr. Greville, a recognised authority as to Diatoms, fully agreed with me as to this. I am not disputing that Dr. Bastian saw these minute and various little *Naviculæ*, but I do say he is building his theory upon what *seemed* to be, not on what really were, the facts. The influence of light and quiet in bringing these little forms out of their recesses in the *mud*, was well illustrated in an experiment I once performed. An immense number of minute *Naviculæ* were very carefully scraped of the blue mud of a river bottom, in shallow water, and transferred to a phial; of course, though as great care as possible was used to get them pure, the mass when shaken up appeared quite slaty. Observing that a leaf, when lifted from the hard bottom, left its form outlined distinctly, the Diatoms coming up to the light all around it, I tried the following experiment:—The mud (and Diatoms, together, of a slate colour) was spread in some thickness upon a strip of glass, and a number of pieces of moistened blotting-paper laid upon it. The slide was then turned over, and a pattern (lace) placed on the glass, and the whole exposed, as in printing a photograph. In something like half an hour the pattern was removed, and the outlines were distinctly shown by the little Diatoms coming up towards the light. It is a quite common dodge to separate, and get Diatoms pure by exposing the material containing them to a strong light, in a saucer under a glass cover; and if Dr. Bastian wishes, I could show him many excellent specimens thus prepared. I proceed, however, in connection with the appearance of these "young (?) *Naviculæ*," to refer to figure 84, *q*, vol. ii, where in explanation, it is stated, "*q* Resolution of *Euglena* into Diatoms"!! In the text, however, the author says, "I have only distinctly observed appearances *indicative of this transformation on one occasion*, but in this case, the whole of the contents of a Euglena *seemed* to have been resolved into seven distinctly striated *Naviculæ*. * * *Although the earlier stages of the transformation were not seen* (italics ours), I have no doubt that the Diatoms originated in this way." Vol. ii, p. 441. He is more easily satisfied that a *Euglena* can transform into a Diatom, which posseses a wonderful, silicious, and beautifully sculptured epiderm, than he is that *Bacteria* come from air-germs. It will not do to trust "the misguiding influence of a treacherous analogy" in Dr. Bastian's case, more than in that of the panspermatists, and to decide that, because these

seven *Naviculæ* were in what appeared to be the thickened envelope of a *Euglena*, about the size of an encysted form, figured near by (*b*) they came from the transformation of such a cyst.

As to Dr. Gros' observations about *Gomphonemæ*, they are simply absurd; and the packing of *Naviculæ* (and other forms) into the empty (*Vaucheria* or other alga) filaments is quite a common occurrence. The true explanation of the encysted *Naviculæ* I can easily give, and I have by me at present a slide with over two hundred of these cysts upon it. In this case, the Diatom is *Colletonema vulgare*, but I have seen it also with *Synedræ*, *Cocconemæ*, *Gomphonemæ*, and, though more rarely, with mixed forms. To the same category belong figures iii, iv, v, of Smith's 'British Diatomaceæ,' vol. ii, pl. C, p. 221, and very erroneously referred to by him as resulting from the sporangium, figure ii. So, also, pl. B, fig. 89, same vol., referred to erroneously as conjugation of *Synedra*. All these, as well as Dr. Bastian's solitary example, are readily explained, and I have repeatedly witnessed the whole phenomenon. It is the work of an *Amœba* (or an amœboid mass) no way connected with any development, evolution, or transformation. I have an elaborate series of representations, carefully drawn, showing the progress and mode of encysting. Of course, from its very nature, as will be shown, the phenomenon involves that the encysted forms shall be mostly, if not altogether, of one species, and so we find it. Clusters of sessile *Synedræ*, or of stipitate *Gomphonemæ*, or *Colletonemæ*, or small *Naviculæ*, the tubes of the former having become, by quiescence, an amorphous jelly; and in such a formless, gelatinous-looking mass, the small *Naviculæ* (*Frustulia* of older authors) are often imbedded. I have repeatedly observed the *Amœba*, while moving freely through the field over and along the stems of Confervæ and often throwing out long, thread-like arms (?) of sarcode like that of Rhizopods, the moment it reaches a mass of Diatoms (frequently even one or two), whose bright, clear endochrome showed active life, spread itself out over, *completely encysting them*. The Diatoms soon after change in appearance; the clear yellow and olive tints disappear, and only small dark red masses remain, somewhat like Smith's figure, 'British Diatomaceæ,' vol. ii, pl. B, fig. 89. Meanwhile, what is not the least remarkable, a transparent wall, of some tenacity, apparently, forms around the Amœboid mass. After a long period the *Amœba* escapes by rupturing this outer shell, often at only one point, out of which the mass issues, as a long string, soon gathering itself up, how-

ever, to travel on in search of new food. The encysted mass, after the escape of the *Amœba*, remains, *showing the envelope*, and the frustules are stuck, or half fused, as it were, together. After treatment with acids, &c., in the usual way, for preparing the frustules, this outer envelope disappears, but the frustules still cohere in bunches, as though the silex had been partly dissolved, and they had thus been cemented. I have slides as well as materials showing this, in abundance. Sometimes, after thus encysting, the *Amœba* mass will remain for days, showing no disposition to move away. I think that it will be quite evident that Dr. Bastian's seven encysted *Naviculæ* belonged to the group I have just explained, and are no development of a *Euglena*.

I pass on to figure 85, p. 447, the title of which is, "Origin of Diatoms, Desmids, Pediastræ and Algæ from Euglenæ and other vegetal Matrices," and at p. 444, under the heading, "Transformations into Diatoms," it is stated, that "some of them (Euglenæ) are apt, at certain times, to be converted into large Diatoms." The authority for this astounding statement is Dr. Gros, for Dr. Bastian is careful to say, p. 445, "Whilst I have not myself been fortunate enough to trace the actual origin of any of these large Diatoms, I have, on several occasions, been struck with the comparatively sudden appearance of very large specimens (about $\frac{1}{500}''$ in length) of *Navicula librilis* still presenting an embryonic appearance, in vessels containing Euglenæ and Vaucheria." Dr. Bastian apologises for Dr. Gros' nomenclature (foot note, p. 412), but surely he himself was not writing at a period when "precision was not given to nomenclature," "and in a region in which books of reference were not accessible." True, "what's in a name?" yet we fancy that the old names, "hartshorn" and "glue-like," would not suit Dr. Bastian, or modern science, as well as "Ammonic carbonate," or "colloid," and so it would have been quite as well to give the true name to what is no *Navicula* at all. But this after all is of little moment, since we know that the Diatom meant is *Cymatopleura solea*, one of the most persistent forms, if, indeed, there is any difference at all in this respect among the Diatomaceæ. I think that Dr. Bastian cannot consider what he has seen, as to this Diatom, is really of any value in proving transformation of *Euglenæ* into Diatoms. As to the transformations of *Euglenæ* into Desmids, he has been equally unfortunate; but then, indeed, "Dr. Gros has observed it on several occasions"!!

Figure 85 *e* (from Gros, of course) shows a *Euglena*, no doubt; it is about the length of *f*, which is also, no doubt, a

Closterium. Moreover, the *Euglena* is represented with a central transverse blank space, very like that of the *Closterium;* but, unfortunately, both ends of a *Closterium,* as everybody knows, are alike, so are not both ends of a *Euglena.* Now, the *Euglena does* undergo many changes, no doubt, but I believe generally, if not invariably, it passes from the long spindle shape into the ball. The very slight resemblance as to length and central blank space, is really all that Dr. Gros has to build his transformation upon! Dr. Bastian says, p. 448, "Although I have never seen the final stages of this transformation, I had, even before becoming aware of Dr. Dr. Gros' views, noticed the curious fact that very small specimens of Closteria were never to be seen. * * * So that, just as in the case of the large Diatoms already alluded to, their origin by metamorphosis is much more reconcilable with these facts (transformations of Euglena) than with the notion that they are derived from small germs, more especially since no one has ever seen or knows anything about the mode of production of such germs in *Closterium.*" The authority for this latter statement is given — Pritchard's 'Infusoria,' 4th Ed., p. 12. I do not question that such a statement *is* in Pritchard, for there are many erroneous statements in it; it is not on page 12, however; but no doubt this is a misprint, peculiar to American editions. As to the small Closteria, considering that Dr. Bastian is not "where books are not easily accessible," we refer him to Ralf's 'British Desmideæ,' T. xxvii; Rev. W. Smith, 'A. N. H.,' 1850, p. 4; and Pritchard's 'Infusoria,' 4th Ed., p. 15. I do not of course vouch for Mr. Jenner's observation, but I think it entitled to as much respect as Dr. Gros', to say the least; and, if such be the office of the sporangium of *Closterium* (result of conjugation), to serve as a resting spore over the winter, and the final production of broods of young *Closteria,* then we have, in the very marked difference between the results of conjugation of Desmids and Diatoms, one of the strongest proofs of their complete dissimilarity: for the sporangium of the latter which is generally of much larger size than the parent frustules, serves to restore again the cycle by commencing self-division in a large form, when, by the act long repeated, the frustules had become very small. No doubt many do go on until myriads of small forms appear, thus self-dividing until finally they die out, without any renewal by conjugation.

To finish my remarks on figure 85 (reproduced from Dr. Gros), *b* and *c* are referred to as two forms of Datoms which *may* arise from transformed *Euglenæ;* these are probably

Naviculæ, one in front, the other in side view; but what *Naviculæ* this representation is too imperfect for decision; *d* and *d'* are called *Chlamidomonas* (*sic*), giving origin to Diatoms. Dr. Gros' account of this transformation is too absurd to be worth repeating here. *d* is very much like a small *Amphora*, or possibly a *Cocconema* self-dividing, and *d'* may be the dorsal aspect of a *Cocconema;* any resemblance to *Chlamydomonas*, or any reason to infer development from this, is most fanciful. In view, then, of this entirely insufficient evidence, how can Dr. Bastian say, p. 420, that "the actual transformation has been witnessed by independent observers, whereby algoid or Euglenoid corpuscles are bodily converted into Diatoms or Desmids?" I have dwelt so long and particularly on one special portion of the book, because upon this I felt best qualified to act the part of an honest critic. I have no "constitutional objections," or "religious scruples" about accepting anything which can be proved, either as to Archebiosis or Heterogenesis, but we want no fancy pictures. As to other parts of the work, upon which I am not so capable of judging, *e. g.* such as refer to the development of *Nematoids* from spores of *Vaucheria*, I have no doubt there are many bubbles that might be pricked. This I leave for others to do, and begging a correction in the Chart, facing p. 552, where it is stated that Diatoms produce *two* embryos, much larger than their parents, which is only partially true, for often there is but one sporangium, and often but one parent frustule, I stop, sorry that the author has been led so much astray, and has spoiled what promised to be a good book.

A Résumé *of recent* Observations *on* Parasitic Algæ.

By W. Archer, M.R.I.A.[1]

That Green Algæ, contrary to what has been generally supposed, can, though no doubt exceptionally, lead a parasitic life, has been recently pointed out by several observers. The subject is one which may not inappropriately be brought before the readers of this Journal connected with the new Schwendenerian theory as to the nature of Lichen-gonidia, upon which it seems possibly to have a certain amount of indirect bearing.

[1] Read at the Meeting of the British Association at Bradford, Sept. 19.

Professor Cohn,[1] in describing an interesting new *Chlorophyllaceous* parasitic alga, in the second number of his 'Beiträge,' remarks, amongst the physiological and vegetative distinctions between the class of Fungi and that of Algæ, the most important is the absence in the former of chlorophyll, and that, on account of this want of chlorophyll, it is assumed that fungi depend for their nutrition upon organic compounds and hence must live a parasitic life, since they have not the power like the green plants to assimilate inorganic compounds in sunlight.

Later researches, however, go to show that the fungi obtain their nitrogen in the same manner as green plants, but that they cannot, like them, decompose carbonic acid, and hence that they depend for their carbon upon the assimilation of carbonic compounds already formed in organisms. To the green plants, and especially to the algæ, on the other hand, the faculty of assimilating such organic compounds is, as a rule, denied. But certain true phanerogamic parasites produce chlorophyll; the assumption may then be correct that the presence of chlorophyll may not be incompatible with the assimilation of organic compounds. Many animals (Infusoria, Zoophytes, Turbellaria) produce chlorophyll, alike in all respects to that of plants, and these in no way differ in their power of assimilation from the colourless or brown species.

Now with respect to the Green Algæ it has been generally assumed that they exclusively build up their cells from carbonic acid and ammonia and saline matters, but, on the other hand, do not assimilate more complex carbon compounds, and that hence they never can be true parasites. But some recent observations have shown that a parasitic mode of life may not be foreign even to members of this class.

Cohn points out that epiphytic algæ,[2] even though some are found, it may be exclusively, only upon certain other species, are beside the present question, as well as the smaller algal forms which occasionally nestle amongst the mucous envelopes of others.

The relation in which certain green algæ stand to the closed thallus of various Florideæ in which they have been

[1] Cohn: "Ueber Parasitische Algen," in 'Beiträge zur Biologie der Pflanzen,' Heft II, 1872, p. 87.

[2] These are numerous, many of common occurrence; but the scientific world at large may not even yet, possibly, be aware of the newest "facts" put forward by Dr. Bastian as to their nature and origin ('Beginnings of Life'), who explains all in the most off-hand manner by gravely assuming that the varied and often very heterogeneous epiphytic *fringes* of algal forms which are met with attached to higher plants are developed as merely *heterogenetic* outgrowths from the latter!

found is apparently different. Prof. Cohn on a previous occasion[1] describes the occurrence of green (amylaceous), narrow lanceolate or broadly pyriform cells, drawn out at the lower end into a long solid cellulose stipes, occurring amongst the densely packed filaments of a *Cruoria*. These green cells are so regularly placed that at first Cohn, as well as probably other observers, regarded them as normal reproductive cells of the *Cruoria*, but they are, he now holds, without doubt foreign Endophytes.

Mettenius[2] found another alga of the same Order, *Polyides lumbricalis*, beset through its dichotomously branched thallus by green cells, single or more or less crowded, and with their narrow end directly standing upon the *Polyides* and surrounded by the substance of the alga. These were taken by Mettenius for the mother-cells of the spores of the *Polyides*; but Cohn holds wrongfully so, as they seem foreign to the *Polyides*, and to be rather the young plantlets of a chlorosporaceous alga. In this latter view Thuret (in a letter) coincided, and indeed identified the plant as *Cladophora lanosa*. He found the germinating zoospores amidst the closed cortical tissue of the *Polyides* as green oval or round cells, increasing in size for some time and without dividing, until towards the end of winter, when the end-cell elongates outwardly, breaks through the tissue of the *Polyides* and develops outside into a little *Cladophora*-tuft. Cohn met with a similar plant afterwards at Heligoland, but did not find that it formed septa or became branched or broke through the tissue of the *Polyides*; hence he regards this as probably a distinct Endophyte.

Prof. Cohn goes on to remark, that the question of the relation of the Lichen-gonidia to the hyphæ of the Ascosporeæ, whose constant associates they are, gives to the researches upon the endophytic mode of life of green algæ a special interest. According to the Schwendenerian hypothesis the algæ become involved in the mycelium of an ascomycetous fungus and their mutual relationship is dependent upon the fungus conducting the crude inorganic nutriment to the algæ, whilst the former has presented to it by the latter the organic compounds necessary for its existence. Hence the fungus vegetates parasitically upon the algæ, and is nourished directly or indirectly by the organic nutriment produced by the activity of their chlorophyll.

[1] Cohn: "Ueber grüne Schläuche der '*Cruoria pellita*, Fr.,' in Rabenhorst's 'Beiträge zur näheren Kenntniss und Verbreitung der Algen,' Heft II, Leipzig, 1865.

[2] 'Beiträge zur Botanik' (Heft I, p. 39, T. IV, f. iii, 1), 1850.

After referring to Reess and Schwendener's memoirs (already cited in this Journal, on *Nostoc* and *Collema*),[1] tending to show that *Collema* originates by the penetration from without of a parasitic ascomycetous fungus into a *Nostoc*, the author then goes on to allude to two records of new and surprising observations evidencing just the contrary—that is, representing *Nostochaceæ* as endophytic (parasitic) in higher plants. Reinke describes the occurrence of parasitic Nostochaceous algæ in the stems of five species of *Gunnera*.[2] The alga lives at first in the mucus produced from the glands on the back of the young leaves ; this mucus infiltrates afterwards amongst the epidermal cells of the *Gunnera*, resolved themselves, too, in great part into mucus, and with it the clusters of algal filaments gain an entrance. The passage to the *Nostoc*-clusters becomes closed by newly-formed parenchyma replacing the previous glandular tissue, and the alga is completely imprisoned, so that henceforth its nutriment is obtainable only from the sap of the *Gunnera*. Thus the relation of the "gonidia" in the *Gunnera* is just the reverse of that assumed by Schwendener for the lichens.

The memoir of Janczewski[3] treats of the endophytic occurrence of a *Nostoc* in the interior of the tissue of certain Liverworts. Milde was the first who noticed these, but they seem previously to have been taken by others for broodcells or male elements. Janczewski, however, showed that in *Anthoceros* they were truly *Nostoc*-colonies belonging to a species growing on the same soil, and which penetrated through the stomata on the underside of the thallus of the liverwort, and became spread through its intercellular spaces. He also infected certain plants artificially. By and by the chlorophyll and protoplasm of the neighbouring cells of the liverwort became destroyed ; nor could the imprisoned *Nostoc*, which could only gain its nutrient substances from the *Anthoceros*-thallus, make an exit except by the breaking up of the tissue of its host. The *Anthoceros* has no need of the *Nostoc* in order to draw either its crude or elaborated nutrient substances, neither, *vice versâ*, apparently has the *Nostoc, per se*, need of the *Anthoceros*—both can and do live separately. Both, however, lived and flourished for a time

[1] 'Quart. Journ. Mic. Sci.,' n.s., vol. xii, p. 367, vol. xiii, pp. 226 and 235.

[2] Reinke: "Ueber gonidienartige Bildungen in einer dicotylischen Pflanze," in 'Göttinger gelehrte Anzeigen,' No. 25, 1871; also 'Ueber die anatomischen Verhältnisse einiger Arten von *Gunnera*," same Journal, 1872.

[3] Janczewski: "Zur parasitischen Lebens weise des *Nostoc lichenoides*," in 'Bot. Zeit.,' 2nd Feb., 1872: since more fully set forth, with illustrations, in 'Annales des Sciences Naturelles,' Bot., T. xvi, p. 306 (1872).

together, the *Nostoc*, however, seemingly, now truly parasitic, the *Anthoceros* becoming locally disorganised and, as we are to gather, ultimately decomposed. The same author found Nostochaceous filaments, having made an entrance (through the existing opening) into the spiral cells of *Sphagnum*.

Whilst, however, these observations only show the *Nostoc*-colonies to have penetrated into the intercellular spaces, those of Reinke, as mentioned, show they gain access into the cells themselves of the *Gunnera*-parenchyma. Still, to both the objection might be raised that the *Nostoc*-colonies, which, indeed, live outside such hosts, may have penetrated therein only by accident, and hence that a true parasitism may not be unquestionable. But to the new chlorosporaceous alga which Cohn describes he holds that that objection could not be applicable.

Upon microscopically examining, in the month of May, some leaves of *Lemna trisulca*, which he had had growing through the winter in a glass vessel along with certain other water plants, Cohn observed numerous little cells embedded in the interior of the parenchyma, some of which were of an intensely emerald-, others of a verdigris-green.

The thallus of *Lemna trisulca* is covered on both sides by an epidermis formed of tabular cells with an irregular zig-zag contour, and clothed by a cuticle. Along the margin of the thallus between the layers of the epidermis there occurs only a simple stratum of parenchymatous cells, leaving between them, at the angles, considerable air-spaces; at the thickened middle of the thallus are found larger hexagonal spaces, in two series under one another, mutually separated by simple walls; stomata are absent. The whole of the cells, except those containing raphides, contain chlorophyll-globules, or afterwards starch-granules, which become used up for the formation of the lateral offshoots.

The "emerald-green" parasites are propagated by zoospores, and with these Cohn begins his account of this interesting new form. These attach themselves externally upon the *Lemna*, sometimes in many hundreds, and always at the boundary between two epidermis-cells; before the swarming they are pyriform, green, and with a colourless beak; after germination, round, and coated with a thick, colourless membrane, subsequently swollen, and multi-laminated. Each now puts forth a tube, whose apex pushes asunder the lateral surfaces of the two epidermis cells—assuming in the act a figure-of-8 shape—the one portion above, the other below the epidermis. The lower portion, now of a clavate shape,

pushes deeper and deeper into the interior; the green contents descend from the upper portion, which now appear like an external colourless knob. The lower portion expands and fills up and dilates an intercellular space, and even distorts the adjacent parenchymatous structure of the *Lemna*, or may become itself ultimately misshapen or variously figured. The membrane of the parasite becomes thicker and more evidently laminated, the chlorophyll contents become very dense, and starch-grains also appear, until it becomes dark green and almost opaque. Presently, a peculiar kind of free cell-formation takes place, the contents become divided into an immense number of segments, breaking up finally into a great number of pyriform zoospores (of 4—5 microm. in diam.), of clear bright green, their colourless beak mostly directed outwards. In the mean time the inflated lower portion of the parent-cell has put forth one or several neck-like processes, splitting the epidermis and opening outwardly, through which by and by the zoospores rapidly make their exit and leave the empty colourless mother-cell behind in the *Lemna*-thallus; the author had not, however, been able to witness them during exit, nor has he observed the number and attachment of the cilia. Many differently advanced stages of this alga occur in the same *Lemna* simultaneously. Not all the zoospores, however, which become attached upon the surface of the *Lemna* arrive at perfect development; many perish after germination or after the formation of a short neck, remaining thus as little colourless knobs upon the epidermis. It likewise sometimes happens that not all the zoospores find an exit through the neck-like opening, and the few remaining behind assume a regular globular shape without much increase of size, and become coated with a wall. These look like certain protococcaceous cells, and do not appear to put forth a germinating tube or neck. The author thinks it likely they may be considered resting-cells.

From the foregoing description it seems to follow that this green endophyte is to be regarded as an independent organism, a chloro-zoosporaceous alga. The epiphytic genus *Hydrocytium* (Al. Br.)[1] approaches the nearest to this endophytic form; but this latter characteristic points to an affinity with the colourless group, *Chytridieæ*, frequently referred, indeed, to an Order of Fungi. Amongst these, *Synchytrium*, established by de Bary and Woronin, has been shown by Schroeter and Schneider to be a widely represented genus infesting land plants. Dr. Schroeter[2] divides this genus into three sections:—

[1] A. Braun: 'Alg. unicell. gen. nov.,' p. 24.

[2] Schroeter: "Die Pflanzerparasiten aus der Gattung Synchytrium," in Cohn's "Beiträge zur Biol. d. Pflanzen.,' Heft I, p. 1.

Eusynchytrium; protoplasm yellowish-red; zoospores penetrating the cells of living plants, becoming dilated, and their contents breaking up into a cluster of zoosporangia, certain of the zoospores forming at the close of the vegetative period resting-spores with a thick dark brown membrane. *Chrysochytrium;* protoplasm yellow or reddish-yellow. *Leucochytrium;* protoplasm colourless. In both the latter sections the zoospores penetrating the host-plant at once become resting-spores, which upon the breaking up of the host-plant become free, and their contents after a period of rest make their exit and become divided into zoosporangia. Compared with these the present plant in its development approaches most to the first section, as is seen, but it seems to differ in the absence of a membrane round the segmented portions of the contents, which afterwards break up into zoospores. In this it agrees with *Characium*, but there the division of the contents is successional, and the author had not made out a regular division in powers of two in *Chlorochytrium*, by which name he proposes to designate his new plant. Nor in it, though, as is seen, it is probable, has he ascertained the occurrence of resting-cells.

In spite, however, of these analogies with the *Eusynchtyriceæ*, they are separated by the important character of the presence of chlorophyll in the protoplasm of *Chlorochytrium*, as well as by the formation of a germinating tube whose apex becomes enlarged as a special cell, the rest remaining outside the host-plant, both characters absent from *Synchytrieæ*. The latter are intra-cellular in their growth, the new plant is inter-cellular. Cohn regards *Chlorochytrium* as holding a place between *Hydrocytium*, *Characium*, and the *Chytridieæ*.

The following is his description:

Chlorochytrium (n. g.), Cohn.

Planta endophyta viridis unicellularis, globosa ovoidea vel irregulariter curvata bi- tri- multiloba, dense conferta plasmate viridi, primum in segmenta majora diviso, dein secedente in zoosporas innumeras pyriformes virides processibus tubulosis extus emissas.

Ch. Lemnæ, n. s. Zoosporis extus ad epidermidis superficiem ad cellularum dissepimenta affixis, post germinationem in tubos excrescentibus, qui inter laminas dissepimentorum intus usque ad parenchyma mesophylli provecti, in lacuna intercellulari aucti, in utriculos globosos vel elongatos vel

irregulares excrescunt; cellularum adultarum diameter ad 0·1 mm.

Habitat in *Lemna trisulca*, Bresl., 1872.

That *Chlorochytrium* is a *parasite* cannot be doubted, for although its presence in the *Lemna* does not, beyond its mechanical distortion of the tissue, exert any very injurious effect, neither do the undoubted parasites, the *Synchytrieæ* and *Peronosporeæ*. Nor can the intercellular vegetation be adduced as against its parasitic nature, for most *Peronosporeæ* develope their mycelia in the intercellular spaces of their host-plants. Since this endophyte is encompassed by the tissue of the *Lemna*, the formative substances necessary for its increase can manifestly be brought to it only by means of the neighbouring cells.

It might be assumed, indeed, that these green *Chlorochytrium*-cells, notwithstanding their endophytic position, take up from the surrounding tissue of the *Lemna* only inorganic compounds (crude nutritive substances), but no organic juices, and that even their cell-membrane or their protoplasm, by virtue of some peculiar molecular structure, would exclude organic nutriment, and admit only the inorganic by endosmose. It is known, indeed, that living cells, such as those of the roots in Phanerogamia, cause such a dialysis of their nutrient fluids, or that, *vice versâ*, certain organic solutions become retained by the protoplasm in the interior of a living cell, whilst inorganic compounds make their exit without difficulty. It is, however, just as probable (especially having in view the green phanerogamous parasites) that the presence of chlorophyll in the cells of a plant does not exclude the faculty to take up certain organic juices; as, indeed, manifestly the green tissue of ordinary leaves must have taken up at least a portion of their formative substances in the assimilated form. In any case *Chlorochytrium* is so far the most interesting of all known endophytes, as it is the only one hitherto known which is chlorophyllaceous. Perhaps the green Endophytes observed by Cohn in *Cruoria* and *Polyides*, as regards their development-history, are related to *Chlorochytrium*; the thick solid stipes of that appertaining to *Cruoria* is strikingly like that part of the structure in *Codiolum*, *Characium*, and *Hydrocytium*, whose relations to *Synchytrium* Schröter has already set forth.[1]

But the *bluish-green* globes also enclosed in the *Lemna* fronds were different things. They were certainly also algæ —mostly *Nostochaceous* algæ, which represented various genera; these at first find themselves on the surface of the

[1] L. c., p. 48.

Lemna, and then betake themselves through its burst tissue as well as the empty recesses of the *Chlorochytrium* into their cavities, and, having entered, soon entirely fill up the space, as if favoured by the protection afforded by this new nidus. It is striking that the thickened cell-wall of the empty *Chlorochytrium* assumes a brown colour, possibly through the chemical action of phycocyan from dead filaments. Even green algæ make their way in, such as *Raphidium*. All these, however, are but secondary endophytes: *Chlorochytrium*, on the other hand, is a primary parasite.

As regards the question of the nature of the green lichen-gonidia, these observations have only a bearing in so far as they are evidence that chlorophyll-containing algæ can live as endophytes within foreign plants.

Prof. Cohn concludes his account of his *Chlorochytrium* by remarking that he has found *Lemna trisulca* to afford a nidus for other Chlorosporaceous plants, partly in the interior of the parenchyma and of the epidermis-cells, partly in the intercellular spaces, in the latter forming Confervoid green tubes, breaking up into smaller segments, of one or many series, and forming an intercellular network somewhat like a *Hydrodictyon*; but he has not yet been able to follow out the development-history of these endophytes.

To the *résumé* of recorded cases of undoubted "Parasitic Algæ" Professor Cohn might have added yet another, if not two. The algæ referred to (unlike those mentioned and described by him), are not endophytic indeed in higher plants, but make a portion of an animal tissue their peculiar nidus—two of these are the closely allied forms *Pleurococcus Bradypi* et *Cholopei* (Welcker et Kühn)—the other the *Zooglœa capillorum* (Buhl).

The two former were found by Welcker[1] in the tissue of the sheaths or cortical layer of the hairs of the two- and three-toed Sloth; if sometimes sparing in quantity or absent in the examples examined, they were on the other hand mostly present in very great numbers. Kühn, to whom they, upon even a first examination, were submitted for a critical examination of the alga, readily satisfied himself that this was indeed a vegetable structure foreign to the tissue of the hair. However, in order to test the matter more closely, he tried the experiment of boiling the hairs so affected in solution of caustic potash, for so long as to cause the substance of the

[1] Welcker: "Ueber die Entwickelung und den Bau der Haut und der Haare bei *Bradypus*, nebst Mittheilungen über eine im Innern des Faulthierhaave's lebende Alge" in 'Abhl. der Naturf. Gesellschaft zu Halle,' Bd. xx, Heft. I, p. 59.

hair still to cohere as a mucous fibre, but not so long as to cause it to break up; the structure of the hair itself was then no longer perceptible, whilst the questionable cells, as was to be expected, remained perfect and became sharply and distinctly recognisable. There was hence no doubt but that these cells were of vegetable nature. Although himself satisfied these were algal not fungal in nature, Kühn applied dilute sulphuric acid and iodine to the previous preparation, and the membrane assumed a blue colour, showing they were not of fungal nature; for though *Peronospora* in the spore-bearing portion of its mycelium reacts as most plants, its intramatricular portion does not. Further, in the present plant the absence of a mycelium speaks against its fungal nature, nor could it be at all taken for spores of Coniomycetes. As will be seen, these showed manifold stages of division similar to *Protococcaceæ*, and not the mode of growth of fungal spores. But above all, the presence of Chlorophyll places their algal nature beyond doubt.

These cells occurred embedded in various parts of the cortical tissue of the hair, more or less abundantly, sometimes more at one side, sometimes more at the other, occasionally causing even little cushion-like elevations, or again, by their numbers filling certain furrows or channels of the hair. The normal form of these cells in the hairs of the three-toed sloth was round, in the two-toed elliptic, but both often somewhat bluntly angular, owing to mutual pressure; the cell-nucleus very large. Like other allied forms, these showed a subdivision in all directions of space, as well as a mode of increase by many daughter-cells being formed within a common mother-cell (Sporangium, Kühn), by and by set free by the dissolution of the latter and seeming finally to burst through and away from the tissue of the hair. These two forms would agree generically with the genus *Pleurococcus*. Numerous adult specimens of sloths examined by Welcker all presented this parasite; they were not found in newly-born individuals.

The parasite in question, as regards its transmission, could gain access at first to the hairs through certain splits in the spongy cortical portion of the developed hair, which possesses hollow spaces. These they fill up by their growth and development.

One of these forms is marked by its sporangia containing 8—16 "spore-cells," furnished with sharply-bounded nuclei, the other by 16—32 spore-cells. He had not been able to distinguish cilia or any movement in those of these algæ.

He found two species, one occurring in *Bradypus*, the other in *Choloepus*, characterised as follows:—

Pleurococcus Bradypi, n. s., Kühn.

Vegetative cells mostly combined in groups, more rarely single, rounded, 0·009 mm. in mean diam.; sporangia numerous, round, 0·01 mm. in size; spore-cells numerous, rarely under 16, mostly as many as 32 in a sporangium; nucleus round, minute.

Pleurococcus Choloepi, n. s., Kühn.

Vegetative cells combined in groups, more rarely single, oval, or elongate, mostly irregularly angular owing to mutual pressure, 0·01 mm. long, 0·005 mm. broad, sporangia less frequent, oval, 0·0013 mm. long, 0·01 mm. broad; spore-cells less numerous, mostly 8, never more than 16, in a sporangium, nucleus of considerable size, roundish, laterally compressed.

This will be probably the first case of a *Chlorophyllaceous* alga occurring as a parasite in an *animal* tissue, if, indeed, *Zooglœa capillorum* be not a case in point.[1] That plant, occurring in human hair, between cortex and cuticula, was called by its discoverers a fungus (although agreeing in general characteristics with *Palmella*), mostly on account of its peculiar habitat. Kühn rather supposes even the yellowish-red colour appertaining thereto may be, not as Buhl assumed, dependent upon nutriment drawn from the hair and blood-colouring substances, but upon the presence of erythrophyll (as in *Palmella cruenta*, &c.) He takes this, therefore, for a *Palmella*, and more correctly to be designated *Palmella capillorum*, than as *Zooglœa capillorum*.

Two other forms, referred to algal genera, have been recorded as occurring attached to human hairs used in the manufacture of "chignons," and it is unnecessary to do more in this place than refer thereto—*Pleurococcus Beigeli*, Rabh. et Küchenm. and *Glœothece tricophila*, Rabh.[2] It is just possible that the precise nature of these, judging from the figures given by Dr. Beigel,[3] may be questionable, as they seem somewhat heterogeneous, for along with "unicellular," they represent likewise "mycelioid," growths, which latter would seem foreign to a *Pleurococcus*. The cells are not distinctly described as *green*, even by Dr. Rabenhorst (*P. Beigeli* "sometimes with a light green tinge"). It would

[1] A. Martin: 'Zeitschrift für rationelle Medizin,' 3. Reihe, Bd. xiv, p. 359.

[2] In "Sitzungsberichte der 'Isis'" (1867, April—June, p. 51).

[3] "On the so-called Chignon fungus (*Pleurococcus Beigeli*, Rabh. et Küchenm.)." 'Journal of Botany,' vol. v, pp. 189, 312; see also pp. 246-8.

appear, however, that these plants are truly parasitic, and being described as algæ, an allusion to their literature would seem to be called for in the present *résumé* of recorded Parasitic Algæ.

Contributions *to the* Anatomy *of* Auerbach's Plexus *in the* Intestine *of the* Frog *and* Toad. By Dr. E. Klein. (With Plate XVIII.)

Besides the well-known, larger or smaller isolated groups of ganglion-cells occurring in the branches of Auerbach's plexus there exist other ganglion-cells between the circular and longitudinal muscular coat of the large intestine of the frog and toad, which seem to represent a system of their own. Before entering this subject I will first describe the method by which I have succeeded in demonstrating these cells.

The large intestine of a toad or frog—the latter being preferable—is cleared of its contents by injecting through it ½ per cent. saline solution by means of a glass tube provided at one end with an india-rubber tube and drawn out at the other end like a canula and bent near this end at an obtuse angle. This is, in short, a glass tube like the one I use for injecting the frog's bladder with chloride of gold (see 'Handbook for the Physiological Laboratory,' p. 53). The large intestine is ligatured at both ends, one end being tightened, the tube, filled with ½ per cent. solution of chloride of gold, is introduced at the other end, and the intestine is moderately distended by injecting the gold solution into its cavity. The second ligature having been then tightened as well, the large intestine is excised and placed in ½ per cent. solution of chloride of gold for forty-five minutes, then, the intestine having become stiff, it is cut open and placed in distilled water for two or three days until it has assumed a perfectly dark colour.

The intestine is divided into several pieces, from which the peritoneum, together with the longitudinal muscular coat, is stripped off by means of pointed forceps. With a little practice one can obtain large continuous strips; it is, however, difficult to succeed in this if the intestine has been excessively distended. The strips, which contain also the circular coat, are of no great use, though it is in many instances possible to remove the latter by stripping it off in

bundles. I must, however, add that it is of no use to strip off the peritoneum with the longitudinal muscles without removing, at the same time, Auerbach's plexus; it can be easily recognised, even with the naked eye, as a plexus of cords stained red or violet. If successful strips or membranes are obtained they are placed in methylated spirit for five to ten minutes, and are then transferred into a weak solution of logwood. This latter I use for this and any other object, *e. g.* for staining all kinds of sections of injected or uninjected tissues, in a somewhat similar way to that prescribed by Professor Arnold.[1] I prepare the staining fluid in the following manner:—Six grammes of hæmatoxylin extract are mixed thoroughly in a mortar with 18 grammes of powdered alum; 28 c. c. of distilled water are gradually added while stirring; the whole mass is placed on a filter, and to the filtrate 1 drachm of spirit of wine is added. What remains on the filter can be replaced in the mortar and mixed thoroughly with 14 c. c. of distilled water, which is also gradually added. This is again filtered, and to the filtrate half a drachm of spirit of wine is added. The two solutions, being of nearly the same strength, can be mixed without difficulty. The mixture is kept in a stoppered bottle, and can be filtered again when, after a certain time, it contains a precipitate. The solution is used thus:—A common-sized watchglass is half filled with distilled water, to which are added, by means of a capillary pipette, six to eight drops of the staining fluid; the object to be stained is kept in this mixture for from a half to one hour; then it is placed in distilled water for a few minutes, and treated according to the manner in which it is intended to mount it.

This, our present object, viz. the muscular coat with the plexus of Auerbach, is at once mounted in glycerine. In such preparations the muscular fibres come out with very great distinctness, their nuclei being of a bluish tint. Not less distinct is the plexus of Auerbach; it can easily be traced, even to its minutest branches; they come out by the treatment just mentioned with very great clearness; they are of a reddish tint. As may be seen in fig. 1, each of the band-like branches contains within a sheath (which has remained unstained, and which is only recognisable by its numerous oblong nuclei), a various number of minute fibrils. Each branch represents, therefore, according to its breadth, a larger or smaller bundle of primitive nerve-fibrils. Where several such branches meet a more or less complicated decussation of

[1] See 'Quart. Journ. Micr. Sc.,' 1872.

these bundles takes place. A simple glance at fig. 1 will make this point much clearer than any long description.

The meshes which result from this exchange of smaller and larger bundles of fibrils within the individual trunks of the plexus contain ganglion-cells, either isolated or in groups, according to the number of those meshes, or, properly speaking, according to the complication of the decussation. These ganglion-cells, which vary in size, are easily recognisable by their large, generally spherical, sharply outlined nucleus, with its single or double nucleolus, and by their protoplasm being of a more or less dark purple colour. The smaller cells generally appear to possess only one process which can be traced from the protoplasm of the ganglion-cell between the fibrils of the nerve-trunk. The larger are distinctly multipolar, their protoplasm being provided with a number of fine processes, or, as is oftener the case, with one large and several small processes.

In many instances I have been able to distinguish around these ganglion-cells a capsule of a spherical or ovoid shape. In these cases the body, as well as the processes of the ganglion-cell, were lying within the capsule.

This we may call one system of ganglion-cells, as being situated in meshes, which are formed *by the individual bundles within the nerve-trunks*. But there is a second system of ganglion-cells, situated in meshes, which are formed *by the nerve-trunks of the plexus themselves*. These ganglion-cells[1] are of a much larger size than the former; they are twice and three times as large, are multipolar; their protoplasm, which is distinctly fibrillar with granules between the fibrils, is provided with one or two long, thick processes and several short and thin ones; generally also the processes are branched. The general shape of these cells is oblong, the latter being commonly provided on two opposite poles with a thick long process. The cells are generally isolated, sometimes situated in the centre of a mesh, or more commonly near a nerve-trunk that borders the mesh on one side. In some preparations I have met them more numerously than in others, and in several cases I have found that almost every larger mesh of the plexus contained one of these cells. The relation of their processes to the surrounding tissues is a very interesting one; it is this: every one of the ganglion-cells is connected with a nerve-trunk of the plexus by at least one process.

[1] The first occasion on which I saw these ganglion cells was when examining a preparation of the large intestine of a common frog, prepared by Mr. E. A. Schäfer.

In fig. 3 such a ganglion-cell is seen to join a small nerve-branch by means of two short fibrillar processes. The other processes are of a twofold character; some are seen to dip between the muscular fibres after a longer or shorter course, gradually becoming thinner; others—generally the longer ones—spread out at once into a thin plate, which for the most consists of an indistinctly fibrillar substance. This latter contains an ovoid or spherical, distinctly outlined nucleus with clear contents, and one or two nucleoli. In many cases the nucleus is marked by a more or less deep constriction. What becomes of the substance of these placoids at the distal end I am unable to say, for there its outlines, gradually fading away, cannot be traced with sufficient distinctness (see fig. 2).

In a very few of the nerve-trunks of the general plexus isolated medullated nerve-fibrils are seen to pursue an almost straight course from one trunk into the other (fig. 1), and dividing into two. I first thought that there existed, perhaps, some relation between these medullated nerve-fibres and the ganglion-cells last mentioned; soon, however, I found that this is not the case, for not only is a connection between them not to be demonstrated, but also the very limited number of the former is in no proportion to the number of the latter.

A Further Contribution *to the* Natural History *of* Bacteria *and the* Germ Theory *of* Fermentative Changes.

By Joseph Lister, F.R.S., Professor of Clinical Surgery in the University of Edinburgh. (With Plates XIX, XX, XXI.)

In April of this year I communicated to the Royal Society of Edinburgh some of the results of a protracted investigation into various circumstances connected with the appearance and growth of minute organisms in fermentable substances.[1] During the time that has since elapsed I have continued to prosecute the enquiry, and have obtained various new and striking confirmatory facts, a selection from which will form the subject of the present paper.

In the former communication observations were related

[1] For an abstract of the chief points of this communication "On the Germ Theory of Putrefaction and other Fermentative Changes," see 'Nature,' July 10th and 17th. It is in course of preparation for publication *in extenso*, in the 'Transactions of the Royal Society of Edinburgh.

which led me to conclude that in some minute species of hyphomycetous fungi, the spores (conidia) produced upon their filamentous branches germinate in three distinct ways; first, they may form comparatively thick sprouts, which become young plants, like the parent; second, they may multiply by pullulation like the yeast plant, and under some circumstances this toruloid growth[1] may continue for an indefinite period, though the resulting progeny will, under favouring conditions, reproduce a fungus like the original; and, thirdly, the conidia may shoot out sprouts of exquisite delicacy which break up into bacteria. In accordance with this mode of origin of bacteria it was shown that such organisms, like the fungi from which they are derived, are of various totally distinct kinds, manifesting their differences both morphologically and still more physiologically by the characters of the fermentative changes to which they give rise, and by the circumstance that some sorts refuse to grow at all in media in which others thrive. Some of the species exhibited most remarkable variations in size, form, and movement when introduced into different media, and sometimes gave indications of their fungoid origin by indubitable branching, and, in the thicker forms, by the presence of nuclei or vacuoles. Yet, however much any such modification might differ from the form in which the species was seen in another medium, the latter variety could be reproduced at pleasure by reintroduction into the habitat in which it was originally seen.

Hence any classification of bacteria hitherto made, from that of Ehrenberg to that of Cohn,[2] based upon absolute morphological characters, is entirely untrustworthy. In order to determine the species of any particular specimen it is necessary to take into account not merely its appearance, but also the character of the medium in which it occurs. Even then mere morphology will often entirely fail us unless we are able to ascertain the physiological characters. And even these appear by no means constant; for we shall in the

[1] Considering the differences among authors in the use of the term torula, it seems justifiable for the sake of convenience to retain the old sense, as applicable to organisms like the yeast plant.

[2] It is, however, only just to Prof. Cohn to state that he dwells largely upon the different physiological effects of different supposed species of bacteria, and sometimes makes them a ground of classification, more especially in the group of "pigment bacteria," which he distinguishes from others on account of the remarkable colouring matters to which they give rise. Nevertheless he relies in the main on absolute morphological characters. See "Untersuchungen über Bacterien," von Dr. Ferdinand Cohn. 'Beiträge zur Biologie der Pflanzen,' Zweites Heft, Breslau, 1872.

[For an abstract of this memoir see *supra*, p. 156.—Eds.]

present paper see reason to believe that one and the same bacterium may differ at different times in its fermentative effects on one and the same organic solution.

It is obvious that to trace the modifications of any one such organism through a series of successive habitats would be an utter impossibility if bacteria or any kind of fungi were liable to be evolved from the mere chemical constituents of the liquids employed; and thus the investigation, though not undertaken for the purpose of combating the doctrine of spontaneous generation, has afforded the strongest possible evidence against it, and in favour of the germ theory of fermentative changes. For even in organic liquids such as milk, in which spontaneous generation has been said to be most liable to occur, it required only a rigorous attention to experimental details to ensure the complete absence of either organic development or fermentative change, except where organisms were intentionally introduced. But when this was done, the particulars species used for inoculation grew unmixed with others, attended by the chemical alterations characteristic of it.

In order to enable the reader to give credence to my statements, it is essential that I should describe in detail the mode of procedure in its most improved form. Let us take, as an example, the case of boiled milk. The first thing to be done is to ensure that the interior of the vessel in which the liquid is to be heated shall be free from any living organisms. This is done by subjecting a Florence flask to a very high temperature, after providing that the air which enters on cooling shall be effectually filtered of living dust by passing through asbestos which, I find, answers this purpose quite satisfactorily. The asbestos is placed, in a mass about a quarter of an inch thick, between two layers of tin foil sufficiently broad and long for wrapping round the junction of the neck of the flask and a glass cap that covers its mouth; and when it has been so arranged, fine iron wire is tied tightly round, so as to compress it firmly as well as retain it in position after the outer layer of tinfoil has been dissipated by fusion and oxidation. The flask, previously mounted in strong wire for convenience of holding with forceps and for suspension, is then roasted thoroughly over a large Bunsen's burner and hung up by its wire mounting to cool.

The next point is to introduce the milk without contaminating any part of the flask except the lower portion that receives the liquid. For this purpose a funnel is used sufficiently long to reach from some distance above the mouth of the flask to its bottom; and the exterior of the tubular

part of the funnel is freed from living organisms by wiping it with a cloth, soaked in a strong watery solution of carbolic acid (one part of the acid to twenty of water), and drying it with a carbolized rag prepared by immersing it in a solution of one part of the acid in a hundred parts of anhydrous sulphuric ether and allowing the ether to evaporate.

This is much more convenient than heating the thick glass of the funnel, as I did in my earlier experiments; and I may add that throughout this investigation I have found great advantage from thus substituting the use of chemical antiseptic means for the employment of a high temperature when the former happens to be more convenient. And I may remark incidentally that the results have afforded most conclusive evidence of the efficiency of a strong watery solution of carbolic acid for destroying minute organisms; for throughout the whole course of the experiments I have found cleansing with such a lotion exactly on a par in this respect with exposure to the gas flame.

The tube of the funnel, thus freed externally from living germs, is passed down to the bottom of the flask, the asbestos having been previously removed and the glass cap lifted after wiping its margin with carbolic lotion for the chance of any organism having been applied to it in the process, and a piece of carbolized rag being wrapped round the mouth of the flask and the funnel to exclude living dust, the milk is poured in so as to fill not more than the lower half of the body of the flask. The funnel is then withdrawn through the rag, scrupulous care being taken that its extremity, now contaminated with the milk, does not touch the side of the flask. A substantial piece of cotton-wool carbolized in the manner above described is then tied over the mouth of the flask to filter any air that may regurgitate during the next stage of the process, the heating of the milk. This is done by immersing the body of the flask in a saucepan of boiling water and retaining it there for about an hour, care being taken that the boiling water never sinks below the level of the milk. By this means we are sure that the milk has been throughout exposed to the temperature of 212° F. for the period desired, while the earlier parts of the process give us equal assurance that the whole interior of the flask above the milk is free from living organisms. The immersion of the flask in a bath of boiling water, for which I am indebted to a suggestion of Mr. Godlee, of University College, London, has three advantages over boiling by direct flame; it avoids frothing, which in the case of milk is extremely troublesome, and also the sputtering to which Dr. Roberts, of Manchester,

has drawn attention;[1] it prevents any loss of water by evaporation, and so disposes of the vexatious question of whether the specific gravity of the liquid has not been so raised as to render it unfavorable for organic development; and lastly, it avoids any "burning" of the milk with its accompanying chemical changes.

The milk having been allowed to cool completely, a portion of it is decanted off into experimental glasses. These are plain "liqueur glasses," each provided with a glass cap shaped like a small evaporating dish (made to order at any glass work) and covered with a small glass shade standing on a square piece of plate glass. The glass plate has the double advantage of allowing the glass to be removed without disturbing the glass shade, and also of preventing the air beneath the shade from acquiring an accidental odour such as is derived from wood or other porous substances, and interferes with judging of chemical changes by the sense of smell.

The glass shade and glass cap have in combination the effect of perfectly excluding all living dust, although, as neither cap nor shade is made to fit closely, a constant free interchange by diffusion between the air in the liqueur glass and the gases of the atmosphere is permitted. Hence, provided always that the liqueur glass and its cap are free from living organisms to begin with, and that the contained liquid is similarly circumstanced, the latter will remain for an indefinite period unchanged except by gradual loss from evaporation, till at length in the course of months it dries up into a solid mass.

Further I have found as a matter of experience that if the glass shade and cap are raised, in a part of the room free from draughts, for the purpose of inoculation of the liquid or withdrawal of a small quantity for examination, there is practically no risk of the accidental introduction of organisms, provided of course that the operations be nimbly executed and that any piece of apparatus introduced into the glass have been suitably purified. For it appears that organic germs are not nearly so abundant in the atmosphere as is sometimes assumed, and only a very small fraction of the portions of dust with which the air of an occupied room is loaded have such germs adhering to them. Thus, in one instance the sole result of exposure of a glass of uncontaminated urine for half-an-hour in my study was three plants of three different kinds of filamentous fungi, each growing from one point and enlarging thence in all directions, while the liquid remained otherwise unchanged in aspect, a fact which may

[1] See 'Nature,' Feb. 20th, 1873.

probably be stated equally truly by saying that, of all the many particles of dust that fell in during that period, only three at most contained a germ capable of growing in urine.

Hence while it is most true that scrupulous care must be taken in these experiments, and that forgetfulness or slovenliness in their execution would be absolutely fatal to success, yet it is equally true that by the very simple means which I am now describing the observations may be made with a facility and precision that leave nothing to be desired.

The glass plate and shade are simply washed and dried with a towel, but the liqueur glass and its cap must be purified by heat like the flask. This is very simply done by bringing both to a high temperature over two spirit lamps or Bunsen's burners, the liqueur glass being held in the hand by its foot and the cap in a pair of forceps; and the cap having then been placed on the glass, a substantial piece of cotton-wool with a bit of muslin beneath it (neither carbolized) is placed on the cap and tied firmly with fine iron wire round the glass beneath. The heat of the glasses ensures the destruction of organisms in the lower part of the cotton, which acts as a perfect filter during cooling; and though the muslin may be browned by the high temperature, no empyreumatic odour is occasioned in the glass nor any deposit on its sides.[1] The glasses having cooled, the wire ligature is cut and the cotton carefully removed, the muslin beneath serving the purpose of clearing off all portions of cotton at once, and the glass and cap are immediately placed on the glass plate beneath the shade.

A series of experimental glasses, say a dozen, having been thus prepared, it remains to charge them by decanting from the flask, and this is a matter which, at the risk of appearing tedious, I am compelled to describe in minute detail.

The process is effected by means of a syphon of glass tube with a calibre of about one eighth of an inch, the shorter leg rather longer than the height of the flask, and the other leg four or five inches longer. I find the most convenient way of purifying the syphon is to boil it, and in order to adapt it for packing into a saucepan, the glass tube is interrupted at intervals of about four inches with pieces of caoutchouc tubing, the shorter leg having one such india rubber hinge, and the longer leg two. They are tied firmly on the tube

[1] It is only in my more recent experiments that I have thus employed the cotton, but there can be no doubt that while it scarcely adds to the trouble of the process, it must materially increase its security.

with fine wire, silver wire being used for the shorter leg, where iron might rust. In the longer leg one of the caoutchouc junctions serves the further important purpose of enabling the assistant to control the flow through the syphon by compressing the india rubber between the finger and thumb. A fourth piece of caoutchouc tubing is applied, without tying, to the end of the longer leg for adapting a syringe. The syphon thus constructed is filled with water and boiled for half an hour, and while it is still in the hot water, one of the caoutchouc junctions of the longer leg is seized with catch forceps (previously heated) to prevent the syphon from emptying itself when taken out. The longer leg being now raised from the saucepan by aid of another pair of heated forceps, the syringe, which has been washed out with carbolic lotion, and the nozzle passed through the flame, is applied to the terminal caoutchouc adapter. The shorter leg is next raised, and at once slipped through a hole in the middle of a piece of carbolized cotton wool, and then into the flask (whose cotton cap has been previously loosened, so as to be ready for removal), and the end of the leg being kept a little above the level of the liquid, to avoid mingling of the water in the syphon with it, the cotton is tied round the neck of the flask and the syphon. Then, as the syphon is intended to be left permanently adapted to the flask to serve for future decantings, it is needful to provide against the access of organisms to the moisture between the india rubber junctions of the longer leg and the glass tube. For this purpose, the catch forceps being removed, carbolized cotton wool is wrapped round each junction, and a piece of rag over this to enable it to resist wear, and tied securely round the glass tube above and below the caoutchouc. The syphon is now emptied of its water by means of the syringe, and the shorter leg being pushed down till its extremity is in the liquid, the syringe is again brought into operation till the syphon is seen to be full of milk. The assistant then compresses one of the caoutchouc junctions through its cotton investment, to prevent the milk flowing out when the syringe and its adapter are removed. This is done with fingers dipped in the carbolic lotion, and the apparatus is completed by slipping upon the glass tube that now terminates the syphon a circular piece of thin caoutchouc, about two inches in diameter, with a hole in the centre just large enough to admit the tube, so that it remains in position without further fixing. This caoutchouc plate is to serve as a screen to keep dust out of the glasses while they are being filled. To keep it level it is strengthened by a fine wire run through and

through near its margin, and, to ensure freedom from living organisms, it is steeped for half an hour or so in the strong carbolic lotion; after which, as caoutchouc has the property of imbibing carbolic acid into its substance, the screen when dried retains a sufficient quantity of it to ensure the destruction of organisms that may come in contact with it. The experimental glasses, which as yet are covered with their shades at as short a distance as possible from the syphon, are successively exposed and charged, each being brought close to the syphon before the glass cap is raised, and then at once placed with its margin in contact with the caoutchouc screen, while the end of the syphon extends into the glass. The assistant is now directed to relax his hold upon the caoutchouc junction above, when the milk at once flows into the glass, and when this is about two-thirds filled, the flow is again arrested by the assistant, the glass removed, the cap, held in the other hand of the operator, is reapplied, and the glass placed again under cover of the shade.

All the glasses having been charged, the caoutchouc screen is slipped off and a piece of carbolized cotton tied over the end of the syphon, which being raised to a higher level than the fluid in the flask the assistant finally relaxes his hold and the syphon empties itself into the flask, becoming occupied by air filtered by the cotton tied over the extremity. When at any future time another set of glasses are to be charged, all that is needful is to remove the cotton wool from the end of the syphon, readapt the syringe by means of a caoutchouc adapter, steeped for a short time in carbolic lotion, and then proceed as before. In this way we avoid the great loss of time involved in providing a fresh syphon for every fresh decanting, as I did in the earlier experiments.

The other experimental fluids employed in the observations about to be related were Pasteur's solution, turnip infusion, an "artificial milk," consisting of a solution of sugar of milk and white of egg in water, and urine.

In preparing the Pasteur's solution for this set of experiments I deviated from Pasteur's formula in two respects; viz. the proportion of the water and the source of the mineral salts. I doubled the quantity of water, so as to make the liquid, as I hoped, more favorable for the growth of some organisms, more especially after loss by protracted evaporation as occurs in my experiments, and tap water was employed instead of distilled, so as to afford greater variety of saline material. For the yeast ash, which every one who has tried must have found extremely troublesome to prepare, I substituted the same weight of ashes left after burning a

large amount of loppings from various kinds of trees and shrubs; the liquid obtained by lixiviation being filtered, and a quantity used in proportion to the estimated weight of dissolved solids. It seemed to me that the salts obtained in this way would be more likely to afford suitable pabulum for the growth of different organisms than those derived from one particular species of fungus. Thus, my Pasteur's solution had the following composition :—

Water from the tap . .	5000 grains.
Lump sugar	250 ,,
Tartrate of ammonia . .	50 ,,
Salts from wood ashes . .	5 ,,

It happened that the alkalinity of the ashes exactly counterbalanced the acidity of my specimen of tartrate of ammonia, so that I had a perfectly neutral solution to work with. The flask was prepared and the fluid introduced as above described for milk, but the boiling was done by the direct flame and was continued only ten minutes.

The turnip infusion was prepared by boiling peeled white turnips, in about enough water to cover them, till they were soft, reducing each to a mash with a little additional water, filtering, and keeping the filtrate at 212° F. for half-an-hour, as in the case of the milk.

The "artificial milk" required special preparation. A solution of 160 grains of milk sugar in ten ounces of tap water, which is about the proportion in milk according to 'Miller's Chemistry,' was subjected to the temperature of 212° F. for an hour and a quarter in a flask prepared and arranged as for the milk. Next day, the fluid being of course cold, I added five drachms of the white of a raw egg, the shell of which had been treated twelve days before with one to twenty carbolic acid solution for an hour and twenty minutes and then wrapped in carbolized cotton, a process which, I may remark, preserves eggs from putrefaction, apparently for an unlimited period, although the carbolic acid leaves the cotton in a few days, and that which was applied to the egg shell does not penetrate sufficiently to produce any coagulation whatever of the albumen; and I have lately eaten an egg which had been prepared in this manner more than three months before, and for the last fortnight had been kept at 100° F. A large pipette having been purified by heat, and protected from the entrance of dust in cooling by means of carbolized cotton, a plug of which in the upper end served the further purpose of preventing the entrance of organisms into it from a syringe with which it was connected by

means of a caoutchouc adapter, a small hole was made in the shell of the egg with carbolized fingers and heated knife, and the narrow end of the pipette being inserted between the yolk and the shell, and a piece of carbolized cotton wrapped round the pipette so as to cover the orifice and exclude dust, almost all the white was extracted without interfering with the yolk, and transferred at once to the sugar of milk solution in the flask, the cotton round the pipette serving as a temporary screen, for which a substantial cap of the same material was substituted on removal of the pipette. Twenty-four hours later, the flask having in the interval been occasionally agitated to diffuse the albumen, a syphon was introduced with the peculiarity that a piece of sponge was tied over the end of the shorter leg to serve as a filter for excluding the shreddy undissolved residue of the albumen, the sponge being of course purified by the boiling. The artificial milk was thus obtained with only trifling turbidity when decanted into experimental glasses, and the stock in the flask has remained unchanged to the present time (Sept. 1873) more than three months after it was prepared.

The urine was not boiled at all, but was obtained altogether unaltered by a very simple process, depending upon what appears to be a fact of high interest both physiologically and pathologically, that a mucous canal in a state of health does not permit the growth of foreign organisms in its immediate vicinity, so that preliminary external application of a carbolic lotion (1 to 40) is sufficient to ensure an uncontaminated state of the fluid, which, with its unaltered mucus, is a much more favorable nidus for organic development than after boiling.

One other piece of apparatus requires a short notice, viz. that used for withdrawing fluid from the experimental glasses for inoculation or examination. The most convenient means for this purpose I have found to be what may be called a "syringe pipette," consisting of a small syringe with a piece of glass tube connected with it by a caoutchouc adapter, the junction being self-supporting but yielding (as distinguished from rigid). This last property permits the use of a very delicate tube without risk of breakage when it touches the side of a glass; and it is of great importance that the tube should be of as *thin* glass as possible. It can then be heated fully when dry by once drawing it quickly through the flame of a Bunsen's burner, and a few seconds suffice for its cooling. The tube, which is about a line in diameter, is drawn out a little at the end, and is bent at an obtuse angle about two inches from the syringe; so that the latter is not held over the liquid during the process. Care

is taken not to drive any air from the syringe into the tube after heating the latter, and rather more of the liquid than would suffice for inoculation is taken up, so that the part left in the tube may protect that which is ejected from air from the syringe.

To the general reader these details may seem almost unpardonably minute, but for any one who is desirous to repeat similar experiments I venture to hope they will not be found so.

On the 14th June I drew off for the first time some milk from the flask which was exhibited to the Royal Society of Edinburgh in April as having its contents still fluid, and therefore probably unaltered, though prepared seven weeks previously, and under difficulties as compared with the material of later experiments, inasmuch as it was boiled by the direct flame of the lamp, the extreme inconvenience occasioned by the frothing of this flask having led to the suggestion of the boiling water bath above described. Also the cotton wool over the mouth was not carbolized, a piece of muslin between the cotton and the flask being alone treated with the etherical solution of the acid. Nevertheless, the cotton filter had proved efficient in spite of the often repeated rapid rushing of air into the flask which must, of course, have occurred whenever the lamp was removed to prevent the froth from reaching the cotton. For the milk when decanted just four months after the boiling, proved perfectly good, having a slight flavour of turnip as might be expected of winter milk; its reaction showed the peculiar character now known to be possessed by that fluid when fresh, purpling blue as well as red litmus paper, and the microscope showed no appearance of organisms or of the granular masses of deposited casein often seen as an early indication of fermentative change, while the milk globules were bright and unaltered.

These observations were made upon the first two or three drachms that flowed from the syphon, received into an unprepared glass, as should always be done to wash out any residual water from the tube, and thus ensure uniformity of the contents of the experimental glasses. Of the latter, one was at once exposed in my study by removing the shade and glass cap to receive any organisms that might fall into it, and was covered again with cap and shade after fourteen hours, including the night and early morning in which the furniture was "dusted" with a cloth by the servant, but the glass carefully avoided. It was then placed beside the other glasses in a cupboard, the temperature of which varied from about 65° to 70° F.

On the 20th of the month I observed for the first time a delicate filamentous fungus on one part of the side of the glass, extending upwards from the milk for about an eighth of an inch; and at the same time a semitransparent layer which had been noticed for about two days previously at the surface of the milk was found to have increased in thickness. Two days later this layer had attained a depth of $\frac{1}{6}$ inch, and I proceeded to investigate its nature, thinking it probable that it might be a change induced by the growth of the fungus. But on trying to take up a portion with the syringe pipette, I encountered a most unexpected difficulty in extreme viscidity of the liquid. I had before observed the effects produced upon milk by thirteen different organisms, including six distinct kinds of bacteria, but though the products had differed extraordinarily in colour, reaction and consistence, viscidity had in no case been witnessed. Here, however, the upper part of the milk had been converted into the most viscid substance I ever saw. When I at length succeeded in extracting the pipette without any of its contents getting upon the outside of the glass, I found that on touching any object with the delicate end of the tube and withdrawing it, the tiny drop became extended into a thread a foot and a quarter in length, as delicate as the finest spider's web and barely visible from its tenuity. I afterwards amused myself with spinning webs from one object to another. When dry they exhibited considerable tenacity, and thicker ones broke with an audible snap when subjected to longitudinal traction, while the finer ones floated like gossamer in the air. Here, then, was an amazing chemical change effected in the milk, and one of great interest with reference to the elaboration of mucus and other viscid secretions in the animal economy. On applying the microscope I found no fungus filaments, but multitudes of motionless bacteria, such as are represented in Pl. XIX *m*, very minute and delicate, and often showing a peculiarity only badly represented in the specimens drawn, viz. that of having one part of the organism of much higher refractive power than the rest. In the lower part of the glass similar bacteria were seen in active movement, often curiously wriggling and sometimes rotating completely round a transverse axis. The reaction of the milk was also changed, distinctly reddening blue litmus paper and not affecting red.

Next day I introduced into another of the glasses of milk a morsel of the viscid substance by means of a pair of mounted needles passed through the flame. A glass of the

artificial milk above described, which had been decanted for seventeen days and had undergone no change, and a glass from a flask of Pasteur's solution which had been prepared on the 11th of February and remained brilliantly clear, were also similarly inoculated.

In the course of two days observing a translucent layer, about a line in thickness, at the top of the milk in the second glass, I removed some for examination. It was distinctly acid in reaction but uncoagulated, and when a drop was diffused on a glass plate the liquid was seen to be generally thin and turbid, but studded with transparent specks which, when touched with the point of a needle, could be drawn out into threads like the viscid material of the first glass On applying the microscope to one of the transparent specks, multitudes of motionless bacteria were seen, such as are represented at *o*, Pl. XIX, shewing in a striking manner the peculiarity before described, of having their extremities of different refractive power from the rest. The thin turbid part, on the other hand, was a finely granular fluid in which similar bacteria were seen in much smaller numbers, some of them moving freely, while others were motionless, the latter being each surrounded with a transparent halo of greater or less extent as is shewn at *p* and *q*, Pl. XIX, and in some cases, the transparent areas surrounding different bacteria were confluent. These were evidently miniatures of the transparent specks visible to the naked eye; and they seem to me beautiful examples of a change effected by bacteria in the surrounding medium, whether due to vital action of the organism or to some substance (a so-called chemical ferment) emitted from it during life or after death.

The moving bacteria, it is to be remarked, had no transparent area around them, nor were they able to penetrate those that surrounded the motionless ones, proving the substantial character of the latter.

The artificial milk and Pasteur's solution were turbid the day after inoculation: and in the former, which I examined microscopically, were seen active bacteria of extreme minuteness, looking like mere pairs of granules, which on the following day had given place to others of larger size and of the same sort of characters as those of the milk, as shown at *n*, Pl. XIX. Similar bacteria were also seen at this time in the Pasteur's solution. But neither then nor at any subsequent period was there any viscidity of the general liquid in either of these glasses, implying that the viscid substance was no essential appendage of the organism, but the result of its fermentative action upon particular materials.

It is, however, to be added that in the course of the next month a deposit occurred upon the sides of both these glasses such as I never saw under any other circumstances, constituting a film which, in the artificial milk, resembled coagulated fibrin in its toughness, and in the Pasteur's solution was tenacious though not viscid, as if the motionless bacteria which constituted the deposit in each case had been glued together by a minute quantity of some intervening substance.

The next observation which I have to record has reference to the origin of bacteria. It will be remembered that a filamentous fungus made its appearance on the interior of the first milk glass six days after its exposure. The growth continued to spread, and by the tenth day, as it had a bloom indicating probable fructification, I scraped off a small portion from the glass by means of a tenotomy knife washed with strong carbolic solution and dried in the flame, and examined the specimen in a drop of water with the microscope. It proved to be a fungus of great beauty composed of very delicate branching filaments (*a*, Pl. XIX), bearing spores (conidia) often septate, characterised by a raw sienna tint (*c*, Pl. XIX) which was often distinctly seen to be confined to an external envelope, affording, what is unusual with fungi of such minuteness, the means of definite recognition, and of ascertaining with precision the three modes of germination above alluded to (see p. 381). Many of the spores were seen to have produced thick sprouts to form young plants. Of these *d* has been sketched because it happened that, while part of the brown envelope had been consumed in the process of germination, a portion still remained for identification. Other spores were observed in toruloid pullulation, as is seen at *e* in a mass still connected with the parent filament, and at *g* in a free and septate spore, while *f* was either a spore multiplying by pullulation, or a young plant of a brown colour. For here and there young plants were seen like *b* retaining the brown investment of the spores; and hence, as a dark coloured coat of threads and spores is the special character of the order Dematiei among hyphomycetous fungi, and as de Bary has given the name *Dematium pullulans* to a closely allied microscopic fungus,[1] I have ventured to suggest for this species the name *Dematium fuscisporum*. Further, the spores were often seen to give off exquisitely delicate threads as at *i* and *k*, while in *h* we have a combination of this delicate sprouting with toruloid pullulation

[1] See 'Morphologie und Physiologie der Pilze,' &c. Von Dr. A. de Bary, Leipzig, 1866, p. 183.

in the same spore. Finally, there were observed in abundance among the filaments free bodies like *l* exactly resembling in form, size and refractive power portions of these delicate sprouts. Some of them, not sketched, were seen to be branched, and yet, though in this respect and in the absence of the double rod-like character they deviated from the most typical form of bacteria, their bacteric nature was rendered indubitable by characteristic movement observed in several instances. I may add that in *k* that which is sketched as a branch of the delicate sprout was seen to oscillate from the position indicated to that of the dotted line, as if about to detach itself; though this is an observation to which I do not wish to attach much importance, as the same appearance might possibly result from accidental adhesion of a previously free bacterium. Taking the observation as a whole it affords proof positive of three distinct modes of germination of the spores of one and the same fungus, while there seems little reasonable doubt that the third mode was the source of the bacteria.

It will be remarked that the bacterium which grew thus abundantly among the filaments of the *Dematium* on the dry glass differs entirely in appearance from that which was found in the milk and produced (as I think we are justified in saying) the viscous fermentation. And there is reason to think that they were in reality two entirely different species, and that the one derived, as it appears, from the *Dematium*, (or some other exactly like it morphologically), which I have indicated in the plate as *Bacterium No. II*, existed in the milk along with that of the viscous fermentation (*Bacterium No. I*), though the latter took the precedence in development, so that the former escaped notice in the first instance; as so commonly happens when germs of different kinds are introduced together into the same medium. For having inoculated a glass of fresh urine on the 30th July with a portion of the viscid material from the second milk glass, the product which first showed itself five days later by dimness of the liquid had none of the characters of *Bacterium No. I*, but resembled in elongated and curved form as well as in dimensions the one derived from the *Dematium*, see Pl. XIX, *Urine, 4th August*. It was of course conceivable that the appearances in question might be merely the result of a modification of *Bacterium No. I* by the new medium in which it grew; the other alternative being that two bacteria had existed together in the milk, but that *Bacterium No. I* was either incapable of growing in urine or had lost its vitality during the five weeks which had elapsed since its

introduction, while *Bacterium No. II* had survived. The last appears to have been the fact; for on inoculating milk and Pasteur's solution with the new Bacterium, while it throve in both it retained the characters that it had in the urine and occasioned no viscidity of the milk. And further, when introduced into artificial milk, in which *Bacterium No. I.* grew so rapidly, *Bacterium No. II* failed to grow at all, the fluid remaining unchanged for the twenty-six days during which it was kept under observation.

Some other points were observed regarding *Bacterium No. II* which appear of sufficient interest to be placed on record. When first seen in the urine it was unbranched, and exhibited rotatory movements; but when again observed two days later it was found of larger size, and often distinctly branched, see Pl. XIX, *Urine, 6th August,* and entirely destitute of motion. On this day a minute drop of the urine containing the organism in this condition was introduced into a glass of turnip infusion decanted from a stock of that liquid which was prepared on the 24th of February, and had then furnished the supply for twelve experimental glasses, but which retained its original characters as regards aspect, fresh odour, and faintly acid reaction, while the microscope revealed no organisms. After two days bacteria made their appearance of the characters shown in Pl. XIX, *8th August,* resembling those first seen in the urine in being unbranched, and even more active than they, with wriggling onward movement. Two days later the bacteria were again motionless and of larger size, and often manifestly branched, see Pl. XIX, *10th August,* the turnip infusion having now acquired a smell like that of strong turnip soup. Again four days later, the glass shade having lost all smell, I supposed the fermentation to be over; but on examining a drop I was surprised to find that bacteria were present in abundance, but that all the large and branched ones had disappeared, and in their place was a progeny more minute than any seen before, showing sometimes the double rod form most characteristic of bacteria, see Pl. XIX, *14th August,* and exhibiting active movements of rotation and wriggling. The only explanation that suggested itself to my mind was that some material of limited amount in the turnip infusion yielded under the fermenting influence of the bacteria a volatile product (the same, perhaps, that caused the soupy smell) which, while it remained, exercised a modifying influence upon the organism, resulting in the branched and motionless variety, but on escaping, as indicated by the odourless state of the fluid, left the bacteria to return to their former shape

and active movements. And this view was confirmed by the result of inoculating a second glass of the turnip infusion from the first on the 14th August, when the bacterium had the minute and active state for the second time. For precisely the same series of changes of the organism was then repeated, as is sufficiently shown by the sketches, Pl. XIX, *August 15th, 18th, and 20th.* I dwell upon these circumstances because they afford an example of modification of bacteria under different conditions of the same medium, and also an instance of branching, which has been spoken of by Cohn in his recent work as something altogether foreign to this class of organisms.[1] I also venture to hope that facts like these will tend to give the reader additional confidence in the trustworthiness of the mode of investigation.

One other circumstance with regard to *Bacterium No. II* seems deserving of mention. As already stated, when introduced into a glass of boiled milk, it grew rapidly, having after three days the appearances shown in Pl. XIX, *Milk, 18th August,* with active movement. There was, however, up to this time no change in the aspect, odour or reaction of the milk. But in the course of a few days the upper part of the liquid assumed a peculiar golden yellow tint, and a fortnight after inoculation the appearance was almost as if the yolk of a bantam's egg were floating on the surface, while there was also some similar yellow material deposited at the bottom of the glass, and the main body of the milk had assumed a cream colour. The reaction was now distinctly though not strongly acid, but the glass shade had no sour smell, a very faintly urinous odour being the only one perceptible. The main body of the milk was a very soft coagulum, but the upper part was a thin transparent liquid, the bright yellow material being deposited at the junction of the two. On examining a portion of the yellow substance with the microscope, I could discover nothing but a mass of motionless but unbranched bacteria such as are shown in Pl. XIX, *1st September,* and I could only conclude that the bacteria were themselves of yellow tint though too minute to show it under the microscope. Yet it is a curious circumstance that the same bacterium in Pasteur's solution had not this colour, but produced a pale pink tint by the deposit which it formed at the bottom of the glass. At this period I was obliged to suspend my observations, but from what had been seen in the last few days it appeared that the bacteria were converting the coagulum into a transparent

[1] Op. cit., p. 139.

liquid, for the upper translucent layer was daily increasing in thickness. On looking at this time at the second milk glass, in which the viscous fermentation had occurred at an earlier period, I found that the viscid upper part had changed to a similar golden yellow colour, and under the microscope I found that *Bacterium No. I* had disappeared, and given place to *Bacterium No. II.* This yellow colour in milk I never saw caused by any other organism.

The last observations which I have now to relate refer to the commonest of all the fermentative changes to which milk is liable, that which results in the rapid evolution of lactic acid, and consequent precipitation of the casein in the form of curd, a change which was attributed by Pasteur, so early as 1857 to the operation of a special organism.[1] The frequency of this change in milk does not, however, appear to depend on specially extensive dissemination of the ferment, but rather upon the circumstance that the organism which we are about to study, when it does gain access to milk, takes the precedence of others in development, and that dairies being places in which this particular ferment abounds, the milk supplied from them is sure to contain it, as they are at present managed. For it is a remarkable fact, and one well worthy of the consideration of the dairyman, that while milk supplied for domestic use will turn sour in summer weather within twenty-four hours, yet of all the many instances in which I have observed alterations in milk caused by organisms introduced through atmospheric exposure, in no single case did the true lactic acid fermentation occur. Some organisms have given rise to a primary alkaline alteration, strong or feeble, some have been neutral in their effects, while others have produced an acid condition indeed, but only feeble and slowly developed.

It seemed worth while before closing this investigation, in which fermentative changes in milk had occupied a prominent position, to apply our method of inquiry to the most frequent and therefore the most interesting of them all. Accordingly on the 14th of last month, August, I obtained from a dairy near Edinburgh, pervaded with the usual sour smell, about a pint of milk said to have been taken from the cow four hours previously and tasting perfectly fresh, the dairy woman bailing it out with a tin vessel from the pan in which it stood into a clean glass bottle which I had provided. One hour later about ten ounces were introduced into a flask purified by heat, and were sub-

[1] "Mémoire sur la Fermentation appellée Lactique," 'Annales de Chimie et de Physique,' 3me série, tome lii, 1858.

jected to the temperature of 212° F. for three-quarters of an hour, the arrangements being such as have been fully described above, see p. 382, and on the following day four experimental glasses were charged each with about half an ounce of the milk by means of a permanent syphon (see above). The first milk that came from the syphon, received into another glass, had the taste of perfectly fresh boiled milk, it purpled both blue and red litmus paper, and exhibited under the microscope nothing but milk globules of all sizes including extreme minuteness. Meanwhile, the milk remaining in the bottle had undergone the usual change. At noon, twenty-three hours after it was taken from the cow, it tasted distinctly sour though still fluid, and sharply reddened blue litmus, and on microscopic examination motionless bacteria were seen in considerable numbers, of soft or delicate character, in pairs, fours, and chains (*Leptothrix* filaments) as represented at *a* in Pl. XX. The milk examined was in a wine glass into which it had been poured from the bottle, and this was kept covered till 5 p.m. when a small drop was taken out for inoculation of one of the glasses which we may term *Boiled Milk I.* It was now more sour to the taste, and more sharply acid to litmus, and when diffused between plates of glass exhibited small white masses which the microscope showed to be granular (deposited casein) while the motionless bacteria before observed were again seen in abundance. The glass also contained some larger portions of soft curd. Next day at 8.30 a.m., or fifteen and a half hours after inoculation, *Boiled Milk I.*, though unaltered in appearance, had communicated a faintly sour smell to the air under the glass shade, while the smell of boiled milk was gone. A drop removed by pipette reddened litmus more than on the previous day, though still faintly blueing red paper, and under the microscope motionless bacteria were seen in considerable numbers exactly similar to those observed in the fresh milk, except that there was greater variety in their size, some being considerably larger, as shown in the plate at *b*. At 5 p.m., twenty-four hours after inoculation, the glass shade gave a pleasant smell of slightly sour milk, and the reaction was sharply acid, but the milk was still fluid, and next morning rather more than thirty hours after inoculation the milk had set into a solid mass.

On the same day (15th Aug.) that Boiled Milk I was inoculated as above mentioned, parallel experiments were made with turnip infusion and with urine, each of which received a minute drop from the same glass of sour milk. The turnip infusion was from the stock prepared in February,

having both naked-eye and microscopic appearances unchanged; and the urine was a glass prepared at the same time as that used for *Bacterium No. II*, retaining unimpaired in every respect the characters which it then had, seventeen days before. Neither of these glasses showed any signs of bacteric development on the 16th, the day after inoculation, but on the following day both were manifestly nebulous, and both exhibited under the microscope numerous motionless bacteria. There was, however, a remarkable difference between the organisms in these two glasses. In the turnip infusion the bacteria did not differ very greatly from those in the boiled milk, except that the leptothrix form was very seldom seen, and that the segments of the pairs were sometimes of greater length, while unjointed specimens, also pretty long, made their appearance, as at *c*. In the urine on the other hand the deviations from the form in the milk were most remarkable, as will be sufficiently evident from an inspection of the plate under *Urine I.* Some indeed, like *d*, were not very different from the original leptothrix form, but even such specimens often exhibited, as that one does, an elongated state of some of the segments of the chain, thus forming connecting links between the leptothrix and the widely different spirillum-like specimens such as *e*. Next day the same sort of appearances were again seen, and an observation made on the previous day was confirmed, viz. that vacuoles were present in the thicker specimens. This is well shown in the sketches *g* and *h*, in all of which there is also a further deviation from the type which has been lately held to be invariable in the entire group of bacteric organisms, and from whence the name schizomycetous, as applicable to a totally distinct order of fungi, has been derived, that is to say these bacteria, instead of multiplying by trasverse fission, are plainly increasing by pullulation, that is to say, by shooting out buds after the fashion of the yeast plant; and it will be observed that these sprouts are by no means always in a line with the long axis of the organism from which they spring. Yet that they really were the same bacterium was evident, not merely from transitional forms, but from specimens such as *f*, in which in one and the same chain we have the leptothrix character combined with the long and thick vacuoled and pullulating organism. Similar observations were made on the following day; and now even the smallest and most bacteriform specimens sometimes exhibited a minute vacuole, as is shewn at *i*. These appearances did not startle me as much as they would have done had I not seen something

almost exactly similar in an earlier part of the investigation, though in another species of bacterium under totally different circumstances.

Thinking it worth while to try how this organism would behave if transferred from the urine to Pasteur's solution, I used for that purpose some of the old February stock, still perfectly bright, inoculating on the 18th. Next day the fluid was distinctly nebulous as examined before a candle, and under the microscope I found motionless bacteria, not numerous, but obviously of new formation from the delicacy of their aspect, represented at *k*, in Pl. XIX, where they are seen to be of considerable thickness and length of the segments, which present a curious alternation of lightness and darkness in their substance. Though a pair and three are given in the sketch as well as a single one, solitary individuals were much the most frequent. Such was the appearance twelve hours after inoculation, but when twelve more hours had expired a very great change had taken place. Not only were the bacteria much more numerous, but very much smaller; and instead of being commonly single, were invariably double, having in fact the ordinary appearance of minute bacteria (see Pl. XX, *l*), and to complete the metamorphosis some of these bacteria were seen swimming actively in ordinary bacteric fashion. Two days later the liquid was considerably increased in opacity and I was struck with what I had never seen before in Pasteur's solution, a sort of dirty or dingy appearance, as if a very small quantity of ink had been mingled with the liquid, and the deposit at the bottom of the glass, which was white on the previous day, had now the same dingy cast. Under the microscope the bacteria appeared much as on the last occasion, except that some were even more minute than any then were, so that it was impossible to say, except by their movements, that they were anything more than mere granules (see *m*, in Pl. XX). At the same time active movement was more frequent than before.

I now thought it well to ascertain whether these minute and active bacteria would reproduce in urine the same sort of organism as that which we could not but believe to have been their parents in that fluid. On this occasion, having no more of the fresh urine, I adapted a syphon to a flask which was prepared on the 1st of March, and had furnished the material for numerous experiments, yet retained its original brilliancy as well as odour unaltered, was distinctly acid to litmus and displayed no organisms under the microscope. Twelve hours after the inoculation on the 21st the liquid was already manifestly nebulous, and on examination with the

microscope bacteria were found, four or five in every field, differing from those that had been introduced in being very rarely double but long and large and often curved *(vide* Pl. XXI, *a,)* having thus returned to a considerable extent to the condition before seen in urine, but now differing from their former state in that fluid in frequently exhibiting characteristic though languid movements. After twelve hours more the previous condition in urine was still more closely approximated by greater length in the segments, as illustrated by *b,* sketched because it happened to be at rest, though by no means having the longest unbroken segments that were observed. I now inoculated from this glass of urine another (*Urine III.*) that had been decanted on the same day and had remained till then unchanged; and twelve hours afterwards I sketched from this second glass the magnificent example of unjointed spirilliform organism represented at *h.* At the same time languid movement was seen in many specimens.

To complete the history of the behaviour of this organism in urine it may be added that, after the lapse of another fortnight, the bacteria in this glass were found again motionless and comparatively small, scarcely differing in appearance from those originally seen in the sour milk *(vide* foot of Pl. XXI).

With the view of determining precisely the identity of the minute organism in the Pasteur's solution with the large one in urine, I stocked as follows, on the 21st August, a "glass garden" consisting of a massive piece of plate glass excavated by the lapidary into a broad and deep ditch around a central island, the ditch to serve as a reservoir of air. This glass, together with a thin covering glass, had been exposed to a high temperature between metallic plates to diffuse the heat and avoid cracking, and cooled without access of dust. With heated forceps the covering glass was raised and, a minute drop of the Pasteur's solution with its organism having been mingled with a large drop of urine on a glass plate purified by heat, a little of the mixture was placed on the island. The covering glass was then luted down with melted paraffin, applied, with a hot steel pen, after a drop of water, boiled and cooled under the protection of carbolized cotton, had been placed in the ditch with the pipette to ensure a moist atmosphere. Immediately after this had been done I examined with the microscope and saw the minute bacteria of the Pasteur's solution as shewn at *c,* in the Plate, in active movement. On looking again five hours later I found those bacteria replaced by large ones as seen at *d,* still moving

though the movement was now languid. Within this short time the one variety of the organism had been converted into the other. Even if we supposed that the thick ones were of a different kind and that one of them had been present originally in the garden unobserved by me, their large numbers at the end of five hours and the vanishing of the small ones would be equally inexplicable. Hence, I think, we may regard it as demonstrated that the minute bacteria of the Pasteur's solution and the coarse ones of the urine were one and the same organism.

Other more remarkable facts, however, remain to be recorded. On the morning of the 22nd August, wishing to ascertain whether this organism, after being so strangely modified in urine, in Pasteur's solution, and then again in urine, retained the property of inducing the lactic acid fermentation in milk, I introduced a minute drop of *Urine No. II* into a second glass of milk decanted at the same time as the former, and which we may designate *Boiled Milk II.* Nine hours later test paper already indicated a slight degree of acidity, and bacteria were found, five or six in each field, about as thick as those in the urine of inoculation, and also pretty long, generally single, but sometimes double as shewn at *e*, Pl. XXI. On looking at the same slide four hours later I found that other bacteria, much more minute and shewing active progressive or rotatory movements, were also to be seen, and next morning such minute and active ones were alone discernible in another drop taken for examination. The acid reaction was now more marked, and the acidity continued afterwards to increase, till within three days the milk had set into a solid mass.

But along with the lactic acid fermentation another and very different change took place in the milk during the first twenty-four hours. On first looking at the glass on the morning of the 23rd, twenty-one hours after inoculation, I was amazed to see at the bottom of the glass a deposit about a line in apparent thickness as black as pitch, shewing out in a glaring contrast to the white milk. The black material did not appear to undergo any increase in the course of the day or at any subsequent period. But there was a peculiar sickly, almost putrefactive, smell mingled with the sour odour of the air in the glass shade in the course of the next twenty-four hours, though this afterwards passed off, and by the time that curdling was complete a pure smell of sour milk was alone perceptible. On the 26th I turned out the curd to investigate the black substance. I found it adhering firmly to the bottom of the vessel so that it could be completely cleansed of the curd with a camel's

hair brush without being detached; and when I picked it out with a knife its lower surface had a brilliant polish corresponding to that of the glass. It constituted a tough scale, between horny and leathery in consistence, and its upper surface presented numerous smooth round depressions with intervening ridges; and it was plain that the pigment had been precipitated in the form of a heavy liquid, the particles of which had coalesced at the bottom of the vessel and afterwards solidified. The intensity of the colour was strikingly brought out by microscopic examination under my highest power, when even parts of extreme tenuity, as at *g*, Pl. XXI, distinctly shewed the sepia tint of the mass. These very thin parts also afforded the opportunity of ascertaining that the substance was perfectly homogeneous and structureless. In other words, the dark substance was not a coloured organism, but a pigment formed from the milk as the result of the growth of an organism in it. The small amount of the material at my disposal permitted me to ascertain only that it was insoluble in water, spirit of wine, anhydrous ether and a strong solution of caustic potash, both in the cold and boiling states of these fluids, and was also unaffected by cold nitric acid, but was dissolved by boiling nitric acid, to which it communicated a yellow colour. Heated in a glass tube with access of air it burnt without fusion, leaving a white ash.

The question of course presents itself, what was the cause of this remarkable formation of pigment from the milk? That it was induced by an organism introduced into the milk we cannot doubt. But was that organism the same bacterium that in the former glass of boiled milk, as in the original stock of unboiled milk, produced only the lactic acid fermentation, but altered in function while modified in form by its residence in the other media, or was it some other species, some "pigment bacterium," to use Professor Cohn's expression, coexisting with the lactic acid ferment? Before discussing this question I must direct attention again to the glass of Pasteur's solution from which the second urine glass was inoculated. It may be remembered that at the time of that inoculation there was already present a dingy or dirty aspect about that glass such as I had never before seen in Pasteur's solution. Next day this peculiar appearance was considerably increased, and on applying a pocket lens, I discovered a number of minute dark brown specks disseminated over the glass, even close to the level of the liquid where the surface was vertical; each brown point having a tiny brown streak extending downwards from it. I succeeded in picking up one of these

brown specks with the attenuated end of the pipette, and on examination found it made up of a mass of motionless bacteria of ordinary form, themselves colourless, but having sepia-coloured particles disseminated among them of the same tint and intensity of colour as the pigment from the milk, very irregular in form and varying in size from mere points, much smaller than the bacteria, to masses considerably larger, as is seen at *n*, Pl. XX, showing that the pigment, though produced under the influence of the bacteria, as seems clearly indicated by its existing specially among the bacteric masses, yet was, as in the milk, a mere amorphous and unorganized product. Thus, we trace back the pigmentary function to the Pasteur's solution, through the urine, although in the latter no pigment whatever was formed. This is in itself a point of interest, as indicating that the formation of pigment is not essential to the organism, but, just as in the case of the viscid substance produced under the influence of *Bacterium No. I*, occurs only when the medium in which the bacterium is growing is of a nature fitted for furnishing the requisite materials. Further the knowledge that the organism which produced the pigment was present in the Pasteur's solution and in the urine will aid us in considering the question whether that organism was or was not a different one from the lactic acid ferment, and this we may now proceed to discuss.

Supposing it to have been a separate organism, it is not at all likely that it found its way by accident into the first urine glass or the Pasteur's solution during the brief periods of exposure for inoculation or withdrawal of fluid for examination. For in no single instance have I known bacteria introduced before in this way. Nor can it have existed diffused through the original supply of milk, seeing that no pigment was produced in that stock or in the glass *Boiled Milk No. I* inoculated directly from it. We can only imagine it introduced from the original supply by supposing that it had entered the unboiled milk immediately before the inoculation of the first urine glass, and was all taken up in the drop used for the purpose; a contingency possible but not probable.

But even if we admitted that, in spite of the slenderness of the chance of such an occurrence, a separate "pigment bacterium" had made its way accidentally into the first urine glass or that of Pasteur's solution, we should find ourselves confronted by a further series of improbabilities. We should have to suppose that the two bacteria thus coexisting in the two fluids were both modified in form in the same manner by

the two media, both becoming coarse and long-segmented in urine, and both minute and of ordinary bacteric aspect in the Pasteur's solution; for none of the minuter kind were seen in the former fluid, nor any of the coarser sort in the latter. Further, we should have to suppose that the "pigment bacterium," when introduced into milk, grew with great activity for twenty-four hours and then suddenly perished. For we have seen that no further deposit of pigment took place after the first night, although the milk remained fluid considerably longer, and on microscopic examination of a drop from the upper part of the glass next day, when granular masses of casein showed that coagulation had begun, I discovered not a vestige of pigment in it. And in further proof that the pigment bacterium, supposing such a separate organism to have been present, had died, I found that a bit of the curd introduced from this glass at the close of the third day into another glass of the same boiled milk, gave rise to the lactic acid fermentation, pure and simple, with no formation of pigment, and none of the putrid odour that had attended the pigmentary formation in the other glass. It may, perhaps, be suggested that the "pigment bacterium" was poisoned thus early by the lactic acid generated under the influence of the other (supposed) organism. But unfortunately for such a view, we find the same transient character of the pigmentary function in urine as in milk. For, as has been before mentioned, the day after the inoculation of *Boiled Milk No. II* from *Urine II* (resulting in the pigmentary fermentation), I introduced a drop from *Urine II* into another glass of the same urine with the result of reproducing in great beauty the long unjointed form of the bacterium. After two days more I inoculated from this *Urine No. III* a fourth glass of the boiled milk, in the hope of getting back the pigmentary formation. But no such thing occurred, merely the lactic acid fermentation. Now it is scarcely conceivable that the "pigment bacterium" (supposing it present) should have perished so quickly in the urine as well as in the milk. For it is to be remarked that the urine was but little changed by the bacteric development that followed the inoculation, retaining its acidity at the close of the two days, while little effect was produced upon its odour. Besides this it must be borne in mind that, if the supposed "pigment bacterium" was derived from the original stock of sour milk, it had before survived a residence for three days in urine, which was the fluid originally inoculated.

On the other hand, if we admit that there was only one organism present, but modified in function as in form by the

different media, the course of events is exactly what we might have anticipated. It was in the Pasteur's solution that the pigmentary function first manifested itself, not indeed during the first thirty-six hours, during which it is distinctly recorded that the deposit in the glass was *white*, but in the course of the next day; and it is natural to suppose that it was in this medium, in which the form became so greatly modified, and at the same time the function of active motion conferred upon the previously motionless organism, that the faculty of pigmentary fermentation was also acquired. Then, just as modifications of form assumed by a bacterium in any one medium are more or less quickly lost when the organism is restored to its previous habitat, so should we expect it to be with altered function, and this bacterium, when transferred from the Pasteur's solution to either milk or urine, would more or less quickly lose the new fermentative property which it had acquired.

One clear instance of acquisition of a new function by the bacterium is presented by the power of active movement which shewed itself for the first time in the Pasteur's solution; so that if we were to adopt the language of some authors who have attributed a most exaggerated importance to movement as a distinctive character, we should say that the organism was converted in that fluid from a bacteridium to a bacterium. But when restored to urine, the organism moved but languidly and after about two days became again motionless. In milk, on the other hand, the power of motion was more permanently retained, and active movements were observed both in the third and the fourth glass of boiled milk as late as five days after the organism had left Pasteur's solution.

There is another consideration which seems strongly confirmatory of the argument against a distinct "pigment bacterium" as the cause of the black deposit in the milk. If it were true that such an organism existed, which, when introduced along with the lactic acid ferment, would produce this striking effect, black milk would be a thing of frequent occurrence; whereas this is, so far as I am aware, the first time such a thing was ever seen. But if it be asked, why was it that this unheard of appearance showed itself in my experiment? the answer is that the conditions of the experiment were such as to afford the organism opportunities which it had probably never had before. Never before, in all probability, was this organism allowed to develop unmixed with any other in urine and Pasteur's solution consecutively. For while this ferment takes the precedence of others in

milk, such is far from being the case in urine, and very probably in Pasteur's solution also. How far the previous residence in urine may have predisposed this bacterium to assume the pigmentary fermentation in Pasteur's solution, further experiment can alone decide. Suffice it to say, meanwhile, that the conditions under which the organism grew were novel, and therefore novel appearances need not surprise us. The case seems exactly parallel to that of *Bacterium No. I.* Never before, perhaps, was milk converted into so viscid a material as it was under the influence of that organism, simply because other organisms which would have interfered with the viscous fermentation were for the first time excluded.

I have dwelt at what will, I fear, be thought tedious length upon this discussion, because the conclusion arrived at seems to me of extreme importance. For if the same bacterium may, as a result of varied circumstances, produce in one and the same medium fermentative changes differing so widely from each other as the formation of lactic acid and that of black pigment in milk, it becomes readily conceivable that the same organism which under ordinary circumstances may be comparatively harmless, may at other times generate products poisonous to the human economy. We can understand, for instance, a thing that has at an earlier period of my practice as a surgeon often puzzled me, though now, happily, under the antiseptic system of treatment, I never have occasion to witness it, viz. the development of hospital gangrene beneath dressings left for a long time unchanged, whereas in the same hospital ward sores dressed daily continued healthy. Assuming what analogy leads us to suspect, that some organism is the cause of the disease, why should the special virus of hospital gangrene become introduced into a sore under the former condition more than under the latter? We now see that it is not essential to assume the existence of a special virus at all, but that organisms common to all the sores in the ward may, for aught we know, assume specific properties in the discharges long putrefying under the dressings. Similarly, we can imagine the unhealthiness of an old uncleansed hospital as caused not by the introduction into it of new organisms, but by a modification of those common to it and to freshly built institutions. I take these illustrations from surgery; but to the medical reader others of equal importance will readily suggest themselves from physic.

Another peculiarity of the glass of Pasteur's solution remains to be mentioned besides the formation of pigment

in it, viz. a *putrid* smell which I never observed before in that fluid, and at the same time, a remarkable taste, a combination of slight bitterness with astringency, the latter so marked as to lead me to test for gallic or tannic acid with a persalt of iron, though without effect.

Admitting then that we had here to deal with only one bacterium, it presents such peculiarities both morphologically and physiologically as to justify us, I think, in regarding it as a definite and recognisable species for which I venture to suggest the name *Bacterium lactis*. This I do with diffidence, believing that up to this time no bacterium has been defined by reliable characters. Whether this is the only bacterium that can occasion the lactic acid fermentation, I am not prepared to say; but it seems most unlikely that any other kind will be found combining all the peculiarities of that which we have studied. What fungus it is derived from, if, indeed, it have come from any (for it would be rash to assume that such an origin is universal), I have no means at present of knowing; but, however that may be, it cannot but be right, where we have definite characters of bacteria, to speak of them as species as a matter of convenience, just as is done of various hyphomycetous fungi known to be only inferior varieties of ascomycetous forms.

What are the functions of bacteria with reference to the physiology of fungi, and whether a bacterium derived from a fungus is ever capable of returning to the form of its parent, are questions on which my investigation has thrown no light.

The sketches which furnished the illustrations were all drawn on the scale given at the foot of Plate XXI, either by camera lucida or, in a few cases where the objects were in motion, by eye-piece micrometer, the magnifying power being 1140 diameters. The object-glass which I employed was a tenth immersion lens manufactured by Messrs. R. and J. Beck, the beautiful definition of which was distinctly enhanced by the use of the higher eye-piece.

On a Peach-coloured Bacterium—*Bacterium rubescens*, n.s. By E. Ray Lankester, M.A., Fellow and Lecturer of Exeter College, Oxford. With Plates XXII and XXIII.

In the histological laboratory of Exeter College I observed, during the past summer, in two jars of river water which had been standing on a window-sill, but were protected from direct

sunlight, and which contained the putrescent remains of some caddis-worms, besides various minute algæ, a purple-red growth. This growth consisted of a film which spread itself over the decaying matters at the bottom of the water, and of patches forming an irregular coating on the glass of that side which was most exposed to the light. During the long vacation the same growth developed itself in great abundance in a large vessel in which I had left two crayfish (*Astacus*). The animals had died and were far advanced in putrefaction when I returned to Oxford in August, and at the same time the whole of the sides of the vessel and the remains of the *Astaci* were coloured with a film of a fine purple-red tint.

The observations which follow on the characters of the organism to which this purple-red film was due were made upon samples from the first-mentioned jars. The spectroscopic characteristics of the colouring matter to which they give rise were not ascertained until the second larger quantity of the growth came to hand. I am unable to determine whether this organism has been described before. As far as the identification with any previously existing name is concerned this is not a matter of any moment. What I am desirous of recording with regard to this claret-coloured growth is of interest in connection with the natural history of those highly important and as yet little known plants—the Bacteria; for the organism which gives rise to these coloured films belongs to that group. It is of very little consequence whether we have here to do with a new species or not.

The publication of Schröter's and Cohn's observations on chromogenous Bacteria—an abstract of which appeared in the April number of this Journal—must everywhere have excited a new interest in the investigation of animal and vegetable colouring matters.

The application of the spectroscope to the examination of the coloured substance contained in the blood of various animals, of the green substance of plants, and of various blue, green, and red bodies, characterising various animal and vegetable organisms, has yielded a number of curious results, which are at present, and must probably long remain, in the condition of isolated facts, devoid of any explanation or general significance.

The observation of the formation of coloured products characterised by definite absorption-bands, by the agency of Bacteria, offers new data for speculation as to the chemical antecedents of such products, and the significance of colour in organisms generally. But though I was led to examine the purple films in my jars of putrescent matter chiefly

through the hope that the colouring matter might exhibit some characteristic optical properties, the morphological features of the growth have proved to be of more importance than what relates to the special properties of the colouring matter belonging to it. In fact, owing to their possession of this characteristic colouring matter, I have been able to recognise a number of *phases* or *form-species* of this remarkable *Bacterium;* and I shall first speak of these, and then allude to the spectroscopic characters of the colouring matter. On account of the deep purple-red colour which this *Bacterium* gives rise to when *en masse*—very similar in appearance and mode of occurrence to the crust in a bottle of Burgundy —I call it *Bacterium rubescens,* though, as seen through the microscope, the masses have a warm peach colour.

Bacterium rubescens.

Forms assumed by the plastids or units.—Those who are acquainted with Cohn's paper ('Beiträge zur Biologie der Pflangen,' Breslau, 1872), or the abstract in this Journal, April, 1873, will probably suppose that the generic term Bacterium is here used as limited by him. I am, however, obliged to use it in a wider sense, since the organism which is here spoken of as *Bacterium rubescens* appears to give evidence that a "natural species" can exhibit many of the forms which Cohn has classified under distinct genera and families. It will be remembered that Professor Cohn distinctly guards himself against implying by his proposed nomenclature that the forms thus distinguished by him are *natural* or *physiological* species; and on this account it is, I think, a matter for regret that he should have used some terms which were employed by their authors with the implication which he disavows. The series of forms which I have found in the growth of *Bacterium rubescens* leads me to suppose—what has appeared probable to other persons in regard to the Bacteria generally—that the natural species of these plants are within proper limits 'protean.' Like the protean organisms most recently exhibited in their true light —I mean the Calcareous Sponges so thoroughly worked out by Ernst Haeckel in his beautiful monograph—the Bacteria present themselves in a certain number of forms, and these forms differ in the features of the form-units (spherical, oblong, &c.) and in the mode of aggregation of the units. Similarly the Calcareous Sponges exhibit a certain amount of variation in the elementary forms of their cup-like "persons," and a large variety of modes of conjunction or aggregation of those persons to form complex sponge-masses.

Amongst the Calcispongiæ these architectural characters had been to some extent distinguished by zoologists and generic and specific distinctions were based upon them, on the supposition that *natural* species were thus indicated. In the same way among the Bacteria we have Cohn's four tribes and their included genera, based on distinctions of elementary form, (which, however, he does not put forward as necessarily a natural grouping), and he has recognised certain of the modes of aggregation of these form-units, *e. g.* Mycoderma-form, Zoogloœa-form, &c., as belonging to the category of "phases of growth," which might be presented by any number of natural species.

The natural species among the Calcispongiæ have been shown by Haeckel not to correspond at all with the series of *forms* distinguished by his predecessors. He finds in the characteristics of the peculiar spicules produced by the plastids of the sponge-mass the surest indication of specific distinction, because such spicules are in their nature less dependent on the variations of surrounding conditions than is the mode of extension or aggregation of the sponge-mass, and are rather the expression of the most intimate inaccessible chemical life of the plastids. Nevertheless, he is careful to retain a complete and concise enumeration of the various "artificial species," "form-genera," or "phases," in which the natural species may manifest themselves, and, consequently, for the Calcispongiæ there results a double nomenclature, a series of natural genera and species, and a cross series of artificial genera and species.

It seems exceedingly probable that the same manner of regarding the Bacteria will have to be adopted, Cohn's tribes and genera taking the position of an artificial or formal system, whilst the natural species must be based upon some of those more profound characteristics which Cohn has himself indicated to us in his divisions—saprogenous, chromogenous, pathogenous.

The indications of the limits of natural species do not lie under our hands in the case of the Bacteria, as they do in the Sponges with their spicules, but have yet to be sought out.

It is, perhaps, hardly necessary to remark that the actual demonstration that such-and-such a series of very different-looking forms all are phases of one and the same natural species, really involves the observation of the development of the forms in question one from another *under the eye* of the observer. When such observation is made, we have evidence of a superior class, which at once supersedes all inference

from assumed chemical or structural race-marks. The series of constantly recurring forms which constitute the ontogenetic cycle of the several species of plants and animals have, in a large number of cases, been actually followed with the eye from hour to hour, if not in the same individual specimen, at any rate in specimens as to the practical identity of which there is not a shadow of a doubt. The series of forms or phases, on the other hand, which a Protean species—and all species are so to some extent, but some species to a very obvious and readily appreciable extent—may exhibit have not an invariable recurrence as part of the fixed "life-cycle" of the species, but depend for their manifestation upon the occurrence of conditions in the environment of the species, which in most cases are not ascertained, and concerning which one thing is very clear, namely, that they do not recur in definite order. The forms of a Protean species are a series of *adaptations;* the forms exhibited in the development of a species from its egg are a series of *hereditary recapitulations.* In consequence of the irregular or, so to speak, accidental occurrence of Protean forms, those which are admitted by zoologists or botanists as belonging to one species — for example, some of the Sponges—have not been traced as "continuous" by simple watching of their growth. But, on the other hand, owing to the fact that the varied forms exhibited by the species are due merely to rapid adaptation, essential race-marks or characteristics (such as those of the spicules and canal system in the Sponges) are quite uninfluenced by the superficial changes of form, and enable the observer to *infer* with confidence the specific continuity of the series. The recapitulative series of an individual development, on the other hand, exhibits forms which have not the remotest indication, in any point of structure or composition, of their continuity, and that continuity can only, in the first place, be established by the direct observation of it, which is immensely facilitated by the orderly recurrence of the series.

The variety of forms (see Pls. XXII, XXIII) which I found in the purple films produced in the jars of putrescent animal matter at Oxford might be regarded each as representing a species of Bacterium-like plant. Such species could only have the value of artificial or form-species, for there is no evidence that these distinct forms reproduce themselves, and retain, as a race, their characteristics. On the contrary, various conditions were constantly observed intermediate between two of the more dominant forms. Accordingly, the assemblage of forms may be regarded in some way as continuous, and in that case they must either be a series of steps

in the ontogenesis of a specific form, or they are a number of phases, or "form-species," of a Protean organism.

I adopt the latter supposition because I have no evidence whatever of the definite recurrence of a particular order of succession in the production of these forms one from another.

The feature presented by the series of minute plastids occurring in the purple film, which, more than the occurrence of occasional transitions between different forms (consult the figures in Pls. XXII, XXIII), leads to the supposition of their specific continuity, is the possession of the peculiar purple-red colouring matter described below as Bacterio-purpurin. This colouring matter I regard in them as of the same kind of value as that which the particular spicule-forms have in the species of Calcispongiæ. It is the deep-rooted emblem of their common parentage, their race-mark. Had the series of forms figured in the accompanying plates been colourless, as are, for instance, the plastids of *Bacterium termo*, *B. lineola*, *Bacillus subtilis*, *Vibrio*, *Spirillum*, and others, I should scarcely have ventured to propound the hypothesis of their specific continuity. The jars in which the purple growths occurred teemed with other colourless forms belonging to the Cohnian genera and species above named, and every scrap of the growth examined under the microscope abounded with such colourless forms, in addition to the peach-coloured ones. Hence it was through their colour, and their colour alone, that the specific association of the various purple plastids was suggested, and it is only if the possession of this purple colouring matter be admitted as warranting the assumption of specific continuity that my observations have any further interest. It is, of course, possible to suggest various hypotheses of a contrary tendency. The colouring matter may be suggested to be independent of any of the bacterian forms, and merely to have impregnated them, or it may be developed by one form alone, and be acquired through imbibition by the others. Certain facts as to the diffusibility of the colouring matter of the *Monas prodigiosa* of Ehrenberg (*Micrococcus prodigiosus*, one of the *Sphærobacteria* of Cohn), mentioned by Schröter are suggestive of this. Fungi spores and mycelia were coloured by contact with a growing mass of this *Bacterium*. But in opposition to such an explanation of the occurrence of the various peach-coloured forms to be detailed below there are the following facts:—*a*. The colouring matter (Bacterio-purpurin) is insoluble in water, acid, or alkalies, only partially so, and with change, in alcohol or chloroform. *β*. Colourless growths of some extent (zoogloœa-condition of Vibrio and of Spirillum) occurred in intimate contact with

the purple films, and were quite free from any tint. It might also be hazarded that there is no reason why several distinct species should not, under the influence of common conditions, take on the formation of Bacterio-purpurin in their protoplasm. The reply to this is that in attempting the explanation of a phenomenon we are most likely, in the end, to obtain a true result if the simplest hypotheses are accepted, until it has been shown that more elaborate suppositions are necessary.

Hence I shall assume that the various aggregates of the scattered individual, peach-coloured plastids which occurred in the purple films, belong to one species, which, on account of the form of its most frequent phase, is best put under the old genus *Bacterium* of Ehrenberg, and may be called *B. rubescens*.

The size of the isolated cells, corpuscles, or *plastids*, as it will be most convenient to call them, of *Bacterium rubescens*, varies from less than the $\frac{1}{20000}$th of an inch in the case of those which have a spherical form, to the $\frac{1}{3000}$th of an inch, or somewhat more, in the case of oval, biscuit-shaped, or bacterioid forms, and in those which take on an elongated growth the long diameter is sometimes much larger than this. The variations in size are best seen in the plates, the figures on which have been drawn to a scale there given.

The forms of the plastids might be made the basis of a series of "form-genera," but I prefer to speak of them as—

1. Sphærous (figs. 3, 16, 18, 20, 26).
2. Biscuit-shaped [1]or Bacterioid (figs. 10, 11, 12, 13, 14, 15, 19, 21, 22).
3. Filamentous (figs. 24, 25).
4. Acicular (figs. 2, 28, 29).

To make this series of descriptive terms complete, so as to include all the known forms of plastids of which Bacteria of other species give rise to, we must add—

5. Bacillar.
6. Serpentine.
7. Spiroid.
8. Helicoid.

Inner structure.—The differentiation of the substance of individual plastids gives rise to characteristics which are among the first to strike the eye in the examination of growths of *B. rubescens*, and can be recognised also in other species of Bacteria, such as, for example, *B. lineola*.

The plastids may be cleanly moulded in free particles, or

[1] The 'biscuit' is the shape of the small sponge-cakes sold by confectioners; it is seen in the transverse section of the mammalian red blood-corpuscle.

they may be surrounded by a transparent, viscid substance, to which they themselves have given rise, as is frequent enough with the lower Algæ. Then they may be devoid of all further indication of differentiation—optically simple oblong or spherical particles of living substance—or they may exhibit a distinction into a "wall" and "contents," and the contents may occupy but a single chamber surrounded by the "wall," or there may be several minute chambers (looking like granules) excavated in the wall-substance. Thus, as far as the structure of the plastids is concerned, they may be—

1.—*a*. Naked (figs. 2, 11, 12, 13, 18, 19, 22, 27), or *β*. Glœogenous (figs. 10, 14, 15, 17).

2.—*a*. Homogeneous (figs. 10, 14, 15), or *β*. Loculate (figs. 3, 13, 18, 19, 21).

And these either unilocular or multilocular (figs 3, 12, 16, 17, 20, 22, 23, 24, 25, 28, 29).

The glœogenous and the loculate conditions are no doubt, to a certain extent, antagonistic. The loose jelly in which Bacteria make their appearance, as Cohn's Zooglœa-condition, is a phase of the development of the outermost layers of the living plastid; and where the growth of this part is such as to form a dense capsule or "cell-wall," the production of the looser jelly will not proceed to any extent. It is the fact that the largest amounts of jelly in *B. rubescens* are produced by homogeneous plastids. The addition of a coloured fluid to the microscopic preparation in which they are being studied renders a certain amount of such jelly also obvious around aggregations of multilocular plastids such as fig. 17. It would probably be wrong to deny in any case, except that of the motile forms, the existence of small quantities of viscid matter, forming a film on the surface of the plastids, by which even the most strongly-walled plastids adhere to form "mycoderma," or tesselate films (fig. 21).

Coloration.—The disposition of the colouring matter in the plastids of *B. rubescens* is a feature which naturally comes under consideration in connection with their structure. The colour is either—

1. Diffuse,
2. Locular,
3. Both diffuse and locular.

It never extends to the jelly in the glœogenous condition, and this is what would be anticipated from the fact that such "exudations" or "conversion substance" as these jellified cell-walls are not when once formed the seat of further nutritional operations, whilst, on the other hand, the Bacterio-pur-

purin is not diffusible, and is probably developed as the result of the most active chemical processes in the living matter where it is seen. If, as the analogy of such colouring matters suggests, we may regard the staining of the parts of the Bacterium-plastid with Bacterio-purpurin as evidence of the vitality of the parts so stained, we shall find interesting evidence of the gradual loss of living properties by the plastid wall in the different samples of homogeneous and loculate plastids given in the plates.

The homogeneous bacterioid forms in fig. 10 have a diffuse coloration, therefore a diffused colour-producing activity. Those drawn in figs. 12 and 20, whilst possessing the diffused property, have, nevertheless, developed special points of activity—as many as eleven in one plastid—where the coloration and, in all probability, the chemical movement of the plastid is of a more intense character. From such plastids we find a transition—(I do not mean to suggest that the one ever passes into the other; they may both diverge from the simple bacterioid)—to the spherical and bacterioid unilocular or capsulate forms by such monstrous growths as those drawn in fig. 22. By the coalescence of a number of minute irregularly placed cavities of activity the large cavities occupying the axes of these overgrown plastids are being formed, or the condition may be regarded as developing in the reverse direction, namely, small, irregularly placed cavities of activity may be supposed to be developing here and there in the greatly thickened wall-substance of a unilocular biscuit-form, which has taken on excessive individual growth.

One of the forms less frequently detected, and perhaps the most interesting of any—that drawn in figs. 2, 28, 29—the acicular multilocular plastid, precludes the supposition that the uncoloured part in the plastids of *Bacterium rubescens* has *necessarily* passed into an inert or dead condition.

Though no longer the seat of those nutritional changes which result in the production of Bacterio-purpurin, the colourless matter which, in this case, forms the length of the needle-like particle, studded at intervals with purple globules, exhibits an unmistakable sign of vitality. These acicular plastids *move* continually with great activity, wagging one of their extremities from side to side as represented in fig. 29, *a*. The movement is sometimes sufficiently slow for one to clearly follow with the eye the oscillations of the motile portion. The gliding of the minute needle-like bodies has a character of its own, distinct from the coarser and more violent locomotive efforts to which the broader bacterioid and bacillar Bacterians are addicted. It is clear that the con-

tractile property of these motile acicular forms of *Bacterium rubescens* must reside in the continuous colourless substance in which the row of four, five, or six purple dots is placed at intervals.

Aggregation.—The plastids of *B. rubescens* do not occur, as a rule, isolated, but forming films, encrustations, or tufts. With the exception of the acicular forms just described, they do not exhibit vital movement, and, consequently, are never to be found swimming in water, but accumulated in masses of growth. Quantities of plastids occur, which are free, but *associated* with other free plastids or aggregates (figs. 12, 20, 22, 26). Most are *aggregated* in adherent masses.

The forms of aggregation which can be recognised, and which may also be recognised (all or some of them) in other species of Bacteria are—

1. Linear.
2. Stellar.
3. Globose.
4. Massive.
5. Arborescent.
6. Catenular.
7. Reticular.
8. Tesselate.

The best example of linear aggregation of Bacterian plastids is seen in some of the so-called Leptothrix filaments of *Bacterium termo* (fig. 6) and *B. ulna* (fig. 4). When such filaments appear absolutely structureless, with such a magnifying power as Hartnack's No. 10 immersion, the addition of nitrate of silver, half per cent. solution to the slide, will, after a few minutes, bring out very clearly the outlines of the constituent bacterioid units. Bacterioid plastids are frequently thus linearly aggregated, and so are sphærous plastids, whence some of the forms of *Bacterium termo* known as rosary chains (Rosenkranz-ketten of Cohn). The transition from the filaments so constituted to filamentous plastids is complete. It is a simple question of the partial segmentation of the plastid as it grows, or the retention of its linear continuity. Hence the forms of *Bacterium rubescens* drawn in figs. 24 and 25 may be regarded either as filamentous multilocular plastids, or as linear aggregates of sphærous plastids. From the approximation of some of the purple "loculi" or cavities of activity it is apparent that they are increasing by a process of division, which may be compared to that of the division of chlorophyll-granules, now observed in so many plants. Since growth thus proceeds from each of the iso-

lated points of coloration, it will probably be right to consider the filaments as linear aggregates of sphærous plastids; but the forms drawn in fig. 22 show that the distinction has only a terminological value, as is also shown by the structure of the motile acicular plastids with their coloured beads, which are connected through these somewhat coarser beaded filaments with the whole of the remaining forms. The reticular aggregate of trilocular bacterioid plastids given in fig. 23 leads from these filamentous forms through fig. 19 to the simple unilocular bacterioid of fig. 21 or fig. 13, and the segmentation of the colourless wall of the filaments 24 and 25 around the spherical loculi would give a chain or catenular aggregate of such simple sphærous plastids as is figured in fig. 3. The sphærous plastid with a well-marked wall occurring either aggregated in simple chains, in arborescent chains (fig. 3), or in compact, irregular masses (fig. 18), is one of the most abundant, and, perhaps, the most typical of the phases of *Bacterium rubescens*.

The *stellar* condition of aggregation is only exhibited by the acicular plastids (figs. 2 and 28), and when in this condition they do not exhibit movement. Whether their radiate disposition is due to their having developed in this way by the modified growth of other plastids, or whether they have moved into these positions, I cannot determine, but the former supposition appears to be the more probable.

Globose aggregates are formed, both by the spherical and bacterioid plastids; but they are especially characteristic of the medium sized ($\frac{1}{5000}$ inch long) homogeneous plastids of the bacterioid form, and are in this case largely glœogenous (figs. 1, 14, 15).

In the first growth of *Bacterium rubescens* which I examined—that which appeared in a jar containing dead caddis-worms and living algæ—the globes of this character formed an encrustation over the glass side of the vessel, whilst other forms made their appearance more abundantly in the films which spread at the bottom of the vessel over dead twigs and other débris.

Fig. 1 represents some of these globose aggregates with accompanying green algæ, as they appeared on the field of the microscope, affording the brilliant contrast of colour which is seen in the plate. The globes vary in size, from the $\frac{1}{1000}$th of an inch in diameter, built up of but few plastids to those of the $\frac{1}{300}$th inch. Still larger globose aggregates occur, which are compound; that is to say, are themselves formed by the aggregation of smaller globose aggregates. Some of these are seen in fig. 1, and they attain

dimensions such as to render them visible to the naked eye as patches the size of small pins' heads. Such patches should be described as discoidal rather than globose.

The morphological doctrine of individualities is illustrated in a very simple way by these aggregates of Bacterian plastids: a single plastid becoming larger by growth than is compatible with the cohesion of its viscid substance, divides into two, four, eight, or more plastids held as one spherical aggregate of the second order by their glœogenous property. In time, owing to the fissiparous multiplication of the plastids, the secondary aggregate becomes too large to retain its individuality. Its cohesion in turn fails at various points, and the mass re-arranges itself, chiefly in virtue of the common property of adhesion, into a congeries of smaller spheres, held in a common jelly—a tertiary aggregate, but certainly one of a very low degree of integration.

The form of plastid which builds up these globose growths is apparently the most abundant of the forms assumed by *B. rubescens*. It seems to multiply with the greatest rapidity, and produce the largest amount of colour. These plastids are homogeneous oblong bodies of the form of *B. lineola*, and with a uniformly-diffused peach colour. In some the tint appears bluer, in others redder, perhaps due to variation in quantity merely, though more probably to the predominance of certain red or blue elements of the colouring matter. These plastids are not always packed in globose glæogenous aggregates, but are loosely scattered, sometimes in a thin jelly (fig. 10). They then attain larger dimensions than when closely packed, and are to be seen in obvious process of transverse division. Still larger plastids of the same general form occur (figs. 11, 12), free from jelly and isolated. In these cases "loculi," or globules of darkly-tinted matter, are to be observed, and I am inclined to think that the process going on in them is comparable to free cell-formation in a blastema. The "loculi" indicate as many "active" points, corresponding to so-called "nuclei" arising in "homogeneous protoplasm."

Smaller oval and sub-spherical plastids, having the same general characters and diffused coloration as those just described, also occur, forming irregular and globose aggregates (figs. 16, 17, 20). In some parts of the red growths plastids of this character were found in great abundance, and there can be little doubt that each plastid represents, *potentially*, as many units as it possesses coloured loculi. I did not observe any transitional state of development in which these multilocular plastids were breaking up into separate unilocular sphæroid plastids, which is a condition we might

reasonably expect to find. If these forms are, therefore, not simply in course of breaking up, but represent a more permanent condition, it is not, I think, difficult to suppose that a certain state of the environment might cause the assumption, on the part of the plastids, of this enlarged, internally segregated condition. The globose aggregates formed by these plastids, as well as the far more abundant ones formed by the homogeneous plastids spoken of previously, are glœogenous—they would be spoken of by Cohn as zoogloœa-conditions, a term which it seems advisable to replace by an equivalent adjective, especially since so many different forms of bacterian plastids may throw out a jelly-like matrix. It is desirable to notice that the homogeneous biscuit-shaped plastids and the multilocular plastids are both coloured throughout their substance by Bacterio-purpurin, and are, at the same time, both largely glœogenous. The entire coloration of the plastids, as stated above, would seem to indicate that there is in them nothing corresponding to a dead cell-wall; hence the multilocular plastids drawn in figs. 11, 12, 20, 22, have "wall" and "contents" in a relative and morphological sense only. But some of the smaller sphærous and bacterioid plastids have dense, highly-refringent walls, quite free from colour, and such wall-substance is relatively inert and incapable of giving rise to the quantity of mucilaginous jelly which is found around the through-stained plastids, but is almost entirely absent from the plastids with dense walls.

Globose aggregates are sometimes formed by such walled or unilocular sphærous plastids (*e. g.*, fig. 18), and we must suppose the surface of these units to be viscid in order to maintain the state of aggregation.

Massive aggregates of irregular form (fig 1, *c*), and often large enough to be visible to the naked eye, are the aggregates most frequently formed by the sphærous unilocular form of *B. rubescens*. In both the jars in which I found the purple films last summer such aggregates occurred, but in the second they appeared almost exclusively, and formed a dense scum on dead leaves and twigs under the water. The masses so formed have a very dark, deeply-stained appearance, and on account of the small size of the plastids forming them are, at first view, with a quarter-inch objective, hardly to be recognised as similar in nature to the paler masses of larger homogeneous bacteroids.

The sphærous plastids of the same form also form *arborescent* masses and *catenular* groups (fig. 3), and there are gradations between such arrangements, through loosely-packed aggregations, to the densest forms. In some of the

looser conditions I have seen the walled plastids embedded in gelatinous matrix, though there was not very much of it.

Arborescent aggregation is not unfrequently exhibited by sharply cut bacterioid plastids, with well-marked wall and contents, such as are seen in fig. 19. This figure is not a sample of that form of aggregation, but is composed of the same prismatic bacterioids. The dendritic aggregates exhibit forms not unlike those crystalline deposits to which the same term is applied.

Of the *catenular* aggregation often exhibited by the sphærous and bacterioid plastids mention has been made above. The term may be used, in distinction from *linear*, for those aggregations in single series which do not form rigidly straight lines.

The term *Reticular aggregations* I apply to the examples figured in fig. 19 and fig. 23. Neither of these figures do justice to the very remarkable and beautiful appearance sometimes presented by such reticular aggregates of prismatic bacterioids. They occur but rarely, and often are of very small size, but I have a drawing of one mass of the kind which measured $\frac{1}{50}$th of an inch across. In fig. 19 are represented a few only of the meshworks of one of these masses. They are, in the complete masses, not confined to one plane, as are those of the fragment figured, but form *basket-like* enclosures, or balls; and the meshes are often of great regularity of form, either pentagonal or hexagonal. A considerable amount of colourless jelly-like matrix is connected with the plastids of these basket works, which have, however, very sharply defined colourless "walls" and coloured "contents."

The specimen drawn in fig. 23 is chiefly remarkable for the trilocular condition of the prismatic plastids. Such a triple segmentation of the bacteroid or biscuit form of bacterian plastid is not unfrequently to be observed in growths of *B. termo*. It connects the bacteroid form readily with the rosary-chains and multilocular filaments (fig. 24).

Tesselate aggregations of the most marked kind did not occur in my growths of *Bacterium rubescens*. Fig. 19 presents the nearest approach to the condition which is typically exhibited by the "mycoderma-phase" or pellicle of *Bacterium termo* (see this Journal for April, Pl. V, fig. 10). In this form of aggregate the plastids are set together side by side, so as to form a sheet, and are definitely packed or adjusted, so as to form something like a pattern, whence the comparison to mosaic work.

Some physiological properties of B. rubescens.—All these various forms of the plastids of *B. rubescens* occur submerged

in putrescent liquids, forming incrustations on surfaces. They do *not* float in the water.

Light is probably not necessary for their development, but in glass jars they form a denser coating on that side exposed to the light than on that turned from it.

Artificial culture of *B. rubescens* has not at present been successful. I have failed to cultivate it in ammonium tartrate with ash-salts, and also have failed in turnip-infusion. On the other hand, wherever it has made its appearance, *animal* matter has been present in a state of putrefaction, accompanied by *B. lineola*. However, I may mention that, in the spring, my deeply-regretted friend, Dr. Pode, of this University, observed on slices of boiled potato (which we had exposed, as recommended by Cohn and Schröter) patches of a peach-coloured growth, which circumstances prevented me from following up closely. They were, I believe, caused by dense aggregations of the minute, sphærous, unilocular plastids of *B. rubescens*, which was not then known to us.

At present I do not see a prospect of examining more carefully the special activities of *B. rubescens*, though the inquiry is a most interesting one. It should be possible, by continued experiment and observation, to ascertain the conditions which favour the growth of each of the particular plastid-forms and aggregation-forms which I have above pointed out.

With regard to that matter, I am only able to adduce the following facts:

In the large jar left during the summer months with decaying *Astaci*, there was an excess of putrescible matter, and no living green algæ were present. The water after two months swarmed with active *B. lineola* and *Vibrio serpens*. *Vorticella* was also present. The red-coloured growth was in great abundance on the glass slides of the vessel, and consisted almost exclusively of plastids of the large, homogeneous, biscuit or bacterioid form (figs. 10, 14), aggregated in great irregular masses.

In a smaller jar, in June, where the putrescence was scarcely perceptible by any odour, and where various green algæ were abundant, the first films of the peach-coloured Bacterium were seen spreading over some dead twigs and leaves. In this case the growth consisted at first entirely of irregular and globose aggregates of the small, sphærous, unilocular plastid (fig. 3 and fig. 18). A few days later were detected in this jar small, multilocular sphæroids (fig. 16, and some of the reticular growth, (fig. 19). In a second small jar, which like the first bore but slight evidence of putrescence,

beyond a black-coloured sediment and a Bacterium-scum on its surface, and in which various green Algæ were developing under the influence of sunlight, the peach-coloured growth was not detected until it had been long and well established, being spread over the matters lying at the bottom of the jar, and forming a growth on the sunny side of the glass. This growth was almost exclusively of the character drawn in fig. 1—glæogenous globose aggregates of homogeneous biscuit-shaped plastids. But from the films at the bottom of the jar all the other forms were obtained in abundance, as well as the globose aggregates, and they were repeatedly examined, measured, and drawn.

There is some reason to suppose that there is a physiological variation in the tint of the colouring matter of *B. rubescens*. It varies from a pale blue peach colour to a deep red peach colour, and this variation is not due merely to variation in amount, though that will, of course, account for some effects of the kind. Such colouring matters as Bacterio-purpurin are seldom in the living organism of a simple nature, for they are each really the expression for a group of complex bodies, which are continually and gradually built up as the result of one series of chemical changes in the organism, and as continually and gradually are broken down in connection with another series. Thus it is with chlorophyll, and, probably, with all its representatives. Bacterio-purpurin, as its three-banded absorption spectrum would by some authorities be supposed to indicate, is probably a collection of two or three blue and red coloured bodies, almost identical in chemical constitution, and giving rise one to the other. Slight changes of physiological conditions may give an excess of the blue element (which we may suppose is a step in the ascending or building-up process), or an excess in the red (which we may suppose to be a step in the descending or breaking-down process). The growths from the sides of the glass jars, especially the luxuriant growths in the large jar, had a somewhat redder tint than the other films, though the particular form of plastid occurring in them is *not* invariably of a redder tint than others, *e. g.* fig. 10.

In dying the plastids assume a brownish tint, and this will tend to modify the aspects of old crusts.

When a jelly-like crust of *B. rubescens* is taken and agitated in water in a test-tube the whole material is diffused through the liquid. In a few moments a subsidence commences, which proceeds in a curiously selective manner; for invariably the browner particles of such a crust fall the first, and leave a stratum above the sediment of partially subsided

brightly-coloured particles. In this way I have obtained the most brilliant samples of the colour formed by *B. rubescens*; that is to say, a translucent stratum extending half an inch or so above the actual sediment in the test-tube, of an intense red peach colour, caused by the suspended plastids. It is such a stratum of coloured material, which I found most convenient for examination by transmitted light, though the growths themselves *in sitû* on the sides of the glass jars also gave the absortion-spectrum with intensity.

I have already spoken of the only manifestation of contractility presented by *Bacterium rubescens*, viz. the motions of the acicular form. No other form of *B. rubescens* was seen by me to exhibit vital movement, though it is quite likely that such plastids as those in fig. 10, which are very like actively moving forms of *B. lineola*, may, under certain conditions of oxygen supply, &c., exhibit movements.

Hence we have some of the facts of, *a*, growth and reproduction; *b*, metamorphosis of substance; *c*, contractility observed in the life history of *B. rubescens*. The phenomena of a fourth activity, namely, of chemical exchange, with the environment which have been to some extent observed in the case of *B. termo*, are as yet not known at all in this case.

Zooglœa Form or Glœogenous condition of Spirillum.

In the small jars with *Bacterium rubescens* occurred quantities of a species of *Spirillum*, probably *Sp. undula*, as adopted by Cohn. These were at first observed in the glœogenous state as figured in figs. 8, 9. I give these figures because Cohn states that he has not observed the zooglœa condition in the genus *Spirillum*.

Forms of Bacterium termo and B. lineola.

In the plates I have given a few figures of these species for comparison. The filaments, composed of biscuit-shaped units in line, I have already alluded to (figs. 4 and 6). In fig. 7 are drawn sphærous and biscuit forms, both of which I consider as belonging to the natural species *B. termo*.

I may say that there appears to be some reason in the view that *B. termo* and *B. lineola* are two distinct natural species of saprogenous powers. I believe that I can distinguish the odour which is characteristic of each, but more watching of these forms is certainly necessary for anything like a definite conclusion.

It also seems probable that each of these species has a

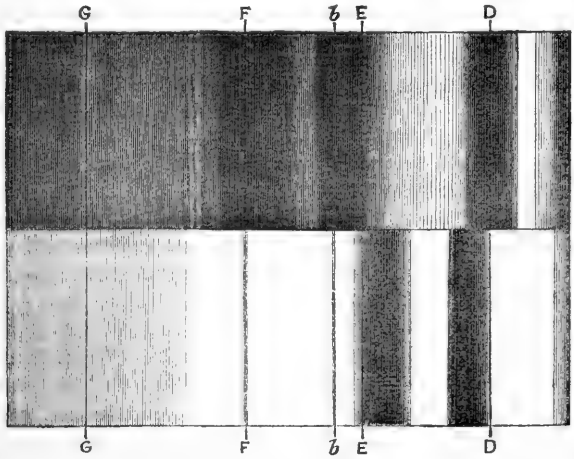
sphærous, a biscuit-shaped, a bacillar, a serpentine (*Vibrio*), and possibly a spiral (*Spirillum*) form or phase of plastid.

The range of variation which I have brought evidence to establish in the case of *Bacterium rubescens* renders the hypothesis of such an extent of variation in *B. termo* and *B. lineola* justifiable.

Physical Characteristics of Bacterio-purpurin.

This name is used to indicate, not a definite chemical compound, nor a body of constant crystalline form, but a natural pigment occurring in the plastids of *B. rubescens*, and *definitely characterised* by the absorption-spectrum figured in the woodcut. The absorption-spectrum of oxy-hæmoglobin and the chief solar lines are given to fix the position of the bands.

This colouring matter is insoluble in water, alcohol, chloroform, ammonia, acetic acid, sulphuric acid, singly or in combination. It is changed into a brown substance by hot



alcohol, which is slightly soluble in that agent. Chloroform changes it to an orange-brown, and dissolves the substance so produced; but this body gives no detached absorption-bands.

Bacterio-purpurin differs, therefore, greatly in spectroscopic and solutional properties from the red colouring matter of *Monas prodigiosa* (Ehr.) described by Schröter.

REVIEW.

Illustrated Guide to the Fish, Amphibian Reptilian, and supposed Mammalian Remains of the Northumberland Carboniferous Strata. With Atlas of Ten Plates. By Thomas Pallister Barkas, F.G.S. London, W. M. Hutchings.

Mr. Barkas does not seem to be at all sure as to what his work ought to be called, for while the designation given above is quoted from the title-page, we find impressed on the outside boards the alternative title of 'Manual of Coal-Measure Palæontology.' The book consists, however, of notes on such fossil vertebrate remains from the Northumberland coal-field as have come under his own observation, and can in no sense be called a "manual" even of any one department of the somewhat extensive domain of "Coal-Measure Palæontology." The remains described in the ten chapters are for the most part very fragmentary, and have been also mainly derived from one stratum of shale known as the Low Main Coal Shale. Mr. Barkas describes and names as new some fishes, and several Labyrinthodonts. He claims the discovery of true reptilian remains in the Northumberland coal-measures, and even advances the theory that certain footprints and a fragment of a jaw are mammalian. Should the latter supposition prove true Mr. Barkas is certainly on the high road to eminence as a palæontological investigator. We are certainly glad to see these discoveries published in a compact and accessible form, seeing that many of the original descriptions first appeared in the 'English Mechanic,' a periodical which most people would consider a somewhat strange medium for the publication of original palæontological work.

Mr. Barkas has "refrained as much as possible from all attempts at generalisation, and he is wise to do so, and to adhere for the present to his classification of fossil Amphibia into "large" and "small" (p. 116). The atlas is certainly the best part of the work, though there seems no reason why a maxillary bone of *Palæoniscus* should have been drawn upside down.

Although we regret that we cannot apply very high praise to this work as a scientific production, yet it must be frankly admitted that Mr. Barkas has shown for many years most enthusiastic perseverance in the prosecution of palæontological studies.

NOTES AND MEMORANDA.

Carmine Staining.—The difficulty in obtaining uniformly good results with carmine, especially when the tissues to be stained have been hardened in chromic acid, has led to the employment of many other staining agents. These are of more or less value, but it is generally admitted that none of them can be compared to carmine for brilliancy and permanency. It is with a view to overcome this difficulty that I venture to suggest the following slightly modified plan of carmine staining, which I have rarely found to fail with any tissues. Immerse the tissue to be coloured in Beale's carmine fluid, diluted with two or three times its volume of water, and filtered. In this it should remain until it is thoroughly stained, the time required being, of course, variable, but usually from four to twelve hours will suffice. Then wash and transfer the tissue to proof spirit (methylated) containing ½ per cent. of hydrochloric acid, and allow it to remain therein from five to ten hours, when the staining will be found no longer diffuse, the carmine having been extracted from all but the protoplasmic portions. Lastly, wash and preserve in spirit until convenient to mount. The tissue may be left for a longer period in the staining fluid without detriment, but if left too long in the acid solution the colour loses its brilliancy. In making Beale's carmine solution for this purpose it is well to adopt Mr. Atkinson's plan of preserving as much as possible of the ammonia in the fluid.

Chromic Acid and Spirit for Hardening.—The following hardening fluid will be found exceedingly useful in the preparation of all delicate tissues, and especially for those which are liable to be injured by long maceration in watery solutions:—Chromic acid, 1 part; water, 20 parts; rectified spirit (methylated), 180 parts. Dissolve the chromic acid in the water first, and then add the spirit (violent action will ensue if the dry chromic acid be added directly to the spirit). The colour of the solution soon becomes brown. If, after a few days, it turns semi-gelatinous, it should be changed for fresh. From a week to ten days is required to harden such

tissues as retina, cochlea, &c., for which this fluid is particularly well adapted. URBAN PRITCHARD.

The Movements of the Glands of Drosera.—The peculiar movement of the glands which cover the margin and the upper side of the leaf of the sundew has often attracted the attention of botanists. During the past autumn in Westmoreland the following observations were made on *Drosera rotundifolia*. It should be noted that the glands are in no sense hairs, that is, cellular expansions of the epidermis of the leaf. They have been shown by Grœnland and Trecul to be an integral part of the leaf itself, penetrated by a fibro-vascular bundle with spiral vessels (in other words, by a vein on nerve of the leaf) from one end to the other, and even furnished with stomata on their surface. They terminate in a pellucid knob, within which is formed their peculiar viscid secretion. Under a low magnifying power this secretion may be seen collected about the knobs, and stretching in thin glutinous strings from one to another. The secretion has probably an attraction for flies and other small insects, for, if the plant is examined in its native bogs, scarcely a leaf will be found in which an insect is not imprisoned, and one leaf will very often show as many as three or four. The experiment was made of placing a very small insect—a species of Thrips—on a leaf at that time quite unencumbered, beneath a low power of the microscope. Immediately on coming into contact with the viscid secretion, it made vigorous efforts to escape, but these efforts only seemed to entangle it all the more deeply. The contact of the insect appeared to excite a stronger flow of the secretion, which soon enveloped the body of the animal in a dense and almost transparent slime, firmly glueing down the wings, and rendering escape hopeless. It still, however, continued its struggles, a motion of the legs being still clearly perceptible after the lapse of three hours. During all this time the insect was sinking lower and lower down among the glands towards the surface of the leaf, but only a slight change had taken place in the position of the glands themselves, which had slightly converged so as to imprison it more completely. But after the struggles of the prisoner had practically ceased, a remarkable change took place in the leaf. Almost the whole of the glands on its surface to its margin, even those removed from the body of the insect by a distance of at least double its own length, began to bend over and point the knobs at their extremities towards it, though it was not observed that this was accompanied by an increased flow of the secretion from

them. The experiment was made in the evening, and by the next morning almost every gland on the leaf was pointing towards the object in the centre, forming a dense mass over it. The sides of the leaf had also slightly curved forwards so as to render the leaf itself more concave. The nearly allied Venus's fly-trap, or *Dionæa muscipula* of the United States, which imprisons flies by a much more sudden motion of the sides of the leaf, collapsing when irritated on the upper surface, is said to digest and absolutely consume the insects thus entrapped. What becomes eventually of the prisoners of the sundew my experiments have not yet been carried sufficiently far to ascertain. It will be seen that the most singular feature in the phenomena described is that the motion of the greater number of the glands did not begin till after the insect had become comparatively motionless; and therefore it is very difficult to attribute it to the excitement caused by the struggles on any "contractile tissue" at the base of the gland, an explanation which has been offered for the sudden and rapid motions of the stamens of *Berberis*, or the leaves of *Mimosa*. It is also quite certain that the impinging of raindrops on the surface of the leaf causes no similar motion —a peculiarity similar to that which Darwin has observed in the case of the motions of tendrils and climbing stems. In order to determine what share in these motions of the glands was due to the organic nature of the substance imprisoned and to its power of motion, the following experiments were also made:—A small piece of raw meat was placed on another leaf similar to the first. No immediate change was observable, and no increased flow of the secretion; but after the lapse of a few hours a perceptible inclination towards the object of the more distant glands took place. The next morning the piece of meat was found, like the fly, sunk down to the surface of the leaf, with almost the whole of the glands converging towards it and above it in just the same manner. The changes here were, therefore, perfectly of the same kind as in the case of the fly, though, apparently, somewhat slower. After the lapse of twenty-four hours the piece of meat appeared decidedly lighter in colour; but an accident prevented the process of digestion being further traced. On other leaves were placed a minute piece of wood and a small piece of worsted; and in neither of these cases was the least change perceptible. It would appear, therefore, as if the organised structure of the fly and of the piece of raw meat had some power of exciting this motion which is not possessed by matter of a different description.—A. W. Bennett, *Brit. Ass.*, Sept. 20.

QUARTERLY CHRONICLE OF MICROSCOPICAL SCIENCE.

EMBRYOLOGY.

On the Fecundation and Development of the Ovum of the Rabbit.—Dr. Carl Weil writes on this subject in the *Medicinische Jahrbücher*, vol. i., 1873.—Since the discovery of spermatozoa in the ovum of the rabbit by Barry (*Philosoph. Trans.* 1838-40), it has been held by all embryologists that the spermatozoa form the most important part of the sperma as regards the fecundation. Barry's observation has been confirmed by a great number of embryologists, amongst whom we may mention Bischoff, Lehmann, and Meissner. Spermatozoa have been found by those observers in the albuminous envelopment of the ovum of different mammals during its passage through the oviduct, farther in the zona pellucida, and between the latter and the germ (yolk) itself. They have been seen in the latter place, not only previously to the cleavage process—when the germ (yolk) had retracted itself from the zona pellucida—but also in a later period, when the germ was already divided into a number of cleavage globules. In all these instances, however, the spermatozoa were motionless. Dr. Weil has observed in a number of ova, taken from the oviduct between the 17th and 46th hours after fecundation, spermatozoa in very lively movement in the albuminous envelopment as well as within the zona pellucida. Weil gives an account of four instances where he has seen unchanged spermatozoa *in the substance of the germ* itself, besides numbers of moving spermatozoa between the germ and the zona pellucida. There were to be found in these and other instances filaments either isolated or in bundles inside the germ, which Weil regards as the tail of spermatozoa. In later periods, when the ovum had already reached the uterus, no spermatozoa were to be found, neither outside nor inside the germ. From these facts, Weil takes it as probable that the spermatozoa, after having penetrated

the germ, vanish completely, and that this intimate union of the spermatozoa with the germ forms the most material part of the fertilisation of the ovum. Consequently, the inheritance of faculties from the father may be in this way explained.

Weil confirms the assertion of Bischoff that the coitus in rabbits is not to be regarded as the chief cause of the extrusion of the ova from the ovary; but that, if there exist a relation between the coitus and the extrusion of the ova, it is only in so far as the former takes place a few hours before or after the latter.

According to Weil, each ovum possesses two vesicular nuclei before the cleavage process commences (an observation made by E. van Beneden with regard to the ovum of mammalia in general in his 'Recherches sur la Compos. et la Signif. de l' Œuf. Extrait du Tome XXXIV des Mémoires couron, de l'Acad. Royale de Belgique,' 1870.—Ref.)

As to the earliest changes of the ova on their way through the oviduct, Dr. Weil's observations confirm those of Bischoff, fully described in his great work on the development of the rabbit's ovum (1842), which may be briefly described as follows. The germ is first closely surrounded by the zona pellucida; then the germ retracts itself from that membrane; farther, the germ divides itself into two halves, each of these again into two halves; then the germ consists of eight cleavage-globules, and finally of sixteen. In ova taken from the uterus (four days), the germ is already transformed into a vesicle (*vésicule blastodermique* of Coste) which exhibits on its surface a mosaic of cells, and on one place a mass of opaque elements, projecting into the cavity of the vesicle. In a later stage (five days) this vesicle consists only of one layer of cells; that is to say, the elements which result from the cleavage of the germ have arranged themselves in one layer, which encloses the cavity of the vesicle. In a still later stage (seven days) the vesicle shows a circular opaque spot which consists of two layers of cells, whereas all other parts of the vesicle have only one layer. (This spot represents the future *area germinativa*, in which the rudiment of the embryo appears.—Rep.)

Not being able to find the above-mentioned mass of opaque elements at the stage when the germ-vesicle was seen to consist of only one layer of cells, Weil takes it as improbable that this mass of elements participates in the formation of the *area germinativa*, and is, therefore, in agreement with the earlier assertion of Bischoff and that of Remak, and against the later assertion of Bischoff and that of Coste.

Weil does not give any explanation of the spherical finely granular bodies that are to be found between the germ and the zona pellucida previously to, as well as in, the earlier stages of the cleavage-process. He does not think it necessary to conclude that they stand in a relation to the germinal vesicle (viz. the nucleus of the unfertilised germ), which, according to some authors, leaves the germ before the cleavage-process commences. The method employed by Weil in his researches is as follows. Within the first twelve hours after littering, the female is coupled with the male; from twelve until about eighteen hours after coition, the oviduct and ovary of one side are excised from the living animal, which is then allowed to live, the wound being treated according to the ordinary rules; the other oviduct may be made use of at a later period. For observation of the ovum from eighteen hours to seven days after coition, the cornua uteri are excised. In the first instances the oviduct is freed by the aid of forceps and scissors from the surrounding fat and peritoneum, and opened on a glass slide with fine scissors inch by inch. Whether ova have left the ovary can easily be recognised by the presence of blood-stained specks on the ovary—the openings of ruptured Graafian follicles. The folds of the mucous membrane being stretched with a pair of needles, ova can be discerned under a lens as spherical bright bodies. After being spread out as much as possible, the oviduct is covered with a cover-glass without the addition of any reagent, and can be examined under the microscope even with the highest magnifying powers.—(E. Klein, M.D., in *Medical Record.*)

PROCEEDINGS OF SOCIETIES.

Medical Microscopical Society.

The sixth ordinary meeting of the above Society was held at the Royal Westminster Ophthalmic Hospital, on June 19th, at 8 p.m., Jabez Hogg, Esq., President, in the chair.

The minutes of the last meeting having been read, the President read a paper on "The Histological Difference between Croup and Diphtheria." The paper will be published in full elsewhere.

In the discussion that followed Dr. Pritchard considered that the presence of the oidium albicans in diphtheritic membrane indicated only a deteriorated condition of the blood as it was to be found at times in cases of blood poisoning. He also thought that inclusion of the nerve fibrils in a mass of inflammatory tissue scarcely sufficient to explain the paralysis of diphtheria. Dr. Bence, in cases of croup that he had examined, had never found epithelium in the membranous exudation, as Mr. Hogg had described; but had observed infiltration of the submucous connective tissue with exudation cells.

Mr. Golding Bird agreed with the last speaker about the presence of exudation cells (white blood-corpuscles) in the croup membrane; and these he had noticed arranged in a linear manner, of two or three deep, between the corrugations of the apparently structureless membrane. He had never noticed epithelium.

The President, in reply, stated that his specimens had been chiefly obtained from patients by means of emetics, and was almost inclined to agree with a member who had stated that another epidemic of diphtheria was needed before the histological differences between croup and diphtheria were fully understood. He thought the paralysis of the latter affection was owing to disintegration of the nerve fibrils and not to pressure.

The meeting then resolved itself into a conversazione. The President exhibited specimens of croupous and diphtheritic membranes illustrative of this paper.

The seventh meeting of the Society was held on Friday, July 18th, at 8 p.m. No paper was read. The meeting was entirely conversational in character.

Dublin Microscopical Club.

20*th March*, 1873.

Hormospora mutabilis, Näg.—Mr. Crowe showed *Hormospora mutabilis*, Näg., a pretty and rather scarce alga; though found in localities widely remote, it seems always scanty. This is the typical *Hormospora*—that shown at the last meeting by Mr. Crowe, *H. plena?* being as yet only conjecturally congeneric.

Docophorus platyrhynchus—from ouzel—was shown by Dr. J. Barker.

Identity of Schizonema laciniatum and S. implicatum.—Rev. E. O'Meara showed *Schizonema laciniatum*, from Dr. Harvey's Collection, and *S. implicatum* (Moore), in order to point out that, after the most careful examination of outline and striation in those two so-called species, he could find no reliable characters sufficient to separate them as truly distinct.

Volcanic "Hair."—Dr. Frazer showed specimens of the so-called "volcanic hair," from Hawaii (Pele's Hair), resembling spun-glass, forming long slender threads, often with a clavate head at one end, not unlike some kind of hair with its bulb. The bulbous globular masses appear to have formed in the manner of "Rupert's Drops" at the ends of the fibres.

Euastrum binale var. *insulare* (Wittrock).—Mr. Archer was now able to show examples of a minute desmid appearing identical with *Euastrum binale* var. *insulare* (Wittrock), except that in the centre of the disc of each segment a minute scrobiculus occurred not shown by Wittrock. This may not have been noticed by that observer, as it is not very perceptible except when looked for on the empty membrane. This little character was, so far as may be judged, hardly sufficient to distinguish this from the Gotland form, but still Mr. Archer thought the var. *insulare* was a really distinct thing from the true *E. binale*.

Epidermis of Lycopodium.—Professor M'Nab showed a preparation of the Epidermis of a species of *Lycopodium* from New Zealand. The walls of the epidermal cells are irregularly thickened, the pores or pits being of comparatively small size. These pits are chiefly met with in the side wall of the cells. The external or free surface is also thickened in the inner side, but not to any great extent; whilst the deep-seated wall—that next the mesophyll of the leaf—does not seem to be thickened. The stomata are peculiar: single epidermal cells have divided by a wall parallel with the long axis of the leaf. The half epidermal cell thus forms one side of the stomatic opening. The opening is elongated and more or less funnel-shaped—the wide opening externally, a narrow slit-like aperture visible below. On application of reagents no small cells surrounding the stomatic opening can be brought to view, the stoma being thus formed by the division of a single

epidermal cell. No thickening is observable in the stomatic cell, but the wall adjoining it is thickened, but only on one side. Stomata occur on both sides of the leaf, but more abundantly on the lower. Both the epidermal and stomatal cells contain granular materials and chlorophyll. The thickening of the epidermal cells, and the relation of the thickened cells to those in the stem, and the peculiarity of the stomata, have probably an important connection with the absence of a true dermatogen-layer in these plants.

Dr. Bastian's "Fig. 84" not truly a Metamorphosis of Vaucheria.—Mr. Archer drew attention to a not very uncommon algal form —*Apiocystis Brauniana* (Näg.)— showing its *balloon-shaped* "fronds" growing seated upon various supports—other algæ, *bits* of Entomostraca, &c. It may be found growing on such aquatics (and of large size often) as *Ranunculus aquatilis* and others. Now the form figured by Dr. Bastian as growing "on Vaucheria" (fig. 84), and which he assumes as truly an outgrowth from, and transmutation of, the substance of the *Vaucheria* is very probably merely a young *Apiocystis*. Just as in the instances now drawn attention to, there could be no doubt but that this little algal form so figured by Dr. Bastian was seated on its support by the sheerest accident; both inhabited the same waters, and, the habit of the former requiring a support, the *Vaucheria* was there to lend its timely aid—any other basis would have answered. It is no doubt quite possible that *Apiocystis* may be an "alternation" of some higher type, or it may yet be found to produce its own true spores (other than the already known zoospores); but to suppose, with Dr. Bastian, that it owes its origin to a metamorphosed outgrowth—and the same may be said of the other foreign growths seated on the alga, in his figure referred to—from the various fulcra on which it grows is not reasonable; nay, it is not going too far to say it is wholly inconceivable.

Zygospore of Staurastrum Brébissonii.—Mr. Archer showed the Zygospore of *Staurastrum Brébissonii*. Of this Professor Cleve gives a figure in his 'Bidrag till Kännedomen om Sveriges Algen,' which is correct.

17*th April*, 1873.

Closterium rostratum, conjugated; also occurrence of Cosmarium plicatum, Reinsch.—Dr. J. Barker showed conjugated examples of *Closterium rostratum* (Ehr.); this, though not a common species, is not unfrequently met with conjugated; as is known, its zygospore is of singular form.—Dr. Barker exhibited also the, with us at least, rare *Cosmarium plicatum* (Reinsch); like *Staurastrum pileolatum* (Bréb.), this latter form seems mostly to occur on rocks.

Cosmarium Hammeri, Reinsch, *new to Ireland.*—Mr. Crowe showed specimens of a *Cosmarium*, doubtless identical with *C. Hammeri*, Reinsch ('Die Algenflora des mittleren Theiles von

Franken,' p. 109, t. ix, fig. 1), new to this country. As the gathering, made from a wet rock, lay through an accident for some days corked up at the hotel in Arklow, the specimens were considerably deteriorated, and in no case could the *contents* be seen in their normal condition. It is to be hoped that more specimens may hereafter be forthcoming from the same site in order to pursue the examination of this form. Reinsch does not say anything as to the arrangement of the chlorophyll-contents in his form, as to which there may possibly exist some point to be determined, whether parietal or central; he describes the membrane as smooth; in Mr. Crowe's specimens the membrane is punctate, but, as regards the identity of the forms, the description in this regard might not be incompatible.

A "mechanical finger."—Mr. Porte showed a "mechanical finger" which he had himself constructed after a design published in the 'Lens,' with improvements, which he had found most manageable and useful in transferring objects under the microscope.—Rev. E. O'Meara showed a specimen of *Navicula didyma*, Gregory, in the mounting of which the "finger" had been employed with great efficiency.

Animal Hairs exhibited.—Dr. Richardson showed specimens of various animal hairs mounted in Canada balsam, and forming interesting and pretty objects; these were otter, jerboa, racoon, sheep wool, goat, ichneumon, deer, beaver, squirrel, zebra, bear, dromedary, contimonte, dingo, sloth.

Notes on the female flower of Carex pulicaris.—Prof. McNab exhibited preparations of the young female flower of *Carex pulicaris*. The perigynium was seen to develope as a single bract, surrounding the whole secondary axis. In the axil of the perigynium the female flower was developed (as a tertiary axis), while the rest of the secondary axis produced very imperfect perigynia and the rudiments of flowers. The study of the development showed that the perigynium was equivalent to the sheath of the foliage leaf of *Carex*, the lamina remaining quite undeveloped. The notch in the margin of the perigynium, which the older botanists considered to indicate the double nature of that structure, is quite similar to the notch at the upper part of the sheath of the foliage leaf. On opening the perigynium from the anterior or outer side, the rachis (or secondary axis) is brought into view, completely enclosed within the perigynium, whilst the flower (or tertiary axis) is developed behind the rachis, close to the primary axis.

Sporangia of Equisetum.—Prof. McNab also exhibited a preparation of the sporangia of *Equisetum*, made by Prof. W. C. Williamson, of Manchester, which afforded a beautiful demonstration of the spiral cells in the walls of the spore-cases.

Spirogyra calospora (Cleve), *new to Britain.*—Mr. Archer showed conjugated examples of *Spirogyra calospora* (n. s.), Cleve, and pressed out a zygospore in order to exhibit the scrobiculate mesosporium, which presents a beautifully reticulate marking.

Prof. Cleve alludes to two forms in his work ("Försök till en Monografi öfver de Svenska Arterna af Algfamiljen Zygnemaceæ," in 'Nova Acta Reg. Soc. Sc. Upsal,' Ser. III, Vol. VI.), α and β, that is, a form with one or more spires, or with one only in each cell, in separate filaments. Here, however, the same filament presented joints directly abutting on one another, with one and with several spires. That observer describes the joints as always infolded the ends; here some joints were so, others not at all. Does the form now shown indicate that *Sp. alpina*, Näg. et Crasner may after all be one and the same species? The characteristic distinction would seem to be reduced to the scrobiculate mesosporium. This peculiarity, from the scrobiculi being deep, and lending an almost "honey-combed" appearance to the spores, gives them a very characteristic aspect. In this species the joints are very long, the spores elliptico-cylindrical, and the enclosing cell not inflated. It is possible this may be a more widely distributed species than the few localities in which it has been noticed might lead one to suppose, though, of course, in the unconjugated state it would be quite impossible to arrive at any more than an approximate decision as to specific identity of *Spirogyra*-forms.—Mr. Crowe likewise exhibited examples of the same species taken on the same occasion.

Pleurophrys fulva (Arch.), *exhibited*.—Mr. Archer showed the rhizopod *Pleurophrys fulva*, ejus, a form he had not met with for a considerable time; this minute species seemed, however, to maintain its characteristic appearances very constantly. It does not appear to have been, as yet, detected by other observers, so far as he was aware; with us it certainly does not appear to be at all common.

15*th May*, 1873.

Staurastrum Meriani (Reinsch), *exhibited*.—Mr. Crowe exhibited *Staurastrum Meriani* (Reinsch), from a moist rock near Woodenbridge, Co. Wicklow. This seems with us to be a very rare form. Mr. Archer had met it very sparingly on two occasions—in Co. Westmeath and at Connemara (Co. Galway); it seems thus to be widely distributed, though extremely scanty; it does not appear to have been elsewhere detected save by Reinsch himself. It differs even in the same gathering in the number of angles, but the form could hardly be mistaken for any other, except, perhaps, *Cosmarium cylindricum*, but that is smaller and covered by pearly, not opaque, granules.

Laticiferous Vessels from Onion.—Professor McNab exhibited a preparation of the "Schläuchgefässe" (of the German writers), from the Onion. These are peculiar laticiferous vessels discovered by Hanstein in 1859; the walls are pitted and resemble the "Siebröhren" or cribriform vessels. The simplest mode of demonstrating these laticiferous vessels is to take a thin section of the scale of an onion-bulb and boil for about one minute in

dilute caustic potash, place on a slide, and examine with an amplification of about × 400.

A New Revolving Disc for Objects.—Dr. John Barker showed a rotating glass disc, fitted to be readily attached by its centre to the stage of the microscope, and intended to receive the preparations (say of a series illustrative of a group, or of the different related portions of some given tissue), arranged in a circle, so as to bring each in turn and at once under view, thus greatly facilitating demonstration to a class.

Fluid Cavities in Granite.—Prof. Hull showed sections of granite showing fluid cavities in the silica.

Amici's Prism.—Mr. Robinson exhibited in use a conveniently mounted Amici's prism, and he showed certain of the finely-marked diatomaceous forms by its aid under a $\frac{1}{16}$ Gundlach's objective, with excellent effect; in fact, for lined objects this would appear to be an exceedingly valuable adjunct.

Himantidium arcus, from Sulu.—Rev. E. O'Meara brought under notice a freshwater gathering from Sulu Islands, made by Capt. Chimmo, R.N. It contained *Himantidium arcus*, identical in its details with that form so common in our own country.

Encysted or "Resting State" of the Cells of Draparnaldia.—Mr. Archer showed what he had no doubt was an encysted state of the cells of the branches of *Draparnaldia glomerata*—thick-walled, brown, and globular. At first sight it was not a little puzzling *what* they could be. The groups of cells, as would be due to their origin, were arranged in branching tufts; the larger cells near the base and gradually diminishing in size upwards. The assumption of the nature of these cells he held to be quite confirmed by examples taken from another gathering, made on the same occasion, showing states intervening between that above mentioned and the ordinary condition. Indeed, even the most densely encysted examples showed on the smallest and terminal cells the still adherent "chætæ," but in the first and most marked example the central "stem" cells had disappeared, adding to the puzzling appearance of the whole, and being calculated to prevent the true nature of the object presented to view striking the observer, at least at a first glance. An encysted state of the cells of *Draparnaldia* had before been drawn attention to by Currey ('Quart. Journ. Micro. Science,' Vol. VI, O.S., p. 207), but the condition depicted by him did not seemingly exhibit the extremely dense, brown, very thick-walled cells now exhibited, in which, indeed, the identity of the most extreme state, without reference to any intervening, so far as an *à priori* examination might lead one to suppose, might be said to have become lost. One might surmise, doubtless, that at some period these resting-cells would germinate, and give out each a zoospore, as the ordinary branch-cells might without having previously passed into the "resting" thick-walled condition.

East Kent Natural History Society.

Honorary Secretary, George Gulliver, F.R.S.

June 18th, 1873.

Hairs of Deutzia and Crystals of Platino-cyanide of Magnesium.—Colonel Horsley showed the effects of polarized light on these hairs and crystals, and how well these objects are adapted for this purpose, when viewed either by transmitted light or on a dark ground.

Muscular Coat of the Poison-bag of the Wasp.—This was shown by Mr. Gulliver, jun., to be formed, in the queens of *Vespa vulgaris*, of transversely-striped fibres, a structure which appears to be constant in this insect, since these striped fibres were plainly seen in every one of those queens which he had examined during the present year, although not at all in the same part of two queens of the honey-bee, which he had examined in like manner a little earlier in the same season; but he considers that the point requires further inquiry, as he finds that the same difference exists between the poison-bags of the workers of these two insects.

Crystals in the Seed-coats of Plants.—Referring to the crystals in the testa of the elm, depicted by his father in the 'Quart. Journ. Micros. Science' of June last, Mr. George Gulliver exhibited similar crystals in the same part of the gooseberry and sycamore (*Ribes grossularia* and *Acer pseudo-platanus*); in the gooseberry thickly studded throughout the seed-skin, and in the sycamore and maple occurring in irregular patches that may not always be readily found. His father had discovered similar crystals constantly in the testa or pericarp in certain species of many orders, as Papaveraceæ, Tiliaceæ, Aceraceæ, Geraniaceæ, Grossulariaceæ, Compositæ, Primulaceæ, and Dioscoreaceæ, whereas in the same parts of numerous other orders such crystals are not present.

July 10th, 1873.

An excursion to the sea-side at Whitstable, when live specimens were examined of Membranipora, Plumularia, Laomedea, Alcyonidium, Clavelina, Beroë, Cydippe, Æquoria, Eolis, Noctiluca, and clusters of infant Balani—all under Col. Horsley's, Mr. Sibert Saunders's, Mr. Fullagar's, and Mr. R. J. Bell's microscopes.

Development of Oyster-spat.—Mr. Sibert Saunders described this, illustrating it by live specimens under the microscope, from the egg to the mature animal, and adding numerous interesting details concerning its spawning and the habits and economy of the young. When the embryo is first extruded, and almost invisible to the naked eye, it has a complete bivalve shell, of which both valves are convex, and not, as in the adult, one convex and the other flat. The young oyster, as soon as parted from its parent, swims freely about, by means of the cilia with which it is furnished even before it is hatched; and, after having become attached and fixed, it increases in the course of two months to the

size of a silver fourpence. While swimming about, according to its habit, freely and vivaciously, the minute young oyster was shown to be a most curious and interesting microscopic object.

August 7th, 1873.

Mrs. Dean contributed several specimens of some flowering plants, which she had lately collected in the district.

Teeth of Gasteropod Molluscs.—Colonel Horsley exhibited many specimens of these in proof of their taxonomic value, and of their excellence as objects for experiments with polarized light.

Iron Ore at Whitstable.—Captain McDakin presented samples of ironstone taken from and near the railway-tunnel at Whitstable, where the bed of ironstone is from two to four feet in thickness, and is similar in appearance to, and of somewhat higher specific gravity than, the famous iron ore of the Northampton sands.

September 4th, 1873.

Queen-bees.—Major Munn brought no less than two dozen queen-bees, and showed their fights when two were put together in a bottle, and the structure and use made by them of their stings. That these are quite powerless to penetrate and so sting even the softest human hand was proved by ladies and others handling these insects with perfect impunity, though they resented this by protruding their stings and ejecting the poison, but could not pierce the skin of the offending person. The queens in their combats with each other inflict quickly fatal injury by injecting the sting-poison into the respiratory apparatus. The comparative structure shows why the worker-bee so easily stings its enemies, which the queen cannot do. As is shown by Mr. George Gulliver's dissections, the sting of the former is quite straight, thin, extremely sharp-pointed, and furnished with from eight to ten barbs; while the queen's sting is curved, bluntish at the point, and possessed of only from two to four barbs. All these facts were shown by extemporaneous dissections under the microscope. At the same time were explained the tricks of the famous bee-master, Thomas Wildman, who flourished in the latter part of the last century, and was so much the wonder for his surprising command over bees that he was wont to exhibit, surrounded by them, to the king and nobility, by whom he was offered a hundred guineas for his secret, which he refused to part with, and which Major Munn declared was simply using only queen-bees.

Pebbles and Flints.—Colonel Cox showed many specimens of beautiful coanites, landscape, and other pebbles, from Dover and Hastings, proving that the coast there is richer than is commonly supposed in those pebbles, and that their structure, with the included organic remains, especially of sponges, is most interesting for microscopic examination.

Mrs. Cole brought a large flint, which had been fractured at some remote time, and the fragments since reunited by a deposit of siliceous earth—a fact considered interesting as regards the still vexed question of the formation of flint-nodules.

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VOL. XIII, NEW SERIES.

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Fig 1

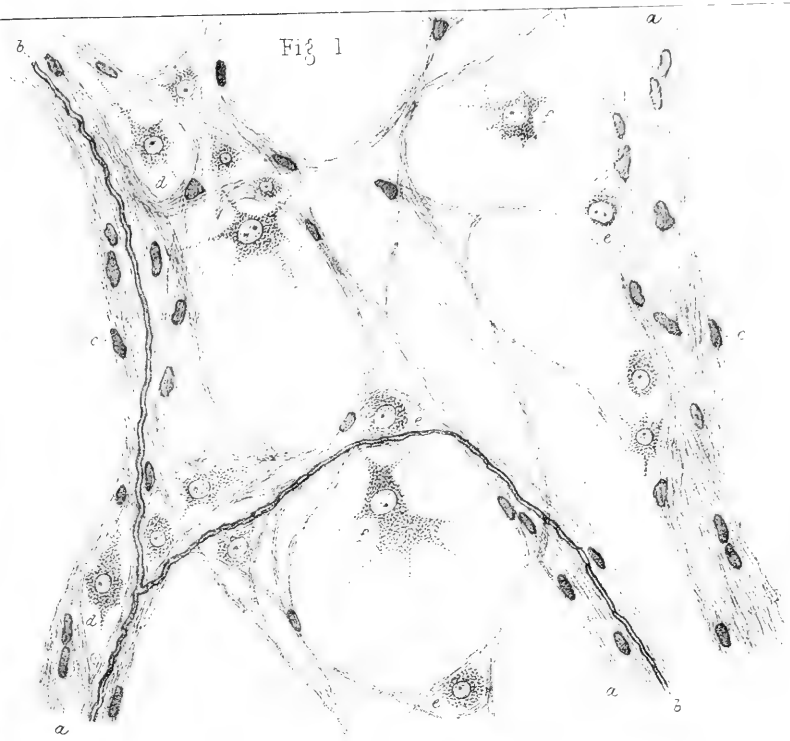


Fig 2

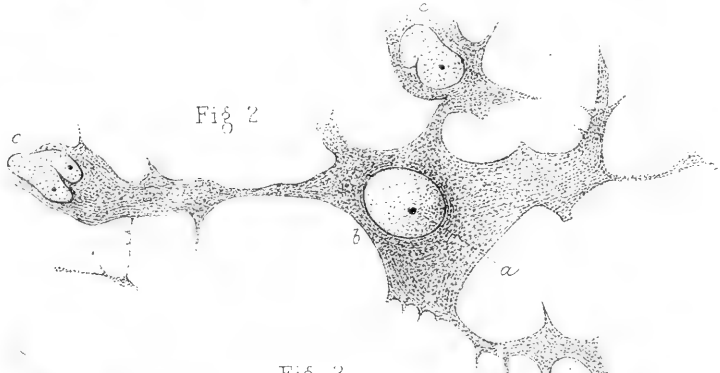
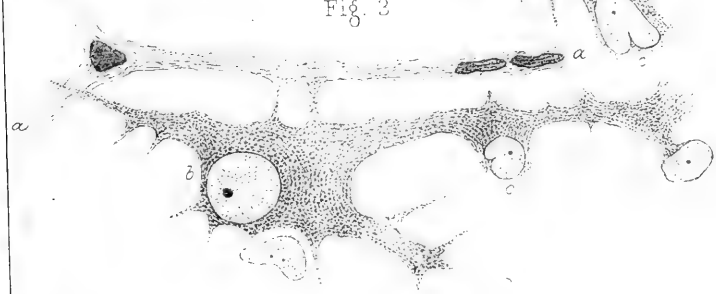


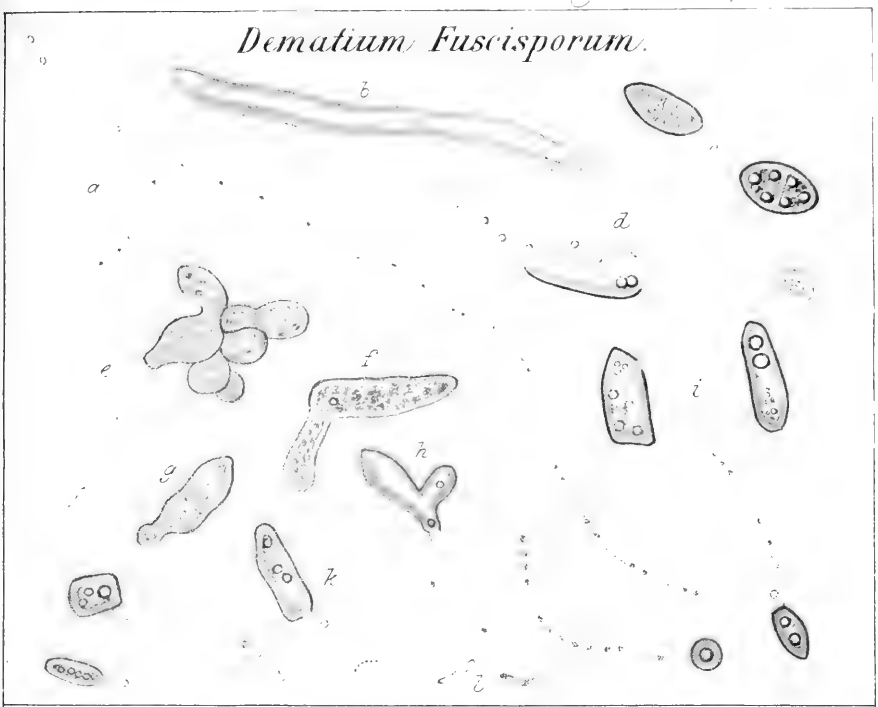
Fig. 3



E. Klein, del.

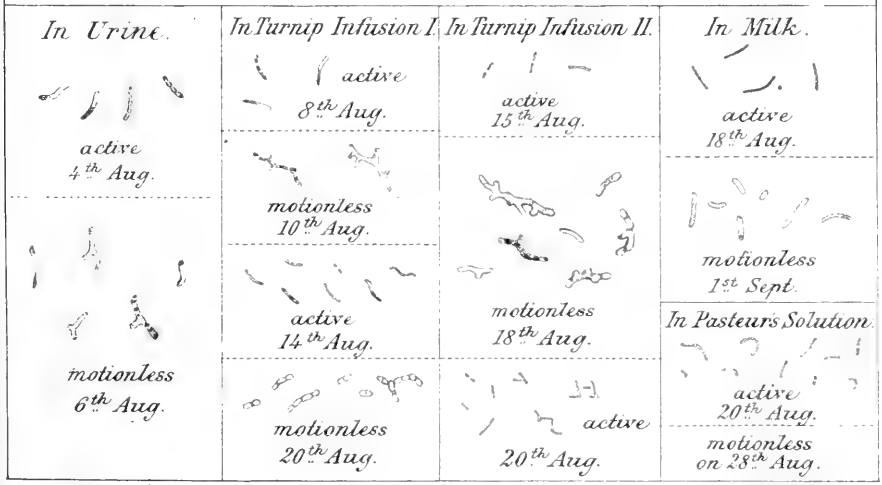
M.c Farlane & Erskine, Lith.rs. Edin.r

Dematium Fuscisporum.



Bacterium N.° I.

Bacterium N.° II.



J. Lister ad nat. del.

M.c Farlane & Erskine, Lith.rs Edin.r

Bacterium Lactis.

In Sour Milk
used for inoculation

motionless 15th Aug.

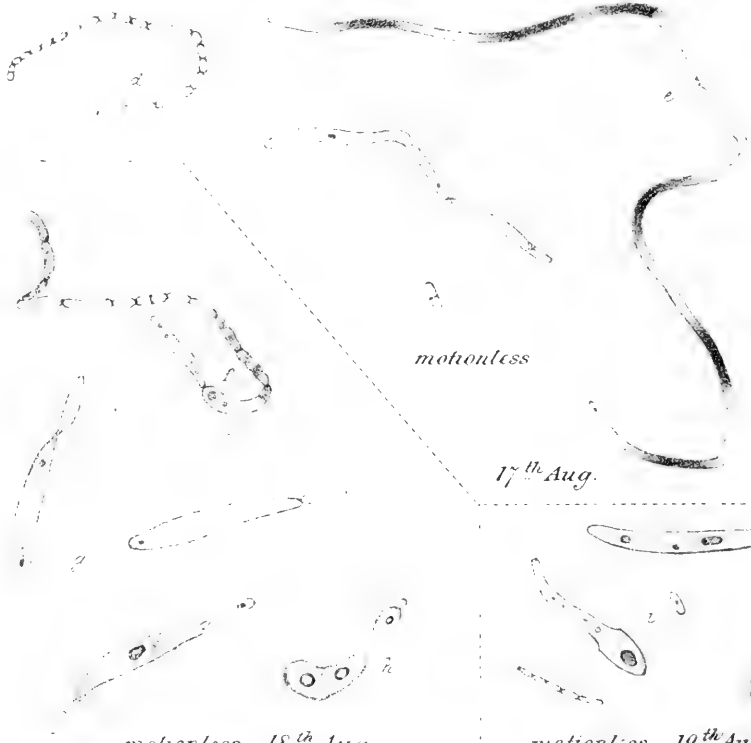
In Boiled Milk I

motionless. 16th Aug.

In Turnip Infusion

motionless, 17th Aug.

In Urine I

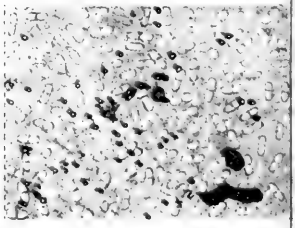


In Pasteur's Solution inoculated from Urine I

motionless
19th Aug 8.15 a.m.

active, 19th Aug 8 p.m

very active, 21st Aug.



motionless
25th Aug.

J. Luster, ad nat. del.

Mc Farlane & Erskine, Lith.rs Edin.r

Bacterium Lactis. (continued.)

In Urine II inoculated from Pasteur's Solution

languid motion
21st Aug. 12 hours after inocul.n

languid motion
22nd Aug. 24 hours after inocul.n

In "Glass Garden" of Urine inoculated from Pasteurs Solution

movement active
immediately after inoculation

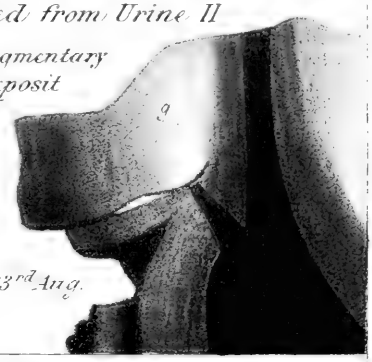
movement languid,
five hours after inoculation.

In Boiled Milk II. inoculated from Urine II

motionless but smaller ones active
22nd Aug.

active
23rd Aug.

Pigmentary
deposit



of 23rd Aug.

In Urine III, inoculated from Urine II.

languid movement, 24th Aug.

motionless
8th Sept.

one thousandth of an inch.

J. Lister ad nat. del.

EXPLANATION OF PLATE XVIII.

To illustrate Dr. E. Klein's contributions to the Anatomy of Auerbach's Plexus in the Intestine of the Frog and Toad.

FIG.

1.—*a*. Nerve branches.
b. Medullated nerve-fibre.
c. Nuclei of the sheath.
d, *e*, *f*. Ganglion cells.
Magnifying power: Hartnack's eye-piece III: Obj. 7.

2.—An isolated ganglion cell like *f* in fig. 1.
a. Nucleus.
b. Cell substance.
c. Nucleated placoids, into which some of the processes spread out.
Magnifying power: Hartnack's eye-piece III: Obj. 9.

3.—A similar ganglion cell as in fig. 2.
a. A nerve branch, with which the ganglion cell is in connexion.
b, *c*. As in fig. 2.
Magnifying power: Hartnack's eye-piece III: Obj. 9.

The explanations of Plates XIX, XX, and XXI, illustrating Professor Lister's paper on Bacteria and the germ theory, are given on the plates themselves.

EXPLANATION OF PLATES XXII & XXIII,

Illustrating Mr. E. Ray Lankester's paper on a Peach-coloured Bacterium.

All the figures, with the exception of fig. 1, were drawn from specimens under observation with Hartnack's objective '10*a* immersion.'

Fig.

1. Portion of a growth of *Bacterium rubescens* from the side of a glass jar. *a*. Homogeneous biscuit-shaped plastids, forming single and compound, dense, glœogenous, globose aggregates. *b*. Homogeneous plastids of larger size, forming loose glœogenous masses. *c*. Unilocular sphærous plastids, forming a very dense, dark-coloured, irregular, slightly glœogenous mass.
2. Acicular multilocular forms of *B. rubescens*, and multilocular sphærous forms attached to a linear growth (leptothrix-form) of a colourless species.
3. Unilocular sphæroids of *B. rubescens* in catenular aggregation.
4. Linear aggregate of *B. lineola*. The units are biscuit-shaped (bacterioid).
5. *a*. Bacillar-form of a colourless species. *b*. Biscuit-shaped form of ditto.
6. Linear growth of a colourless form smaller than fig: 4. The units are generally biscuit-shaped, but have themselves the aspect of being formed by fusion of from two to three sphæroids.
7. *Bacterium termo* from a turnip infusion, sp. gr. 1016, after twenty-four hours' standing in an uncleansed test-tube. *a*. *B. termo* as limited by Cohn. Biscuit-form of this memoir. *b*. *Micrococcus* sp., Cohn; sphærous-form of this memoir. *c*. More minute condition of *b*.

8—9. Glœogenous *Spirillum undula*, forming *Zooglœa* masses.

10 to 29 are all plastids of *B. rubescens*.

10. Homogeneous biscuit-forms, loose, glœogenous.
11. Large ditto, with two highly refringent loculi.
12. Large ditto, with diffused coloration and numerous deeply-coloured loculi.
13. Walled or unilocular biscuit-forms.
14. Globose glœogenous aggregates of large homogeneous biscuit-forms.
15. Smaller ditto.
16. Separate multilocular sphæroid forms with the colour confined to the loculi.
17. Globose glœogenous aggregate of the same.
18. Globose naked, aggregate of unilocular or walled sphærous plastids. Colour entirely locular.
19. Reticular aggregate of unilocular, prismatic, biscuit-shaped plastids.
20. Multilocular sphæroid plastids, in stages of sub-division.
21. Part of a tesselate aggregation of walled, biscuit-shaped plastids (*mycoderma*-phase).
22. Large unilocular or walled bacterioids or biscuit forms developing secondary loculi in their walls. Some of these are of excessive size and irregular growth.
23. Reticular aggregate of trilocular biscuit-shaped plastids.
24. Multilocular filaments or linear aggregates of unilocular sphæroids; subdivision of the coloured loculi is in process.
25. Ditto, of smaller diameter.
26. Unilocular or walled forms of plastid; sphærous, biscuit-shaped, and irregular.
27. Unilocular sphærous forms tending to the biscuit shape.
28. Stellar aggregate of the acicular plastids.
29. Isolated actively swimming acicular plastids. *a*. The dotted part indicates the mode of vibration of the extremity.

Fig. 1.

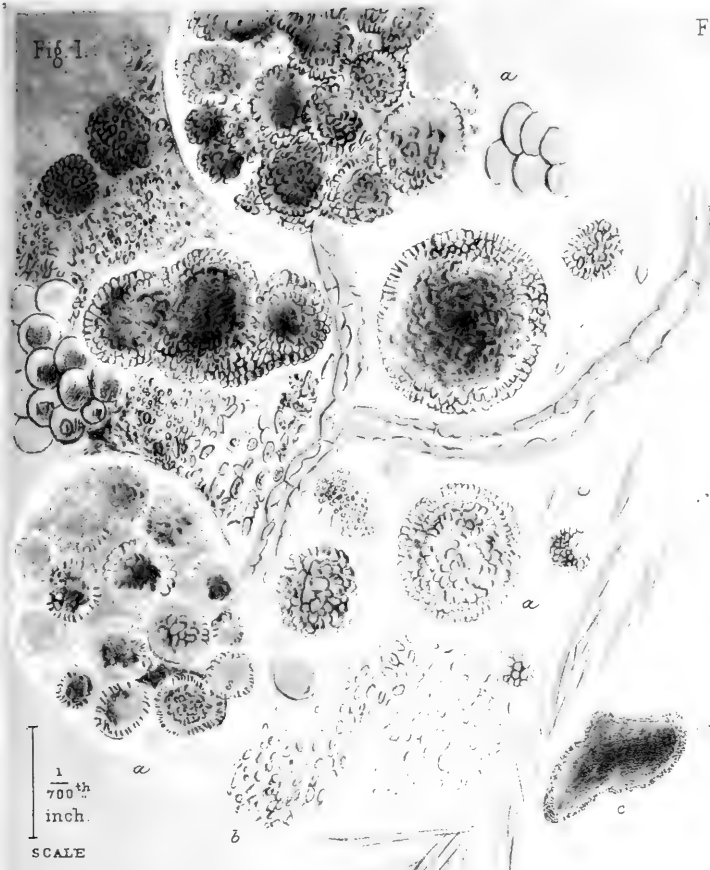
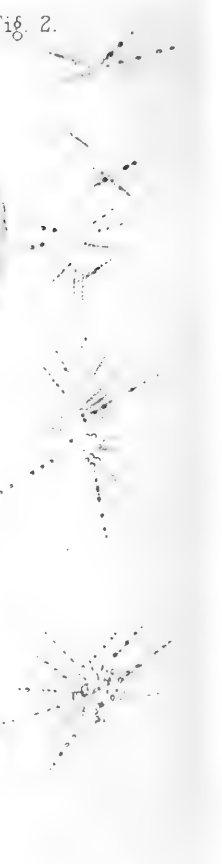
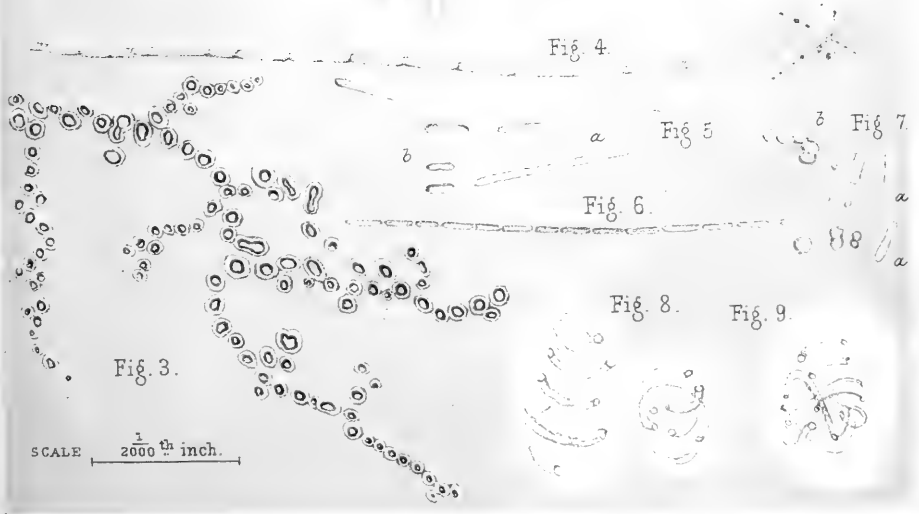


Fig. 2.





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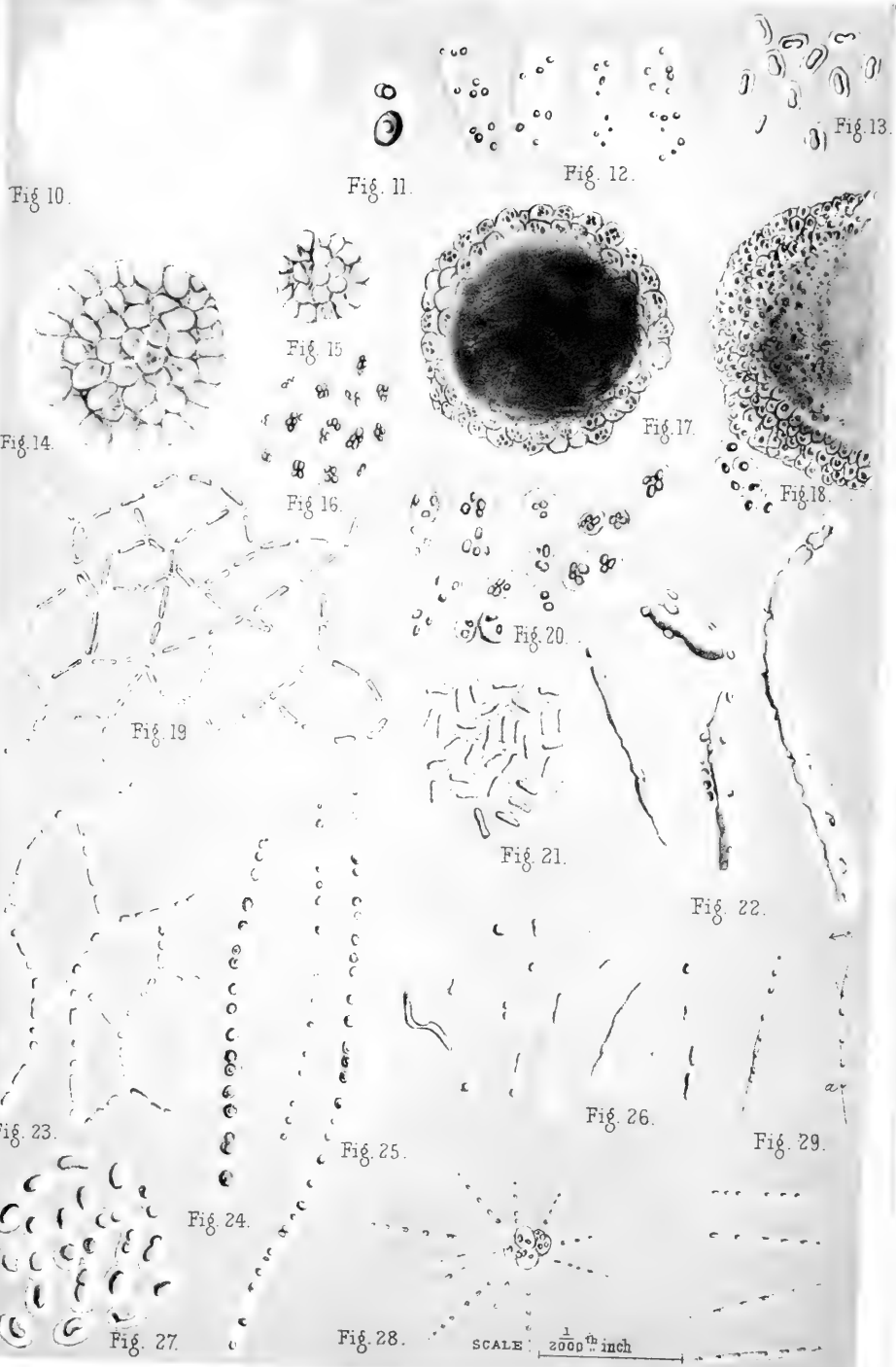

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