# THE MONTHLY MICROSCOPICAL JOURNAL:



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RECORD OF HISTOLOGICAL RESEARCH

AT HOME AND ABROAD.

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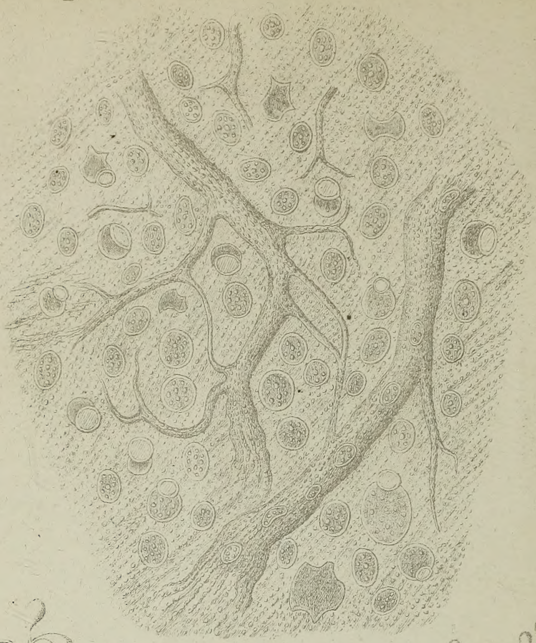

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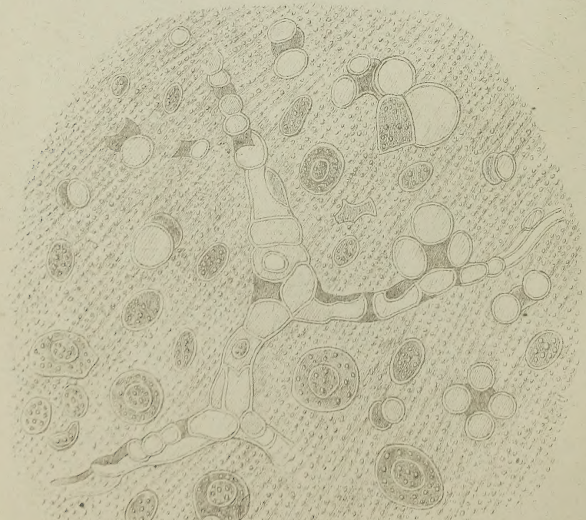

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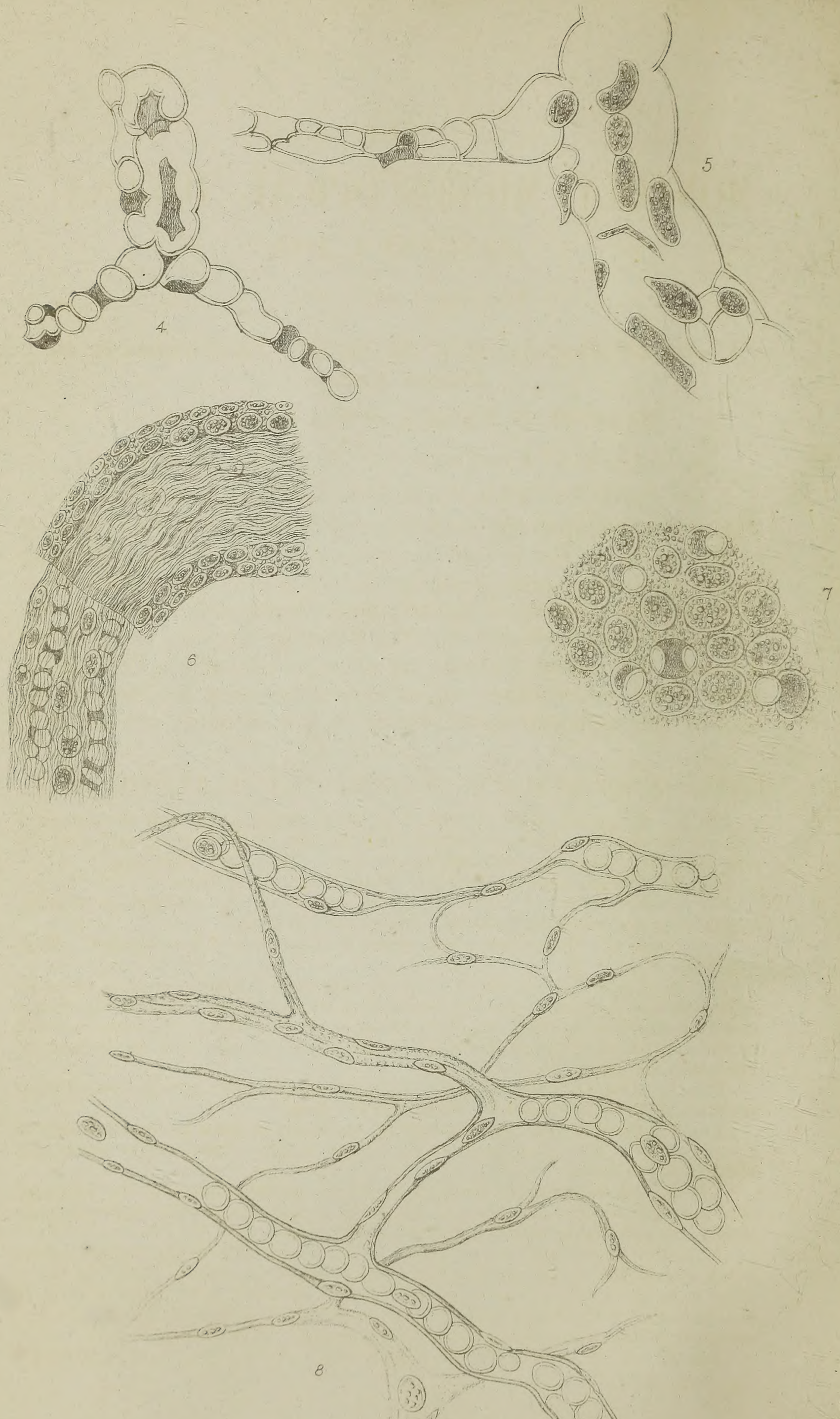
The development of the smaller bloodvessels in Man.

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THE

MONTHLY MICROSCOPICAL JOURNAL.

JANUARY 1, 1875.

## I.—*On the Development of the Smaller Blood-vessels in the Human Embryo.*

By Dr. H. D. Schmidt, of New Orleans, U.S.A.

(*Read before the* Royal Microscopical Society, *Dec.* 2, 1874.)

Plates LXXXVII., LXXXVIII., LXXXIX., and XC.

The researches upon the formation and development of the smaller blood-vessels in the human embryo, described in the following pages, were made some years ago simultaneously and on the same material with those upon the origin and development of the blood-corpuscles in man, published in the February number, 1874, of this Journal. The coloured blood-corpuscles, as set forth in that article, were found to originate within certain primary follicles in the wall of the umbilical vesicle which, in this instance, was attached

EXPLANATION OF PLATES LXXXVII., LXXXVIII., LXXXIX., and XC.

Fig. 1.—Fragment of the chorion of the small human ovum with the abnormal embryo, showing the manner in which the villi arise by small roots, and also the process of multiplication of nuclei by gemmation. Magnified 465 diam.

Fig. 2.—Various forms of the villi. Magnified about 6 diam.

Fig. 3.—Formation of embryonic blood-vessels by the coalescence of cells, arising from the nuclei by budding. Magnified 465 diam.

Figs. 4 and 5.—Embryonic blood-vessels in the course of development. Magnified 465 diam.

Fig. 6.—Fragment of a villus from the chorion of a human embryo, about 16 millimètres in length; *a*, portion denuded of its epithelium, showing its fibrous structure with two capillary vessels in the process of formation; *b*, portion covered by the epithelium, which, out of focus in the middle, is only seen at the margin. Magnified 265 diam.

Fig. 7.—Deeper layer of epithelial nuclei of the same villus, showing their multiplication by gemmation. Magnified 465 diam.

Fig. 8.—Formation of blood-vessels by the fusion of spindle-shaped bodies in the pia mater of the spinal marrow of a human embryo about twelve weeks old. Magnified 465 diam.

Fig. 9.—Blood-vessels of the pia mater from another embryo of the same age. The granular fibrillous bundles, of which the larger vessels were originally composed, are still distinguished. Magnified 465 diam.

Fig. 10.—Artery from the pia mater of the spinal marrow of a human embryo about nine to ten weeks old, showing three coats of the vessel. The interior is filled with blood-corpuscles, the greater portion of which became discoloured by the action of the chromic acid solution, in which it had been laying; the rest, comprising the younger ones, remained unaffected. Magnified 465 diam.

Fig. 11.—Vein from the same preparation. Among the blood-corpuscles a

to an imperfectly developed embryo of an ovum of very small dimensions. The embryo, arrested in its growth, consisted only of a mass of embryonic cells, nuclei, fibrils, &c.

It was in the outer membrane of this small ovum, which, like the umbilical vesicle, appeared to be normal in its development, that I observed the primary formation of the smaller blood-vessels, as well as a certain process of multiplication of nuclei, which, as far as I know, had up to that time not been observed in the tissues of vertebrated animals.

From the examinations made on the chorion of this ovum, together with others of older embryos in various stages of development, it appeared that the formation of the smaller blood-vessels occurred in two different modes. The one, belonging to the earlier period of development, was observed to consist in a coalescence of certain cells, while by the other, at a somewhat later period, the formation of the blood-vessels was effected by the fusion of certain spindle-shaped bodies.

A short time ago, however, long after these observations were committed to the paper, a human ovum, still smaller than the one already mentioned, was put into my hands, only a few hours after its abortion. This specimen was the smallest I had ever seen, for the embryo which it contained scarcely measured 6 mm. in length; it was perfectly normal and fresh, and still buried in the membrana decidua, from which I had to remove it.

While the examination of the umbilical vesicle of this embryo corroborated my former statements regarding the origin of the coloured blood-corpuscles, as we shall see hereafter, that of the

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number of mother-blood corpuscles are seen, and also some free blood-crystals. Magnified 465 diam.

Fig. 12.—Blood-vessel from the pia mater of the spinal marrow of a human fœtus 5½ months old; *a*, nuclei belonging to the fibrous coat; *b*, nuclei belonging to the muscular coat; *c*, the same, bearing vesicles; *d*, cells of the epithelium in its formation. Magnified 465 diam.

Fig. 13.—Small human ovum; *a*, membrana decidua vera; *b*, decidua reflexa, separated from the ovum and pinned down. The light spot upon it represents the embryo seen through the membrane. Natural size.

Fig. 14.—The embryo of this ovum. Natural size.

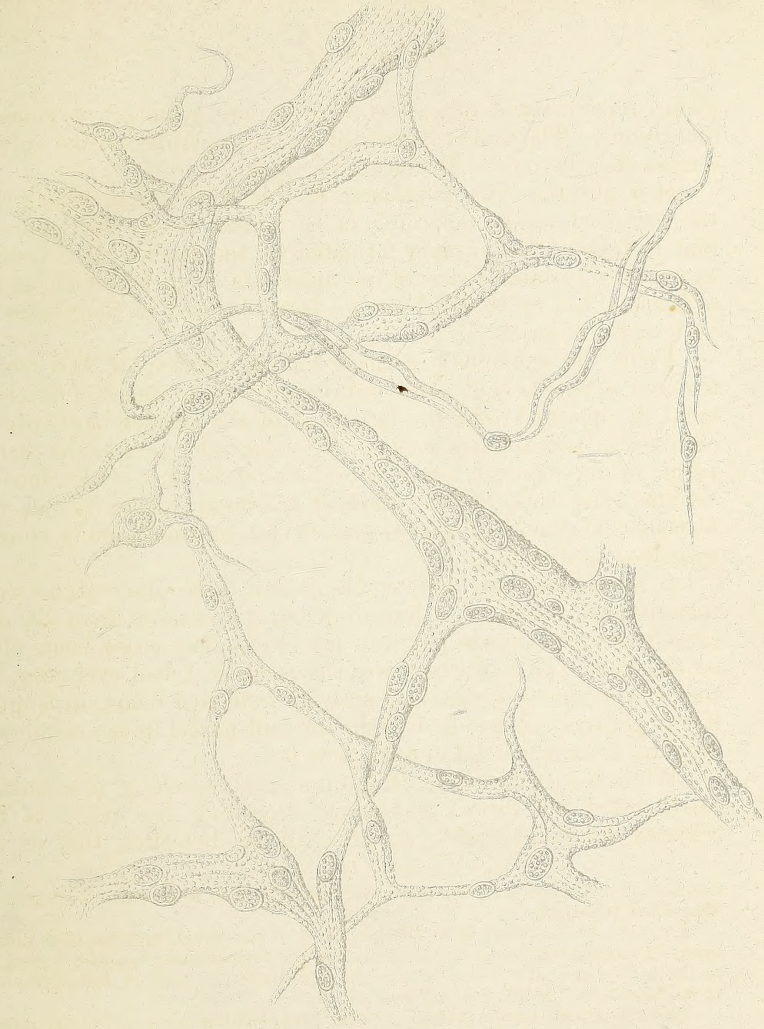
Fig. 15.—The same enlarged; *a*, the elongated coccyx, making a spinal turn.

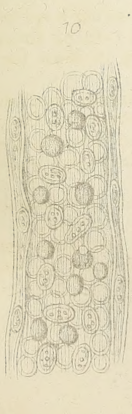
Fig. 16.—The same, showing the left side. The spinal turn of the inferior portion of the spinal column has been straightened, in order to show its proper shape; also enlarged.

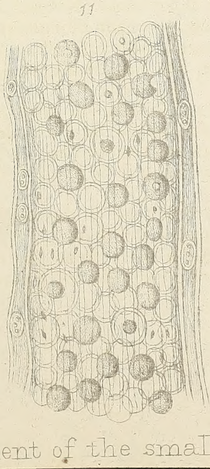
Fig. 17.—The same embryo (enlarged), after having remained about eighteen hours in a weak solution of chromic acid. Its various organs described in the text are here exhibited; the umbilical vesicle has, for the sake of completeness, been represented entire, though at this time a piece of it had already been removed for examination.

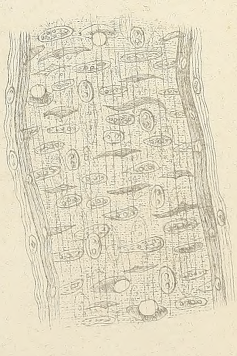
Fig. 18.—Represents the structure of the umbilical vesicle of this embryo, as described in the text. Magnified 465 diam.

Fig. 19.—Embryonic blood-vessels from the chorion of the same specimen, containing embryonic blood-corpuscles in the form of granular nuclei. Magnified 465 diam.



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The development of the smaller bloodvessels in Man.

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chorion, however, showed the formation of the smaller blood-vessels not to take place, as I had observed in the former cases, by the coalescence of certain round cells, but by the fusion of spindle-shaped bodies. From this apparent discrepancy, observed in the formative process of the smaller blood-vessels of the chorion of these two different ova, it must, however, not be inferred that my former observations were incorrect, but we must rather seek for an explanation elsewhere. For this reason, too, I shall in the following pages confine myself to an accurate statement of the phenomena I observed, from which the reader may draw his own deductions.

In returning now to the first ovum, containing the imperfect embryo, we shall commence our statements with a description of the structure of its wall. This consisted of two membranes, an outer and stronger one, the so-called chorion, giving origin to the villi, and an inner one, consisting of an exceedingly delicate loose fibrous tissue. The outer surface of the chorion, including its villi, was covered by a comparatively thick layer of primary epithelial cells or nuclei, imbedded in a granular matrix. In removing this layer, it was found that its base, the membrane itself (Fig. 1), consisted of an amorphous finely granular substance, in which, however, a considerable number of fine granular fibrillæ were crossing each other in various directions. These represented the elementary form of the later fibrous or connective tissue. A considerable number of certain forms of nuclei and also cells were imbedded in the membrane. The most interesting of these elements were a considerable number of nuclei, distinguished by bearing on their surfaces concave depressions, corresponding to the segment of a sphere, either empty or occupied by double-contoured vesicles or cells (Figs. 1 and 3). They remind us of those mother-blood corpuscles, described in the paper before mentioned, having, as will be remembered, similar depressions on their surfaces, which, however, differed in this respect, as they were produced by the separation of a consistent, though soft body (the embryo blood-corpuscle), in the case of the nuclei in question, they were caused by the separation of a small vesicle, arising from the substance of the nucleus; in other words: in the one case, the product was an embryo blood-corpuscle, while in the other it was a vesicle or cell, destined, as we shall see, to take part in the formation of the primary embryonic blood-vessels, or to develop, perhaps, into a nucleus itself. The process of multiplication of the nuclei and cell, therefore, instead of consisting in a division of the old, we find here to take place by a separation of the young brood in the form of a vesicle or bud, being in reality a process of *budding* or *gemmation*. The whole process takes place as follows: on the surface of the nucleus, and partially imbedded in its substance, a small globular body appears, which, gradually enlarging, is developed into a clear double-contoured cell. This may either remain in its place and take part in the building up of the small blood-vessels, or be thrown off from its mother-body and be developed into a nucleus itself. In the latter case its contents become more opaque and small granules appear within them.

The concave depression which the young nucleus leaves behind on the surface of the mother-nucleus never disappears entirely, no matter how much the latter may change its original form by the production of others. For this reason the productive force of the mother-body can always be inferred from the number of depressions. Several vesicles or young nuclei may arise simultaneously from the old one (Fig. 3); accordingly, I observed in the chorion of the ovum in question a number of them, bearing three to four vesicles, and, judging by the number of depressions, one nucleus may even produce five to six of its kind. Along with the production of new vesicles the substance of the old nucleus seems to be rendered more dense, its double contour and the granules it contains become less distinct, and the whole body assumes at last a shining, greenish tint. This is especially the case in specimens which have been laying for a few days in a weak solution of chromic acid; in these, the vesicle, at its first appearance, is distinguished by a reddish tint. In consequence of the concave depressions, produced by the formation of the vesicles, and in proportion to the diameter of the latter, the mother-nuclei appear in many fold, mostly unsymmetrical forms (Figs. 3, 4, and 5), and it is difficult to find an object of suitable comparison. In many cases the vesicle is developed, as in the formation of blood-vessels, into a large, clear, double-contoured cell. The greater now the diameter of the cell, the greater is also the depression observed on the nucleus, and it is for this reason that, after the production of a number of cells, very little of the substance of the nucleus is left. Equally as varying as the form of these nuclei were their diameters; while, namely, those of the smaller ones only reach to about $\frac{1}{100}$ mm., those of the larger attain a size of about $\frac{3}{100}$ mm. in length, by $\frac{1}{100}$ mm. in breadth. The latter are found in connection with the formation of the first embryonic blood-vessels. The more the entire embryo advances in its development, the more also the size and fertility of these mother-nuclei are reduced.

Long before I discovered these mother-nuclei, bearing buds in the form of cells, in the membranes of the ovum, I had met with them in the nervous tissues as well as in the pia mater of those human embryos on which I had worked. But as they appeared here in a smaller form, and with smaller vesicles or corresponding depressions, I failed to recognize their true nature, and considered them passingly as old nuclei, deformed by degeneration. Nevertheless, however, did I observe in their immediate neighbourhood, their offsprings in the form of small, round, and pale bodies, which, as I could nowhere discover evidences of a process of multiplication by a direct division of the nuclei, almost inclined me to think more seriously of a primary formation of nuclei from the plasma itself. It was not until I met in the epithelium, covering the villi of the chorion of a human embryo of about 16 mm. in length, with nuclei, provided with vesicles or their corresponding depressions, that my attention was called to the process of multiplication by budding or gemmation. But further proofs in corroboration of this idea were still wanting, until chance threw the small human ovum under consideration into my hands, in the membranes of which I observed the whole process in full operation.

Besides these mother-nuclei, a considerable number of others, of a more or less oval form and bearing no vesicles, were imbedded in the membrane; they were distinguished by a double contour and a fine granular aspect, containing a number of fine granules. A small number of nucleated cells of a granulous appearance were further observed here. The concave depressions, which many of these bodies showed upon the surface, inclined me to look upon them as mother-cells.

The villi, arising from the chorion of the ovum, were tuberous bodies, connected to each other in a manner similar to that of the branches of a tree, by shorter or longer stems. (Fig. 2.) To the larger bodies, smaller pear-shaped ones were attached by means of fine pedicles. The entire tree-like body thus formed seemed to be connected by a fine stem to the membranous chorion. But by a closer examination, and after the removal of the epithelium, I found that this stem arose in reality from the membrane, by a number of fine branches or roots. (Fig. 1.) The tissue of the villi resembled that of the chorion itself; it consisted of very fine granular fibrillæ, with a number of more or less oval nuclei imbedded in it. The epithelium covering the membranous portion of the chorion, and extending over the villi, consisted in this small ovum of a granular matrix with oval nuclei, a considerable number of which were provided with the above-described vesicles and concave depressions. This fact proves that it is a production of the ovum itself.

The inner membrane of this ovum consisted of a loose, spider-web-like connective tissue of fine, wavy fibrillæ, containing a number of free nuclei and nucleated cells. Some of these latter were open, and apparently in the act of setting free their nuclei, showing that here the process of multiplication took place by the endogenous mode. Besides these, however, a number of small nuclei-bearing vesicles were also observed. No trace of epithelium could be discovered on the inner surface of this membrane.

Let us now direct our attention to the formation of the primary blood-vessels, such as I observed it to occur, not only in the chorion of the small ovum above described, but also in that of somewhat older ova, as well as in the pia mater of the spinal marrow of a very small embryo. As has been remarked before, some of the vesicles or cells, springing from the mother-nuclei, were probably developed into ordinary nuclei, pertaining to the fibrous tissue of the membrane; a portion of these might even assume the function of mother-nuclei and give rise to other vesicles. The rest, however, especially the larger ones, were observed to take part in the formation of the embryonic vessels of the membrane. This seems to be effected by a number of these cells, arising from their respective nuclei at different points, and meeting each other in such a manner as to form a row. (Figs. 3, 4, and 5.) To form this row, however, they were not placed, as might be expected, in regular order, cell by cell; on the contrary, the nuclei, from which they were seen to arise, were distributed throughout the membrane without any special arrangement or order; neither was their size and form more regular. The formation of a vessel, of course, is brought about by the coalescence of cells arising from neighbouring nuclei, and opposite to each other. The tube of the vessel is not necessarily formed by one single row of cells; for when, by the meeting of two cells unequal in diameter, a vacancy is left, this will be filled up by a third cell, arising from the nucleus of one of the former. Rarely less than two cells were seen to arise from one nucleus, but more frequently five to six. Often, large irregularly-formed cells were met with in the course of a vessel, the nucleus of which was marked on its margin by a number of crescent-shaped indentations. In these cases, the large cell was formed by the absorption of the contiguous walls of a number of small ones, which originally had arisen from one and the same nucleus, of which the crescent-shaped indentations were indicating the place of their origin. (Fig. 4.) After the tube of the vessel is fully formed by the absorption of those portions of the cell-walls contiguous to each other, the nucleus assumes, as it seems, by a gradual rounding of its indentated margin, a round or oval form. Very frequently, however, by the production of a number of cells, it is deprived of so much of its substance, that finally nothing of it remains but a long indentated body, which by the rounding of its angles assumes an oblong form. This form is often met with in the walls of young vessels, long after the cellular formation of the blood-vessels has ceased to occur. In some instances, especially with the smaller vessels, we meet with two cells arising from two opposite points of one and the same nucleus, the latter occupying the whole diameter of the row (Figs. 3 and 4), thus seeming to form an obstacle to the opening of the vessel. This, however, is overcome, I think, by the growth of the cells, at the expense of the substance of the nucleus, of which at the end of the process only little will be left. Thus the absorption of the entire nucleus will sometimes be effected. In the nervous tissues of the human embryo, I observed the nuclei, belonging to the primitive fibrils of the axis cylinder, disappearing during the process of development by fusing with the latter. In the small ovum under discussion, the formation of the blood-vessels, as described above, was observed to occur throughout the entire chorion to the roots of the villi, but had as yet not extended into the latter.

In examining the chorion of an embryo of about 16 mm. in length, we still find a considerable number of mother-nuclei with vesicles, budding from their surfaces, imbedded in the tissue. The structure of the membrane is on the whole further developed, for the fibres of the connective tissue are more definite in their form, and the cells of the epithelium placed nearer to each other. The stems of the tuberous villi, arising from the surface of the membrane, have gained in thickness, also their epithelium, which now consists of two layers of cells. (Figs. 6 and 7.) The fine fibres of the connective tissue of the villi themselves are fully developed and have lost their granular aspect. But the smaller blood-vessels, although more numerous, and having penetrated into the villi, still consist of cells, only partially fused with each other. (Fig. 6.)

Among the blood-vessels of the pia mater of the spinal marrow of embryos of about the same period as the preceding, some, especially the larger ones, were found to contain a considerable number of coloured blood-corpuscles, showing that the circulation of the blood through them had already been established; while others, especially the capillaries, still showed their original formation by the cellular structure of their walls, and the entire absence of blood-corpuscles in their interior.

The muscular structure of the heart at this period consists of embryonic muscular fibrillæ, each of them representing a row of simple granules. The walls of the larger blood-vessels, such as the aorta, umbilical vessels, &c., consisted of a granular, amorphous, already partially fibrous matrix, in which a considerable number of oval or even round nuclei were imbedded. On their inner surface a number of clear cells, containing a coarsely granular nucleus, devoid of an enclosing membrane, were met with. These cells, being present in various other tissues of the embryo, probably stand in some relationship to the multiplication of the nuclei.

Judging from the development of the heart and the larger blood-vessels of this period, it would seem that the circulation of the blood were already complete; that this is not the case is proved by the almost entire want of fully formed capillaries in a number of organs which I examined. For this reason I am inclined to believe that it occurs only through anastomosing branches of the smaller arteries and veins. Further, the fact that the smaller vessels of the chorion, and particularly those of its villi, still mostly consist of cells, the contiguous walls of which are as yet not fused, proves sufficiently that even here the circulation is not fully established, and that accordingly the embryo does as yet not receive its nutriment from the mother directly through the circulation of its own blood.

From these observations on the formation of the primary embryonic blood-vessels we may infer that the larger vessels are very probably formed, in the same manner and simultaneously with other parts and organs of the embryo, from the embryonic cells of the *area germinativa*, while those smaller ones, imbedded in membranes, are formed by the coalescence of larger and smaller cells, as above described.

Continuing our examination of the blood-vessels of the pia mater of the spinal marrow, which membrane in regard to its delicate structure and transparency is better adapted than other tissues for this purpose,—at a later period, in embryos, about eight to nine weeks old, we find that the embryo has entered another stage of development, in which these vessels are no more formed by the coalescence of cells. The pia mater here is still represented by an amorphous, granular membrane, in which, however, a thin layer of delicate connective-tissue fibres has already been developed, and containing besides a considerable number of oval or round nuclei. The latter consist of small granules, surrounded by a thin membrane. Among them, however, a number of mother-nuclei, with small buds arising from their surface, are also observed, showing that multiplication of these nuclei still occurs by the process of budding or gemmation. The blood-vessels within the pia mater show different stages of development.

The formation of those vessels described in the preceding pages was brought about, as we have seen, by the coalescence and subsequent fusion of cells, and so were the greater number of those within the tissue of the pia mater of embryos from nine to ten weeks old, through which, as they were filled with blood-corpuscles, the blood had been evidently freely circulating. A smaller portion of these, however, still in process of development, were observed to be formed by another process. Where, in the former instance, the formation of the tube of the vessel was accomplished by the coalescence of cells, it is now by the fusion of elementary fibrils. The process of gemmation, though still occurring, as far as the multiplication of nuclei is concerned, has ceased for the formation of vessels; the *fibrillous* formative process of the blood-vessels has now commenced. This consists in the formation of granular fibrillæ, lying parallel to each other, and becoming in the course of their development fused into the form of a tube.

Postponing at present a more minute demonstration of this process, let us return to the already formed blood-vessels of the pia mater of the last-mentioned embryos. The greater part of the vessels, imbedded in this membrane, embracing, as it seemed, a considerable portion of the larger and smaller capillaries, consisted of tubes of about $\frac{3}{100}$ to $\frac{1}{100}$ mm., the wall of which represented a clear, very transparent membrane. In this, a number of oval and irregular oblong nuclei were observed. The irregular form of many of these bodies lead me to regard them as the remains of those mother-nuclei from which, in the chorion of the smallest embryos, those cells, forming the primary embryonic blood-vessels, were seen to arise. The larger vessels were filled with blood-corpuscles, which, by the action of a weak chromic acid solution, in which the specimens had been preserved, had lost their colouring matter, and thus appeared now in the form of clear double-bordered cells. The smaller ones, on the contrary, contained only a few single corpuscles, which appeared to have been forcibly pressed into their interior. This circumstance points to the probability, already mentioned in connection with the development of the coloured blood-corpuscles in man, that these bodies penetrate with every contraction of the heart only gradually into the newly formed but still closed blood-vessels. Among the blood-corpuscles, contained within larger vessels, a number of mother-corpuscles, bearing one embryo-corpuscle, were always observed.

In examining the larger vessels, imbedded in the pia mater, of this period, viz. the small arteries and veins, their structure was already found considerably more complicated. Their walls, namely, consisted no more of a homogeneous membrane with large nuclei, but of two distinct layers. In place of the large nuclei, often irregular in form, a larger number of smaller ones, of an oval or roundish form, and imbedded between the two membranes, are now observed. These I presume are the descendants of a small number of mother-nuclei with small vesicles budding from their surfaces, always found in company of the smaller nuclei, which they seem to have preceded. The inner membrane probably represented the original tube of the vessel, while the outer one, distinguished by fine, longitudinal, fibrillous lines, represented its later fibrous coat. In the arteries, the fibrous character of the latter was more strongly marked than in the veins. In the larger arteries and veins of the pia mater these two layers, forming the wall of the vessel, and distinguished by their regular, sharply-defined double contour, were found to be separated from each other by a third layer of irregular thickness. (Figs. 10 and 11). The latter most probably represented the muscular coat of the vessel in progress of formation. In those cases where it was found, the nuclei were imbedded in it, and a number of them were observed to assume a position in which their long axis comes to lay at right angles with the axis of the vessel. No muscular fibres, however, could as yet be distinguished.

The greater number of the vessels just described were filled with coloured blood-corpuscles, of which the greater portion had lost their colouring matter by the action of the chromic acid solution, and appeared in the form of clear double-bordered cells, while the rest, comprising the younger corpuscles, had remained unchanged; in many vessels they were crowded in such a degree that the former, by mutual pressure, had assumed a hexagonal form. A number of mother-blood corpuscles were always found among them; they had also become discoloured, while their embryo-corpuscles had retained their colouring material. In many of the larger vessels, besides the blood-corpuscles, a number of coloured molecules and also small blood-crystals were found. Sometimes even blood-corpuscles were met with, in the substance of which small crystals were found to be imbedded. Many vessels were completely coloured by the hematin escaped from the blood-corpuscles.

The blood-vessels were probably found, as already mentioned, by the coalescence of cells. But besides these, I observed in the same pia mater, a number of capillary vessels in progress of formation, the fibrillous composition of which attracted my attention. The result of a series of close examinations, which I subsequently made on the smaller vessels of the pia mater of this embryo, as well as of a number of others of different periods of development, convinced me that these vessels were formed in a different manner from the preceding, to which I have already alluded. This consists in the formation of granular fibrillæ, laying parallel to each other and becoming eventually fused into the form of a tube. (Figs. 8 and 9.) In connection with the development of the coloured blood-corpuscles, I have already pointed to the different modes of origin and development during the earlier and later periods of embryonic life. In the development of the embryonic blood-vessels, according to these observations an analogous change in the mode of formation seems to take place. The exact time at which the first or *cellular* process of formation is superseded by the second, or *fibrillous,* I am at present unable to state.

In an embryo of 16 mm. in length, I found all the vessels of the liver still to consist of granular fibrillæ, while those of the chorion and its villi, as well as those of the pia mater, were formed by the *cellular* process, above described. It appears, therefore, that the formative process of the vessels is not always the same in different organs. In the pia mater, as far as I am able to judge, the fibrillous formative process seems to have already commenced about the eighth or ninth week, and to be in full operation some weeks later. We will now describe its course, as I observed it in the pia mater of human embryos of about twelve weeks, where, at this period, the small arteries and veins, as well as the capillaries, prove to be quite numerous.

The primary elements of the vessels to be formed represent granular, spindle-shaped bodies, with an oval nucleus in their centre; they are formed by the granules of the plasma, arranging themselves into rows at the opposite poles of the nucleus. The latter, therefore, forms the basis of development. After one row has thus been formed by the mutual arrangement and attachment of these granules, and attained a certain length, a second one, starting also from the nucleus, begins to be formed in the same manner; after this, a third and fourth, until their aggregate breadth equals that of the nucleus. But, as with the beginning formation of the succeeding rows of granules, or fibrils, those first formed continue to elongate by the attraction and appropriation of new granules, a spindle-shaped body is the result. In the formation of a vessel by a greater or smaller number of these bodies, they join—already during their own formation—each other laterally in such a manner that a part of the length of one overlaps another; they finally are fused with each other into the form of a tube.

In consequence of the various directions in which these spindle-formed bodies, or the bundles formed by them, are developed, they frequently cross or meet each other. When this occurs, it is usually found that the point of the smaller body or bundle adapts itself to some extent to the lateral border of the larger. As the result of a number of such connections, or points of fusion, the meshes of the capillaries are formed. (Fig. 8.) In this manner, capillaries, still in progress of development, are frequently observed to be fused with larger vessels, through which blood-corpuscles have already been circulating. The small arteries and veins are formed by the fusion of a number of bundles, composed of the spindle-shaped bodies. The contiguous borders of the latter can still be recognized on the vessels of the embryo of twelve weeks. (Fig. 9.) Frequently one of the larger vesssls is observed to be formed by the union of two smaller ones, the latter still being in process of development themselves. In the pia mater of human embryos of this age, the fusion of the spindle-formed primary elements into bundles, and of these into larger or smaller vessels, is observed to take place in an irregular manner, and also, as already mentioned, in various directions. It is owing to this circumstance that up to this period the form of the meshes, and even that of the vessels still in process of development, is quite indefinite. Equally irregular are the numerous nuclei, contained in the walls of the arteries and veins, in size as well as distribution.

The tube of the vessel being thus formed by a parallel apposition and subsequent fusion of those fibrillous spindle-shaped bodies, is opened and distended by the blood-corpuscles penetrating into it from the neighbouring more fully developed vessels; thus we meet in the newly-formed capillaries only a few blood-corpuscles, which have evidently been forced into them from larger neighbouring vessels, as they are seen to distend the calibre of the vessel only to a certain distance. (Fig. 8.) The further development of the vessels is moreover promoted by the nutrient matters contained in the liquor sanguinis. While a number of blood-vessels are thus gradually approaching their final development, and in consequence the circulation of the blood is further extended, others are still formed by the same process.

At what period the formation of new vessels in the human embryo ceases to take place I have not determined, as my examinations concerning this subject were extended only to the vessels of the fœtus, about $5\frac{1}{2}$ months old, in the pia mater of the spinal marrow of which new capillary vessels were still seen in the process of formation.

The further development of the newly-formed capillary vessels consists, as already mentioned, of the fusion of the granular fibrils of which they still consist, into a structureless membrane. In the small arteries and veins, formed by the *fibrillous* process, the granular fibrils are developed into permanent, smooth fibrillæ of connective tissue, while the diameter of the vessel increases by the continuous formation of new fibrils from the plasma of the blood circulating within them. The multiplication of the nuclei takes place by gemmation.

In the pia mater of the embryo of about nine weeks we found, as will be remembered, the largest vessels already composed of three distinct layers, though in the middle and inner one no particular structure could as yet be recognized; the outer layer alone was composed of delicate fibres of fibrous tissue. In the succeeding periods also, it is especially this layer which is most distinctly developed. The first traces of the formation of the muscular layer consist in the appearance of a larger number of oval nuclei, the long axis of which lays at right angles with that of the vessel. The inner layer, however, which in the beginning was equal in thickness to that of the outer one, is gradually rendered more indistinct, perhaps by the increasing development of the latter.

In the fœtus of $5\frac{1}{2}$ months, finally, the outer coat of the larger vessels of the pia mater has attained sufficient consistency to permit its separation from the loose areolar tissue by which these vessels are surrounded, thus affording an opportunity for a closer examination of their respective coats. In the walls of such vessels of about $\frac{6}{100}$ mm. diameter, and freed from the loose fibrous tissue surrounding them, only two layers, distinct from each other, are recognized (Fig. 12). The outer of these, representing the fibrous coat of the vessel, consists of delicate wave-like fibres of connective tissue, holding numerous, more or less oval nuclei, their long axis being parallel to that of the vessel. They are distinguished by a fine double contour, and filled with small granules. The inner layer, which appears somewhat darker than the outer, represents the muscular coat, already tolerably advanced in development; it also contains a considerable number of nuclei, with their long axes, however, at right angles to the course of the vessel. A number of these nuclei, though somewhat larger and of an oblong form, show, like those of the outer coat, a fine double contour, and are filled with granules. The rest, however, represent mother-nuclei, being distinguished by a greenish lustre. A number of these bear those characteristic small vesicles or buds, which show that the process of their multiplication is still in active operation: the remaining ones appear in the form of irregular spindle-shaped bodies, with crescent-shaped notches, the traces of their former activity, in their contours. The muscular coat itself appears as a fine granulous substance. Besides those nuclei just described, a number of others of an oval form are observed on the inner surface of the vessels; these probably represent the epithelium of the vessel in the process of formation.

I shall next proceed to give an account of the results obtained from the examinations made on the very small ovum, alluded to in the beginning of this article.

This was the smallest specimen of the human ovum I had ever seen. (Fig. 13.) Being removed from the membrana decidua, which had come away entire, it was found to be perfectly fresh and normal. Slightly oval in shape, it only measured 13 by 11 mm. in diameter. The oval form, however, may have been produced by a stretching of the membrana decidua, which was pinned down upon a cork during the operation of its removal. Its surface was almost entirely covered by villi. The embryo it contained (Fig. 14), measuring only 6 mm. in length, was bent upon itself, the inferior part of the spinal column making a spiral turn to the left side of the body. The rudiments of the vertebræ when examined through a lens (Fig. 15), manifested themselves in the form of opaque spots. The superior portion of the body was divided into three segments, the upper and larger one of which represented the future cranium, the two others visceral arches; there was no trace of the formation of the eye or ear to be seen. Directly below the visceral arches, and occupying the middle portion of the body, there were three round prominences, which, as I suppose, represented the rudiments of the heart. (Figs. 15 and 17.) They were of a reddish colour, and the larger of them was, by a subsequent examination, found to be hollow. Below these prominences the pedicle of the umbilical vesicle was seen to arise from the rudimentary alimentary canal; and still lower down, within the curve of the spinal column, also the umbilical vessels. The extremities manifested themselves only in the form of four small delicate buds; the upper ones laterally and behind the above-mentioned prominences, and the lower ones within the curve of the inferior portion of the spinal column. That portion of the vertebral column situated beyond the rudiments of the lower extremities and representing principally the coccyx, formed an appendix of such a length that it might, without any impropriety, and in support of Darwin's theory, well be looked upon as a tail. (Fig. 16.) The embryo itself was attached to the walls of the egg by a delicate membrane, which, after arising gradually from the abdominal surface, and including, but *not* closely surrounding, the umbilical vessels, was soon blended with the walls of the egg. (Figs. 16 and 17.) The rudimentary heart, alimentary canal, and umbilical vesicle, were not embraced by this membrane, but, laying at its right side, were projecting into the general cavity.

Referring, for a better understanding of the general form of the embryo, the reader to the drawing accompanying this article, which I carefully took from the object in its fresh condition, while immersed in water, I shall now state and consider the results of the microscopical examination, regarding the structure of the umbilical vesicle and chorion of this interesting specimen of human embryo, as they really formed the principal object of the investigation. To do so, however, I must somewhat deviate from the object, in order to recall to our mind the structure of the umbilical vesicle, in which I discovered the origin of the coloured blood-corpuscles, described in my treatise on that subject.* It will be remembered that the embryo to which it belonged had been arrested in its growth, being represented only by an accumulation of embryonic cells. Further, that the wall of this vesicle was found to be a system of primary follicles and canals, the interior of which was lined by large hexagonal cells, in which the blood-corpuscles originated in the form of pale double-contoured nuclei. The interior of the follicles and canals were occupied by considerable accumulations of fully-developed coloured blood-corpuscles.

The examination of the umbilical vesicle of the small normal embryo, described above, became therefore a matter of interest, as its results would either disprove or corroborate the correctness of the conclusions which I had drawn from the observations made on the former specimen. They proved to have been correct, for in examining a portion of the wall in its fresh unchanged condition, the individual follicles with their intervening canals could be distinctly recognized. As in the former case, the follicles consisted of the same large hexagonal cells (Fig. 18), which also contained, besides a number of small nuclei, one or two larger ones. But there were no fully-developed coloured blood-corpuscles to be seen either within the follicles or the canals. In place of these, however, a considerable number of free small round nuclei, containing a number of granules, were observed. The same kind

* 'M. M. J.,' February number, 1874.

of nuclei I found within the cavity of the rudimentary heart and also within the newly-formed blood-vessels of the chorion. (Fig. 19.)

In examining the chorion I found it to consist of an amorphous, granular membrane, covered by an epithelium, consisting of a decidedly granular matrix, in which a considerable number of small nuclei were imbedded; the villi consisted of the same elements. Within the structureless membrane of the chorion I observed, to my astonishment, a considerable number of the smaller blood-vessels and capillaries (Fig. 19), some of them far enough developed to allow the circulation of the blood, others still in process of development. They, however, extended not into the villi. As their structure showed, they had been formed by the fusion of spindle-shaped bodies. A number of those small granular nuclei, above mentioned, were met with in their interior. A considerable number of oval nuclei were distributed throughout the membrane, occupying the meshes of the capillaries. Some of these latter nuclei I discovered in the act of multiplication, not by the process of budding, however, as in the chorion of the abnormal embryo, described in the first pages of this article, but, on the contrary, by that of direct division.

In reviewing now the different facts, regarding both the mode of origin of the blood-corpuscles within the follicles of the umbilical vesicle, as well as the development of the earliest embryonic blood-vessels in the chorion, elicited by the above examinations, and comparing them with those observed in the same parts of that abnormal embryo, some discrepancies existing between them will become apparent, and it would almost seem as if my former observations had been incorrect. But this is not the case, and for this reason we will give an explanation of the probable cause of these discrepancies, consisting firstly in the presence of granular nuclei within the follicles of the umbilical vesicle and in the blood-vessels of the chorion of the normal embryo, instead of fully-developed coloured blood-corpuscles, as were met with in those organs of the abnormal, undeveloped embryo; and secondly, in the mode of development of the embryonic blood-vessels.

In comparing the diameters of the two ova, we shall see at once, that while the former abnormal one measured 2½ ctm. in diameter, the size of the latter normal one only amounted to about half of this, that is, 13 mm. in length to 11 in breadth. It becomes evident, therefore, that the abnormal one, containing the undeveloped embryo, must have been considerably older than the latter. The structure of the umbilical vesicles of the two ova being found to be the same, we must presume that the fully-developed blood-corpuscles found within the follicles of the umbilical vesicle of the older ovum, had, at an earlier time, likewise been represented by granular nuclei as in the other case, and by a gradual continuous development only attained the more perfect form in which they were discovered. In consequence, however, of the embryo being arrested in its development at a very early period, no heart or blood-vessels were formed to communicate with the system of canals of the umbilical vesicle for the establishment of the provisional circulation. The primitive blood-corpuscles, in the form of round nuclei therefore, being thus prevented from escaping from the canals, accumulated there, as well as in the interior of the follicles, where they were eventually developed into perfect coloured blood-corpuscles. The development of the umbilical vesicle with its follicles, and the coloured blood-corpuscles found within the walls of the latter, of course, must be regarded as an abnormal phenomenon of nature, almost in the same light as those isolated parts of embryos, as fragments of jaws with teeth, &c., found sometimes in the interior of ovarian cysts.

As regards the presence of round granular nuclei within the follicles and canals of the umbilical vesicle, as well as within the rudimentary heart and the primary vessels of the chorion of the small *normal* embryo, it seems to indicate that at an early period, when the blood commences to circulate through the vessels of the provisional circulating apparatus, it carries small round granular nuclei, which are either gradually metamorphosed into coloured corpuscles, or replaced by such originating directly in the cells of the follicles. The latter seems to be the more probable, as a number of pale, smooth nuclei were observed in some of the cells.

The formation of the primary blood-vessels in the chorion of this embryo was observed to be effected, as mentioned before, by the fusion of spindle-shaped cells. In the chorion of that abnormal ovum, however, the blood-vessels, as my former observations showed, were formed by the fusion of certain round cells, arising in the form of buds from the nuclei, distributed throughout the membrane. Now, it might be presumed that, as the development of the embryo in this case was abnormal, the mode of formation of the blood-vessels would probably be the same; but remembering that I observed this same process still taking place in the chorion and the pia mater of much older embryos, of from 16 to 18 mm. in length, this argument loses its force. With these facts before us, we can only presume that the blood-vessels of the provisional circulatory apparatus, during the earliest period of development of the human embryo, are formed by the fusion of spindle-shaped cells, while somewhat later the formation of the permanent vessels is effected by the fusion of smaller or larger cells or vesicles arising by the process of gemmation from the nuclei, as described in the first pages of this article. At a still later period this process, too, ceases in its turn, and the vessels are formed, as we have seen, by the fusion of granular spindle-shaped bodies.

Of the morphological development of the embryo of the small normal ovum, I have little to say; as it was not my particular subject of study in this case. I rather directed my attention first to the examination of the umbilical vesicle in its fresh condition. This examination, together with the removal of the ovum from the membrana decidua and the execution of the sketches, occupying all the time which I had at my disposal until twilight, I was obliged to put the specimen in a weak solution of chromic acid for preservation. In resuming my examinations on the next day, the body of the embryo had so much lost in transparency by the action of the chromic acid, that I was obliged to use compression in order to render it sufficiently transparent for microscopical observation. The compression of course would obliterate the form of any rudimentary internal organ. The whole embryo seemed to consist only of a multitude of small embryonic nuclei imbedded, or held together by a homogeneous plasma. With the fact of the developed blood-vessels in the chorion, however, before us, there can remain no doubt as to the existence of such within the body of the embryo; though I believe, as I have said before, that the larger vessels, as aorta, &c., being developed simultaneously with other internal organs, consist at this period, like the latter, only of embryonic nuclei imbedded in the plasma, until a differentiation of the various tissues commences.

Those three prominences on the anterior side of the body of the embryo, representing, as I supposed, the rudimentary heart, were, as before mentioned, of a decidedly reddish tint, which was fading towards the body of the embryo. The question arises here, whether this tint was the natural colour of the tissues, or whether it was due to the blood, which had been circulating within the tissue. As has been stated, my examination of the walls of the umbilical vesicle showed no coloured elements whatever; still, as the specimen was examined in water, we might venture to suppose that those small round nuclei, which undoubtedly represented the coloured corpuscles in their earliest condition, had already possessed the characteristic colouring material, and lost it again by the action of the water. This supposition would explain the phenomenon, as in the absence of any muscular fibre formed, the reddish tint could hardly be due to the embryonic tissue itself, without the presence of the colouring element of the blood.

During the whole course of my researches made on embryonic tissues, I have endeavoured to discover some general law concerning the different modes of multiplication of nuclei during embryonic life; that is, whether one or the other process of multiplication would regularly appear only during a certain period of the development of the embryo. This seems to be the case, however, only to a limited extent. During the earliest periods of embryonic life, the multiplication of nuclei seems to be solely effected by the process of fission, or direct division. Having observed in the chorion of the small normal embryo, above discussed, a number of nuclei in the act of multiplying only by this mode and no other, I suppose that in the human embryo this process of multiplication of cells and nuclei, beginning with the cleavage of the yolk of the egg, prevails only throughout the first weeks of embryonic life, after which it is superseded by that of gemmation or budding. I base this supposition upon the fact that, with the exception of the cells of cartilage, I have throughout all succeeding periods of development of the human embryo, never been able again to discover a cell or nucleus dividing by simple fission. In that abnormal specimen of human ovum, probably one or two weeks older than the other, I observed, as already stated, the multiplication of nuclei in the chorion as well as in the embryo stunted in its growth, only taking place by budding or gemmation, and the same process I observed in the tissues of other embryos up to the age of $6\frac{1}{2}$ months. During a certain period, however, the multiplication of nuclei seems to be effected also by the endogenous mode. In various tissues of human embryos, of about from 16 to 20 mm. in length, as, for instance, the brain, spinal marrow, skin, &c., I met with a number of double-contoured cells, which were filled by small nuclei of a greenish lustre. On some of them the double contour had disappeared, showing the obliteration of the cell-wall for the purpose of liberating the nuclei. Another kind of clear cells, containing a coarse granular nucleus, which in many instances is seen dividing into a number of fragments, is met with in various tissues of the embryo; these cells stand very probably also in some relation with the multiplication of nuclei.

## II.—*On Pigment-Flakes, Pigmentary Particles, and Pigment-Scales.* By Joseph G. Richardson, M.D., Microscopist to the Pennsylvania Hospital.

The present paper is designed to direct attention to what I conceive to be an egregious error, by which several microscopists of acknowledged ability have been ensnared,—namely, a belief in the importance of the "pigment-cells" or "scales" described by Frerichs, of Berlin, as occurring in blood;* of similar bodies found by Drs. Meigs and Pepper, of this city, under like circumstances;† and of the "pigmentary particles" or "celloids" figured by Dr. William Roberts, of Manchester, England;‡ most, perhaps all of which I assert to be simply and solely *accumulations of dirt* (especially the remains of red blood-corpuscles) in the little excavations on slides in ordinary use.

Such an accusation as this will, no doubt, at first excite astonishment or even ridicule, but of course no sane man would dare to bring forward a charge of this kind without strong evidence in its favour. This evidence I ask each one of my readers to furnish me after trying this simple experiment:

Examine an ordinary *plate*-glass slide microscopically for *dirt-pits* containing brownish-red matter which may be oxide of iron (the remains of the polishing powders used in its manufacture), or, if the slide has been long in use, old red corpuscles. If there are none already filled up with "pigment," rub in faithfully a little blood, by which means you can sometimes fill the shallow cavities with the débris of the red disks, and so imitate quickly the effect probably often produced in a gradual manner by frequently wiping small quantities of blood over the glass. Lastly, clean off the slide perfectly bright (so as to be sure you leave nothing but artificial *cells* upon it), and examine with a power of 250 diameters.

The bodies you probably find are accurately described by Dr. Roberts as follows: § "Pigmentary particles; these objects deserve a passing notice from the fact that they are frequent, almost constant, if not absolutely constant, objects in urinary deposits, and have not hitherto been described. . . They never exist in such quantity as to form the entire (*sic*) of a visible urinary sediment; they are only to be recognized by the microscope. They appear especially under two conditions—namely, as free amorphous par-

* 'Clinical Treatise on Diseases of the Liver.' Sydenham Soc. Translation, London, 1860, vol. i., p. 320.

† 'Pennsylvania Hospital Reports,' Phila., 1868, p. 108.

‡ 'Urinary and Renal Diseases,' second American edition, Phila., 1872, p. 125.

§ *Op. cit.*, p. 124 *et seq.*

ticles and cell-like bodies (or celloids). . . . The cell-like particles have a peculiar appearance, very difficult to explain. They never present an unmistakably cellular character; they appear flat, never spherical. Their outline is generally an oblique ovoid. Within this outline, which is generally of exceeding delicacy and of perfect definition, lie masses of red or orange pigment, exactly resembling the free amorphous particles already described."

Frerichs, after pointing out somewhat similar objects, says* that accurate diagnosis can be made in malarial fever by examining the blood for them, since a few drops "are sufficient to determine the presence or absence of large quantities of pigment."

Drs. Meigs and Pepper report finding pigment-particles in the blood of eighty-nine patients; but later these acute observers seem to have had shrewd misgivings respecting their importance, although without feeling satisfied as to their real origin.

My own suspicions were excited years ago by Frerichs's pigment-scales, and experiments on hundreds of specimens of blood from malarial and other cases convinced me of their delusive character.

Very recently, Dr. James Tyson, of this city, whilst examining in committee some ovarian fluid, pointed out to me several of Roberts's pigment-flakes, and said he had prepared drawings of these bodies for his forthcoming work. His statement naturally led me to a careful and prolonged study of the objects in question, and this in turn forced upon me the conviction above expressed.

Excluding carbon-particles (from the air), which can generally be found in fluids which have not been secluded from the atmosphere, I attribute the peculiar shape of pigment-flakes which Roberts finds so "very difficult to explain" (admirably shown by Dr. Tyson in his plate), to the conchoidal figure of the minute chipped-out cavities in plate-glass; which little pits have, indeed, proved veritable pitfalls to unwary travellers over the microscopic field. These same shallow shell-like excavations, before being filled up with dirt, are, probably, Frerichs's "coagula of a hyaline character, which resemble in form" (as they have a perfect right to do) the pigment-flakes, and are also Roberts's "bluish mother-of-pearl" celloids.

Dr. Roberts concludes, "I have been in the habit of noticing these objects for many years, and have regarded them as derivatives of hæmatin, but how they come to assume their peculiar forms I cannot conjecture." With him, I believe them occasionally to be "derivatives of hæmatin," but only by the *rubbing process* detailed above; and I trust that my "conjecture" as to how these hæmatin-flakes "come to assume their peculiar forms" will be satisfactory.

It seems almost incredible that the recorded appearance of these

* *Op. cit.*, p. 355.

"flakes" in such various and inconsistent localities—viz. in blood, urine, the brain, in tumours, and even in the breath—has hitherto aroused no suspicion of their true nature; and it is only when we remember how few investigators have minds achromatic enough to enable them to see objective facts without subjective colouring, that we can offer a plausible explanation of this remarkable phenomenon. Does not the delusion which, if I am correct, has thus entangled several eminent observers, form one of the most curious episodes in the history of medical microscopy? and should it not serve as a warning to future generations of students?

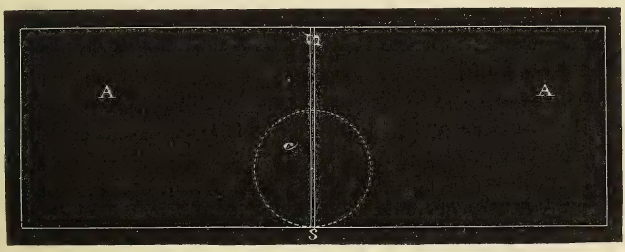
Nevertheless, being always open to conviction, I hereby challenge any devout believer in pigment-flakes to bring me an honest specimen of urine, or blood from any ordinary case of disease, in which can be demonstrated either pigment-flakes, pigmentary particles, or pigment-scales.

PHILADELPHIA, *November* 7, 1874.

## III.—*On a Modification of the "Slit" for Testing Angle.*

By R. B. TOLLES, Boston, U.S.A.

IT is only within a few days that I have used the "slit" devised by Mr. Wenham to cut off false light, as he says. Having always made the test of the verity of my measurements this condition, viz. that the object be in focus and in view with *the extremest rays traversing the sector*, there could remain no chance of my being mistaken. But in tracing a diagram to demonstrate the effect of aberration when the slit was used without cover and at the closed point, I hit upon a ready means of use of the slit *to test angle with cover, and without, almost simultaneously.*




An ordinary glass object-slide, silvered on its upper surface, A. S, the slit cut through the amalgam (or silver). *c*, covering glass, with test diatoms in balsam between cover and slide, in the slit. (Also, a central portion of the cover *might* be a *dry* mount of the same diatoms.)

My course is to find out, in the first place, what the field of the objective to be measured is, by means of a stage micrometer. I then cut the width of the field or less, as I choose, through the silvering, guiding the knife against a metal straight-edge. A little dilute acid cleans the slit-space. In the case of the $\frac{1}{6}$th measured in London the breadth of slit required was closely to 0·015″ *to span the field,* an unneeded breadth for the purpose of angle-measurement.

I will now relate my experience with the apparatus in a trial of it on a $\frac{1}{6}$th closely similar to the one Mr. Wenham tried his invention on. I induced the owner of the objective, a resident of this city, to bring it in and witness the trial.

Thickness of cover used $\frac{1}{40}$ inch, which the objective at "closed," just focussed through upon the objects in the balsam, or in the dry mounted portion, with air contact. The edges of the slit trenched slightly upon the field of view in the eye-piece, a "B" of medium aperture.

To get the angle as Mr. Wenham got it, the *uncovered* portion of the slit was brought into view. A clear air-space between, of course. The angle was less than 100°,—scarcely 90°. The covered portion of the slit was then brought into view, *cover dry.* The angle was all my thin* stage would admit of, about 160°, but the aberration even with the very thick cover and *contact* (with brass setting of front lens) rendered the trial indecisive as to useful angle.

But observe; with *water contact* and the object-slide having been transferred to *bottom* of the thin stage, the objects were nicely defined even with light incident *almost* exactly parallel with the surface of the slide.

Furthermore, with a slit one-half or one-third the breadth of the field, the result was just as satisfactory *with water* contact; but with air only intervening, the angle was reduced, accordingly *as the field* was reduced, from less than 100° (small enough!) to one-half or one-third *that* angle.

Properly used, it is evident that for taking angles a *narrow slit* of field-aperture is as good as more, and therefore a slit at the focus suitable for $\frac{1}{12}$th might serve for $\frac{1}{4}$th. In fact, what suits a $\frac{1}{20}$th ought to serve for *all powers below.* The thinnest covering glass will do, that being needed merely to secure the objects, as tests of definition, in place. The space may be filled up with glycerine to contact as well, ordinarily. Always, where only infinitely near to 180° of aperture is to be measured. Water has too low refraction to *correct* in place of glass.

* Not "*zinc*"! stage; see 'M. M. J.' for August, 1874, p. 65.

I may as well state here what is of *real* importance in *using such* an objective as the $\frac{1}{6}$th, i. e. of maximum (or large), angle and long working distance through cover. That objective goes best with the *thicker* covers, therefore the thin covers $\frac{1}{300}$ to $\frac{1}{100}$ should be supplemented with glycerine instead of water. This gives best command of *all* thicknesses of cover, notably if the objective is *corrected for best work* through the nearly *thickest covers* it will penetrate.

BOSTON, *Nov.* 25, 1874.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*Have the Lungs on their ultimate Alveoli Squamous Epithelium?*—This question has been often asked for the past thirty years, and has been answered both in the affirmative and the negative. However, now a Mr. Henry Brown, of Northallerton, seems to have decided the question. In a letter to the 'Lancet,' of Nov. 7th, 1874, he says of this question that "when we find such men as Waters, Kölliker, Rossignol, Eberth, Hirschmann, and Arnold advocating its presence, and Rainey, Todd and Bowman, and Zenker denying its existence, what are we to say? With your permission I shall briefly point out how the examination of the pulmonary tissue should be conducted, and I shall explain how I and others have failed to observe the epithelium, and how by a little careful attention and manipulation any person possessing a good microscope may, with great ease and facility, demonstrate its existence. Reasoning from analogy, I considered the ultimate lung tissue could not be an absolutely bare and structureless membrane, upon the walls of which the capillaries ramified. Accordingly I obtained some lung tissue from a recently killed pig, and examined it after the least delay possible. I excised a portion from the thin edge of the lung, and, placing it over the index finger of the left hand, made slight pressure upon its upper surface, and then seized it with the thumb and middle finger of the same hand, so as to secure it firmly, and by means of fine curved scissors snipped off small pieces. My object in so doing was to make the pieces as thin as possible, and I have found this mode of procedure preferable to any other. The thin pieces of lung were washed in distilled water for about fifteen minutes. I used a deep watch-glass for this purpose, and by means of two needles I was able to wash the pieces most effectively. I then transferred the small pieces of lung to another watch-glass containing some distilled water, and, after stirring them about for a minute or so, I found that very few air-bubbles made their appearance; and taking up a small piece of lung, transferred it to a glass slide, and placed upon it a thin glass cover. This I carefully examined under a power of 310. The appearance was different from what I had before seen, and I resolved to apply a very weak acetic acid solution. For this purpose I added ten drops of glacial acetic acid to one ounce of distilled water, and, by means of a Clark's stopper, allowed a drop to pass between the cover and glass slide. The effect was truly charming. Beautiful epithelial scales with a nucleus presented themselves. The reason why I have formerly been unsuccessful in demonstrating the epithelium of the alveoli of the lungs is this: that the acetic acid employed was too strong, and immersion of lung tissue in moderately strong acid causes disintegration and solution of the epithelium. I think, Sir, this point should now be finally settled, and I shall most willingly send further particulars to any person interested in this vexed question. I have carefully measured the epithelium, and observed its disposition, and

I recommend the authors of our English Physiologies to *overlook* the stereotyped engravings in their several works on the subject. Not one, with the exception of Waters, gives a faithful delineation. I shall not trouble you with a description of how dry lung is affected with acetic acid of the above strength. Suffice it to say that turpentine, glycerine, Canada balsam, dammar varnish, and other materials, all fail to bring out the epithelium of the alveoli."

*Reproduction of Desmids.*—Prof. Leidy, at a late meeting of the Academy of Natural Sciences of Philadelphia, made some remarks on the mode of reproduction and growth of the Desmids, which are reported as follows by the 'American Naturalist,' Nov., 1874. In illustration he described a common species of Docidium or Pleurotænium. This consists of a long cylindroid cell constricted at the middle and slightly expanded each side of the constriction. When the plant is about to duplicate itself the cell-wall divides transversely at the constriction. From the open end of each half-cell there protrudes a colourless mass of protoplasm defined by the primordial utricle. The protrusions of the half-cells adhere together and continue to grow. The bands of endochrome now extend into the protrusions and subsequently keep pace with their growth. The protrusions continue to grow until they acquire the length and form of the half-cells from which they started. The exterior of the new half-cells thus produced hardens or becomes a cell-wall like that of the parent half-cells. In this condition two individuals of Docidium are frequently observed before separation. During the growth of the new half-cells the circulation of granules in the colourless protoplasm is quite active. In a species of Docidium $1\frac{1}{2}$ mm. long by $\frac{1}{10}$ mm. broad, the growth of the new half-cells was observed to be at the rate of about $\frac{1}{3}$ mm. in an hour.

*The largest Apyrenœmatous Blood-corpuscles.*—In a paper read at a late meeting of the Zoological Society, Professor Gulliver stated that in the Apyrenæmata or Mammalia the largest red corpuscles of the blood are those of the two elephants, the Aardvark, two-toed sloth, and the walrus; and that it is remarkable that the largest apyrenæmatous corpuscles should occur in three such different orders as Pachydermata, Edentata, and Feræ. In Pachydermata, excepting the elephants, the corpuscles are by no means so large, not even in the hippopotamus, the corpuscles of which he had then measured for the first time. But the order Edentata is characterized by the largeness of the corpuscles; while among the Feræ there are very large and very small corpuscles, the large ones being quite characteristic of the Pinniped family, as was shown by his recent measurements of the red blood-corpuscles of *Otaria* and *Trichecus*. In the human subject the corpuscles are exceeded in size by those of only eight or nine exotic Mammalia, and not equalled in size by the corpuscles of any British animal of the class. And this fact, independently of its physiological interest, may prove important in medico-legal inquiries; since by it alone Dr. Joseph G. Richardson states, in the September number of this Journal, that he has correctly distinguished dried stains of human blood from those of the ox and sheep.

*Comparative Microscopic Rock-structure of some Ancient and Modern Volcanic Rocks.*—Mr. J. Clifton Ward lately (Nov. 4, 1874,) read a valuable paper on the above subject before the Geological Society. He stated at the outset that his object was to compare the microscopic rock-structure of several groups of volcanic rocks, and in so doing to gain light, if possible, upon the original structure of some of the oldest members of that series. The first part of the paper comprised an abstract of what had been previously done in this subject.

The second part gave details of the microscopic structure of some few modern lavas, such as the Solfatara trachyte, the Vesuvian lava-flows of 1631 and 1794, and a lava of the Alban Mount, near Rome. In the trachyte of the Solfatara, acicular crystals of felspar show a well-marked flow around the larger and first-formed crystals. In the Vesuvian and Albanian lavas leucite seems, in part at any rate, to take the place of the felspar of other lavas; and the majority of the leucite crystals seem to be somewhat imperfectly formed, as is the case with the small felspar prisms of the Solfatara rock. The order of crystallization of the component minerals was shown to be the following: magnetite, felspar in large or small *distinct* crystals, augite, felspathic or leucitic solvent. Some of the first-formed crystals were broken and rendered imperfect before the viscid state of igneous fusion ceased. Even in such modern lava-flows as that of the Solfatara considerable changes had taken place by alteration and the replacement of one mineral by another, and this very generally in successive layers corresponding to the crystal outlines. The frequent circular arrangement of the glass and stone cavities near the circumference of the minute leucite crystals in the lava of 1631 was thought to point to the fact that after the other minerals had separated from the leucitic solvent, the latter began to crystallize at numerous adjacent points; and as these points approached one another, solidification proceeded more rapidly, and these cavities were more generally imprisoned than at the earlier stages of crystallization. In the example of the lava of 1794, where the leucite crystals were farther apart, this peculiar arrangement of cavities was almost unknown.

The third part of the paper dealt with the lavas and ashes of North Wales; and the author thought that the following points were established:

1. Specimens of lava from the Arans, the Arenigs, and Snowdon and its neighbourhood, all have the same microscopic structure. 2. This structure presents a hazy or milky-looking base, with scattered particles of a light-green dichroic mineral (chlorite), and generally some porphyritically-imbedded felspar crystals or fragments of such, both orthoclase and plagioclase. In polarized light, on crossing the Nicols, the base breaks up into an irregular-coloured breccia, the colours changing to their complementaries on rotating either of the prisms. 3. Finely-bedded ash, when *highly altered*, is in some cases undistinguishable in microscopic structure from undoubted felstone. 4. Ash of a coarser nature, when highly altered, is also very frequently not to be distinguished from felstone, though now and then the outlines of some of the fragments will reveal its true nature. 5. The

fragments which make up the coarser ash-rocks seem generally to consist of felstone, containing both orthoclase and plagioclase crystals or fragments; but occasionally there occur pieces of a more crystalline nature, with minute acicular prisms and plagioclase felspar. 6. In many cases the only tests that can be applied to distinguish between highly-altered ash-rock and a felstone are the presence of a bedded or fragmentary appearance on *weathered* surfaces, and the gradual passage into less altered and unmistakable ash.

In the fourth division of his paper the author described some of the lavas and ashes of Cumberland of Lower Silurian age.

With regard to these ancient lavas the following was given as a general definition: The rock is generally of some shade of blue or dark green, usually weathering white round the edges, but to a very slight depth. It frequently assumes a tabular structure, the tabulæ being often curved, and breaks with a sharp conchoidal and flinty fracture. Silica 59–61 per cent. Matrix generally crystalline, containing crystals of labradorite or oligoclase and orthoclase, porphyritically imbedded, round which the small crystalline needles seem frequently to have flowed; magnetite generally abundant, and augite tolerably so, though usually changed into a soft dark-green mineral; apatite and perhaps olivine as occasional constituents. *Occasionally* the crystalline base is partly obscured and a felsitic structure takes its place.

The Cumberland lavas were shown to resemble the Solfatara greystone in the frequent flow of the crystalline base, and the modern lavas generally in the order in which the various minerals crystallized out. In *external* structure they have, for the most part, much more of a felsitic than a basaltic appearance. In internal structure they have considerable analogies with the basalts. In chemical composition they are neither true basalts nor true felstones. In petrological structure they have much the general character of the modern Vesuvian lavas; the separate flows being usually of no great thickness, being slaggy, vesicular, or brecciated at top and bottom, and having often a considerable range, as if they had flowed in some cases for several miles from their point of eruption. Their general microscopic appearance is also very different from that of such old basalts as those of South Stafford and some of those of Carboniferous age in Scotland.

On the whole, while believing that in *some* cases the lavas in question were true basalts, the author was inclined to regard most of them as occupying an intermediate place between felsitic and doleritic lavas; and as the felstone lavas were once probably trachytes, these old Cumbrian rocks might perhaps be called Felsidolerites, answering in position to the modern Trachy-dolerites.

A detailed examination of Cumbrian ash-rocks had convinced the author that in many cases most intense metamorphism had taken place, that the finer ashy material had been partially melted down, and a kind of streaky flow caused around the larger fragments. There was every transition from an ash-rock in which a bedded or fragmentary structure was clearly visible, to an exceedingly close and flinty felstone-like rock, undistinguishable in hand specimens from a true contemporaneous trap. Such altered rocks were, however, quite distinct in microscopic structure from the undoubted lava-flows of the same district, and often distinct also from the Welsh felstones, although *some* were almost identical microscopically with the highly altered ashes of Wales, and together with them resembled the felstone-lavas of the same country.

The author believed that one other truth of no slight importance might be gathered from these investigations, viz. that neither the careful inspection of hand-specimens, nor the microscopic examination of thin slices, would in *all* cases enable truthful results to be arrived at, in discriminating between trap and altered ash-rocks; but these methods and that of chemical analysis must be accompanied by oftentimes a laborious and detailed survey of the rocks in the open country, the various beds being traced out one by one and their weathered surfaces particularly noticed. A very interesting discussion followed the perusal of the paper.

*The Pathology of the Blood.*—M. Laptschinsky, of St. Petersburg, contributes a paper to the 'Centralblatt,' on the microscopic changes undergone by the blood in various diseases, which is thus given in the 'Lancet,' October 31. He finds that in various diseases in which marked febrile symptoms are present, the microscopic aspect of the blood is essentially different from that of health. The changes consist in the blood-corpuscles not running into regularly formed rouleaux, but accumulating in heaps or clumps of various size and shape. The individual blood-corpuscles frequently appear swollen and cloudy, and their contours less distinct than natural. Small corpuscles, one-third of the normal size, are often met with, some of which exhibit a more intense colour than natural, whilst others are completely pale. In the interspaces of the clumps of red corpuscles, great numbers of white corpuscles may be seen, often coalescing to form groups of from 3 to 8. In typhus he counted from 60 to 80, and more, in one field of vision; in cholera from 110 to 130. Careful enumeration of the relative numbers of white and red corpuscles four days after death in the above cases showed that there was 1 white to 60 red corpuscles in the case of typhus, and 1 white to 23 coloured in the case of cholera. In a very anæmic woman, suffering from suppuration in the knee-joint, the proportion of the white rose to 1 to 13 red. The white corpuscles in these cases presented unusually active and extensive amœboid movements. The nuclei of the colourless corpuscles took a part in the amœboid movements, and could be seen altering their position and form in the interior of the white corpuscles. The thorn-apple or horse-chestnut like form of the red corpuscles he did not find to be unusually frequent. He found, however, large quantities of granular or detritus-like material in the blood of febrile, but not much in the blood of cachectic and anæmic patients. From his enumerations he feels satisfied that in febrile diseases, and in Bright's disease, the conversion or development of white corpuscles into red is either materially retarded or is entirely arrested.

*Palmodictyon viride in Britain.*—Mr. Edward Parfit has written to 'Grevillea' saying that he has found this plant in the Exeter canal. He says: "Not knowing the plant myself, and after searching all the works on the subject I had at my command, I forwarded specimens to my friend Professor Dickie, of Aberdeen, who kindly writes me this: 'The plant is *Palmodictyon viride* (Kützing), and so far as I know new to the British list.' The plant, where it has sufficient room to develop itself, spreads over the bottom, in water about six inches deep; beyond this it comes in contact with *Elodes canadensis,* over which it creeps, and extends its growth from branch to branch into deeper water. In this extension it has first the appearance of a *Conferva,* which I at first took it to be; but the moment I touched it, after taking some from the water, I found from the soft slimy feel that if a *Conferva* it was new to me, and the microscope soon revealed the true character. When the plant grows on the bottom it shows one continuous green membrane, stretched tight over the bottom, but when it comes in contact with other plants it throws out filaments, the thickness of which is difficult to make out on account of their adhesive nature; for wherever they touch it is matter of impossibility to separate them. The membrane forming the filaments is structureless, but the spherical cells form more or less moniliform threads sometimes running in parallel lines, at other times forming an irregular net-work on the inside of the filaments. These cells sometimes divide into two portions, at others into four, and in most of the mature cells may be observed four cellules."

*Formation of Fibrin from the Red Blood-corpuscles.*—M. Landois, according to the 'Medical Record' of November 18, describes the formation of fibrin as being dependent on the dissolved corpuscles. If a drop of defibrinated rabbit-blood be brought into a drop of frog's serum, the cells aggregate together, and become sticky on their surfaces. The cells soon become globular, and those cells lying towards the periphery allow the blood-colouring matter to pass out. This discolouring gradually extends towards the centre of the drop, and at last only a heap of stroma remains. The stroma-substance is very tough and viscid. At first the contours of the cells can be detected; and, when the stroma has been agitated to and fro, the cellular contours disappear, and viscous fibres and stripes are observed. Step by step the formation of fibrous masses from the dissolved mammalian cells can be observed. The author thinks this fibrin should be called "stroma-fibrin" in opposition to the ordinary fibrin or plasma-fibrin, which is formed without solution of the blood-corpuscles. The two kinds of fibrin may possibly be chemically distinguished from each other. In transfusion, if dissolution of the cells occur, then, of course, the formation of stroma-fibrin may take place. The coagulation occurs the sooner, the more serous the blood. Animals in a state of asphyxia, into whom heterogeneous blood was introduced, showed the most extensive coagulation.

*The Anatomy of the Ear.*—At a meeting of the Medical Society in Vienna, in the beginning of October (a report of which is given in the 'Allgemeine Wiener Medizinische Zeitung' for October 20), Professor Politzer gave the result of some investigations which he had recently made into the anatomy of the ear, which was thus given in the 'Medical Record,' December 2. He finds that, in newly-born children, the cavity of the pyramid containing the stapedius muscle is separated only at the upper part by osseous tissue from the canal through which the facial nerve passes, while the lower part of the cavity communicates freely with the same canal, and thus allows, at this spot, the muscle and nerve coverings to come into actual contact with each other. In the adult, the amount of direct communication between the cavity and canal is very various, ranging from a small opening sufficient for the passage of the nerve to the stapedius, to a large irregular opening. The styloid process, he avers, arises from a cartilaginous body, which not only in the fœtus, but also in the newly-born, is to be found as an isolated cartilaginous formation; and the upper end of the process does not terminate at the external visible base, but passes through a thin osseous lamella along the posterior wall of the tympanic cavity, reaching as far as the eminentia stapedii. In the adult, the process is sometimes solid, sometimes hollow, but generally there is a cellular structure with or without a central canal.

*Action of Electricity on Frog's Spawn.*—M. Onimus, in a recent communication to the Société de Biologie, of Paris, states that by electrifying the eggs of the frog, the development of those which are in connection with the negative pole will be accelerated, whilst the hatching of those in connection with the positive pole will be either retarded or stopped.

*What is a Bacterium?*—Dr. W. A. Hollis has written a paper lately (Nov. 21) on the above question which may be of some interest to our readers. He says that the question, What is a bacterium? is thus answered by Ehrenberg in his great work on Infusoria: * "Animal e familia Vibrioniorum divisione spontanea in catenam filiformem rigidulam abiens." Dujardin accepted this definition without alteration, although he modified somewhat the other genera of the family. The derivation of the word itself (from Βακτηριον, a little rod) corresponds well with the characteristic features of the organism above given. For several years the accuracy of Ehrenberg's definition was unquestioned; eventually, however, from the observation of the behaviour of these organisms with certain chemical reagents, and mainly also from the elaborate researches of Professor Cohn regarding their morphology, their animal nature was disputed. It was found that they were unaffected by boiling with potash water, and they were further said to behave somewhat as cellulose does when they were treated with sulphuric acid and iodine, although from their extreme minuteness any changes which take place in their tissue under such conditions are very difficult to observe.

For many years past Professor Cohn, of Breslau, has published in occasional papers the results of his investigations on the subject. He has made one great step in advance of previous observers in ascertaining so much of the history of the bacterium as that it arises from the

* 'Die Infusions-thierchen,' 1838, p. 77.

gelatinous scum seen floating on the top of water containing putrescent organic matter, and this he named "zooglœa." He then described the *Bacterium termo* in these words: * "Cellulæ minimæ bacilliformes, hyalinæ gelatina hyalina in massas mucosas globosas, uvæformes, mox membranaceas consociatæ, dein singulæ elapsæ, per aquam vacillantes;" and he considered them as of decidedly a vegetable nature, and as allied to the Oscillatoriaceæ. In a more recent pamphlet he placed them amongst the family Phycochromaceæ, in a natural order named Schizosporeæ. His last investigations have led him to divide Bacteria into four groups and six genera, as follow:—†

| | | |
|---|---|---|
| I. Sphæro-bacteria. | Genus 1. | Micrococcus char. emend. |
| II. Micro-bacteria. | Genus 2. | Bacterium char. emend. |
| III. Desmo-bacteria. | Genus 3. | Bacillus n. g. |
| | Genus 4. | Vibrio char. emend. |
| IV. Spiro-bacteria. | Genus 5. | Spirillum, Ehr. |
| | Genus 6. | Spirochæta, Ehr. |

Of these genera the Bacterium, Vibrio, Spirillum, and Spirochæta were in the original Vibrionia family of Ehrenberg.

Cohn considers the ferment of contagion to be due to the presence of a variety of the Sphæro-bacteria, the micrococci of Hallier. The whole group he divides into three: the chromogen, zymogen, and pathogen—the micrococci of pigmentation, of ferment, and contagion respectively. These organisms are exceedingly minute, darkish, or coloured granules, so small as to be immeasurable. They frequently present the appearance of beaded chains, or the form of aggregations (colonies). They are motionless, and are occasionally found with the *Bacterium termo* in putrefying organic liquids. Among the pathogen micrococci I may mention the *M. vaccinæ*, observed by Chauveau and Sanderson in the vaccine lymph; the *M. diphthericus*, which is probably the same organism as that described by Professor Eberth, of Zurich, as attacking first the epithelial elements of a part, and subsequently the deeper tissues, and which led him to say "the metastatic pyæmia is for the most part a diphtheria with numerous localizations;"‡ and, lastly, the *M. septicus*.§ found, according to Cohn, in the miliary eruption of typhus, pyæmia, and other diseases. The chromogen, or pigmentary micrococci, have occasionally been the means of working miracles. Several instances of bread exuding blood, under supernatural circumstances, are related by Rivolta.|| Ehrenberg found this colour on some bread in the house of a patient who had died of cholera, and he ascertained the pigment to be due to the presence of the *Monas prodigiosa*—small roundish bodies, which Cohn classes with the micrococci.

The true bacteria Cohn divides into two species, the *B. termo* and *B. lineola*. The *B. termo* are small dumb-bell-shaped organisms,

* Nova Acta,' xxiv., p. 123.
† Cohn, 'Beträge zur Biologie der Pflanzen,' Breslau, 1872.
‡ Eberth, 'Zur Kentniss d. Bacteritischen Mykosen,' Leipsig, 1872, p. 15.
§ The *Microsporon septicum* of Klebs.
|| 'Del Parassitti Vegetali,' Turin, 1873.

having a slowly vacillating motion, and about $\frac{1}{9000}''$ or $\frac{1}{12000}''$ in length. I have elsewhere given Cohn's description of these micro-bacteria and their zooglœa. They are essentially the ferment of putrefaction, and it is doubtful whether putrefactive changes can take place without them. It is probable that Ehrenberg confounded this Bacterium with the *Vibrio lineola* in his plates in the work before noticed. The *B. varicosum* of some writers is possibly this species, although, when fresh names are introduced in classification without sufficient description, some doubt will always be cast upon the accuracy of the investigation.

The *B. lineola* is somewhat larger than the preceding species. It is endowed with stronger and more rapid to-and-fro movements. It is rod-shaped, and is essentially the ferment of sour milk. It is equivalent to the *Vibrio lineola* of Ehrenberg, the *V. tremulans* and *B. triloculare* of the same author, and to the *V. lineola* of Dujardin.

The Desmo-bacteria, or "linked rods," are distinguished, as their name implies, from the true bacteria by being occasionally united together in chains. They are thus separated: the filament transversely lined—Bacillus; the filament cylindrical and curved—Vibrio.

The Bacilli Cohn divides into three species:

The first, the *B. subtilis*, is the *Vibrio subtilis* of Ehrenberg. It is a slender supple thread found in stale boiled milk. Its length is about $\frac{1}{500}''$. It moves with a pausing motion, "like a fish forcing its way through reeds."

The *B. anthracis* of Cohn is the *Bacterium carbuncolare* of some writers. It is described by Rivolta * (following Davaine and Delafond) as an immovable, oblong, highly refractive body, found in the blood of animals affected with the disease. Its size (according to Davaine) varies much, from $\frac{1}{10000}''$ to $\frac{1}{4000}''$ or even $\frac{1}{2000}''$. It is unaffected by water, alcohol, ether, acetic, nitric, or phosphoric acid, or soda, potass, or ammonia. Sulphuric acid readily destroys it. It is occasionally found united in chains of two or three links.

Lastly, the *Bacillus ulna* is distinguished from the *B. subtilis* by the greater thickness of its filaments and by its rigidity. Its length is about $\frac{1}{650}''$. Cohn found it in a stale infusion of boiled egg.

The Vibrios are distinguished from all the preceding genera by their rotary motion. This motion, which most writers had restricted to the Spiro-bacteria, Cohn, I think, rightly applies to the movements of the Vibrio. The *V. rugula* is generally seen with one or two slight curves in the form of the signs ) or ʃ. A flexible thread, $\frac{1}{2500}''$ to $\frac{1}{1200}''$ long; rotation slower than in the following species. This organism was found in the evacuations of cholera and diarrhœa by Leeuwenhoek, and by Davaine in the pus of balanitis also.† The second species, the *V. serpens*, is distinguished by the greater number and regularity of its curves, by the rigidity of the filament, and its more rapid rotation. The thread is also considerably thinner than the *V. rugula*, and its length is about $\frac{1}{2000}''$. The motion is serpentine in appearance.

The Spirilla (including the *Spirochæ taplicatilis*, for I do not

* Rivolta, *op. cit.*, p. 47.

† Davaine, 'Entozoaires,' 1860, p. 5.

think Cohn is justified in separating the two genera) of Dujardin * are distinguished by the greater regularity and closeness of the curves of the spiral, and their uniform corkscrew motion. The distinguishing character of the flexibility or rigidity of the threads in the genera Spirochæta and Spirillum respectively, insisted upon by Ehrenberg and followed by Cohn, is rightly set aside by Dujardin as superfluous. All the Spirilla, of which Cohn gives three species—*S. tenue*, *S. undula*, and *S. volutans*—were found by him in the decomposing tissues of a fresh-water snail. They are distinguished mostly by their size from each other. The *S. volutans* is by far the largest of all the bacteria, if we apply the name to the genus at all. It is thus described by Ehrenberg, "Filis valde tortuis robustis et elongatus." Cohn fancies that he has found traces of organization within it.

He states that having above given a short *résumé* of the labours of the most trustworthy naturalists upon the morphology of bacteria, he will now only add a few remarks upon the limitations we should place on the term.

In the first place, then, it seems right to consider bacteria as strictly forming part of the vegetable kingdom, and this, as I have before remarked, is the opinion of all the most trustworthy authorities of France, Germany, and Italy. I should have included our own country in this geographical list had I not lately been somewhat startled to find a learned Professor in a recent lecture at the Royal Institution † reported to have represented bacteria to be "animalcules." Secondly, I think the name bacteria ought to be restricted to those minute rod-like hyaline bodies, the *B. termo* and *B. lineola* of Cohn. They have a more or less rapid to-and-fro motion. The so-called "locomotive bacteria" of some physiologists are probably in many instances specimens of the larger *V. regula*. Rivolta considers that the true bacteria have no proper locomotive powers, only the vacillatory movements common to all small particles of matter suspended in liquids. Thirdly, we must, I think, always associate the presence of the true bacteria (especially the *B. termo*) with putrefactive or analogous changes in organic liquids.

At some future period I hope to give a short account of the etiology of these organisms, and the part they play in the causation of disease.

## NOTES AND MEMORANDA.

**The Society's Universal Screw.**—We quote the following remarks from 'Science Gossip,' as they are of some importance. They are made by M. A. de Sonza Guimaraens. There is a general complaint among microscopists respecting the so-called "universal screw." I have myself felt great annoyance when finding that the screw is *not* universal. Some of my friends' object-glasses (having the "universal screw") do not screw home in the nose-piece of one

* 'Infusoires,' p. 209.

† See report in 'Illustrated London News,' Feb. 14, 1874, p. 162.

of my microscopes, while others fit loosely the nose-piece of my other instrument, although both microscopes have been supplied by the makers with the so-called "universal screw"! Moreover, I have seen modern object-glasses (manufactured since the introduction of the universal screw), by one of the leading opticians, having different gauges of universal screw, and by another not only object-glasses, but adapted for analyzers, Brooke's nose-pieces, &c. When using high powers with a microscope having a concentric rotating stage (which is now considered almost a necessary addition), these variations of gauge render the stage eccentric, and no doubt very often the rotation of a stage is condemned, and the workmanship considered imperfect, when the fault lies in the inaccuracy of the so-called universal screw of either the object-glass or of the microscope's nose-piece, and frequently of both. I am quite aware that the smallest particle of dust in the object-glass screw will cause eccentricity, but this drawback is not a permanent one; it is bad enough to have it when it occurs—there is no necessity to make eccentricity both a feature and a fixture! With a universal screw, if we could not get in every instance perfect concentricity when rotating the stage, we should certainly approach it much nearer than we do now; of course, accurate workmanship being always taken for granted. Besides the above inconveniences, there is another—the great difficulty and trouble in centring achromatic condensers of large angle of aperture with high powers, by different makers, having different universal screws. The Royal Microscopical Society have undoubtedly conferred a great boon upon microscopists by introducing the present "universal screw"; but could not an effort be made to render the screw really universal by causing the Royal Microscopical Society's *gauge* to be adopted by *all* the London opticians? Some technical and practical reasons may be adduced as to the difficulty of making universally true the "universal screw"; but, even admitting the next to impossibility of such an accuracy, why then call the screw *universal* when in reality nearly each maker of microscopes in London has his own gauge of the "universal screw"? It would be also a great convenience to have a universal gauge for the sub-stage fittings, eye-pieces, &c., so that the apparatus of any one maker should fit the microscopes of the others. At present there is a great discrepancy in the diameter and length of microscope-tubes and the gauge of sub-stage fittings of some makers, compared with those of others. Why not make these also *universal?*

**Gilded Glass in the Construction of the Camera Lucida.**—It is known that the construction of the *camera lucida* is founded upon the simultaneous perception of two images—that of the object, and that of the pencil. Various means have been employed to arrive at this result. In that of Soemmering it is a metallic mirror smaller than the pupil; that of Amici is constructed on the principle of reflexion on a plate with parallel faces; that of Wollaston, at present most in use, consists in a prism, of which the edge, dividing the pupil in two parts, permits the object to be seen by the upper half, and simultaneously the pencil by the lower portion. In all these systems the fusion of the images is somewhat difficult to seize, especially for certain points of the reflected image. Signor Govi, Professor of Physics at the Royal University at Rome, proposes to cover with a thin layer of gold the reflecting surface of a prism, and to apply upon this, with Canada balsam, a second prism with like angles. Although this layer of gold is sufficiently transparent to allow the luminous rays to pass, its power of reflexion is considerable, and it gives images of great brightness. We have thus a perfect means of superimposing, without fatigue to the eye, two different images—the one direct, and the other reflected. The principle is the application of that property of thin plates—metallic or otherwise—to transmit simultaneously direct rays, and to reflect rays which arrive obliquely from another source.

## CORRESPONDENCE.

### "Some One"—An Advocate for the 180°.

*To the Editor of the 'Monthly Microscopical Journal.'*

Sir,—With an anonymous correspondent in the 'American Naturalist' I shall not revive a discussion, that has been closed on an optical question.

When I guessed that some one might come forward to argue Mr. Tolles' 180° to be right, I hardly expected that an advocate would appear. This enlightened one states, that with a dry object on the cover with 180° no distance is involved, ignoring the fact that 180° below the surface must be the result of 180° on the front lens; and if there is no distance in the one case, there can be none in the other. But as there is a front distance ·013 in the $\frac{1}{6}$th, the triangle is a practicable fact.

This advocate having, either not the sense or the will to see this, rather than risk his credit, conceals himself; his defence is, however, a superfluous one, for I have no wish to deprive Mr. Tolles of any honorary degrees that his policy may tempt him to claim; and if "Some One" proposes that 180° is to be emblazoned upon his escutcheon, I will be the foremost to raise my hand to vote that it shall be done.

Yours truly,
F. H. Wenham.

## PROCEEDINGS OF SOCIETIES.

### Royal Microscopical Society.

King's College, *December* 2, 1874.

The minutes of the preceding meeting were read and confirmed.

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

The Secretary announced that the Council had unanimously resolved to support a proposal that Col. Dr. J. J. Woodward, of the United States Army Medical Department, be elected an honorary

Fellow of the Society. Dr. Woodward's name was well known to most of them from his many communications, as well as by the numerous photo-micrographs for which the Society was indebted to him.

The President having put the proposition from the chair, it was unanimously resolved that Dr. Woodward's name be suspended in the usual manner, and that it be brought before the Fellows for election at the next ordinary meeting.

The President said that a number of photographic likenesses of their former President, the late Rev. J. B. Reade, had been sent to the Society by Dr. Wallich, for distribution amongst the Fellows. They would be placed upon the table, and the Fellows were invited to take one each at the termination of the meeting. They would doubtless be very glad to avail themselves of the opportunity of possessing a memento of Mr. Reade, and would feel greatly obliged to Dr. Wallich for his kindness in enabling them to do so.

A cordial vote of thanks to Dr. Wallich for the photographs was unanimously carried.

A paper by Dr. Hudson, "On the Discovery of some New Male Rotifers," was read to the meeting by the Secretary, who expressed his great regret that owing to an attack of bronchitis the author was prevented from reading it to them in person. The paper was illustrated by a number of extremely beautiful drawings in white and coloured chalks upon a black ground, representing the rotifers as they would be seen by the paraboloid illumination.

The Secretary called the special attention of the Fellows to the drawings, several of which were exhibited from the chair, whilst the particular portions of the paper relating to them were re-read. He much regretted the absence of Dr. Hudson, and the more so because he believed that there were other illustrations and some additional particulars which would have been brought before them had he been able to come there that evening. The subject was one of very great interest, and he observed that in a note appended to the paper Dr. Hudson mentioned that in all the males figured and described he had seen the motion of the spermatozoa within the testes.

The thanks of the meeting were unanimously voted to Dr. Hudson for his valuable and interesting paper.

Mr. C. Stewart (Secretary) said that they had just received a paper from Dr. Schmidt, of New Orleans, on the Development of the Smaller Blood-vessels in the Human Embryo. Unfortunately the paper had only been placed in his hands that evening, so that he had not had any opportunity of reading it through or becoming acquainted with its contents; under these circumstances he felt that he might be doing it an injustice to attempt to make an abstract then. The better course, he thought, would be to take the paper as read; the text would then appear in the next number of the Journal, together with the very beautiful illustrations by which it was accompanied.

The paper was then taken as read, and a vote of thanks to Dr. Schmidt was unanimously passed.

The Secretary reminded the Fellows that their next scientific evening would be held on December 9, and requested that intimation might be sent to the Assistant Secretary if any special arrangements were required for purposes of exhibition by any of the Fellows of the Society.

The President having expressed a hope that as many of their number as possible would attend on that occasion, and bring with them matters of interest, the proceedings were adjourned to January 6, 1875.

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*Scientific Evening, December* 9, 1874.

On this occasion an unusual number of remarkable objects were brought together for exhibition, as the subjoined list will show. The illustrations of minute anatomy excited special admiration. It will be seen that Dr. Urban Pritchard contributed a valuable and instructive series of preparations and models showing the comparative anatomy of the cochlea, rods of Corti, and other portions of the ear; while Mr. Loy's modestly-mentioned "Dissections of lepidopterous larvæ," comprised a large collection of objects, prepared and mounted with extraordinary skill.

The two marine creatures exhibited by Mr. Browning have not yet been identified by any authority. The "insect" may be a larval form. It has apparently only six legs, and jaws well adapted for biting. Both these creatures are reported to be serious foes to electric telegraph cables, one assailing the hemp, and the other the guttapercha.

M. de Souza Guimaraens exhibited the ovum, larva, and pupa of the *Phylloxera vastatrix*, the cause of so much damage to the vines. He also exhibited the Phylloxera of the oak, including the male, illustrating Balbiani's researches, which will be found in the 'Revue Scientifique,' June 6, 1874.

Messrs. R. and J. Beck exhibited a microscope made for a surgeon in New Orleans. It was on the design of their large best portable stand, with a complete series of object-glasses and apparatus. The limb was made of solid silver, as also the bodies. The pillars and stand were of aluminium bronze, and the movable parts and apparatus of aluminium. The fittings for rack work and the slow motion were of steel.

The distribution of the various metals was arranged so as to endeavour to obtain the greatest stability, freedom from tremor, and minimum of friction. The whole of the apparatus was packed with the stand in an elaborate rosewood case, every block being screwed into an inner carcase.

They also exhibited a beautiful specimen of *Spirogyra dubia*, showing the anatomy of the cells, prepared by Dr. J. G. Hunt, of Philadelphia.

Messrs. Powell and Lealand exhibited two glasses on a new formula; one, $\frac{1}{4}$th, showing the lines of *Amphipleura pellucida;* and the other, $\frac{1}{8}$th, showing *Pleurosigma angulatum* $\times$ 4000. This object was illuminated by direct light. The effect was to show the interspaces remarkably magnified, and the beads comparatively small. They stood out like minute spheres of pink coral on a white ground.

Messrs. Ross and Co. exhibited a new portable microscope of elegant appearance. The stem of the arm to which the body is attached slides through a socket, forming a coarse adjustment. This slides within another socket with a delicate rack-and-pinion movement for a fine adjustment. It has a revolving stage like Nachet's, packs in a very small compass, and has sufficient range of motion to work with a 4-inch objective.

Mr. Moginie showed a new microscope of large size, arranged with folding legs, to pack in a narrow box. From the stretch of the legs when open, and the disposition of the weight in relation to the points of suspension, it is remarkably steady.

The type slide of Holothuria plates by Möller, exhibited by Mr. Baker, afforded a fresh proof of the artist's remarkable skill, and, like his type diatom slides, will be found highly instructive.

Sections of Dictyoxylon from the Lancashire coal-measures, shown by Mr. How, and a section of fossil wood belonging to the genus Araucaria, from Edinburgh, exhibited by Dr. Millar, may also be signalized, and also a piece of limestone wonderfully rich in polyzoa.

In selecting the above for mention, it must on no account be concluded that many others were not well worthy of special description.

The Society was indebted to Mr. Baker and Messrs. How for the loan of excellent lamps.

*Exhibitors and Objects.*

Mr. James Bell: Coffee pure, and adulterated with mustard husks and with locust-bean.

Mr. John Browning: Worm found in hemp of the deep-sea cable, and an insect found in the guttapercha of ditto.

Mr. Charles Baker: Type slide of Holothuria plates, by Möller.

Mr. John Badcock: *Melicerta ringens*, *Floscularia ornata*, and *Actinophrys sol*, alive.

Mr. W. G. Cocks: *Triceratium favus* (hexagonal form).

Mr. Thomas Curties: Dissections of spider, beetles, &c., by Mr. Tatem, and cuticle of *Onosma taurica*.

Mr. Frederick Fitch: Earth mite and acarus.

Mr. J. F. Gibson: Acarus of bat.

Dr. W. J. Gray: Portion of skin from the neck of a fowl, to which in a space not more than one-third of an inch square, are firmly attached, by the insertion therein of their piercing organs, nearly one hundred fleas! from Ceylon.

M. A. de Souza Guimaraens: Ovum, larva, and pupa of *Phylloxera vastatrix* and the *Phylloxera* of the oak.

Mr. F. Hailes: Selected foraminifera, from Jersey.

Messrs. How: Section of *Dictyoxylon*, from the coal-measures, Lancashire; and section of human liver.

Mr. W. T. Loy: Dissections of lepidopterous larvæ; salivary glands of Java cockroach, *Periplaneta orientalis*.

Mr. Henry Lee: Young cray-fish, *Palinurus vulgaris*.

Dr. Matthews: Canadian lichens, illuminated by sub-stage mirror.

Mr. Moginie: Skin from the finger, showing fat-vesicles, &c.

Mr. S. J. McIntire: Foot of West Indian spider, Test Podura scale, with Wenham's reflex illuminator and Nachet's $\frac{1}{8}$th objective.

Dr. Millar: Section of fossil wood from Edinburgh; fossil polyzoa, and corals from the upper carboniferous limestone, Scotland; and fossil foraminifera.

Mr. Thomas Palmer: Sections of ivy and cane and seaweeds, mounted in balsam.

Messrs. Powell and Lealand: *Pleurosigma angulatum* (4000 diameters), with $\frac{1}{8}$th immersion object-glass, on a new formula; *Amphipleura pellucida*, with $\frac{1}{4}$th immersion object-glass, on a new formula.

Messrs. Ross: Rough diamond used for turning glass; and the molecular movement of particles in fluid cavities of quartz; and their new educational microscope.

Mr. W. W. Reeves: A fungus on rotten wood, *Stemonites typhoides.*

Mr. Charles Stewart: *Gyrinus*, showing two of the four sets of compound eyes; those on the upper surface of the head for seeing in air, and those on the lower for seeing in water.

Mr. J. W. Stephenson: Crystals of sulphur.

Mr. H. J. Slack: Vesicular and other forms of silica deposited from silicic fluoride on wet cotton threads.

Mr. Amos Topping: Section of jaw-bone and teeth of hedgehog, injected; ditto of mouse, injected; ditto (transverse) of rabbit, imbibed.

Mr. J. S. Townsend: Leaf of *Oxalis stricta*, showing cell structure most beautifully.

Mr. E. Wheeler: Some whole insects, and some nice slides of Diatomaceæ, including new species of *Triceratium*, *Coscinodiscus*, and *Aulacodiscus.*

Mr. T. C. White: Salivary glands of cockroach; head of cysticercus: and a section of the pad of kitten's foot, doubly stained with picric acid and carmine.

Dr. M. Pritchard: Cochlea of human fœtus, showing organ of Corti, containing air-cells, rods, and membrana reticularis in section; human adult cochlea, showing nerve-fibres and ganglion-cells; cochlea of cat, showing the ciliated cells of Corti; cochlea of kitten, showing membrane of Reissner in position, and the general arrangement of ductus cochlea; cochlea of dog, showing rods; cochlea of guinea-pig, showing a row of outer rods; cochlea of parrot, showing the general view of the straight cochlea of a bird; cochlea of a porpoise, showing immense spiral ganglion and ganglion-cells, &c.; some beautiful models and diagrams.

Mr. W. Fell Woods: Living organisms from the cockle.

Donations to the Library, &c., since Nov. 4, 1874:—

| | From |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| Journal of the Linnean Society, No. 58 | *Ditto.* |
| Quarterly Journal of the Geological Society, No. 120 | *Ditto.* |
| Seventy photographic likenesses of the late Rev. J. B. Reade, for distribution amongst the Fellows | *Dr. Wallich.* |
| Ross' instrument for measuring thin glass | *Dr. Millar.* |

The following gentlemen were elected Fellows of the Society:—John H. Martin, Esq.; John Badcock, Esq.; Alfred Coles, Esq.

WALTER W. REEVES, *Assist.-Secretary.*

## Medical Microscopical Society.

Friday, November 20, 1874.—Jabez Hogg, Esq., President, in the chair.

Dr. Goodhart read a paper "On Ichthyosis Linguæ." He had observed two cases, both men above middle age, both with a history of syphilis, and in both the disease ended in epithelioma; in one the ichthyotic condition had lasted ten years. The naked-eye appearance of the disease is that of a thick hard white coating to the tongue in patches on its dorsum, and sometimes on the cheeks. In one case the patches were of the character of local warty excrescences, a millimètre in height, consisting microscopically of a number of vertically-set papillæ, of fusiform shape and ragged surface; the surrounding epithelium was twice its normal thickness. In the *plâque* the epithelium was much thickened, as also the cutis vera and sublying fibrous tissue: at times the epithelial layer was of uniform thickness, at others it was seen dipping down into the interpapillary spaces and sublying fibrous coat, and was surrounded by a small cell growth: to all these changes the warty appearance was due. All this was explained by over activity of the rete Malpighii, the supply of cells produced being greater than the demand created by wear and tear required.

He had not observed the colossal papillæ described by Mr. Hulke, nor the shrunken papillæ described by Mr. Fairlie Clarke; which latter might be explained by the normal papillæ having been cut obliquely; still, if the interpapillary depressions are for long clogged with excess of epithelium, then the papillæ would seem to be less prominent. The thickening of the subcutaneous fibrous tissue was especially noticed in the condensed fibrous band that normally may be seen running along immediately below the bases of the papillæ. The muscular fibre of the tongue had not been found diseased.

In order of sequence it was difficult to state which ought to be placed first: the epithelial growth or the excess of subcutaneous fibrous tissue; but probably the former.

The incurability of the disease might be owing to its being generally seen when almost in the condition of epithelioma. With regard to this latter affection, it was hard to trace, microscopically, its exact relation to ichthyosis; the general infiltration of the subjacent fibrous tissue of an ichthyotic patch with indifferent cells indicating its presence; in fact, this condition was generally characteristic of epithelioma in this situation, it being comparatively rare to see the so-called "birds' nests" of epithelium. Even before the onset of epithelioma the greatest difficulty in treating an ichthyotic patch with the idea of curing it, would be from the altered habit that the cells must have acquired after a long time, which would have to be counteracted before the normal state of things could be resumed.

In ichthyosis the normal tissues were only in excess; but in epithelioma this was not only the case, but the epithelial cells infiltrated parts foreign to them, and from their very rapidity of growth acquired the characters of "indifferent" cells. A second condition rendering the cure of ichthyosis doubtful—and at the present time impossible—was the increase of fibrous tissue. At first excessive epithelial growth was found: this meant increased blood supply, and this in turn increased development of tissue supplied by the blood: hence the one condition reacted on the other.

The President discussed the paper generally, criticising the use of the term ichthyosis; he thought that of tylosis better. He had not had the opportunity of observing a case pass on to epithelioma; and quoted one where there was no history of syphilis.

Mr. Fairlie Clarke remarked, he had, in adopting the term tylosis linguæ, only reproduced the original name, and that there were strong arguments, clinically, against that of ichthyosis. He had found, microscopically, a thinning and wasting of the papillæ; for not only is there increase of cell structure towards the surface, but it even dips down and spreads laterally between the papillæ themselves; this especially appearing as it approaches the condition of epithelioma. Sooner or later an "ichthyotic" tongue became epitheliomatous; but there is a condition where white patches ("white fibrous cicatrices") are seen on the tongue, which, though incurable, does not lead to epithelioma, and hence requires carefully distinguishing from tylosis linguæ. Epithelioma supervenes in two ways: either by extension of cell growth from the surface, which growth not only is in large quantity, but penetrates into tissues to which it is naturally foreign; or, secondly, it may commence in the underlying structures, as the result of prolonged irritation from the ichthyotic patch.

Palliative measures may relieve in the disease, but as yet we are ignorant of any cure, short of that by surgical interference.

Mr. Henry Morris questioned the connection of cancer and ichthyosis linguæ, remarking that though, in his opinion, quite distinct diseases, yet that both depend upon modified nutrition; this being the production of excessive epithelium, in the case of cancer, heterologous, but not so in ichthyosis. He had observed, at least once, epithelioma follow, as a direct result of irritation to an ichthyotic patch that had shed its scale, the red raised spot becoming a cancerous ulcer. Where epithelioma has followed, it does not spread more rapidly than if it had started quite independently, even though the ichthyosis may have been of long standing. He believed the disease to be like ichthyosis elsewhere; he had seen it on the tongue, while the neck around was similarly affected.

Dr. Allchin asked whether the secondary conditions described were not rather extensions of the ichthyotic growth, and not true epithelioma, in a histological sense, although clinically malignant.

Mr. Needham, in two cases operated on where epithelioma was commencing, had observed hypertrophy of the papillæ, and of the cutis, which was infiltrated with large granular cells; the vessels were also enlarged. He had traced the epitheliomatous growth to the original ichthyotic patch.

Dr. Goodhart, in reply, preferred retaining the term icthyosis linguæ, as one well understood now. Had but once seen the white cicatricial patch described by Mr. Fairlie Clarke; he did not think it cicatricial in character, for it could be scraped off; and suggested its being owing to some chemical change on the mucous surface. It was not the rule to find the "birds' nests" of epithelium in epithelioma of the tongue; but he had usually observed an abundant infiltration of small cells under the epithelium, as in ichthyosis linguæ. The second cause for cancer following this disease, given by Mr. Fairlie Clarke, was useful in explaining those cases where the direct extension of the disease from the original patch could not be observed. He had never verified Mr. Needham's observation of hypertrophied papillæ, though he had heard that condition described before.

## Microscopical Society of Victoria, Australia.

The usual monthly meeting of this Society was held September 24, 1874, at eight o'clock. There was a good attendance of members, and Mr. Ralph took the chair.

Dr. Sturt exhibited some Gippsland limestone, containing foraminifera, pointing out many of its characteristics and demonstrating the mode of manipulation for preparing and mounting the stone. He said the indications it presented showed the deposit had been accumulated close to the beach, but he could not state the exact locality. He did not profess to have examined it thoroughly, but hoped the slight account he had given might induce members to give that and other similar deposits a careful examination, which could be accomplished without much special skill. Dr. Sturt said he had received some specimens from Geelong, and also exhibited some crystalline limestone from the interior, extremely pure in character, and he hoped that persons throughout the colony would forward to the Society any specimens for examination.

Mr. Sydney Gibbons, F.R.M.S., exhibited the cuticle of synapta, with anchors *in situ*—a small marine animal allied to the holothuridæ, or sea cucumbers—one of which was a known article of commerce, under the name of *bêche-de-mer*. The synapta differed from the other echinodermata in not having ambulacra—the little feet by which star-fish, &c., move about. Its motion was vermigrade, creeping along by contractions and elongations, like those of a worm. This mode of progression was facilitated by the skin being studded with minute calcareous plates, in each of which a minute anchor was socketed. Knowledge of the animal in a state of nature was limited, and there was uncertainty as to the species, owing to the difficulty of preserving it for observation, the creature having a trick of committing suicide by bursting itself to pieces when caught. Mr. Gibbons also introduced to the Society the echinococcus (a hydatid), the immature form of a kind of tapeworm, which was only found in the mature state in animals of the dog family. In the larvæ form it was a common tenant of cysts or hollow tumours formed in various cavaties of the human body. He said there could be no doubt that much disease occurred as a consequence of the very common practice of dogs licking the faces and hands of children, and being kissed by them. The tænia was the smallest of the kind, being only about a quarter of an inch long, and the larvæ were often as small as 1-200th of an inch. He showed a specimen about 1-150th of an inch, demonstrating the extremely fine hooklets or barbs which evidenced the creature's presence. They were abundant in the preparations, which were stated to have been recently taken from the human subject.

Mr. Robert Robertson exhibited a tetrarhynchus, an entozoon from the flathead. Mr. Robertson explained that it lodged in the flesh and intestines of the fish, and was supplied with four probosces covered with circular rows of hooks, which were employed for boring through the flesh and tissues of the fish. Mr. Robertson likewise presented some berg-mehl, a mountain meal, from Swan-hill, containing numbers of diatoms.

Some diatomaceous deposits from New Zealand were exhibited and distributed to the members by Dr. Sturt.

The Chairman, on behalf of Mr. Johnson, exhibited an apus found by the latter gentleman at St. Kilda, observing that these creatures existed on tadpoles.

At the conclusion of the exhibits the following gentlemen were elected officers of the Society for the ensuing year :—President, Mr. T. L. Ralph; Committee, Dr. Sturt, Messrs. W. H. Archer and F. Barnard; Hon. Secretary and Treasurer, Mr. Robert Robertson (re-elected).

The meeting then proceeded with some arrangements for the annual conversazione of the Society, to be held in October, and several members present promised their aid on the occasion. It was decided to admit friends (ladies and gentlemen) of members, and for this purpose to give a liberal distribution of tickets; and that the application for tickets of any ladies or gentlemen interested in microscopical research be made to the Hon. Secretary or any of the members. Members were also requested to forward to the Hon. Secretary lists of objects which they would exhibit on the occasion.

The meeting then adjourned.

## The Memphis Microscopic Society, U.S.A.

The Society met at the usual hour on the night of 3rd December. Dr. J. T. Marable, and A. J. Murray, City engineer, were elected active members; and Dr. J. J. Woodward, Assistant-Surgeon, United States Army, in charge of the Army Medical Museum at Washington; Dr. W. B. Bizzell, of Mobile, and Dr. Sterling Loving, of Ohio, were elected corresponding members.

Contributions of unmounted material were received from Rev. E. C. Bowles, of Salem, Massachusetts, consisting of different vegetable fibres used in the manufacture of textile fabrics in India; also, six slides from B. F. Quimby, of Philadelphia, two being crystals of salicine, one crystals of phloridzin, one crystals of chloride of copper, one of fresh-water algæ from the Adirodacks, &c.

Mr. G. W. Morehouse, of Wayland, New York, contributed one dozen slides of fossil and recent diatoms. The specimens contributed by both these gentlemen were much admired for their skilful mounting.

A hearty vote of thanks was returned to each of the donors.

Encouraging letters were read from a number of practical working microscopists, expressive of the kindest hopes regarding the future of the young Society.

A paper, contributed by J. Edward Smith, of Ashtabula, Ohio, on the use of dammar varnish as a mounting medium for test objects, was read to the Society. Mr. Smith has found, from numerous experiments, that the varnish renders objects much more difficult of resolution than balsam. The increased transparency obtained by the use of the varnish seems to him to be the chief cause of the difference. The dammar mounts, according to Mr. Smith's experiments, utterly defeated a Tolles' immersion $\frac{1}{10}$th three system objective, which would readily resolve the same specimens when mounted in balsam. A Tolles' new four system $\frac{1}{10}$th objective, however, readily resolved the dammar slides, though the same result could not be obtained by the use of any of the old objectives from $\frac{1}{10}$th to $\frac{1}{50}$th.

A paper was also read from G. W. Morehouse, of Wayland, New York, on the comparative results obtained by the use of Tolles' old three system, $\frac{1}{50}$th, and the new four system, $\frac{1}{10}$th. Mr. Morehouse states that the best work of the former was unequivocally excelled by the performance of the latter. This is a great gain, as the $\frac{1}{10}$th gives a great increase of light and a better definition, as compared with the $\frac{1}{50}$th. The most remarkable point in Mr. Morehouse's investigation is this: That the optician can, by a new and simple combination of lenses, with a focal distance as low as $\frac{1}{10}$th of an inch, secure better performance than can be obtained by the old system, $\frac{1}{50}$th of an inch focal distance. This seems to be the greatest triumph of the optician's art, as regards the construction of objectives.

A communication from J. Edward Smith fully corroborated the comparative statements of Mr. Morehouse.

Mr. Dod, secretary of the Society, stated that he had ordered one of the new four system $\frac{1}{10}$ths, and that the members could soon have an opportunity of judging from practical demonstration of the value of this new objective.

The Board of Managers reported to the Society that they had purchased one of J. W. Queen and Co.'s students' microscopes, with accessions to the amount of one hundred and fifty dollars, in accordance with the expressed wish of the Society.

The Society then adjourned, and the members proceeded to an examination of the slides lately received, and to a test of the performance of a Gundlach's $\frac{1}{16}$th objective on the Möller probe platte. This was followed by an interesting discussion of the theory of "ultimate atoms," as set forth by the president, Dr. Cutler.


W.West & Co.sc.

Brachionus & Male Rotifers.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

FEBRUARY 1, 1875.

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## I.—*On some Male Rotifers.* By C. T. Hudson, LL.D.

(*Read before the* Royal Microscopical Society, *December* 2, 1874.)

Plate XCI.

It is a trite and very obvious truth, that, in consequence of the great growth of science, anyone who wishes to add to the common stock and to enjoy the pleasure of original discovery, must be content to confine himself not merely to one branch, but even to one twig, or, it may be, to a mere twiglet of the great tree of knowledge.

It is true that there are some whom great natural capacity and the happier accidents of fortune enable not only to keep themselves well acquainted with what has been done and is doing by others, but also to take up with success first one subject and then another; bringing to the investigation of each minds disciplined and enlarged by the knowledge of many others. But such cases are rare. The great majority of us have neither the leisure nor the talents necessary for playing such a part. With ordinary brains, and under the ordinary circumstances of life, anyone who wishes to study natural history, and yet is not content with doing over again what others have done before him, must of necessity be a specialist.

And to be a specialist is to lie under several great disadvantages: it tends to make a man a tedious recounter and a bad listener; for a specialist finds it equally difficult to take an interest in other peoples' hobbies, or to get others to take an interest in his. He is apt too to lose all sense of proportion: to estimate his discoveries

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DESCRIPTION OF PLATE XCI.

Fig. 1.—Ventral view of *Notommata Brachionus;* female.
„ 2.—Dorsal view of the same, to show the muscles. *a*, curved cilia at the base of the buccal funnel.
„ 3.—A setæ-bearing cushion from the head of above.
„ 4.—Enlarged view of the setæ fringing the side of the buccal funnel.
„ 5.—Dead male of *Floscularia Campanulata*—dorsal view.
„ 6.—Side view of male of *Notommata Brachionus.*
„ 7.—Ditto ditto of male of new species of *Asplanchna.*
„ 8.—Ditto ditto of male of *Lacinularia socialis.*
„ 9.—Enlarged view of (*b*) in Fig. 8.

*a.* Testis, *b.* Penis } in Figs. 5, 6, 7, 8.
*c.* Atrophied œsophagus, *d.* Arm-like processes, *e.* Third process } in Fig. 7.

rather by the difficulty they have caused him, than by their intrinsic merit; and to be quite amazed to find that the scientific world are as little startled with his corrections of some predecessor's errors, as the mathematical world was by the laborious gentleman who rightly proclaimed an error in the two-hundredth decimal place of the ratio of the circumference of a circle to its diameter.

Fortunately we are all as well provided with "flappers" to bring us to our senses, as were the sovereigns of Laputa; and though in our case the "flappers" are amateurs and not officials, yet they are not the less efficient on that account. These excellent but unsympathetic friends do good service in preventing us from over-estimating our labours, and in bringing us back from the realms of science to the work-a-day world. Even a man's own household will now and then gently flap him "as though they loved him;" but the outside world knows no such tenderness (at least in the case of Rotifer-hunters), and flaps with the most wholesome vigour. It was only a few days ago that I was hawking with a lens over a bottle of port-wine-coloured water that I had dipped from a farmyard pond, when I became aware that I was being watched by a stout labourer leaning on his pitchfork and standing on the dung heap which had stained the water I was examining. His face was a picture of pitying contempt, and said as plainly as a face could do, "Well! he looks harmless, poor fellow!—but I'm glad I've got my pitchfork:" in fact, a naturalist who goes about with bottles, hunting for little creatures in ponds and ditches, may think himself lucky if he is *silently* treated as little better than an amiable lunatic; for the great majority of mankind seem to make ignorance of natural history a positive merit, by adopting to those who study it a tone of calm superiority, which is at once both amusing and irritating.

To an audience however like the present a naturalist, even if he is a specialist, may turn with no little comfort; for he is sure to find among the members of such a Society many who are familiar with his own subject, and some who have obtained distinction in it; while even those to whom it is comparatively new have minds trained by similar investigations to appreciate his new facts, and to exercise a most useful criticism on his new theories.

It is therefore with great pleasure that I bring before your notice one or two discoveries concerning male Rotifers ("a poor thing, but mine own"), quite free from any of Touchstone's anxieties as to being understood and appreciated, though at the same time thoroughly agreeing with him that "When a man's verses are not understood nor a man's good wit seconded with the forward child understanding, it strikes a man more dead than a great reckoning in a little room."

For ten years after the publication of Ehrenberg's 'Infusionsthierchen,' it was supposed that the Rotifera were all hermaphrodite; and it was not till 1848 that Mr. Brightwell of Norwich discovered a Rotifer with separate sexes in the genus *Asplanchna.* In 1850 Mr. Gosse announced his discovery of the male of another species of the same genus, *Asplanchna priodonta,* and in 1854 Dr. Leydig discovered that of a third, *Asplanchna Sieboldii.* Two years later Mr. Gosse published a paper in the 'Philosophical Transactions' "On the Diœcious Character of the Rotifera," and in it he described and figured the males of several species of *Brachionus,* of *Polyarthra platyptera, Synchæta tremula,* and *Sacculus viridis;* besides stating that he had discovered certain unusually shaped ova (which were possibly male ova) in *Melicerta ringens.* There were also strong grounds for believing that the males of *Hydatina senta* and *Notommata Brachionus,* had been seen and described as new species of female Rotifers.

So the case stood in 1856; and I am not aware of any further addition having been made to our knowledge except my own discovery of the male of *Pedalion mirum.*

Now on looking at the list of species given above in which the males have been observed it strikes one at once that, with the single exception of *Melicerta,* they all belong to one group; namely, to that of the free-swimming Rotifers: moreover, as it is probable from Mr. Gosse's description that the ova he found in *Melicerta* were winter eggs and not male ones, the thought at once occurs that it is possible that the Rotifers may be divided into two great groups, the one diœcious, the other monœcious—the first including all the free-swimmers both loricated and il-loricated, and the second the tube-making Floscules and Melicertans, and the creeping Philodines.

Indeed Professor Huxley, in his paper on *Lacinularia socialis,* made this probability a very strong argument for considering the Rotifers as permanent forms of Echinoderm larvæ—as in these larvæ a similar difference in sexual character accompanies a difference of structure, very like that which separates the free-swimming Rotifers from most of the others.

The argument was one that was hard to answer, for it rested on the supposed monœcious character of some of the largest and most common Rotifers, of creatures that are constantly being watched and studied in consequence of their great size and beauty. Indeed it does seem strange that no one should have seen during the last eighteen years the males of *Stephanoceros* or of the *Floscules,* if these creatures have any; for the adult animals are fixed to the plants on which they are found, are of comparatively large size, and, what is still more to the purpose, have tolerably transparent tubes in which their eggs are deposited and hatched.

*Melicerta* presents a difficulty in the opacity of its tube, and *Conochilus* in its roving habits; but *Lacinularia* is free from each drawback, and yet hitherto its male has escaped observation.

It was only a few weeks ago that Mr. Bolton of Stourbridge very kindly sent me a group of *Lacinularia socialis* on a small piece of myriophyllum; and after spending some time in enjoying the beautiful sight (quite a new one to me) of a fully-expanded healthy cluster of *Lacinularia*, seen with a dark-field illumination under a low power, I changed the objective and illumination, and began a systematic inspection of one of the group. I soon discovered that the animals were loaded with eggs, and almost at the same instant saw a young Rotifer playing round one of the females in the usual male fashion. I at once endeavoured to catch and isolate it, and on succeeding found that it was a male, in the usual aborted condition, but differing in shape and proportions from any that I had seen before. As the weed was perfectly clean, and had nothing on it but this cluster of *Lacinularia*, I had no doubt that it was the male of that rotifer; but to make quite sure I clipped away everything from the group, and then dropped it into a small tube of clean water. In this the eggs hatched day after day, supplying me with dozens of the same male, so that I had every opportunity of studying its form and structure.

Fig. 8 is a side view of this creature. It will be seen that it consists of little else than a large testis (*a*) ending in a hollow cylindrical penis (*b*), and nearly filling the whole internal space of the body. Of mouth, œsophagus, mastax, or stomach, it has not even a vestige. There is a large nervous ganglion (*c*) giving off nervous threads to two red eyes, and a dorsal antenna (*d*).

Tortuous tubes with vibratile tags were visible above the testis, and could be traced partly down the animal's sides; while above the testis between it and the ganglion (*c*) I repeatedly thought that I caught sight of the delicate outline of a contractile vesicle. At first sight this seems a most unusual position, but, if I am right as to its existence, it really holds its normal position with respect to the testis, and is only apparently thrust out of its proper place by the monstrous size of that organ.

Large cilia could be seen lining the passage through which the penis could be protruded, as well as the cup which terminated the short pointed foot.

My good luck with *Lacinularia* encouraged me to make a deliberate effort to find the male of *Floscularia Campanulata*, which was I knew growing in fair abundance in a pond not far off, and I have at last been successful in finding it, and in seeing it (when newly hatched) force its way through its mother's tube. The Floscules were growing on algæ attached to the stems of water-lilies, and the first thing to be done was to cut up the stems in lengths at the pond's side, and by the aid of a lens to pick out the pieces where there were good-sized clusters of them; for these tube-dwellers have a choice as to depth and light, and even as to the side of the stem to which they adhere. Next, every piece had to be hunted over with a low power, in the hope of detecting some specimen with eggs differing in size, shape, or number, from the ordinary female ones. Unfortunately the stems of the water-lily were very thick, and I had to put them in a trough so deep that I was often prevented from using a power sufficiently high to detect differences of size—and as for those of shape or number I could at first find none.

After two or three days of this work, and of finding none but the usual-looking eggs, I at last came upon an empty tube with three eggs in it, and these eggs were perceptibly smaller and rounder than usual. Moreover, one of the eggs already showed the two red eyes quite distinctly, but not a sign of any teeth. I compared this with a female egg close by, and in which the eyes were in much the same state, and in it the teeth were distinctly visible.

This was at three o'clock in the afternoon, so I left the supposed male egg in the middle of the field of view, and at seven returned to see what progress had been made. The frontal cilia were now visible, and the young animal could twitch itself about in its shell—but still there was no sign of teeth. Feeling certain that it was a male, I sat down before the instrument, book in hand, determined to wait patiently for the happy moment.

But I reckoned without my Rotifer. It developed rapidly enough during the next four or five hours, and I could distinctly see through the shell, and at the same time the cilia of both the penis and the foot. But at half-past one it was still twisting about in its shell. It was as an " unconscionable time a-hatching " as Charles the Second was " a-dying "; and as the little wretch would neither hatch nor apologize, I went to bed and left him to his fate —namely, to be dried up.

I confess that I was a little reluctant after such a failure to go back to the pond and begin the whole thing over again ; but I did, and I was rewarded by finding on my return home, on almost the first piece of stem I looked at, two Floscules, each with six or seven of the same smaller eggs in their tubes, and with others in their ovaries. None of the eggs had eyes developed in them, so I knew that I need not trouble myself about them for twenty-four hours ; but next day, soon after I began to look over the eggs in one of the cases, I saw a newly-hatched young one in the other, trying to drill his way through his mother's case into the water. I say " drill," for the word exactly expresses the process. With its wreath of powerful frontal cilia it swept its way through the tenacious stuff of which the tube is composed ; stopping every now and then to take breath, as it were, and to hitch up its foot to get a fresh purchase for a new assault. Its progress was very slow, for nearly twenty minutes elapsed before the head fairly emerged from the side of the case. The instant it had done so the cilia flashed round in a grand whirl, and in a second the whole animal glided swiftly through the opening. I had seen the testis and penis, and the entire absence of teeth or stomach, and could safely say now that *Floscularia Campanulata* was diœcious. Soon after I saw another newly-hatched male attempt the same means of exit into the world from the other tube. But *he* was not so fortunate. Whether the case were tougher or the young one weaker it is hard to say, but I watched the poor fellow working away till he was fairly exhausted; and then he crept back to the side of his mother, and died. Fig. 5 is a portrait after death of this unlucky Floscule.

The diœcious character of at least one of the Floscules, to wit *Campanulata*, and also of one of the Melicertans, viz. *Lacinularia socialis*, having been thus established, it is worth while to revert to Professor Huxley's argument, to state it a little more amply than I have done already, and to show how it is affected by these two discoveries of male Rotifers.

The argument is as follows. The Rotifers have a nervous ganglion (their only one) situated on what is usually called the dorsal surface of the body, and in one group of Rotifers—viz. that of the free-swimmers, creepers, and Floscules—the trochal disk has been so unsymmetrically developed as to thrust the mouth to the opposite side of the body from that on which the ganglion lies, and the anus to the same side as the ganglion; while in another group —viz. that of the Melicertans—the disk has been so developed as to push the mouth to the same side as the ganglion, but the anus to the opposite side. Moreover, " so far as the sexes of the Rotifera can be considered to be made out (approximatively) the diœcious forms belong " to the first group, and the monœcious to the second. " It is this circumstance," says the Professor, " which seems to me to throw so clear a light upon the position of the Rotifera in the animal series. In a report in which I have endeavoured to harmonize the researches of Professor Müller upon the Echinoderms, I have shown that the same proposition holds good of the latter in their larval state, and hence I do not hesitate to draw the conclusion (which at first sounds somewhat startling) that *the Rotifera are the permanent forms of Echinoderm larvæ,* and hold the same relation to the Echinoderms that the Hydriform Polypi hold to the Medusæ, or that *Appendicularia* holds to the Ascidians."

There are other weighty arguments in the same paper for placing the Rotifera among the Vermes, but this is one on which, as will be seen from the above extract, Professor Huxley lays great stress, and which the discovery of the male of *Lacinularia socialis* weakens considerably. For now it has been shown that diœcious Rotifers exist among those whose anus is on the *opposite* side to that of the ganglion as well as among those who have it on the *same* side. Besides, there is now a great probability that all the Melicertans are diœcious, for they resemble each other so much that Mr. Gosse, in the 'Popular Science Review,' has proposed to reduce the whole family to a single genus.

To sum up then we may say that the Rotifers can be divided into the five families, the *Flosculariæa*, the *Melicertadæa*, the *Brachionæa*, the *Hydatinæa* or *Notommatæa*, and the *Philodinæa*, and that among the first four of these five, diœcious genera have been discovered. The family of the *Philodinæa* is the only one in which as yet no males have been found.

It may possibly still be held desirable to rank the Rotifera among the Vermes, with which it must be admitted they have many points in common; but among the reasons for so doing, that of their sexual resemblances to the Echinoderms can scarcely hold a place.

Indeed the very peculiar males of the Rotifers lend no little assistance to those who, like Gosse and Leydig, would place the Rotifera among the Crustacea; for a parallel case to that of their rudimentary condition is only to be found among some of the Cirripedes.

The males of the Entomostraca are often smaller and rarer than the females, and (as among the Rotifera) one impregnation suffices for a succession of generations of females. There are female parasitic Isapods too that have minute imperfect males parasitic on themselves; and among the Vermes there are curious rudimentary males much smaller than the females; but in all the above cases the males possess some sort of mouth and stomach, and are capable of taking nutriment, whereas in that of the Rotifera, and of the males of the parasitic Cirripedes *Alcippe* and *Cryptophialus*, the males have not even a rudiment of mouth or stomach. As Darwin (in his monograph of the Sessile Cirripedes) has pithily said, "they exist as mere bags of spermatozoa."

As the male Rotifers spend their short lives in incessantly chasing one female after another, they are provided with cilia, muscles, eyes, nervous ganglion and nerve-threads (as well as with a depuratory apparatus), all of which the males of *Alcippe* and *Cryptophialus* lack; but in the remarkable absence of any means of procuring nourishment, these Rotifers and Cirripedes at the same time most closely resemble each other, and differ from almost everything else.

It is beyond the scope of this paper to discuss the question of the affinities of the Rotifers, or that of their true position in the animal kingdom; but I may be permitted to add that there is at least one Rotifer—viz. *Pedalion*—which it seems to be impossible to class among the worms, for it has six hollow limbs worked by striated muscles, some of which pass freely through the cavity of the body.

Among the males figured in the Plate is that of *Notommata Brachionus*, Fig. 6. I found the females, Figs. 1 and 2, of this very curious Rotifer for the first time some two years ago, in a large rain puddle lying in a woody hollow at the top of Nightingale Valley. The pool was not more than two or three yards across, and it was often dried up, and very little light could reach it through the overhanging trees; yet when I first found it, it contained swarms of this *Notommata*, many specimens of large *Bursaria*, and a few half-developed ova of some fresh-water zoophyte. The latter were evidently accidental additions to its inhabitants, for none of the adult Polyzoa could have survived a single drying up of the water, and this pool was often dry for a fortnight at a time. It is hard to say how the ova could have got into such a place. Perhaps some bird bathing at the edge of Abbot's Pond (where *Plumatella repens* was then abundant) had entangled the ova in its feathers, and had then washed them off again in the pool, or they might have been carried there in the coat of a roving spaniel.

Any way it was curious, for the two places are more than a mile apart; and though stato-blasts might travel a long way without injury, it does not seem likely that soft ova could be taken far, or could survive after having been dried up.

On one occasion, when I had been disappointed by finding the pool empty, I thought I would try to rear my own Rotifers at home; so I carried away in my tin box a thick sandwich of leaves and soil from the bottom of the pool, and put it into an aquarium full of soft water. In three days' time the *Notommata* made their appearance, as I had hoped, though not in any great numbers; the experiment however was rather too successful if all the creatures that came to life are to be taken into account; for one evening I saw such a forest of long white worms waving backwards and forwards over the rotten leaves, that I hastily emptied my aquarium, and resolved to be contented in future with my pool, no matter how often it might fail me.

The female of *Notommata Brachionus* is so good an example of a typical Rotifer, that its structure requires no further explanation than that given by Fig. 1, which shows the ventral surface.

This *Notommata* has however two peculiarities which are well worth notice. First, that its external shape might almost make one fancy it a hybrid between a *Hydatina* and a *Brachionus;* for it has the peculiar head of the former, and an illoricated body tucked up to resemble the lorica of the latter: while in the second place the setæ and cilia surrounding the mouth, and the funnel-shaped cavity leading to it, surpass in number, size, and variety, those of any other Rotifer that I have ever seen.

Three hemispherical cushions (as in *Hydatina*) crown the head; and the setæ on them, Fig. 3, have distinct bases from which they spring, and *through* which they grow. In the young *Notommata* only the tip of each seta can be seen just above the top of its cylindrical base. Similar partly sheathed cilia are to be seen ranged parallel to one another on each side of the buccal funnel; an enlarged view of these is given at Fig. 4, which also shows a fan of small setæ situated just above the base of each of the larger ones. At the bottom of the funnel also are large curved cilia (figured at (*a*) in Fig. 2), and I have more than once thought that I could detect the presence of minute cilia over the whole surface of the cavity.

I have added to the Plate, Fig. 7, the male of a new species of *Asplanchna*. The female resembles *Asplanchna priodonta*, but differs from it in having an unusually large contractile vesicle, which is kept constantly in motion to and from the ovary with a sort of semi-contraction, but without the distinct spasmodic collapse that usually characterizes this organ. The vibratile tags too are numerous, and arranged in a long straight line down the whole length of the body, as in *A. Brightwellii*. It is, however, only half the size of *A. Brightwellii*, and its peculiar male also shows it to be a different species. I should have thought it to be *A. Sieboldii*, were it not that the male of *A. Sieboldii* has four arm-like processes, and this male has only two. The male is wonderfully transparent and empty; for such organs as it has are small, and its skin is so delicate that it is often difficult to detect the creature in the water with a lens, in spite of its being of the respectable size of $\frac{1}{60}$th of an inch. Its two arms (*d*) and an odd hump (*e*) are only seen when it retracts its head; on doing this they start out stiff from the body in the most comical fashion, and collapse again as the relaxing muscles allow the head to resume its usual position. The atrophied œsophagus is seen at (*c*) attached by a thread to the conical hump: of the mastax, stomach, or salivary glands, there is not a trace. The vibratile tags and contractile vesicle are as well seen as in the female, but I have met with a specimen in which the tags appeared to be quite empty, with the exception of two or three near the contractile vesicle in which the usual cilia were vibrating.

It is hardly possible to consider so rudimentary a creature as this male *Asplanchna* without speculating on the steps that have brought it into so strange a condition. When we find an atrophied œsophagus, closed at both ends, without either mastax or stomach attached to it, but in precisely the same position as the œsophagus of the female, it is difficult to imagine that we are looking at the original state of things: the mind naturally pictures to itself a time when the œsophagus was of real use, when it led from a mouth to a stomach, and when the male, as capable of feeding as the female, lived a much longer life than it does now, as well as a very different one.

What then can have led to so remarkable a degeneration? What can have reduced the male to the rare, rudimentary, short-lived creature that he is at present? It might be suggested that an abnormal development of the special male organs has taken place in some specimens at the expense of the other organs of the body, and that an unusually numerous and vigorous offspring has descended from such specimens; inheriting the peculiarities of their parents, and supplanting the feebler and normal tribes; such a process, if continued, leading in time to the conversion of the male into what is now little more than a movable "bag of spermatozoa."

But this guess, if accepted as an explanation, only meets half the difficulty. How is it that parthenogenesis among the females accompanied degeneration among the males? For, of course, if the males occur only at certain seasons of the year, and live but an hour or so, parthenogenesis is a necessity among females whose lives are so short that scores of generations succeed each other between two appearances of the males. Then too is there any connection between the degeneration of the male and its rare occurrence? And how comes it that some female Rotifer, in no way distinguishable from hundreds of others, should, unlike them, lay male eggs instead of female ones? And why should the same female never lay eggs of different sexes?

Questions such as these are much easier asked than answered; and yet it is the hope of finding a satisfactory answer to them that constitutes the chief charm of natural science, which without its speculations and hypotheses would become a barren record of comparatively uninteresting facts.

I cannot pretend to offer any solution myself of these difficulties; and, indeed, in the case of the Rotifers it seems almost hopeless to expect ever to get a solution of them: for the small size of these creatures prevents us from studying them except under conditions that are very unfavourable both to life and health.

## II.—*On the Invisibility of Minute Refracting Bodies caused by Excess of Aperture, and upon the Development of Black Aperture Test-Bands and Diffraction Rings.*

By Dr. Royston-Pigott, M.A., F.R.S., &c.

(*Read before the* Royal Microscopical Society, *Jan.* 6, 1875.)

The invisibility of minute bodies, subtending a sufficient visual angle to be readily seen if properly defined, is a highly curious and important fact, to which our Hon. Secretary, Mr. Slack, has already drawn the attention of the Royal Microscopical Society.

This invisibility depends upon several causes, which it is now intended to examine.

Minute bodies are often solely distinguished by the sharpness and decision of their outline. The question is—can this outline be obliterated by the conditions of vision and by any relation between the refractive index of the substance and the aperture of the objective employed? A few experiments will now be related which may perhaps help to unravel these points.

First starting with gas bubbles found in plate glass (probably nearly vacuum bubbles)—if glass of very fine quality be chosen, (having surfaces true enough to exhibit Newton's rings * under pressure,) and these be examined with a horizontal microscope placed opposite the window, a very perfect picture of the prospect will be seen in miniature, surrounded by a black band; the field of view will be found precisely *three-fifths* of the diameter of the bubble, or the band one-fifth. The curious fact is, that for this hollow lens the breadth of the band is the same for all objectives, whatever be the aperture.

Not so, however, with a solid spherule of the same size and of the same glass. Two conditions regulate this breadth.

I. The band increases in breadth from nothing till it occupies the whole spherule as the aperture is diminished.

II. The degree of aperture at which this black band first appears varies with the refractive index of the bead.

It results from these principles that the aperture of an objective regulates the appearance or disappearance of the circular black outline of minute refracting spherules, and consequently the black bands of refracting cylinders.

Having thus stated the points to be attended to in observing such bodies, I proceed to describe more in detail the facts from which these conclusions have been deduced.

I. In the case of the air bubbles, in plate glass, care must be

* Of course it is only when one of the surfaces is truly spherical that Newton's rings are developed: in nearly flat surfaces these rings take every imaginable shape under pressure and every variety of prismatic hue.

taken to choose them lying near the surface, and as small as possible. The writer published an article in January, 1870, from which the following passages are now selected.

"Remembering that when a pencil of parallel rays passes through a denser into a rarer medium (supposing common air were enclosed), the focal point for a refractive index 1·5 would be found to lie on the posterior surface of the minute hollow sphericle, i. e. on the surface farthest from the eye of the observer. If the contained gas or air were much attenuated it would approach the centre. . . . The field of view presented is independent of the aperture of the objective, whilst change of aperture has a very surprising effect upon the visible characters of the solid glass spherule. This change, so decided and important in minute research, has been a cause of much surprise and pleasure to the writer, as it appears to open a new mode of changing and selecting definition under novel conditions." . . . . "Remarkable is the result that large aperture destroys the black ring-outline of refracting spherules and black borders of cylindrical fibres. In innumerable instances the only possibility of distinguishing the molecules of organized particles depends upon shadow. . . . A fundamental defect of excessive aperture is the disappearance of these invaluable characters of minute spherules and of fibres capable of refracting light."

"In some cases, therefore, they appear jet black with a small aperture, but most frequently invisible with an excess of aperture."*

The appearances of Mr. Slack's invaluable silica films most opportunely illustrate the effect of aperture. He has observed quite independently that the most minute beading visible with a glass of low aperture vanished under increased aperture.

Now if aperture must be diminished in order to develope the black test-band, it is evident that excess of aperture may destroy it, so that in the case of direct illumination by parallel rays the blackness and breadth of the test-band may wholly depend upon the two conditions already stated.

I have used in these experiments an "iris diaphragm" (constructed for me by Messrs. Beck in 1869); by this instrument the aperture can be instantly reduced from $\frac{3}{8}$ths to $\frac{1}{100}$th inch.

*Experiment* 1.—Select very fine threads of glass, and, holding them like an open fan, rapidly pass the ends through the blue edge of the steady flame of a wax light. On examination with the microscope † under parallel rays from the plane mirror before the window in a good light, fused spherules of glass will be seen, of various sizes and degrees of spherical perfection, in each of which a minute image of the window appears *surrounded by a black*

* 'Quart. Journ. Micro. Sci.,' Jan., 1870.

† It is best to begin with a low aperture objective.

*annulus*, which I shall call the black test-band. If the object-glass aperture be reduced, or if another object-glass be used of much less aperture, this circular blackness will appear much increased in breadth. And upon careful micrometrical measurements being made, it will be found for the same aperture *that the breadth of the black ring is exactly in the same proportion* to the diameters of the spherules. Indeed this phenomenon is so striking that the angular aperture is at once shown by the breadth of the *picture* displayed within the spherule or spherical lens.

Upon increasing the aperture the picture becomes larger and larger, and more and more indistinct and confused, until with a large aperture the ring is attenuated exceedingly; but of course as the aperture is increased the spherules chosen must be smaller in proportion to the power of the glass.

Upon diminishing the aperture exceedingly, the aperture test-band widens so much that only a minute picture is left in the centre, which can be further diminished to a bright dot.

This aperture test-band has a remarkable effect upon definition. If we are observing very minute spherules in a mass, with excessive aperture the aperture bands become almost invisible. The forms of closely-packed beading if refractive and transparent cannot be descried. Each bead under large aperture-vision forms a confused picture; and if there be brilliant illumination the forms under inspection are completely obliterated.

*Experiment* 2.—Globule of glass $\frac{1}{2000}$th of an inch in diameter. Aperture 140°, dry $\frac{1}{8}$th, 1862. Aperture band invisible. A stop is now placed behind the back set of lenses $\frac{1}{100}$th of an inch in diameter; a large, broad, black annulus is instantly produced. The breadth of the aperture band measures the reduction of aperture.

II. Another principle affects the breadth and distinctness of the test-band, viz. the refracting power of the spherule itself.

This band will not be developed in a spherule of water with a greater angle than 60° 16′ of angular aperture.*

A minute spherule of plate glass will begin to show the aperture band at so high an angle as 83°; blue sapphire at 124° 30′. But a heavy glass bead, consisting of two parts lead and one of flint, will begin to develope the aperture band at 164°.

The aperture bands are shown equally well in cylindrical threads as in spherules.

It is evident that, conversely, a coarse measure of the refractive index of any proposed substance can be got from the breadth of this band, provided the substance can be formed into minute spherules or cylindrical threads. There is, here, a nice distinction to be made as regards microscopical definition, in the double effects of variable

* For the mathematical consideration of this point, see the article already quoted.

refractive power and variable aperture upon the character of the defining band developed.

Thus the peculiarly pale appearance at the edges of some silicious forms found in sponges under ordinary apertures is dependent upon their very low refractive index. Indeed, Tabsasheer will not develope the defining band at a greater objective aperture than 25°.

I have not as yet measured the refractive index of Mr. Slack's silica beads, but suspect it is very low. For this reason alone, a very high-angled objective would fail entirely to detect the circular defining band of a very minute spherule. That gentleman has kindly presented me with several of his slides, and left in my care an excellent Zeiss $\frac{1}{6}$ from Jena. Considering the fine performance of this glass notwithstanding it has no adjusting collar, I have instituted experiments on its aperture: and at the same time measured that of Powell and Lealand's glasses.

| Objective. | Focal Length. | Angular Aperture. | Character. |
|---|---|---|---|
| Zeiss .. .. .. .. .. .. | $\frac{1}{6}$ | 68° | Dry lens. |
| Powell and Lealand .. .. | $\frac{1}{8}$ | 98° | Dry, latest construction. |
| Ditto ditto .. .. | $\frac{1}{8}$ | 124° | Immersion, ditto. |
| | | 124° | Immersion without water. |

The angular aperture was measured by laying a tube, into which was screwed the objective, upon a large flat board, then two night-lights were placed at about a foot distance from the nose of the object-glass: the lights were then gradually separated till, upon looking through the tube, both appeared distinctly in miniature at the extreme edge of the field. On replacing the eye-piece and alternately hiding each, the field of view was symmetrically illuminated. Two lines were then carefully ruled from the centre of the front to each light, and the angle subsequently measured by a protractor.

In the case of Powell and Lealand's "1872 eighth," the angle was first measured dry, and then a small piece of covering glass being wetted was attached to the front lens: in each case, as might be expected, the same oblique rays reached the observer's eye at the same position of the lights, viz. 124°.

I may remark here that one of Andrew Ross' finest "quarters" had about the same angle, tested in this way, as Zeiss' "sixth." This mode of testing is altogether different from measuring the angle at which a ray of light emanates from a brilliant particle itself immersed in a highly refracting medium: the two things are totally distinct.

There is another point of view worth considering, viz. the appearance of brilliantly reflecting particles under illumination from above. The peculiar invisibility of minute beading under ordinary wide-angled glasses under reflected light is quite as striking a result as that given by transmitted light. A brilliant scene is lit up upon dark ground. The appearances presented remind me of the beautiful effects displayed in the field of view of a first-rate telescope directed to a dew-drop glittering in the morning sunshine.

No glass yet constructed, whether microscopic or telescopic, has yet been adequate to present to the eye the real size of the image of the sun seen on a small spherule.*

The study of Mr. Slack's films by reflected light on a black ground indeed well repays the observer. Rich fields of sparkling beauty, variegated with tiny stars of various magnitudes down to exquisite groups of star-dust as it were closely resembling resolved nebulæ and cloudlets of nebulosity, doubtless indicating beading still more minute and undefinable—such are some of the lovely pictures formed by these films.

Precisely in the same way, thus illuminated, the spherical silica beads present very beautiful diffraction rings according to the quality of the glass, and the Zeiss "sixth" certainly gave very finely formed concentric ones. On trying a badly-corrected eighth objective, and thought "fine" at the time by the makers, the brilliant speck reflected by a single bead presented an exact representation of Saturn and his ring as it were viewed perpendicularly to the plane of the ring, no division being visible: in fact there were no delicate diffraction rings whatever.

Now in wide aperture glasses it is possible many images may be embraced by the extreme rays of the objective: just as each person views at one and the same time a different set of solar rays reflected from the falling rain-drop.

It would seem that an extremely wide-angled objective is not adapted for defining brilliant points of light reflected from minute spherical surfaces. Nor is it so well adapted for developing the aperture test-band of solid highly-refracting particles. On the contrary, it completely hides it.

I shall beg leave to conclude this note by a further quotation from the article already cited,† (in the case of transmitted light)—

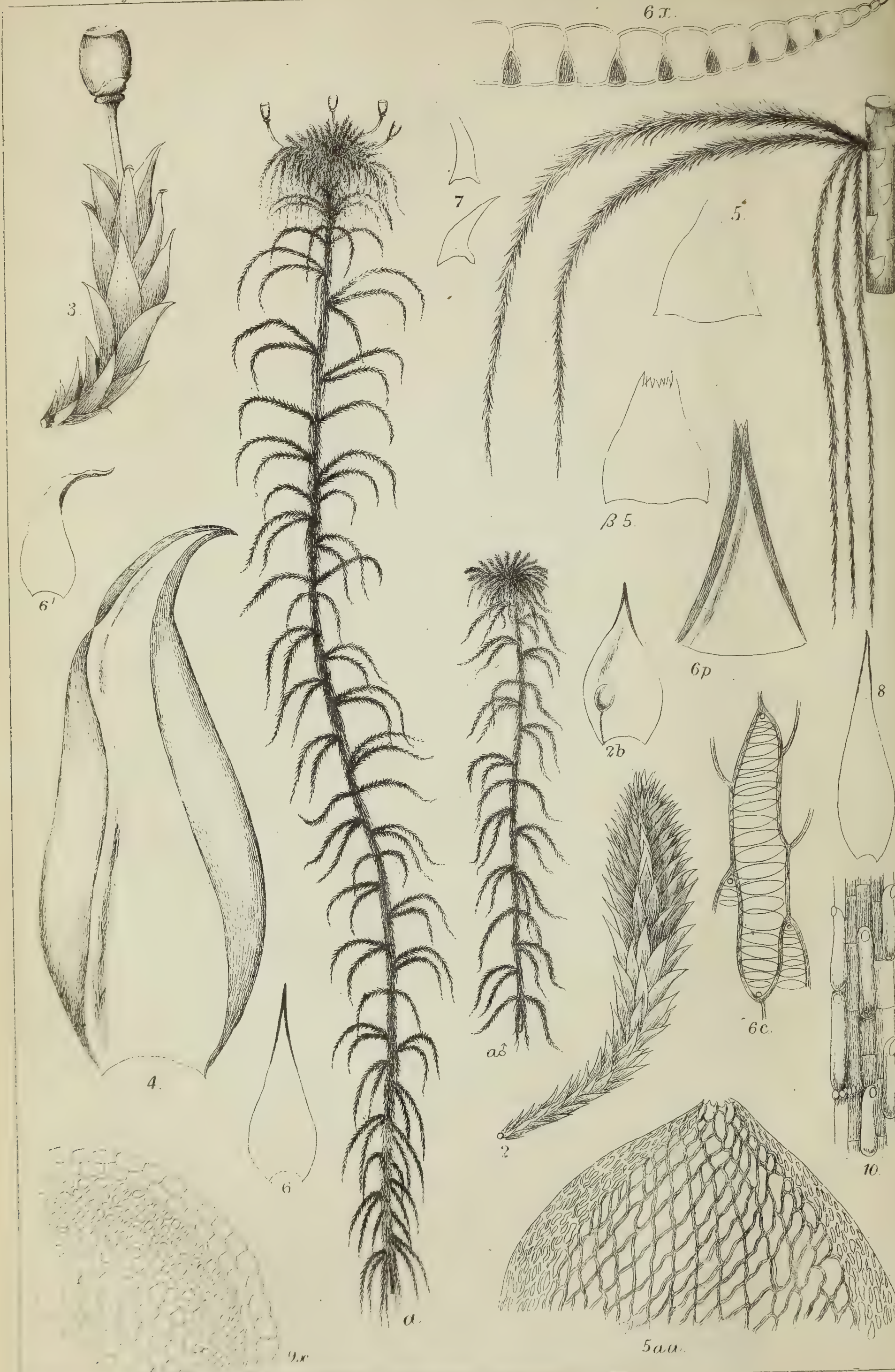
"From these effects of aperture it may now be assumed that

* With the telescope a disk which ought to be the two-thousandth of an inch appears something like the fortieth of an inch in diameter: or the spurious disk is 500 times larger than the reality. It is my opinion, however, from many careful experiments that microscopic object-glasses are more finely constructed than the telescopic, and that great improvements are still necessary in that direction.

† 'Quart. Journ. Micro. Sci.,' Jan., 1870, "Researches on the Errors of Microscopical Vision and on New Methods of Correcting them," by Dr. Royston-Pigott.

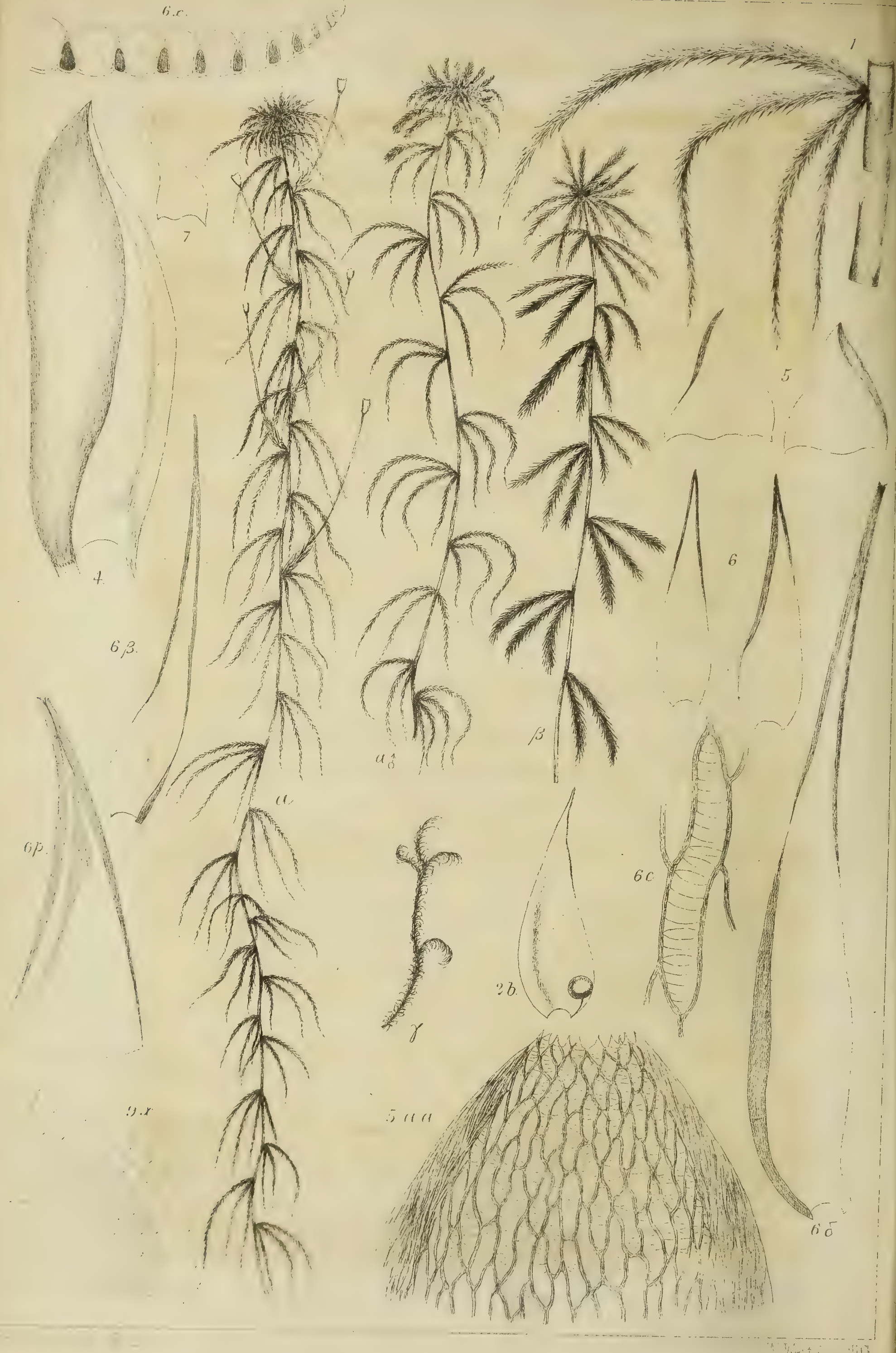
*with a small aperture* a *spherical refractive particle exhibits a dark even jet black terminal annulus.* If a body be studded with such beading it will necessarily appear dark from an assemblage of shadows; when the definition is exalted, these beads considered as lenses exhibit in general too small a central point of illumination to be detected in ordinary glasses. Even if perfectly refracting and unembarrassed with other substrata, beads so small as the thirty-thousandth of an inch form an image of a radiant source of light inconceivably minute.[1]"* "Supposing the glasses free from annular aberration, greater depth of vision with black outlines is given by a reduced aperture with direct light. . . . . On the other hand, enlarged aperture appears to illuminate a dark object, if transparent; it converts apparently, in many cases, opacity into translucence, transforms a reddish-brown scale into a brilliant object, reveals interstitial sparkling, and developes new but often delusive appearances in eidolic forms."

* "[1] The image of a brilliant flame is in all such cases swelled out by the imperfection of the glasses into an exaggerated spurious disk."




W. West & Co. lith.

Sph. intermedium.




Sph. cuspidatum

## III.—*On Bog Mosses.* By R. Braithwaite, M.D., F.L.S., &c.

Plates XCII. and XCIII.

### 18. Sphagnum intermedium Hoffmann.

Deutschlands Flora II. p. 22 (1796).

Plate XCII.

Syn.—*Sphagnum palustre, molle, deflexum, squamis capillaceis.* Dill. Hist. Musc. p. 243, tab. 32, fig. 2 a (1741).—*Sph. recurvum* P. Beauv. Prodr. p. 88 (1805).—Brid. Bry. Univ. I. p. 13 (1826).—Lindb. Torfm. No. 3 (1862).—Berkeley Handb. Br. Mosses, p. 308 (1863). *Sph. cuspidatum* Nees, Hsch. & St. Bry. Germ. p. 23, Tab. IV. Fig. 9 (1823).—C. Müll. Synop. Musc. I. p. 96 (1849).—Schpr. Torfm. p. 60, Tab. XVI, fig. 1 (1858). Synop. p. 675 (1860). Russow Torfm. p. 55 (1865).—Milde Bry. Siles. p. 383 (1869).—et p. p. aliorum auct. *Sph. cuspidatiforme* Breutel Bot. Zeit. 1824, p. 407.—Brid. Bry. Un. I. p. 752. *Sph. Mougeotii* Schpr. in Moug. & Nestl. St. Crypt. Vog.—Rhen. fasc. XIV, No. 1306. *Sph. flexuosum* Dozy & Molk. Fl. Batav. p. 76, tab. III. *Sph. cuspidatum* and var. β, *recurvum* Wilson Bry. Brit. p. 21.

*Dioicous. Plants robust, straight,* in large, dense or lax tufts; *yellow-green, pale green or sometimes pale ferruginous above, pale brown or whitish below.* Stems 6–12 in. high, greenish white; *the cortical cells small, thin, not porose, in 2–3 layers;* woody zone narrow, pale yellowish. *Cauline leaves reflexed, rather small, ovato-triangular, minutely auricled, without fibres or pores,*

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EXPLANATION OF PLATES.

Plate XCII.

Sphagnum intermedium.

*a.*—Female plant. *a* ♂—Part of male plant.
1.—Part of stem with branch fascicle.
2.—Male inflorescence. 2 *b.*—Bract with antheridium.
3.—Perichætium and fruit. 4.—Bract from same.
5.—Stem leaf. 5 *a a.*—Areolation of apex. β 5.—Stem leaf of var. riparium.
6.—Leaf of divergent branch. 6′.—The same in a dry state. 6 *x.*—Section. 6 *p.*—Point of same. 6 *c.*—Cell from middle × 200.
7.—Basal intermediate leaves.
8.—Leaf of a pendent branch.
9 *x.*—Part of section of stem.
10.—Part of a branch denuded of leaves.

Plate XCIII.

Sphagnum cuspidatum.

*a.*—Female plant. *a* ♂.—Male plant.
1.—Part of stem with branch fascicle.
2 *b.*—Bract with antheridium.
4.—Perichætial bract.
5.—Stem leaves. 5 *a a.*—Areolation of apex.
6.—Branch leaves. 6 *x.*—Section. 6 *p.*—Point of same. 6 *c.*—Cell from middle × 200.
7.—Basal intermediate leaf.
9 *x.*—Part of section of stem.
β.—Var. plumosum. 6 β.—Branch leaf of same.
γ.—Var. hypnoides.
6 δ.—Branch leaf of var. Torreyi.

F 2

*broadly bordered with narrow cells,* the apex somewhat obtuse with 3–5 small teeth, not involute at margin.

*Branches* 4–5 *in a fascicle,* two divergent, and arched downward, the rest pendent, attenuated, *closely appressed to the stem and concealing it;* those of the coma numerous, short, obtuse, squarrose-leaved, forming a large dense capitulum; retort-cells elongated, perforated and slightly recurved at apex.

*Branch leaves densely imbricated, erecto-patent, broadly lanceolate, involute in the upper part, when dry with the margins undulate and points recurved;* apex with 2–3 minute teeth. Border of 2–4 rows of extremely narrow elongated cells; hyaline cells of the upper half elongated, filled with annular and spiral fibres, and with a few small pores; of the lower half very long, with annular fibres only and no pores; *chlorophyll cells free on the posterior surface, trigono-compressed in section.*

Male amentula fusiform, subclavate, ochraceous, the bracts ovate, acuminate. Capsules numerous in the capitulum, exserted, and also in the upper fascicles. Perichætium yellow-green, *the bracts broadly oval, pointed, concave, without fibres or pores;* lower ovate, acuminate, recurved at apex, upper elliptic oblong, emarginate. *Spores yellow.*

Var. *β. riparium.*

*Sph. riparium* Ångst. Öfvers. Vet. Ak. Förh. XXI, p. 198 (1864). *Sph. cuspidatum Var. majus* Russ.

Plants taller, dull brownish green. Cauline leaves shortly triangular, erose and somewhat fringed at apex. Branch leaves ovato-lanceolate, without fibres in the apical cells.

Var. *γ. speciosum* Russow.

Plants robust 10–18 in. high, deep green. Capitulum very large; branches gradually attenuated from the middle. Cauline leaves large, elongated-triangular, often deeply fringed at apex. Branch leaves longish lanceolate, with a subulate point, recurved when dry, without fibres in the upper cells.

Hab.—Open moorlands, wet heaths and spongy mountain bogs. Frequent. Fr. July. *β,* in deep pools. Upland and Westrobothnia. In the Riesengebirge, Labiau in Prussia, Livland. Woolston Moss, Cheshire (Wilson). *γ,* Eastern Europe; sparingly in Silesia, Estland, Courland and Prussia.

Prof. Schimper unites this plant with the following as *Sph. cuspidatum* Ehrh., regarding the present as the type of the species, and the other as a submersed variety, and moreover describes them as monoicous. The habit, texture and general facies of the two are so dissimilar, that they appear to me always separable by these characters alone, and in all the specimens I have examined the reproductive organs in both are on separate plants. The points especially noteworthy in *Sph. intermedium* are, the pendent branches quite concealing the stem; the indistinct cortical cells, which scarcely differ from those of the woody layer; the branch leaves undulated and more or less squarroso-recurved when dry, the broadly oblong, apiculate, more densely areolate perichætial bracts, and the pale yellow spores.

The indefatigable Lindberg has satisfactorily settled the nomenclature of both *Sph. cuspidatum* Ehrh. and *Sph. intermedium* Hoffm. from an examination of original specimens of both authors preserved at St. Petersburg; Hoffmann's description is otherwise far too brief for correct determination, and his Var. $\beta$, of *intermedium* is also stated by the same authority to belong to *Sph. acutifolium* Ehrh.

### 19. Sphagnum cuspidatum Ehrhart.

Decades Crypt. No. 251 (1791).

Plate XCIII.

Syn.—*Sphagnum palustre, molle, deflexum, squamis capillaceis, β fluitans.* Dill. Hist. Musc. tab. 32, fig. 2, B. (1741). *Sph. cuspidatum* Ehrh. l. c.—Hoffm. Deutsch. Fl. II. p. 22 (1796).—Smith Fl. Brit. p. 1147 (1804). Eng. Bot. t. 2092 (1809). Turner Musc. Hib. p. 6 (1804).—Bridel Sp. Musc. I. p. 17 (1806). Mant. Musc. p. 2 (1819). Bry. Univ. I. p. 14 (1826).—Weber & Mohr Bot. Tasch. p. 74 (1807).—Schwägr. Supp. I. P. I. p. 16, tab. VI (1811).—Schkuhr Deutsch. Moose p. 16, Tab. 7 (1810).—Röhling Deutsch. Fl. III. p. 35 (1813).—Schultz Supp. Fl. Stargard. p. 65 (1819).—Fl. Dan. Tab. 1712 (1821).—Hüben. Musc. Germ. p. 29 (1833).—Dozy & Molkenb. Fl. Bat. p. 79.—Berkel. Handb. Br. Mosses p. 307 (1863).—Lindb. Torfm. No. 1 (1862). *Sph. laxifolium* C. Müll. Synop. I. p. 97 (1849). Milde Bry. Siles. p. 385 (1869). *Sph. cuspidatum* Var. $\gamma$, Bry. Brit. Tab. IV. *Sph. cuspidatum β submersum* Schpr. Torfm. p. 61, Tab. XVI. fig. 1 $\beta$ (1858). Synop. p. 676 (1860). *Sph. laricinum* Ångstrom, Öfver. Vet. Ak. Förhandl. XXI, p. 197 (1864).

*Dioicous. Plants very soft, in loose submersed or floating tufts;* light green, deep green, or more or less tinged with yellow or brown.

*Stems slender, flaccid, pale green,* 6–18 in. or sometimes several feet in length; *cortical cells not porose, larger, well defined, in* 2–3 *strata.*

*Stem leaves ovate-oblong,* pointed, with the margins involute at apex, *broadly bordered with very narrow cells, the hyaline cells of the upper half with numerous spiral fibres.*

Branches 3–5 in a fascicle, longer, often turned to one side and falcate at points; all divergent, *or* 1–2 *pendent but not concealing the stem,* those of the coma few and more lax.

*Branch leaves laxly imbricated, narrowly lanceolate, flexuose when dry, often somewhat falcato-secund,* 3–6 toothed, and with a broader border of narrow cells; *chlorophyll cells free on the posterior surface, trigono-elliptic in section.*

Capsules in the capitulum, or more frequently scattered on the stem, the peduncles being often much elongated. *Perichætial bracts* *distant from each other, very broadly oval, involute at apex, laxly areolate, with fibres in the upper cells. Spores ferruginous.* Male plants more slender, amentula fusiform, the bracts ovato-lanceolate.

Var. *β. plumosum.*

*Sph. subulatum* BRID. Sp. Musc. I, p. 19. Mant. p. 3. Br. Univ. I, p. 18. SCHWGN. Supp. I, P. I, p. 18. *Sph. acutifolium δ subulatum* Bry. Germ. I, p. 21, T. III, fig. 8***. ROTH Fl. Germ. III, P. I, p. 120. BLANDOW Fasc. V, No. 204. *Sph. cuspidatum* Musc. Brit. p. 4, T. IV. *Sph. cuspidatum β δ plumosum* SCHIMP.

Submersed, slender, elongated; branches decurved, all uniform and divergent, their leaves very long and narrow.

Var. *γ. hypnoides.*

*Sph. hypnoides* BRAUN in Bot. Zeit. 1825, No. 40. BRID. Br. Univ. I, p. 752.

Short, densely tufted; stem simple with simple branches, hooked at apex. Leaves uniform, strongly undulated, narrowly lanceolate, falcato-secund.

Var. *δ. Torreyi.*

*Sph. Torreyanum* SULLIVANT, Mem. Amer. Ac. n. s. IV, p. 175 (1849). Mosses of Un. States, p. 13, No. 16 (1856).

Robust, rigid 10–16 in. high, of a dirty brown colour; branches 4–5 flattened. Branch leaves very large, involute at point, elongated lanceolate-acuminate, spreading, straight, broadly margined, minutely eroso-denticulate at apex; stem leaves without fibrils.

Hab.—Stagnant pools in moorlands, frequent. Fr. July. *β*, in deeper water. *γ*, in Lake Hornsee (A. Braun). *δ*, ponds in pine barrens of New Jersey (Torrey).

The chief points of distinction between this species and the last are these; in *Sph. cuspidatum* the plants are more slender, the pendent branches not closely appressed to the stem so that it is more or less visible; the cortical cells of stem well defined from the thicker woody layer; the longer branch leaves not recurved when dry but slightly flexuose; the stem leaves with larger cells, fibrillose in the upper part, and narrower more elongated ones at the margin; the more obtuse perichætial bracts; and lastly the brown spores. It must also be borne in mind that the two plants not unfrequently grow together, yet each retaining their special features. A gradual transition may be observed between the typical plant and the Var. *plumosum*, a form of which (Var. *mollissimum* of Russow) with densely placed fascicles is remarkable for its yellow, soft, spongy texture, and was found by Nowell near Todmorden. C. Müller regards the Var. *hypnoides* as an abnormal condition of seedling plants.

## IV.—*On the Similarity between the Red Blood-corpuscles of Man and those of certain other Mammals, especially the Dog; considered in connection with the Diagnosis of Blood Stains in Criminal Cases.* By Dr. J. J. Woodward, U. S. Army.

In his recent paper "On the Value of High Powers in the Diagnosis of Blood Stains,"* Dr. Joseph G. Richardson, of Philadelphia, affirms the possibility of distinguishing the blood of man from that of the pig, ox, red-deer, cat, horse, sheep, and goat, by the measurement of the red blood-corpuscles, even in dried stains such as the microscopist is called upon to examine in criminal cases.

The circumstance that Dr. Richardson does not mention any animal whose blood-corpuscles cannot be thus distinguished from those of man, and the warmth with which he combats the prudent counsels which Virchow,† Casper,‡ and Taylor,§ in common with other experts,‖ have offered to enthusiastic microscopists in connection with this subject, led me, on perusing his paper, to fear he would be understood as teaching in a general way, that it can be

* 'American Journal of the Medical Sciences,' July, 1874, p. 102; also the 'Monthly Microscopical Journal,' September, 1874, p. 130. This paper has attracted considerable attention. See, for example, the 'Lancet,' August, 1874, p. 210; the 'Medical Times and Gazette,' August 8, 1874, p. 151; and the 'London Medical Record,' September 9, 1874, p. 560. The last of these journals is the only one to raise a warning voice: "Dr. Richardson's paper is interesting, but we are afraid the question often put, 'What is the source of the blood in a stain?' must still go unanswered. In questions where capital punishment hangs on scientific evidence, that evidence must be of no doubtful or questionable nature."

† Rud. Virchow, "Ueber die forensische Untersuchung von trockenen Blutflecken," Virchow's 'Archiv,' Bd. xii. (1857), s. 334.

‡ J. L. Casper, 'Handbook of Forensic Medicine.' Translation of new Sydenham Society, London, 1861–5, vol. i., p. 138, *et seq.*; also p. 198, *et seq.* See also the new and enlarged German edition of the same by Dr. Carl Liman, 'Practisches Handbuch der Gerichtlichen Medicin,' 5te aufl. Berlin, 1871, Bd. ii., s. 173, *et seq.*

§ A. S. Taylor, 'The Principles and Practice of Medical Jurisprudence,' 2nd edit., London, 1873, vol. i., p. 548.

‖ Among others, I may mention E. Brücke, "Ueber die gerichtsärztliche Untersuchung von Blutflecken," Wiener 'Med. Wochenschrift,' Jahrgang, 1857, s. 425; Hermann Friedberg, 'Histologie des Blutes mit besonderer Rücksicht auf die forensische Diagnostik,' Berlin, 1852; Andrew Fleming, "Blood Stains," 'The American Journal of the Medical Sciences,' vol. xxxvii., N.S. (1859), p. 84; Wharton and Stillé, 'Medical Jurisprudence,' 3rd edit., Philadelphia, 1873, vol. ii., p. 696; M. Z. Roussin, "Examen Médico-Légal des taches de sang," 'Annales d'Hygiène,' tome xxiii. (1865), p. 139. For an elaborate history of the growth of our knowledge on this subject, up to 1860, the reader may consult B. Ritter, "Zur Geschichte der gerichtsärztlichen Ausmittelung der Blutflecken," in Henke's 'Zeitschrift für die Staatsarzneikunde,' 1860; Drittes Vierteljahrheft, s. 31. The chief authority in favour of the possibility of distinguishing the blood-corpuscles of man from those of other mammalia is Carl Schmidt, "Die Diagnostik verdächtiger Flecke," Mitau u. Leipzig, 1848. I have not yet obtained a copy of this paper, but find abstracts of it in Schmidt's 'Jahrbüch' for 1849, p. 258, and Ritter's history, just cited. The reader will also find liberal extracts in Fleming's paper, cited above. The extravagant views of Schmidt are especially confuted by Brücke and Virchow in the papers cited above.

determined by the microscope with certainty whether a given stain is composed of human blood or not; and this fear has been justified by some of the notices of his essay which have since appeared in the medical journals.

Now, this subject is one which, from time to time, becomes of great importance in criminal cases, and justice, no less than scientific accuracy, demands that the microscopist, when employed as an expert, shall not pretend to a certainty which he does not possess. I suppose no experienced microscopist, who has thoroughly investigated this subject, will be misled by Dr. Richardson's paper, but there are many physicians who possess microscopes, and work with them more or less, to whom a partial statement of facts on such a subject as this is peculiarly dangerous; and the object of the present paper is to point out to this class of readers that Dr. Richardson's statement of the case, even if all he claims be granted as true, is, after all, not the whole truth: that there are certain mammals—among them the dog, the constant companion of man—whose red blood-corpuscles are so nearly identical in size with those of human blood, that they cannot be distinguished with any power of the microscope, even in fresh blood, much less in dried stains; and that, consequently, it is never in the power of the microscopist to affirm truthfully, on the strength of microscopical investigation, that a given stain is positively composed of human blood and could not have been derived from the blood of any animal but man.

I must do Dr. Richardson the justice to state, at the outset, that these facts are well known to him, although, from motives of prudence, he has thought proper to be silent with regard to them. In a note dated October 19th, 1874, in reply to one in which I informed him of my intention to write the present paper, he says, "I should be very much obliged to you if you would add to your remarks (in a foot-note or otherwise) that, on communicating with me, you found me fully aware of the difficulty of making anything more than a differential diagnosis even in the cases I specified, and of the impossibility of distinguishing the blood of man from that of a monkey or dog, but that I had refrained from giving prominence to these facts," lest an improper use should be made of them in the defence of criminals.

I must, however, entirely dissent from this view of the matter. I cannot forget that on more than one occasion in the past, witnesses summoned as scientific experts have been so misguided as to go into courts of justice and swear positively, on the strength of microscopical examinations, that particular stains were human blood,* and I think the danger that others may do so in the future,

* Passing by certain American cases, I may refer, in illustration of this statement, to the celebrated English case, Reg. *v.* Thomas Nation (Taunton Spring Assizes, 1857, p. 279), with regard to which the editor of a London medical journal

to the prejudice of innocent men, is more to be feared than the possibility that an acquaintance with the true limits of our knowledge on this subject may sometimes be made use of in the unscrupulous defence of real criminals. I have, therefore, no hesitation whatever as to my duty in speaking of this subject at all, to speak the whole truth so far as it is known to me, and in so doing, I am happy to say I follow the practice of many of the best writers on medical jurisprudence.

In the instance of the dog it might at first sight be supposed from the estimates of the average diameters of the red corpuscles in this animal and in man, as given by Gulliver and Welcker, the authorities most frequently cited in the modern text-books, that a certain small, but constant and measurable, difference existed, which might serve as the basis of a distinction in legal cases. This inference, however, is not only contrary to the facts of the case, but an examination of the original essays of the authors cited, shows that it is not borne out by their observations.

The mean diameter of the red corpuscles of man, according to Gulliver,* is $\frac{1}{3200}$ of an inch ($= \cdot 00794$ millimeter), while that of the red corpuscles of the dog is $\frac{1}{3542}$ of an inch ($= \cdot 00716$ mm.). With regard to his estimates for the human corpuscle, Mr. Gulliver says: † "We are only speaking now of the average size; for they vary like other organisms; so that in a single drop of the same blood you may find corpuscles either a third larger or a third smaller than the mean size, and even still greater extremes." According to this statement, the human red blood-corpuscles may vary in a single drop of blood from $\frac{1}{4800}$ of an inch ($= \cdot 00529$ mm.) to $\frac{1}{2400}$ ($= \cdot 01058$ mm.). Mr. Gulliver tells us further, in the same paragraph, "My own estimate of the average size has been deduced from numberless measurements, frequently repeated during the course of several years, of corpuscles quite fresh and swimming in the blood, and in various artificial mixtures, as well as in the dry state." I have not, however, been able to find, in those of his papers which I

has pithily said, that the testimony of the expert must be looked upon "as most dangerous clap-trap, and rather what we might expect to hear at some popular lecture, where the 'wonders of the microscope' form the theme of declamation to a gaping audience, than the solemn asseveration on oath of a man of science in a court of justice."—'Medical Times and Gazette,' April, 1857, p. 366.

* George Gulliver, F.R.S., "Lectures on the Blood of Vertebrata," 'Medical Times and Gazette,' vol. ii., of 1862, p. 101, *et seq.*; "On the Red Corpuscles of the Blood of Vertebrata," &c., 'Proceedings of the Zoological Society of London,' 1862, p. 91; the Sydenham Society's edition of 'The Works of William Hewson,' London, 1846, p. 216, *et seq.*; Appendix to 'Gerber's Elements of the General and Minute Anatomy of Man and the Mammalia,' London, 1842, p. 31, *et seq.*; "Observations on the Blood-corpuscles or Red Disks of the Mammiferous Animals," 'London and Edinburgh Philosophical Magazine,' vol. xvi. (1840), pp. 23, 105, and 195; also vol. xvii., pp. 139 and 325; also vol. xxi. (1842), p. 107. For a list of other papers referring to the blood-corpuscles of various animals, see 'The Works of William Hewson,' above cited, note to p. 236.

† 'Medical Times and Gazette,' vol. ii., of 1862, p. 157.

have examined, any of the numerical data from which this average size was deduced.

In the table of measurements appended to Gerber's 'Elements,' in which, for the first time, he gave "mean or average sizes" (in previous papers he had only recorded "common sizes," occasionally supplementing these by the extremes observed), Mr. Gulliver explained his method of arriving at the average size, as follows: "The common-sized corpuscles are first set down, then those of small and large size, and lastly the average deduced from a computation of the whole."* In this table the measurements for the common dog are given as follows: †

| | | |
|---|---|---|
| Common sizes .. .. | 1-4000 | of an inch. |
| | 1-3500 | " |
| | 1-3200 | " |
| Small size .. .. .. | 1-4570 | " |
| Large size .. .. .. | 1-2900 | " |
| Average .. | 1-3542 | " |

Where the "average" is simply the arithmetical mean of the several fractions given above, it can hardly, I think, be accepted as the true average size, since as much weight is given, in this mode of calculating, to the rarer as to the more frequent forms. Accordingly, it is not surprising that we find in a former paper ‡ measurements which do not accord very closely with this average. "Domestic dog, old mongrel. Common diameter of corpuscles, 1-4000th to 1-3200th of an inch. Foxhound puppy, one day old, a bitch, 1-3000th and 1-2666th, the most common diameter of the corpuscles. Foxhound puppy, twelve days old, a bitch. Most common diameter of the corpuscles 1-3000th and 1-2885th of an inch. Extreme sizes 1-4000th and 1-2666th. Mongrel puppy, four months old, a bitch; all the following diameters common, viz. 1-3693rd, 1-3554th, 1-3429th, and 1-3200th." The measurements for the second and third of these animals are about as much larger than those for the human species as the others are smaller.

It is interesting to know just how Mr. Gulliver's measurements were made. He tells us he used a glass eye-piece micrometer so adjusted that the divisions had a value of $\frac{1}{4000}$th of an inch each. "If one space and a quarter of this micrometer were occupied by a single globule, this would of course measure $\frac{1}{3429}$th of an inch; if three equally-sized particles lying in a line, and touching at their edges, covered three spaces and a half, the diameter of each of these would be $\frac{1}{3429}$th, if four spaces $\frac{1}{3000}$th of an inch."§ The objectives used were an eighth by Ross and a tenth by Powell. ‖ It is not

* Appendix to Gerber's 'Elements,' cited above, p. 1.

† *Loc. cit.*, p. 38.

‡ 'London and Edinburgh Philosophical Magazine,' vol. xvi. (1840), p. 28.

§ 'London and Edinburgh Philosophical Magazine,' vol. xvi., p. 24.

‖ *Loc. cit.*, pp. 24 and 105.

stated whether these objectives were provided with the screw-collar adjustment for thickness of cover, but they probably were, and if so, doubtless all the measurements were somewhat vitiated, like others of the same date, by the failure to allow for the variations in magnifying power produced by turning the screw-collar. Moreover, it must be clear that practically the fractions of a division of the eye-piece micrometer were only *estimated*, for the case in which a number of "equally-sized" corpuscles would be found "lying in a line," and just "touching at their edges," without overlapping, must have been rare. As to the accuracy of the value assigned to the eye-piece micrometer, Mr. Gulliver himself says: "In the absolute accuracy of any micrometer applied to objects so extremely minute it is difficult to place implicit reliance," and he only claims "relative exactness" for his results.*

Turning, now, to the original essay of Welcker, we find that his observations give even less support than those of Gulliver to the notion that the blood of the dog can be distinguished from that of man by the microscope. Welcker's measurements, as ordinarily quoted in the text-books, are ·00774 of a millimeter for man, and ·0073 for the dog. I find, in his original paper,† that the mean for the dog was derived from the measurement of but ten corpuscles in the blood of a single terrier, the variations in this case being, minimum ·0065 mm., maximum ·0082. Now, if we turn to the table‡ of his own measurements of human blood, we find that in the last measurement of the blood of Dr. Schweigger-Seidel, fifty corpuscles gave the following results: mean, ·00724 mm.: minimum, ·0051; maximum, ·0085, in which case the mean is a trifle less than that found for the dog.

I would commend this table of Welcker's to the study of those who may be disposed to underrate the diversity of size which may be observed among the human red corpuscles; the minimum measurement recorded in it is ·0045 mm.; the maximum, ·0097 mm. The author remarks: "I have always, both in animals and in man, found the transverse diameter of the blood-corpuscles of one and the same individual vary from one-fourth to one-half of the mean measurement; and it appears that all the sizes lying between the two extremes are present in tolerably equal numbers, with the exception of the smallest corpuscles, which occur for the most part singly and at intervals."

I may mention further that the mean dimensions of the human red corpuscles so often quoted from Welcker, viz. ·00774 mm., with a minimum of ·0064 mm., and a maximum of ·0086, were

* *Loc. cit.*, p. 24.

† H. Welcker, "Grösse, Volum und Oberfläche und Farbe der Blutkörperchen bei Menschen und bei Thieren," 'Zeitschrift für Rationelle Medicin,' 3te R., Bd. xx. (1863), s. 237.

‡ *Loc. cit.*, p. 263.

not derived from the whole of this table, but from four sets of measurements of his own blood only, of which two were from dry preparations and two from the moist blood. He tells us that he selected the mean ·00774 mm. because it *was* derived from his own blood, which he had used in a previous research on the number of blood-corpuscles, and thought best, therefore, to use also in the computation of their volume, which is one of the chief subjects discussed in his paper. The mean of eight other measurements from five different individuals was ·00768 mm. The blood of a chlorotic woman gave ·00656 mm. as the mean of the corpuscles examined moist, and ·00693 mm. as their mean when examined dry.

Welcker made his measurements with Kellner's System III., ocular II., magnifying about 620 diameters, and by a delicately ruled eye-piece micrometer, each division of which, with the power used, had a value of ·001723 mm., as determined by the stage-micrometer. A human blood-corpuscle fell within four or five of these divisions, while, on account of the great delicacy of the ruling, fifths or even tenths of a division could be estimated with tolerable exactness. The stage-micrometer itself was a millimeter in one hundred parts ruled by Lerebours, and which Welcker had verified by comparison with a standard scale, in a manner which he describes in full, and which is worthy of study. He measured, as a rule, 50 blood-corpuscles from each sample, and these were not selected, but taken indiscriminately one after the other, as they came under the scale while the specimen was being moved along.

Other observers besides Gulliver and Welcker have recorded minute differences in the average size of the red corpuscles of man and the dog. Thus, Carl Schmidt * estimates the average diameter for man at ·0077 mm.—for the dog at ·0070 mm. A. Kolliker † fixes the mean for man at ·0033 of a Paris line (= ·0751 mm.)—that of the dog at ·0031 of a Paris line (= ·00709 mm.). On the other hand, Friedberg ‡ makes the blood-corpuscles of the dog the largest, stating that he finds the human corpuscles measure from ·0070 to ·0058 mm.—those of the dog from ·0054 to ·0080.

For myself, after repeated measurements of the blood of the dog and of human blood, I can only say that I find no constant difference between them, whether the fresh blood or thin layers dried on glass be selected for measurement. The mean of fifty corpuscles taken at hazard is seldom twice the same, and sometimes that of human blood, sometimes that of dog's blood, is a trifle the largest.

The following measurements, intended to illustrate these facts,

* *Op. cit.*

† 'A Manual of Human Microscopic Anatomy,' London, 1860; pp. 519 and 525.

‡ *Op. cit.*

were made with a glass eye-piece micrometer ruled in two hundred and fiftieths of an inch, and with such a magnifying power that each division corresponded to the fifty thousandth part of an inch (·0005079 mm.). The objectives used were an immersion $\frac{1}{16}$ of Powell and Lealand, and an immersion No. 13 of Hartnack, either of which permitted the above value to be given to the divisions of the eye-piece micrometer by properly adjusting the draw-tube. The stage-micrometer used in effecting this adjustment is an excellent one in $\frac{1}{100}$ths and $\frac{1}{1000}$ths of an English inch, in which the several hundredths and thousandths, as nearly as I can measure, are equal to each other, and the ten divisions of the latter value to any one division of the former, a quality in which the stage micrometers in the market are generally deficient. I have compared this micrometer with a standard scale ruled on silver—a centimeter in millimeters and tenths—the property of the United States Coast Survey, kindly loaned for this purpose by Mr. J. E. Hilgard, who assures me that it is "very accurate." I made several comparisons both by means of an eye-piece micrometer, and by the contact method described by Welcker. These comparisons showed that the divisions of my stage-micrometer were nearly two per cent. (exactly 1·945 per cent.) larger than they ought to be, and this correction was accordingly applied in adjusting the value of the eye-piece micrometer. The value assigned to the divisions of the eye-piece micrometer for these measurements cannot, therefore, I think, differ from their absolute value by a quantity large enough to modify the results appreciably.

As the divisions represent a value twelve and a half times less than that of the divisions of Mr. Gulliver's eye-piece micrometer, and more than three times less than those of Welcker's eye-piece micrometer, I did not find it necessary to estimate fractions of a division, as they did, but read the nearest number of whole divisions corresponding to each corpuscle. Fifty corpuscles, or about that number, were measured in each sample of blood. An assistant noted the number of eye-piece divisions corresponding to each corpuscle as the measurements were made, and the mean was obtained in each case by adding together all the values and dividing by the number of corpuscles measured. Of course, the number of eye-piece divisions found only required to be multiplied by two to convert it into decimals of an inch. I endeavoured at first to make these measurements with a dry Powell and Lealand's $\frac{1}{50}$th of an inch, with the draw-tube so adjusted that each division of the eye-piece micrometer should equal one hundred thousandth of an inch, but I found the outline of the corpuscles, with this power, was not sharp enough to permit me to measure them as exactly as I wished, and I therefore gave the preference to the immersion objectives above mentioned.

Of course, in arranging for these measurements, the effect of the screw-collar adjustment of the objective on the magnifying power had to be taken into account. This was done in the following manner: some thin glass covers, not varying more than a thousandth of an inch from ·012 inch in thickness, were selected from a lot of so-called $\frac{1}{200}$th inch covers by means of a suitable lever of contact.* Some blood being placed under one of these covers, the best adjustment of the screw-collar for definition was found by trial. The stage-micrometer, which is an uncovered one, was then temporarily covered with another of the selected thin glasses, and being duly focussed upon the desired value was given to the divisions of the eye-piece micrometer by the adjustment of the draw-tube, after which the measurements were proceeded with, and the screw-collar was not turned again until they were completed.

The following tables present the several means deduced from these measurements in decimals of an inch, to which, for convenience, I have added the equivalent values in decimals of a millimeter. The number of corpuscles from which each mean was deduced is also given. The measurements made with the Hartnack No. 13 immersion are marked (H), the others were made with Powell and Lealand's immersion $\frac{1}{16}$th :

Measurements of Human Red Blood-corpuscles, from Five Individuals.

| | Number of Corpuscles Measured. | Mean Diameter. | |
|---|---|---|---|
| | | Decimal of an English Inch. | Decimal of a Millimeter. |
| 1. Dr. W., dry .. .. .. .. | 50 | ·000304 | ·00772 |
| 2. Do. moist .. .. .. .. | 49 | ·000292 | ·00742 |
| 3. Do. do. (H) .. .. .. | 50 | ·000300 | ·00762 |
| 4. Do. do. (H) .. .. .. | 50 | ·000289 | ·00734 |
| 5. Dr. McC., dry .. .. .. .. | 50 | ·000288 | ·00731 |
| 6. Do. do. .. .. .. .. | 50 | ·000294 | ·00747 |
| 7. Do. moist .. .. .. .. | 50 | ·000301 | ·00765 |
| 8. Mr. W., dry .. .. .. .. | 50 | ·000298 | ·00757 |
| 9. Do. do. (H) .. .. .. | 52 | ·000297 | ·00754 |
| 10. Mr. T. do. .. .. .. .. | 50 | ·000290 | ·00737 |
| 11. Do. do. (H) .. .. .. | 50 | ·000292 | ·00742 |
| 12. Mr. B. do. .. .. .. .. | 50 | ·000296 | ·00752 |
| 13. Do. do. (H) .. .. .. | 50 | ·000297 | ·00754 |

In each of these measurements of human blood the great majority of the corpuscles ranged from twelve to seventeen divisions of the eye-piece micrometer; that is, from ·00024 to ·00034 of an inch. Out of the whole number measured, six were as small as

* The instrument used was made by Stackpole and Brothers, of New York, after the pattern of the instrument designed by Mr. Ross, which is figured in 'Carpenter on the Microscope,' 4th edit., London, 1868, p. 203.

ten divisions, and one as large as eighteen divisions; large and small forms were not searched for, however. The size most frequently measured was fifteen divisions, or ·00030 of an inch.

Measurements of the Red Corpuscles of the Dog, from Five Individuals.

| | Number of Corpuscles Measured. | Mean Diameter. | |
|---|---|---|---|
| | | Decimal of an English Inch. | Decimal of a Millimeter. |
| 1. Mongrel terrier, dry .. .. .. | 50 | ·000292 | ·00740 |
| 2. Same animal do. .. .. .. | 54 | ·000299 | ·00759 |
| 3. Another mongrel terrier, dry (H) | 50 | ·000290 | ·00737 |
| 4. Same animal, moist (H) .. .. | 50 | ·000288 | ·00731 |
| 5. Scotch terrier do. (H) .. .. | 50 | ·000291 | ·00739 |
| 6. Same animal do. (H) .. .. | 50 | ·000289 | ·00734 |
| 7. Do. do. (H) .. .. | 49 | ·000287 | ·00729 |
| 8. Spitz dog, dry (H) .. .. | 52 | ·000285 | ·00724 |
| 9. Black-and-tan, moist (H) .. | 50 | ·000290 | ·00737 |

In each of these measurements of dogs' blood, precisely as in the case of those of human blood, the great majority of the corpuscles measured from twelve to seventeen divisions of the eye-piece micrometer (·00024 to ·00034 of an inch); out of the whole number measured, four were as small as ten divisions, but nine larger than seventeen were encountered. As with the human blood, however, large and small forms were not searched for, but all the perfectly formed corpuscles brought into view by the movement of the stage, were measured as they passed under the micrometer without selection, until the required number was recorded. The size most frequently measured was fifteen divisions, or ·00030 of an inch, precisely as in the case of human blood.

It will be observed that three of the above means for human blood, Nos. 1, 3, and 7, are a trifle larger than any of those of dogs' blood; and two of the latter, Nos. 7 and 8, are a trifle smaller than any of those for human blood. All the other means for the dog are within the range of the values found for human blood, and the majority of them are each identical, even to the last decimal place, with some one of those found for man.

I may, moreover, remind the reader in this place, that the variations between the mean diameter assigned to human blood by different observers are quite as great as the variations recorded by any of them between the blood of man and the dog, or even greater. Passing by the older measurements, some of which, as a matter of curiosity, I have given in a foot-note,* I may cite, besides

* A list of the more important of these older measurements will be found in the Mensiones Micrometricæ of R. Wagner ('Partium Elementarium Organorum quæ sunt in Homine atque Animalibus Mensiones Micrometricæ,' Erlangen, 1834). Most of these are included in the more complete list given by Louis Mandl

the measurements of Gulliver, ·00794 mm., Welcker, ·00774 mm., and Kolliker, ·00751 mm., which have been already quoted in this paper, the following values: Robin,* ·0073 mm.; Harting, ·0074 mm.; Valentin,† ·0071 mm.; and Austin Flint, junr.,‡ ·00726 mm. ($\frac{1}{3500}$ inch).

I have thus shown that we are not justified, either by the facts of the case or by the authorities supposed to favour the possibility of doing so, in attempting to distinguish between the blood of man and that of the dog by the measurement of their red corpuscles. Mr. Gulliver himself, indeed, appears to have come to a similar conclusion, not only with regard to the dog, but of other animals, for he tells us that the corpuscles of the quadrumana "differ but little from those of man, being only just appreciably, or sometimes not at all, smaller, both in the monkeys of the Old and New Continents," and that "in the seals, otters, and dogs, the corpuscles are about as large as in man."§ I myself have not made systematic measurements of the blood of any of these animals, and am therefore unable to speak as authoritatively with regard to them as I can about the dog. From Mr. Gulliver's detailed measurements, appended to Gerber's 'Elements,' however, I am led to believe that there are several other animals whose blood, even in the fresh state, could not be distinguished by the dimensions of the red corpuscles from that of man. Among the domestic animals I may especially mention the rabbit and guinea-pig as belonging to this category. To these, besides most of both the monkeys of the Old and New World, the seals and the otters, we may add the kangaroo, the capybara, the wombat, and the porpoise. In the case of all these animals we not merely find that the "average size," calculated in Mr. Gulliver's peculiar way, approximates dangerously to the average assigned to man, but the classic $\frac{1}{3200}$th of an inch figures among the "common sizes" recorded by Mr. Gulliver for each.

The foregoing remarks and measurements refer especially to the fresh blood of the animals mentioned, and to thin layers quickly

('Mémoire sur les Parties Microscopiques du Sang,' Paris, 1838, p. 10), from which I take the following, reducing the values which both Mandl and Wagner give in vulgar fractions of a Paris line to decimals of a millimeter: Leeuwenhoek (1673), ·00902; Ib. (1720), ·01327; Jurin (1717), ·00789; Tabor (1724), ·00723; Senac (1749), ·00820; Muys (1751), ·01128; Weiss (1760), ·01085; Della Torre (1763), ·00301; Blumenbach (1789), ·00789; Villar (1804), ·00564; Sprengel (1810), ·00902; Kater (1819), ·00677; Bauer and Home (1818), ·01504; Young (1819), ·00451; Rudolphi (1821), ·00902; Prevost and Dumas (1821), ·00705; Edwards (1826), ·00814; Hodgkin (1827), ·00902; Wollaston (1827), ·00525; Weber (1830), ·00525; Müller (1834), ·00525 to ·00902; Schultz (1836), ·00667 to ·00836; Wagner (1838), ·00645 to ·00752; Mandl himself gives ·00800.

* Charles Robin, "Note sur Quelques Points de l'Anatomie et de la Physiologie des Globules Rouge du Sang," 'Journal de la Physiologie,' tom. i. (1858), p. 283.

† I cite the estimates of Harting and Valentin from Welcker's paper, cited above, p. 258.

‡ 'The Physiology of Man,' vol. i., New York, 1866, p. 111.

§ 'Proceedings of the Zoological Society,' 1862, p. 96.

dried on glass, as is generally practised in making preparations of blood for permanent preservation. In such preparations the corpuscles have almost exactly the size they possess in the perfectly fresh blood. The great majority of Mr. Gulliver's measurements were made upon blood prepared by this method, and at the time he appears to have regarded the results as the equivalent of measurements made on perfectly fresh blood. "In some instances," he tells us, "there was certainly a slight enlargement in the dried corpuscles, as compared with those seen in their own serum immediately after they were taken from the animal. In the greater number of trials, however, the sizes of the wet and dry disks corresponded accurately." * Twenty years later he seems to have modified this opinion somewhat, for he states, "When the corpuscles of man and other mammalia were dried on glass, however quickly, they were usually just appreciably larger than in the liquor sanguinis." † Welcker also found that the mean dimensions obtained by measuring the corpuscles dried in a thin layer was apt to be rather greater than that obtained from the measurement of moist blood, and explains it by stating that "the very smallest, mere spherical corpuscles spread out a little in drying." He regards the difference, however, as so trifling, that he uses measurements of dried specimens indiscriminately with those of moist in obtaining his average. I myself am not satisfied that there is any constant difference, and find, on comparing the mean diameter of fifty corpuscles dry, with fifty moist, from the same individual, that sometimes the one, sometimes the other, is a trifle the largest. The dried corpuscles are very apt to be deformed, and often many of them are quite oval. If the long diameters of a number of such corpuscles are measured, the mean will be of course too great. I do not find it so if the measurement is confined, as it should be, to those corpuscles which have dried systematically and are quite circular. How is it, now, with regard to blood dried *en masse* when sprinkled upon weapons, clothing, wood, &c. Dr. Richardson admits in this case that a *slight* contraction takes place, but evidently regards it as too trifling to interfere with the diagnosis. Carl Schmidt, on the other hand, found the blood-corpuscles under such circumstances contracted to nearly one-half their original size; and gives ·0040 mm. as the mean diameter of the corpuscles of human blood thus prepared, while he assigns ·0077 mm. as the mean of human corpuscles dried in thin layers on glass.‡ It is not necessary for the purpose of the present paper to go into a detailed discussion of this subject, for no one will pretend that it can be any easier to make the diagnosis of such stains than it is in the case of moist blood or

* 'London and Edinburgh Philosophical Magazine,' vol. xvi. (1840), p. 25.
† 'Medical Times and Gazette,' August, 1862, p. 158.
‡ I quote from Fleming, *op. cit.*, p. 111.

of thin films dried on glass; and if it is impossible in the latter case to ascertain by the microscope that the sample submitted is human blood, it would be absurd to hope to do better in the former. I cannot, however, refrain from expressing my conviction that Carl Schmidt was quite as accurate in measuring his samples as Dr. Richardson in measuring his, and that the latter has underrated the variations in size which the dried corpuscles may present under various conditions.

I may also call attention, in this connection, to the effect of water on the diameter of the corpuscles. Mr. Gulliver has pointed out that if "water be mixed with blood, the disks immediately become much enlarged and spherical, quickly losing their colouring matter; and yet if the whole of this be thus removed, after a while the outlines of the disks, very faint indeed, may frequently be recognized, diminished considerably in diameter and apparently quite flat." *

In another place he relates that "some human corpuscles having an average diameter of $\frac{1}{3429}$th of an inch measured only $\frac{1}{4800}$th of an inch after the whole of their colouring matter had been separated in this manner." † Suppose, now, the case of blood mixed with water and afterwards dried, as, for example, in the case of an unsuccessful attempt to wash away the blood while fresh?

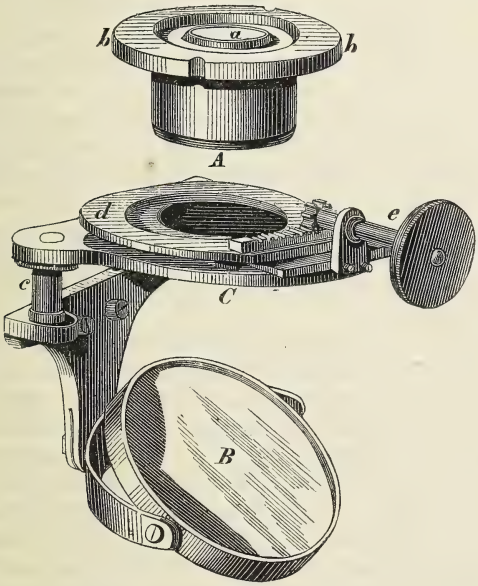
In conclusion, then, if the microscopist, summoned as a scientific expert to examine a suspected blood stain, should succeed in making out the corpuscles in such a way as to enable him to recognize them to be circular disks, and to measure them, and should then find their diameter comes within the limits possible for human blood, his duty, in the present state of our knowledge, is clear. He must, of course, in his evidence present the facts as actually observed, but it is not justifiable for him to stop here. He has no right to conclude his testimony without making it clearly understood, by both judge and jury, that blood from the dog and several other animals would give stains possessing the same properties, and that neither by the microscope, nor by any other means yet known to science, can the expert determine that a given stain is composed of human blood, and could not have been derived from any other source. This course is imperatively demanded of him by common honesty, without which scientific experts may become more dangerous to society than the very criminals they are called upon to convict.

* 'London and Edinburgh Philosophical Magazine,' vol. xvi. (1840), p. 106.
† Ibid., vol. xxi. (1842), p. 108.

## V.—*A New Illuminating Apparatus for the Microscope.*

By Professor E. Abbe, of Jena.

Two non-achromatic lenses, A *a* of the annexed figure, constitute the illuminating part of the arrangement or "condenser," to retain the term in common use. This portion of the apparatus presents the form of a stout object-glass, and has a thick plano-convex lens uppermost, of more than hemispherical form. This part of




the arrangement is set in a brass disk *b b*, by means of which it fits into the stage of the microscope from above. When once inserted into its place for use, it remains permanently fixed in the axis of the instrument. The plane upper surface *a* of the top lens lies in that case only a trifle lower than the plane of the brass disk *b b*, and as the latter, in its turn, is situated accurately in the plane of the stage, the general level remains unbroken, except by the narrow flat groove surrounding the surface of the lens. The focal length of the entire combination amounts to about 15 millimeters, and this cannot be materially increased—although it would in itself be of advantage—without involving dimensions of an inconvenient size. The superior focus lies only a couple of millimeters above the plane surface of the front lens. When, therefore, a slide having an object upon it is laid on the stage of the microscope, the object of its own accord is brought to lie almost in the focal point, by which means the mass of the slide itself, the thin interval beneath it being disregarded, forms the continuation of the upper lens of the condenser. When in special cases it becomes a matter of importance to avoid a loss of light as much as possible, that interval may be filled up by the application of a drop of water.

The condenser is not made achromatic for the reason that, for the effect contemplated, it would be altogether useless to seek to obtain a sharp image of the cloud or other source of light, as it is in like manner quite immaterial whether the image is formed precisely on a level with the object, or somewhat below or above it. On the other hand, attention has been directed to giving the condenser as large an angle of aperture as possible for the superior focus. It furnishes—especially with the design of producing luminous positive images—even rays which in a film of *water* are inclined at almost an angle of 60° to the axis, and which therefore, as the limiting angle of total reflexion is about 48°, could never be conveyed to the object out of an interval of air. If, however, rays of such an inclination have to be had recourse to for any purpose, of course the object must not be mounted in air, and the interval below the slide must in like manner not be occupied by air, but be filled up with water.

The rays of the primary source of light are conveyed to the condenser by a plane mirror B, which does not admit of a lateral movement, but turns only upon a fixed point in the axis of the instrument. The regulation of the illumination is effected by means of a special diaphragm-frame *c*, a few centimeters below the stage of the microscope between the plane mirror and the condenser, near the inferior focus of the latter. The effective portions detached from the entire surface of light available by means of the different diaphragms, act, with respect to the object, as though they were luminous surfaces very far removed, but of a suitable dimension. In order to be able to change the stops rapidly and with readiness the diaphragm-frame is made to turn bodily upon a pin at the side, *c*. It is thus rendered easily accessible, and while it can be readily turned outwards from under the stage of the microscope, a single movement of the hand will restore it to its correct central position. This frame is destined not only for the direct reception of the stops, but also for supporting on it a disk, *d*, provided with a rotating and a slipping motion. When the entire frame, fitted with a diaphragm in its aperture, has been brought into position below the stage of the microscope, it is by means of the motion given to the disk that all the modifications in the direction of the incident rays are brought about. A rotating and a sliding motion are given simultaneously by a projecting handle with a milled head, *e*, below the stage of the instrument. The action of this handle, when turned on its axis between the fingers, is to displace the disk with the entire diaphragm from the centre outwards, while the aperture in the diaphragm, which is thus thrown into an excentric position, admits of being rotated some 120° round the axis of the microscope, by using the handle as a lever to effect a horizontal rotation. The centre of the range of the lateral displacement that the rack admits of, or, in other words, the central position of the diaphragm in use, is indicated to the finger with accuracy by means of a spring-catch. The head of the rack and pinion serves also as a handle for moving the body of the diaphragm-frame forwards or backwards.

Firstly, with regard to the use of the apparatus for ordinary observation with transmitted light. For this purpose a series of circular disks is employed of about 30 millimeters in diameter, having circular apertures ranging from 1 to 7 millimeters in width. With a central position of the rack and pinion, when inserted in succession, they afford every desirable gradation of the ordinary direct illumination. It is true that there is not brought about thereby the continuous alteration of the aperture of the incident cone of rays, which is obtained by employing cylindrical stops provided with an up-and-down motion, yet as the free diameters of the apertures in these disks may differ as little as we please, and as they can be changed with great rapidity when one acquires the knack of using them, the present arrangement for the normal use of the microscope does not appear to stand in an unfavourable light when compared with Oberhäuser's arrangement of the stops, more especially as for central illumination the loss of light in the condenser is inappreciable. The transition from direct to oblique light at any inclination desired is simply effected by manipulating the handle already spoken of, without in any way touching the mirror, and we are thus enabled thereby not only to divert the radiating surface away from the axis as far as the limit of the free aperture of powerful object-glasses, but also at the same time—by a movement of the excentric diaphragm in azimuth—to allow the oblique light to fall in the direction of the object from any side. Hence with the illuminator now described the rotation of the microscope upon its vertical axis may be dispensed with for this purpose. This arrangement allows of the control of the oblique illumination being effected with great ease and certainty, in particular it presents the advantage of enabling us to pass from one modification to any other at our pleasure, without having to trouble ourselves again and again with the adjustment of the light. When the mirror is *once* so placed that the condenser gives its full illumination, this is kept up without change as long as the source of light remains unaltered.

On the other hand, it must be stated clearly that for *very* oblique light—as, for instance, when the matter in hand is to bring into play upon test-objects the utmost resolving power of an object-glass—the apparatus does *not* quite come up to the performance of a concave mirror. The reason thereof lies in part in this, that the rays being considerably inclined suffer a perceptible diminution of their intensity ; it is partly due to the circumstance of the unavoidable reflexions at the surfaces of the condenser, when the aperture of the diaphragm occupies an excentric position, producing secondary images underneath the object, which by the share they take in the formation of the image cannot but impair the effect. This defect could probably be got over only by means too complicated to be of practical use. But since with central illumination this defect is entirely absent (for in that case the images arising from reflexions lie in the axis), and with an incidence of moderately oblique light is imperceptible too, owing to the low intensity of the reflexions, the ordinary use of the microscope is not affected thereby in the least. In view, however, of the exceptional case spoken of above, it is expedient that the arrangement should be so disposed as to allow of the illuminator being replaced by the simple concave mirror without trouble. To ensure this, the entire diaphragm apparatus and the plane mirror belonging thereto form one connected piece, which by means of a groove may be slipped in and withdrawn under the stage of the microscope as quickly and readily as the condenser can be inserted into or taken out of the stage-plate.

When observations are to be made with polarized light, all that is required is that the Nicol's prism, set in a suitable disk of metal, should be inserted in the diaphragm-frame instead of a stop. The action is in every respect similar to that in the ordinary arrangement.

Finally, with respect to the observation of positive images of objects on a dark ground, nothing more is required for obtaining this kind of illumination, as explained above, beyond substituting in the diaphragm-frame, instead of one of the perforated stops, a narrow ring furnished with a central disk of about 12 millimeters in diameter, supported on thin spokes. If the angle of aperture of the object-glass employed is less than the angular diameter of the middle portion of the available surface of light below the object and thus rendered ineffective, no direct ray enters the microscope through the object, and the field remains perfectly dark. The image is produced exclusively by such rays as having traversed the object are thrown down thereon by reflexion at the limit between covering glass and air, and by those which on their path through the object are deviated in the direction of the axis, be it either by refraction or inflection. Either the one or the other has the preponderance as the nature of the object may be; but when an immersion lens is used, of course it is only the light that is deviated in the object that comes into play, inasmuch as the reflexion at the upper surface of the covering glass is almost entirely wanting. If the object be neither too transparent nor too opaque, and when other circumstances favour the effect in question, the pure positive images thus obtained may possess considerable brilliancy. In many cases—for instance, with diatoms mounted dry—the brilliancy is sufficient, with good ordinary daylight, to allow perfectly well of an amplification of from 500 to 600, especially if the interval between the upper lens of the condenser and the slide is occupied by a drop of water.

Should object-glasses having an angle of aperture sensibly exceeding 40° be employed with this kind of illumination, that angle will have to be reduced accordingly by stopping-off the zone of the periphery, partly because otherwise the field cannot be kept dark without reducing too much the effective surface of light in the condenser, and partly also because an excessive angle of divergence of the cone of rays forming the image interferes very materially with its perfection in the case of the great majority of objects. This stopping-off, which is indispensable when tolerably high powers are used, is effected by means of diaphragms with suitable aperture placed above the upper lens. The resolving power of the object-glass is of course reduced thereby to the degree corresponding to the free aperture that remains.

I must leave it an open question whether and how far this class of illumination—which essentially is that of Wenham's paraboloid—may prove of any importance for scientific investigations. With the arrangement above described it, however, is not at any rate in the least liable to the objections raised against it relatively to the perfection of the image, and which are represented by Harting as inherent in its principle. On the contrary, with good object-glasses even with a considerable amplification—supposing the objects to be suitable—not only are very clear and distinct images obtained, but they are also frequently very characteristic; and in consequence of their solid standing-out on the dark ground, may be said often to present decided advantages, at least as far as practical lectures are concerned.

With regard to the use of the apparatus with transmitted light, it may be further observed that when very low powers are employed, if the available source of light is but of limited extent, no uniform illumination of the object is arrived at, owing to the image of the source of light in the focus of the condenser not being large enough to cover the whole extent of the object that is visible. One in this case obtains a field illuminated equally and with sufficient brilliancy by covering the plane mirror with a piece of white paper. If lamplight is to be employed for the observations, what answers best is placing as large a condensing lens as possible in a direct line between the flame and mirror. The simplest method to follow is to interpose a large glass globe filled with water coloured of a moderate blue tint in order to transfer the luminosity of the smaller flame to the more extended surface of the mirror. This is attained when the point of the cone of light issuing from the lens or globe falls on the mirror and renders its entire surface luminous.—*Max Schultze's 'Archiv,'* vol. ix., p. 474.

# NEW BOOKS, WITH SHORT NOTICES.

*The Micrographic Dictionary. A Guide to the Examination and Investigation of the Structure and Nature of Microscopic Objects.* By J. W. Griffith, M.D., and Arthur Henfrey, F.R.S. Third Edition. Edited by J. W. Griffith, M.D., Professor Martin Duncan, M.B., F.R.S.; assisted by the Rev. M. J. Berkeley, M.A., F.R.S., and T. Rupert Jones, F.R.S., F.G.S. Parts XV., XVI., XVII., XVIII., XIX., XX., and XXI. Illustrated by forty-eight plates and eight hundred and twelve woodcuts. London: Van Voorst, 1875.

The microscopical world may well congratulate the author and with him the several editors who have been engaged upon it, on the completion of the third edition of the Micrographic Dictionary. It has been so many years going through the necessary metamorphosis, that we may almost be excused the supposition which we once held, that its completion was not to be witnessed in our days. However, Dr. Griffith has made an apology—that of ill-health—to the subscribers for the prolonged delay, and of course we are bound to accept it; the more so as having intended supervising the completion of the work himself alone, he has relinquished that intention, and has called in to his aid the assistance of Dr. Martin Duncan, F.R.S., who has regularly edited the monthly parts, till in December he issued a double portion, and thus concluded the volume. It will not be amiss if we quote a few of Dr. Griffith's remarks from the preface to this the last edition. He says: "In regard to the alterations made in this Third Edition, it will be noted that nearly one hundred pages of new matter have been added. The original articles have been revised according to modern researches and views, so as to represent, as far as space would permit, the present state of knowledge. When I state that the articles upon the Fungi were entrusted to the Rev. M. J. Berkeley, and those upon the Foraminifera to Professor Rupert Jones, the reader will surely feel confident that they have been carefully and faithfully elaborated. For some valuable notes on the Lichens I have to thank the Rev. W. A. Leighton. . . . . The plates have all been newly engraved upon copper, thus rendering the figures of the objects more sharply defined. Three new plates have been added, and several of the original plates have been rearranged and improved." This is the opinion of one who may in great measure be regarded as the only living author of the work. Still the view that will be held by the majority of readers will, we fear, be much less favourable to the book. We think that the opinion held by many will be something of this sort: "If a new edition were required, surely there was room for infinitely more change than has been made; if not, why impose so expensive a work upon those who already possess the second edition?" And we fear that the answer will not be quite satisfactory. Still we are far from agreeing with some of the reviewers of this work, and we consider that the new edition was justifiable, and that if the author had called in two or three other writers beside Dr. Duncan to aid him in his labours he would have succeeded perfectly in the result.

But it must not be on this account imagined that the book has been badly edited by Dr. Duncan. Quite the contrary. And if we take the majority of the foraminiferal, fungal, and lichenological articles as being fully done, we have but to consider how the rest of the work has been achieved, and to this we shall make answer by an examination in detail of several of the more important sections of the Dictionary. In doing this we do not purpose to go farther in the present notice than Parts XV. and XVI., leaving the remaining six portions to be dealt with in our next number. First, of the subject of Muscle. This is, in our opinion, very good; the structure, especially that relating to the position of the ultimate nerves, is very well given; and although some theories receive no notice, still all that are worthy of attention are given; even Mr. Shäfer's quite recent paper before the Royal Society receiving due consideration. Next comes Haëckel's new genera of Monera, which, though shortly given, have nevertheless as much space as is necessary to give the reader clear ideas on the subject. Navicellæ may be said nearly the same of; and references to Mr. Ray Lankester's and Professor Van Beneden's Memoirs are given. Nerves is the heading of another article of import. We confess that this rather disappoints us, not so much in reference to the structure of the cords, as in regard to the subject, partly touched on, of cerebellar structure. This is completely behind the time; and though the paper in Stricker's Handbook (the best paper in existence) is referred to, we should have had a more complete summary of the recently made out structure of so important an organ as the cerebrum. Under the head of Nucleus we have that of animals and plants described separately, and we do not see why the former is so much more fully and scientifically dealt with than the latter. Yet so it is, most undeniably. The subject of immersion objectives is one which we should have expected to have read an important contribution upon. But truly it is about the worst treated matter in this "part." It really might as well be left out of consideration completely, for the only remark that pertains to originality is that the immersion glass is more readily used than its predecessor, a fact we would certainly call in question. It is much to be regretted that some one like Mr. Wenham was not asked to contribute a short paper on this, at present, important question. Ovary and Ovum, too, are articles that we cannot say have been well executed; much might have been added, and some alteration made. Palmella is, of course, excellent. Parasite, too, is by no means badly done. Pedalion also is briefly but fairly given. This animal, the reader will remember, was first described in these pages, being the subject of a paper before the Royal Microscopical Society, by Dr. Hudson. Petalonema, too, is very good, though short. Photography, we fear, was entrusted to some one unfamiliar with the best workers at the subject. Else, how is it that no allusion is made to any of the splendid photographs, and of the many excellent suggestions made by Col. Dr. Woodward? This is to be regretted. Pitted Structure is well done, too, and it is well illustrated with woodcuts in the text. Plastids is a new paragraph, and though short is a very good one; it shows us what is the relation of the true Amœba to its congeners. Podura is too short and meagre, much that has been done on the group receiving no notice whatever. Lastly, Pollen and Polycistina are capital papers, and Polyzoa is by no means bad. With these we must terminate our present notice. In our next we shall have a word to say on the subject of the plates, and shall conclude our notice of the remainder of the volume.—*To be continued.*

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# PROGRESS OF MICROSCOPICAL SCIENCE.

*Cup-cells of the Conjunctiva.*—In a provisional communication to the 'Centralblatt' (No. 47, 1874), Dr. M. Reich gives the results of his observations on the cup-cells of the conjunctiva, which have previously only received notice at the hands of Steida and Waldeyer. It is well known that these writers, as well as Max Schultze, thought that these cells were unicellular glands, and that they secreted the mucus, since they are found on all mucous membranes. V. Thanhoffer, however, who has particularly examined those present on the villi of the intestines, holds that they are ordinary columnar cells from which the protoplasmic contents have escaped. Reich finds that they are common on the conjunctiva of old people, and in slight catarrhal states. They were remarkably abundant in the conjunctiva of a young man who had suffered from sharp conjunctivitis in consequence of a very prominent staphyloma corneæ.

*The Classification of the Animal Kingdom.*—A paper on this subject was read by Professor Huxley before the Linnean Society at a recent meeting (December 4, 1874), which, however, was not completely written out, so that as yet no exact report of it appears. It was of much interest, since it speculated on the relation of the Amphioxus, and it to a certain extent admitted Professor Haëckel's division of animals into *Protozoa* and *Metazoa.*

*Development of the Teeth in Reptilia and Batrachia.*—At the meeting of the Royal Society, on December 10, an important paper, by Mr. Charles Tomes, was read on this subject. He says that the descriptions given by Arnold and Goodsir of the development of the human teeth have been already demonstrated to be in material respects inaccurate as applied to man and other Mammalia; and the present paper shows that the accounts propounded by Professor Owen, of the process in Batrachia and Reptilia, which are practically an extension of the theories of Goodsir to these classes, are even more at variance with the facts of the case. There is in no Batrachian or Reptile any open groove or fissure ("primitive dental groove"); there are, at no period of development, free papillæ; consequently, the whole process of "encapsulation" has not any existence, but is purely hypothetical. From first to last the whole process of tooth development takes place in solid tissue, beneath an even and unbroken surface; with which, however, the young tooth sacs have a connection through a band of epithelial cells. The first process is a dipping down of a narrow process of the oral epithelium, the extremity of which, after it has penetrated in some, as the snake, to a great depth, becomes dilated, and is transformed into the enamel organ; and this is the case whether a recognizable coat of enamel is or is not to be found on the perfect tooth. Subsequently to the dipping in of the band of epithelium, and concomitantly with the dilatation of its end, a dentine pulp is formed opposite to it. This may constitute the entire tooth sac, which is then wholly cellular, as in the newt; or it may go on further to the formation of a connective-tissue tooth capsule. The external thin structureless coating of the teeth of Ophidia is derived from an unmistakable enamel organ, developed as above described; it is therefore enamel, and not cementum, as it is denominated by Professor Owen. The successional tooth sacs, very numerous in the snakes, are located in a sort of capsule: this character, peculiar to the Ophidia, and most marked in the lower jaw, is of obvious service during the extreme dilatation which the mouth undergoes, as is also the tortuosity of the process of epithelium, before it reaches the collection of tooth sacs. The epithelial band may be traced winding by the side of the older tooth sacs till it reaches the position of the youngest, where it ends in a cæcal extremity, to be transformed into the enamel organ next developed. In fine, the stages of open groove, free papillæ, and encapsulation of the same, have no existence whatever in Batrachia and Reptilia, their existence having been previously disproved in Mammalia.

*New Species of Rhizopods.*—In 'Silliman's American Journal' for January, 1875, there is an account of a recent paper by Professor Leidy on the above subject, which was read before the Philadelphia Academy of Natural Science. Professor Leidy says that among the amœboid forms noticed by him in the vicinity of Philadelphia, there was one especially remarkable for the comparatively enormous quantity of quartozse sand which it swallowed with its food. The animal might be viewed as a bag of sand! It is a sluggish creature, and when at rest appears as an opaque white spherical ball, ranging from $\frac{1}{8}$ to $\frac{3}{8}$ of a line in diameter. The animal moves slowly, first assuming an oval and then a clavate form. In the oval form one measured $\frac{3}{5}$ of a line long by $\frac{2}{5}$ of a line broad, and when it became clavate it was $\frac{2}{3}$ of a line long by $\frac{1}{8}$ of a line broad at the advanced thick end. Another, in the clavate form, measured $\frac{7}{8}$ of a line long by $\frac{1}{3}$ of a line wide at the thick end. The creature rolls or extends in advance, while it contracts behind. Unless under pressure it puts forth no pseudopods, and the granular entosarc usually follows closely on the limits of the extending ectosarc. Generally the animal drags after it a quantity of adherent dirt attached to a papillated or villous discoid projection of the body.

The contents of the animal, besides the granular matter and many globules of the entosarc, consist of diatoms, desmids, and confervæ, together with a larger proportion of angular particles of transparent and mostly colourless quartz. Treated with strong mineral acids so as to destroy all the soft parts, the animal leaves behind more than half its bulk of quartzose sand.

The species may be named *Amœba sabulosa*, and is probably a member of the genus *Pelomyxa*, of Dr. Greef.*

The animal was first found on the muddy bottom of a pond, on Dr. George Smith's place in Upper Darby, Delaware County, but has been found also in ponds in New Jersey.

When the animal was first noticed with its multitude of sand particles, it suggested the probability that it might pertain to a stage of life of *Difflugia*, and that by the fixation of the quartz particles in the exterior, the case of the latter would be formed. This is conjectural, and not confirmed by any observation.

A minute amœboid animal found on *Spirogyra* in a ditch at Cooper's Point, opposite Philadelphia, is of interesting character. The body is hemispherical, yellowish, and consists of a granular entosarc with a number of scattered and well-defined globules, besides a large contractile vesicle. From the body there extends a broad zone, which is colourless and so exceedingly delicate that it requires a power of 600 diameters to see it favourably. By this zone the animal glides over the surface. As delicate as it is, it evidently possesses a regular structure, though it was not resolved under the best powers of the microscope. The structure probably consists of globular granules of uniform size alternating with one another, so that the disk at times appears crossed by delicate lines, and at others as if finely and regularly punctated. The body of the animal measures from $\frac{1}{66}$ to $\frac{1}{50}$ of a line in diameter, and the zone is from $\frac{1}{333}$ to $\frac{1}{200}$ of a line wide. The species may be named *Amœba zonalis*.

The interesting researches of Professor Richard Greef, of Marburg, published in the second volume of Schultze's 'Archiv f. Mikrokopische Anatomie,' on Amœbæ living in the earth (*Ueber einige in der Erde lebende Amœben*, &c.), led me to look in similar positions for Rhizopods.

In the earth, about the roots of mosses growing in the crevices of the bricks of our city pavements, in damp places, besides finding several species of *Amœba*, together with abundance of the common wheel-animalcule, *Rotifer vulgaris*, I had the good fortune to discover a species of *Gromia*. I say good fortune, for it is with the utmost pleasure I have watched this curious creature for hours together. The genus was discovered and well described by Dujardin, from two species, one of which, *G. oviformis*, was found in the seas of France; the other, the *G. fluviatilis*, in the river Seine.

Imagine an animal, like one of our autumnal spiders, stationed at the centre of its well-spread net; imagine every thread of this net to be a living extension of the animal, elongating, branching, and becoming confluent so as to form a most intricate net; and imagine every thread to exhibit actively moving currents of a viscid liquid both outward and inward, carrying along particles of food and dirt, and you have some idea of the general character of a *Gromia*.

The *Gromia* of our pavements is a spherical cream-coloured body,

* 'Archiv f. Mik. Anat.,' x., 1873, 51.

about the $\frac{1}{16}$ of a line in diameter. When detached from its position and placed in water, in a few minutes it projects in all directions a most wonderful and intricate net. Along the threads of this net float minute Naviculæ from the neighbourhood, like boats in the current of a stream, until reaching the central mass they are there swallowed. Particles of dirt are also collected from all directions and are accumulated around the animal, and when the accumulation is sufficient to protect it, the web is withdrawn, and nothing apparently will again induce the animal to produce it.

From these observations we may suppose that the *Gromia terricola*, as I propose to name the species, during dry weather remains quiescent and concealed among accumulated dirt in the crevices of our pavements, but that in rains or wet weather the little creature puts forth its living net, which becomes so many avenues along which food is conveyed to the body. As the neighbourhood becomes dry, the net is withdrawn to await another rain. The animal with its extended net can cover an area of nearly half a line in diameter. The threads of the net are less than the $\frac{1}{30000}$th of an inch in diameter.

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## NOTES AND MEMORANDA.

**A New Local Microscopical Society.**—We learn from the energetic Honorary Secretary, Mr. John Hopkinson, jun., that a Natural History Society, which is intended to embrace also microscopical research, has just been established at Watford. We wish it every success, and we hope ere long to have to report its proceedings in these pages. The evening meetings of the Society will be held (by permission) in the rooms of the Watford Public Library, and during the summer months field meetings will also be held. It is proposed that the annual subscription be ten shillings, without entrance fee. The names of ladies and gentlemen willing to join the Society will be received by Dr. Brett, Watford House; by Mr. Arthur Cottam, St. John's Road, Watford; and by Mr. John Hopkinson, jun., Holly Bank, Watford.

**The Illumination of Difficult Test-objects—The Use of Blue Glass.**—Mr. J. E. Smith [U.S.A.] has recently described a method which strikes us as not particularly novel in principle, though it has some originality of detail. The following is the author's own account of the mode. My present method of using a single plate of blue glass will recommend itself by its perfect simplicity; in fact it is hardly any more trouble than the use of ordinary *diffused daylight.* My method is as follows: Select any ordinary small table, say 2 feet square, with square sides. Provide also a *pane* of blue glass. This pane of glass should be about 12 × 18 inches, although a smaller size would answer. Next provide a cleat of $\frac{1}{2}$-inch wood, about 12 inches long by 3 or 4 inches wide. Fasten this cleat, with two screws, to one end of the table, the top of cleat being flush with surface of table, leaving, however, space enough between the table and the cleat to insert the edge of the 12 × 18 pane, adjusting the screws to the thickness of the pane in such a manner that the pane will be held with sufficient security, and, at the same time, permit it to be instantly removed when required. This simple arrangement it will be seen admits of the glass being placed in position or removed therefrom at a moment's warning.

The table being now properly placed before an *open* window in such a manner that the pane of glass shall intercept the solar beam, and the microscope stand being placed near to and behind the same, the microscopist is ready to use monochromatic sunlight with quite the same facility as diffused daylight; while by the arrangement described the *entire stand is enveloped in an atmosphere of blue light.* After, however, observing for a few moments, owing to the movement of the sun, the entire table will require to be moved. This is scarcely the work of a moment, and need not in any way disturb the "resolution" of the most difficult and severe tests.

A little practice will be found of value in determining the amount of solar beam to be used; and this amount will vary with the strength of the sunlight, as also with the objective and eye-piece used. As a general rule the amount of light should be no greater than what the eye can comfortably bear, anything like a resplendent field should be avoided. I find it preferable to use the mirror; a slight touch of this will quickly produce the effects desired.

The obliquity of the illumination will of course depend (when using the low-angled objectives) on the objective used, but with modern wide-angled objectives excessive obliquity is to be avoided, and in no case should the illumination be so oblique as to badly distort the image. With Mr. Tolles' new 4 system $\frac{1}{6}$th and $\frac{1}{10}$th objectives a beam proceeding from the mirror to the object, making an angle of 40° or 50° with under surface of slide, is quite sufficient to show the most difficult tests known; although with the objectives named a much more oblique angle can be used without sensibly distorting the image.

With the arrangement above described any ordinarily good dry $\frac{1}{8}$th ought to show *A. pellucida's* transverse striæ strongly and with little trouble. A very commonplace $\frac{1}{16}$th of my own and which is greatly inferior to my new Tolles' will give me the transverse striæ on *A. pellucida* splendidly and without trouble.

All observations in the line of advanced research should be made with monochromatic light. In comparison, diffused daylight or the best artificial light sinks into insignificance.

Do not let the beauty of the "resolution" with the blue light result in attaching undue importance to an inferior objective; for a very ordinary glass under these favourable conditions will *seem* to perform finely. For the test of objectives use always artificial light and *balsam mounts,* without condenser. A modern wet $\frac{1}{10}$th should thus go readily and easily through the "Moller's Probbe Platte," showing strongly the striæ on Nos. 18, 19, and 20, either with "D" or $\frac{1}{4}$-inch solid eye-pieces. In short, a good modern immersion $\frac{1}{10}$th should show with lamplight *any known natural test, even when balsam-mounted.*

# CORRESPONDENCE.

## The Measurement of Angular Aperture.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *November* 28, 1874.

Sir,—By the same mail that takes this I send you three object-slides, to illustrate the mistaken measurement of the angle, taken in air, of Mr. Crisp's $\frac{1}{6}$-inch objective, and publicly promulgated through your Journal. You will readily ascertain that the diatoms mounted on the cover over the "slit" of each slide are part in the balsam and part dry mounted, while a portion of the slit is uncovered and *there* gives the very narrow angles, *under circumstances* described in either of two articles sent you the present week. Please bring these slides to the notice of those interested, in your discretion, and good pleasure.

Respectfully yours,

Robert B. Tolles.

*Note by Mr. Wenham.*

Having seen a proof of the above, I beg to append a few lines, as a formal reply is not called for. The subject has been thoroughly explained and need not be repeated *ad nauseam.* That discussion with Mr. Tolles is useless, is proved by his article, page 21 of this Journal for January, 1875, where, in order to show that I am wrong, he first tries the slit without and then with water contact, in the last case measuring not *the internal cone*, or immersion angle, but the increased emergent angle from the under surface of the slide. As for "the mistaken measurements of the angle taken in air" of 180° in the $\frac{1}{6}$th, surely he does not intend to cast aside his laurels, which he can wear so complacently—may they never wither!

The slit, if properly used, is an essential adaptation for measuring large angles accurately. Its use is to prevent rays that give false indications from entering the front lens behind the focal point, and even if this falls exactly on the under surface of an intervening plate of glass in water contact, it will still cut off stray rays within the glass, and give the true *air* angle, as one of final emergence.

## "The Society's Universal Screw."

*To the Editor of the 'Monthly Microscopical Journal.'*

*January* 5, 1875.

Sir,—As you have been so good as to quote my remarks on the above from 'Science Gossip,' you will permit me, perhaps, to add a few words on the subject.

The gauge of the Society's "universal screw" seems to have been considered as forming—(1) the *narrow* gauge adopted by Beck, and

Powell and Lealand—and (2) the *broad* gauge adopted by Ross and the other London opticians, although the discrepancy between the two is such that an object-glass having the latter does not fit any microscope having the former, unless the "universal screw" of either the microscope or of the object-glass has been *considerably* eased. It is perfectly evident that one or other of these gauges is not correct. If the word *universal* is to be taken literally, the *broad* gauge seems to be the correct one, as the object-glasses of Beck, and Powell and Lealand, fit Ross's microscopes, while the object-glasses of Ross do not fit the microscopes of the former unless the screws have been eased "*to order*"*!* On the other side I have heard repeatedly the assertion that the *narrow* gauge is the correct one. Could not the Society settle the discrepancy for the sake of completing the great boon they have conferred upon microscopists in introducing the "universal screw"?

Continental opticians seem to have, in many instances, a *really* "universal screw" for their object-glasses. I have lately received, from Paris, one of Hartnack's No. 7, and from Berlin, one of Bénèche's No. 7—*both these object-glasses have precisely the same gauge of screw*—the adapter made for one fitting exactly the other.

Your obedient servant,

A. DE SOUZA GUIMARAENS.

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# PROCEEDINGS OF SOCIETIES.

## ROYAL MICROSCOPICAL SOCIETY.

KING'S COLLEGE, *January* 6, 1875.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

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

The President reminded the Fellows that the ensuing meeting in February would be their anniversary, and that it would devolve upon them then to elect Officers and Council for the year following. In accordance with their usual custom a "house list" had been prepared of the names of those gentlemen who were nominated by the Council, and he called upon the Secretary to read it to the meeting.

The Secretary read the following list of nominations:

As *President*—H. C. Sorby, Esq., F.R.S., &c.

As *Vice-Presidents*—Dr. Braithwaite, Dr .Millar, Mr. C. Brooke, and Dr. Carpenter.

As *Treasurer*—Mr. J. W. Stephenson.

As *Hon. Secretaries*—Mr. H. J. Slack and Mr. Chas. Stewart.

As *Council*—Mr. F. H. Wenham, Mr. Frank Crisp, Mr. F. H. Ward, Mr. Ingpen, Mr. McIntire, Mr. Hy. Lee, Mr. Loy, Dr. Lawson, Dr. Matthews, Mr. Shadbolt, Mr. C. Tyler, Mr. C. F. White.

The rule relating to the proposal of other names by independent Fellows of the Society having been read, the President invited nominations by anyone present who desired to add any names to the list in accordance with the bye-law.

Dr. W. M. Ord read a paper entitled "Studies in the Natural History of the Common Urates," which he illustrated by drawings, diagrams on the black-board, preparations, and specimens shown under the microscope. Before commencing the paper, Dr. Ord asked for the indulgence of the meeting because of the want of finality in what he was about to lay before them, inasmuch as he had not yet been able to come to any conclusions which he could consider final. He had for this reason some hesitation in coming before them, but had yielded to his friend Mr. Stewart, who told him that as it was possible that he might not get to the end of the study during the whole of his life, he had better give them something as an instalment. He therefore offered the paper as an instalment, towards a solution of the inquiry as to the meaning of the different forms in which the urates were found in organic structure.

The President, in proposing the best thanks of the Society to Dr. Ord for his paper, observed that it opened up many important pathological considerations as bearing upon a number of conditions of disease, and it seemed to him to give promise of results in the future. It also brought out some curious relations connecting those peculiar conditions of matter known as colloid and crystalloid.

Mr. H. J. Slack believed he was right in thinking that Dr. Ord had in his paper brought much light to bear upon the relations connecting the molecular condition of matter with crystalline forms. The modern tendency of science was to break down all those distinctions which some time ago were thought to be definite, such as the distinctions between plants and animals. Some time ago he remembered that a rev. gentleman in that room told them that crystalline forms of inorganic bodies were all angular, and, on the contrary, those which were rounded or spherical were organic; but crystalline bodies readily assumed curved forms. Such facts as those mentioned by Dr. Ord illustrated the transition from the crystalline to colloid conditions. It would be recollected that he had, in some experiments of his own, shown how colloid silica could be mixed with crystalline bodies, and cause them to assume spiral forms. The readiness with which silica could be precipitated in the form of spherules or beads was also very remarkable.

A vote of thanks to Dr. Ord for his paper was unanimously carried.

A paper by Dr. Pigott, "On the Invisibility of Minute Refracting Bodies caused by Excess of Aperture, and upon the Development of Black Aperture Test-Bands and Diffraction Rings," was read by the Secretary.

Mr. Slack said he had brought down with him to the meeting a slide which he thought would very well illustrate some of Dr. Pigott's remarks. It contained an infinite number of minute spherules of silica, which could be seen well with a small-angled glass, by means of Mr. Wenham's dark-ground illuminator for high powers, and a deep eye-piece. With a large-angled glass many of these minute bodies were altogether invisible. He did not know how far they might be seen with so fine a glass as that which was shown at their late scientific evening by Messrs. Powell and Lealand, and under which, although magnifying 4000 diameters, he noticed that whilst the interspaces of a diatom appeared very much larger, its spherules seemed much smaller than when they were seen by a glass of inferior quality and less power. A similar effect was noticed with telescopes; the better the object-glass the smaller the spurious disks of stars appeared.

A vote of thanks to Dr. Pigott for his paper was carried unanimously.

Dr. Ord said that he had placed under one of the microscopes on the table a slide of crystals of urate of soda combined with phosphate of soda which he thought would be worth examination.

The President announced that as their next meeting would be their anniversary, it would be necessary for the Fellows to appoint two gentlemen to audit their accounts, and he therefore invited nominations for the purpose.

Mr. Jackson—proposed by Mr. Ward, and seconded by Mr. Ingpen—and Mr. E. W. Jones—proposed by Mr. Curties, and seconded by Mr. Moginie—were then duly elected Auditors in the usual way.

Some beautiful sections of a foraminifer (Alveolina), both transverse and longitudinal, mounted by Möller, were exhibited by the Assistant-Secretary.

Donations to the Library and Cabinet since December 2, 1874 :—

| | From |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal | *Society.* |
| Popular Science Review, No. 54 | *Editor.* |
| Bulletin de la Société Botanique de France | *Society.* |
| Sketch of the Natural History of the Diatomaceæ. By A. Meade Edwards, M.D., 1874 | *Author.* |
| One Slide of Silica Films | *H. J. Slack, Esq.* |

Henry Pocklington, Esq., was elected a Fellow, and Dr. J. J. Woodward an Honorary Fellow of the Society.

Walter W. Reeves,
*Assist.-Secretary.*

## Medical Microscopical Society.

Friday, December 18, 1874.—Jabez Hogg, Esq., President, in the chair.

Dr. Payne made a communication upon the presence of bacteria in disease.

He remarked that organized bodies were found not only externally, as in parasitic diseases of the skin, but internally, as in malignant pustule, where bacteria in large quantities were seen in the blood and in the discharges, forming the materies morbi of that affection. In the class of specific fevers and in pyæmia from wounds, similar organized structures form the materies morbi also. The presence of these bacteria is best studied in pyæmia.

The majority of these cases are from injury, the disease beginning locally, spreading throughout the body—a proposition not held by all—and hence the old theory that pus was the material carried in the blood current. This is disproved by two considerations: 1st, that the pus actually forms where the secondary deposit appears: 2nd, there is no proof of the absorption of pus.

The nature of the materies morbi and its method of transmission were then dwelt upon; with regard to the latter point, until Virchow's theory of thrombosis, two views were held; one, that pus got into the veins coming from the seat of injury and of suppuration; the other, that the pus began in the veins, the result of phlebitis. According to Virchow, clots were first formed in the veins; they softened, and the products of their disintegration were carried into the system. This explanation not satisfying all cases, especially those of puerperal pyæmia, the same septic matter was suggested as running in the lymphatics; and recent research would seem to show that these latter were really rather concerned in its transmission than the veins. Then followed Virchow's theory of embolism, the complement of that of thrombosis; but even a plug in a vessel failed to explain the general disorder of pyæmia, and hence attention was directed to the nature of the materies morbi itself.

With regard to the nature of this material, Dr. Payne pointed out that bacteria were found in cases of pyæmia, and generally of the specific fevers when properly searched for. The examination of the blood for these did not give constant results, and the morbid granules derived from white blood-corpuscles were not to be confounded with bacteria, nor in examining solid tissues post mortem were the bacteria of putrefaction to be mistaken for those of disease. These last were rod-shaped, those of disease spherical, and resisting the action of alcohol, ether, or caustic potash. Professor Heiberg of Norway found in pyæmia from wounds clots in the veins composed of granular amorphous material, that closer observation showed to be formed of bacteria. In the arteries going to secondary deposits, and in the areolæ around them, in pyæmia, similar masses of bacteria had been seen. These observations Dr. Payne had to some extent verified himself.

In the kidneys the tubes had been noticed plugged by the same material, this being the way of exit of the bacteria from the body, according to Heiberg. In pyæmic meningitis Dr. Payne had seen bacteria in the lymphatic spaces, not in the vessels.

The important and, to some extent, novel feature in Heiberg's observations, was the detection of bacteria in the solid organs at the actual seat of disease. This line of investigation was specially and urgently recommended to medical observers with the microscope.

To examine the bacteria, though visible with $\frac{1}{4}$-inch objective, 600 diameters should be employed at least, and in dealing with the solid organs a softening part should be chosen; a section made at once with a Valentine's knife, and the specimen be immersed in caustic potash and water, in which medium it is best examined, other reagents being applied in the usual way.

The President had observed in putrefying infusions first spherical bacteria appear, and then the oval, and thought that observers were apt to make too many species of these bodies.

## Quekett Microscopical Club.

Ordinary Meeting, November 27, 1874.—Dr. Matthews, F.R.M.S., President, in the chair.

Mr. Ingpen communicated some notes on "Personal Equation," with reference to microscopy. He first explained the use of the term in astronomy, as exemplified in transit observations, and in its more extended application to differences by a constant quantity between observers, short of actual defects of vision. The same causes affected microscopical observation, though they were not so well recognized as in astronomy. The principal points referred to were the following: I. *Mental* equation, as causing differences in interpretation, particularly with regard to test-objects. II. *Nervous* equation, as shown by varied sensibility to tremours, &c. III. *Colour.* Difficulty in estimating colour, as noted in Admiral Smyth's "Sidereal Chromatics."—Right and left eye often differ, in this respect.—Effect of yellow crystalline, referred to by Professor Liebreich in his lecture on "Turner and Mulready."—Difference in visibility of violet end of the spectrum, amounting in some cases to slight fluorescence.—Effect of red and yellow grounds in increasing definition in certain cases.—Effect of bluish mist caused by slight opacity of cornea or crystalline upon estimation of the correction of objectives.—Colour blindness often existing in a slight degree unsuspected, and difficult of detection. IV. *Focal equation.* Differences in effect of long and short sight upon cover correction, &c., also upon depth of focus, and power of resolving surface markings.—Differences in size of images formed by right and left eye, and consequent effect upon binocular vision.—Want of accommodation and pseudoscopic vision, &c. V. *Form.* General tendency of the eye to show ultimate particles circular.—Effect of square and triangular apertures.—Effect of astigmatism upon form, particularly of lines and dots, as seen in different directions.—Reference to Professor Liebreich's lecture.—Effects of diffraction upon points of light, &c.—General considerations of the effects of unnoticed differences of vision producing discrepancies often attributed to other causes.

Dr. Matthews added some remarks as to the effect of quinine in producing fluorescence, and extending vision towards the violet end of the spectrum, shortening the visibility of the red end in an equal degree.

Mr. J. G. Waller differed from Mr. Ingpen with reference to the later pictures of Mulready, which Professor Liebreich considered to show the effect of yellow crystalline; and gave reasons for thinking that the blueness of those pictures was due to their unfinished condition. He thought also that Turner's later pictures showed extravagant mannerism, which could be thrown aside at will.

The meeting terminated with the display of several beautiful preparations by some of the members.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

MARCH 1, 1875.

## I.—THE PRESIDENT'S ADDRESS.

*Delivered before the* Royal Microscopical Society, *February* 3, 1875.

Gentlemen,—In the last anniversary address a well-grounded hope was entertained and expressed that the unquestionable intentions of Mr. Layard, when First Commissioner of Works, &c., to afford to the Royal Microscopical Society some accommodation in the new buildings at Burlington House would ere long be carried out; but this hope has hitherto been entirely disappointed, nor is there at the present time any prospect of its fulfilment.

The war of the "Angle of aperture" has been waged during the past year with considerable vigour; but it appears somewhat to be regretted that a quasi-judicial calmness and courtesy should not at all times have been manifested in the discussion of a purely scientific question. If in the discussion of a question of optics an alleged ignorance of some of the first principles of that science be manifested, and thereby a feeling of irritation rather than one of compassion be evoked, surely the blame must rest more with individual temperament than with the nature of the question at issue.

On this subject there are two propositions so self-evident that they may be taken as axiomatic: first,—no ray can contribute to the visibility of an object that does not *proceed from* the object, and reach the eye; and secondly—that no ray proceeding from an object under a microscope can contribute to its visibility unless it enters the object-glass: consequently all rays that, proceeding from the object, fail to enter the object-glass, as well as all rays entering the object-glass, that do *not* come from the object, are useless so far as vision of the object is concerned, and may be *entirely* left out of consideration. Mr. Wenham is unquestionably right in stating that if an isosceles triangle be described, the base of which is ten times the measured diameter of the face of the front lens, and the altitude ten times the measured distance of the focal point from the same surface, the vertical angle of that triangle will correctly represent the *maximum available* angle of aperture: it is admitted that stray rays may perhaps enter the object-glass at a much greater angle, little less perhaps than 180°; but such are practically useless for any purpose of vision.

The past year has been marked by decided improvements in the construction of object-glasses. A remarkably fine $\frac{1}{8}$th has been made by Messrs. Powell and Lealand, with an avowed single front lens; but how far its principle of construction may agree with, or differ from, that previously enunciated by Mr. Wenham, cannot at present be stated, as its construction has not been made public. The image formed by this lens bears amplification by very deep eye-pieces exceedingly well—than which there can be no more certain a test of first-rate definition.

In the object-glasses constructed on Mr. Wenham's formula considerably increased flatness of field has been obtained by substituting two plano-convex lenses of proportionally less curvature for the single plano-convex posterior lens originally employed. The writer has in his possession a $\frac{1}{7}$th on the improved model, and can without hesitation affirm that it is superior in definition, and far superior in clearness and absence of fog or milkiness, to any other objective he possesses—these comprising a $\frac{1}{6}$th and $\frac{1}{8}$th by Andrew Ross, a $\frac{1}{4}$th and $\frac{1}{12}$th by Thomas Ross, and a $\frac{1}{16}$th and $\frac{1}{25}$th by Powell and Lealand; all considered first-rate by their respective makers. The definition of this lens can by no means be broken down by the sixth eye-piece of Ross, a much deeper ocular power than the writer would ever think of employing except as a test of definition.

As regards fog, this defect is very conspicuous in the $\frac{1}{6}$th by A. Ross. The construction of this object-glass is a single front lens followed by three cemented combinations. There are some reasons for surmising that fog is partly due to the multiplication of cemented contact surfaces; and if this be so, the general principles of analysis would lead to the conclusion that the amount of the defect in question would be in proportion to the square of the number of cemented surfaces, rather than in the simple ratio of that number. Thus this $\frac{1}{6}$th, which has four cemented surfaces, might be expected to present four times as much fog from that cause as the $\frac{1}{7}$th above mentioned, which has only two: this is thrown out rather as a suggestion for observation than as an assertion of a fact.

Your President cannot but regret that in the past year some intemperately conducted correspondence should have appeared in the pages of the 'Monthly Microscopical Journal.' This fact has much strengthened a conviction, long entertained, that whenever this Society may have the good fortune to find itself in a better financial position than that existing at the present time, it will greatly redound to its credit and *prestige* to follow the example of the older Royal Societies and publish its own Transactions, unmixed with extraneous matter, however valuable, or however cognate to the objects and pursuits of the Society.

The Society may be congratulated on the valuable contributions which it has received during the past year from our Transatlantic neighbours; amongst these may be mentioned the paper of Dr. H. D. Schmidt, of New Orleans, on the construction of the dark or double-bordered nerve-fibre, and the deductions in a second paper by the same author from the observations described in the former.

We are also indebted to the same author for his elaborate researches on the development of the human embryo, a paper containing much valuable information.

In a communication on pigment-particles, by Dr. Richardson, of the Pennsylvania Hospital, it appears to be rather heroic to assume that an experienced microscopist can mistake dirt on the slide or covering-glass for pigment-particles between these surfaces: the simple act of focussing up or down would probably be sufficient to determine the locality of the observed object.

The paper by Dr. Anthony on the suctorial organs of the blow-fly is interesting as giving a satisfactory explanation of the function of those well-known forked points that appear on the under surface of the framework of the suctorial tubes, which, without the appendages of soft tissue described by the author, would not be in harmony with the universal type of suctorial apparatus; and have always hitherto remained unintelligible to the writer.

In the same number of the Journal the paper by our esteemed Secretary, Mr. Slack, on beaded silica films artificially formed, is important for the insight it gives into the formation of the valves of diatoms. The well-known markings of some of these are closely imitated: and it appears not improbable that each particular form of beading may be due to minute differences of density, pressure, or molecular electrical state;—such being the normal conditions of the species respectively imitated.

The contributions by Dr. Braithwaite on bog-mosses are careful and elaborate, and well merit the attention of the botanist.

Nor must it be omitted to mention the further papers of Dr. Royston-Pigott on the optical characters of the microscope, on the important points of which time and space forbid an efficient detailed analysis.

A valuable communication on the development of the teeth in the Batrachia and Reptilia having been recently made to the Royal Society by Mr. C. S. Tomes, it may not be uninteresting to the Fellows of this Society to receive some notice of it. That the accounts of the development of the teeth propounded by Arnold and Goodsir was not in all respects correct, was suspected many years ago by some histologists. Nevertheless, their views have passed unchallenged, and are still to be found in most of our text-books. The chief features of the process as described by them were (i) the occurrence of an open furrow extending all round the jaw, "the primitive dental groove;" (ii) the appearance on the bottom of this groove of uncovered papillæ corresponding in number to the future teeth, "the dental papillæ;" (iii) the incapsulation of these free papillæ by the growing up of the sides of the groove, which finally arched over and met above them. Three stages were distinguished, viz. the papillary stage, the follicular stage, and the eruptive stage.

Professor Huxley some years ago demonstrated that the stage of free papillæ at no time existed in the frog and the mackerel, while more recently Kölliker has traced out the process as it occurs in man with great accuracy, and has shown that all the changes take place beneath an unbroken surface of epithelium. As respects Batrachia and Reptilia, the accounts given by Professor Owen, who gives detailed descriptions in all respects according with the theories of Goodsir, are still accepted. According to the researches of Mr. Charles S. Tomes, the following facts may be predicated of the development of the teeth of Batrachia and Reptilia.

There is at no time any primitive dental groove or fissure, but the whole process takes place in the midst of solid tissue, and at a distance from the surface; consequently there is no papillary stage, nor indeed anything that can fairly be called a papilla.

At the inner side of the teeth already in position, where Professor Owen describes a dental groove, is an area occupied by forming teeth-sacs, but these latter lie beneath an unbroken surface of epithelium. From the oral epithelium dips in an inflection, in section like a tubular gland; at the extremity of this, and from it, an enamel organ is developed, and a dentine germ appears beneath the cap of the enamel organ thus formed. The enamel organs of successional teeth are derived from the epithelial necks of their predecessors, and the depth to which these processes of epithelium penetrate is often great. The enamel organs have no stellate tissue, but consist mainly of the enamel cells with the reflected layer or external epithelium of the enamel organ feebly expressed.

The teeth of Ophidia consist of dentine and enamel, and have no cement, as is stated by Professor Owen: that the outer layer is enamel is conclusively proved by its development. Enamel organs are derived from an inflection of the oral epithelium, as in Lizards and Batrachia. Some peculiarities having relation to the great extensibility of a smaller mouth however exist. Thus the inflection of epithelium is tortuous, and the whole series of tooth-sacs are enclosed in an adventitious connective-tissue capsule; moreover, the teeth which have attained some little length become laid down, so as to be parallel with the jaw. The teeth of all Reptilia, which are attached by anchylosis, owe their fixation to a special development of new bone, removed and formed again with the change of each tooth, and not to the ossification of the tooth-capsule, as generally supposed. The several views advocated by Mr. Tomes were fully borne out by numerous microscopic sections of the parts to which his observations referred.

The obituary of the past year is unusually heavy, and comprises the names of several of the veterans of microscopical science.

Dr. Edwin Lankester was born April 23, 1814, at Melton, Suffolk. His school education, at Woodbridge, terminated at the early age of twelve, and it was through his own persistent zeal for knowledge that, in spite of serious difficulties, he attained an important position in the scientific world. According to an obituary notice in the 'Lancet,' he narrowly escaped apprenticeship to a watchmaker, and was articled to Mr. Gissing, a surgeon. After leaving Mr. Gissing he acted as assistant to Mr. Spurgin, surgeon, of Saffron Walden, who was celebrated for stimulating the intelligence and promoting the studies of his pupils. While with this gentleman he acquired a strong taste for botany, which ever after remained a favourite pursuit. He became secretary of a local society of naturalists and curator of the museum. He was so highly esteemed in Saffron Walden that, as the 'Lancet' states, some friends offered a loan of 300*l.* to enable him to go through a course of medical study in London. From 1834 to 1837 he was a hard-working student at University College, where he won the Lindley silver medal, and was elected President of the College Medical Society. Having become a member of the Royal College of Surgeons, and a Licentiate of the Apothecaries' Company, he visited the Continent and graduated at Heidelberg in 1839. His first important public position was that of Lecturer on Materia Medica and Botany at the school adjoining St. George's Hospital, and in 1850 he was appointed Professor of Natural History in New ? College, London. In 1851 he received the degree of LL.D. from Amherst, U.S. In 1853 he lectured at the Grosvenor School of Medicine, and in 1858 became Superintendent of the Food Collection at South Kensington.

Dr. Lankester became a member of the Microscopical Society in May, 1842, was elected President in 1869, and delivered two Addresses published in the Society's Transactions.

In 1853, the 'Quarterly Journal of Microscopical Science,' published by Mr. Highley, and under the editorship of Dr. Lankester and Mr. Busk first appeared, partly as the organ of this Society, and partly as an independent Journal. This connection of the Society with Dr. Lankester lasted till 1868, when it was found desirable to obtain earlier means of publishing the Society's papers than could be afforded by a periodical issued at quarterly intervals, and the 'Monthly Microscopical Journal,' edited by Dr. Lawson, was set on foot by Mr. Hardwicke for that purpose.

Amongst Dr. Lankester's numerous publications may be mentioned, as most in connection with, or nearest allied to, microscopical pursuits, the article "Rotifera," in the 'Cyclopædia of Anatomy and Physiology,' a translation of Schleiden's 'Principles of Botany,' and one of Kuchenmeister's 'Animal Parasites.' In 1859 he published a popular work of great value in spreading a taste for microscopical pursuits, his well-known 'Half-hours with the Microscope;' and it may be mentioned to his honour that for many years, both by writing and lecturing, he occupied a foremost place amongst those social benefactors who have succeeded in causing the physical and descriptive sciences to be recognized as necessary branches of national education.

In 1862 Dr. Lankester was elected Coroner for Middlesex, an office for which he was well qualified, and the duties of which he discharged with a fearless regard for public interests. His genial manners, his readiness to assist others in the pursuit of knowledge, together with his zeal for sanitary and other measures of social amelioration, caused him to be highly appreciated by a wide circle of friends, by whom his death, at a comparatively early age, was sincerely lamented. It may be further mentioned that he acted as Secretary to the Ray Society for many years, and was elected a Fellow of the Royal Society in 1845.

His death occurred, after a debilitating illness, at Margate, October 30, 1874, and he was buried at Hampstead.

Dr. Lankester's zeal for science, and with ampler opportunities, is fully inherited by his son, Mr. E. Ray Lankester, now Professor of Natural History in the University of Oxford.

Mr. Henry Deane was born at Stratford, in Essex, on August 11, 1807. After a very elementary education, he was apprenticed at the age of eighteen to a chemist and druggist at Reading, and subsequently took service in the firm of John Bell and Co., in Oxford Street. In 1837 he commenced business as a chemist and druggist at Clapham, which occupation he continued to the time of his decease. Mr. Deane was best known in his connection with the Pharmaceutical Society, of which he became one of the first members in 1841. In 1844 he was appointed an examiner; he was for many years a member of the Council, and filled the offices of Vice-President in 1851-2 and 1852-3, and of President in 1853-4 and 1854-5.

The relations of Mr. Deane with this Society cannot probably be better expressed than in his own words, as contained in an autobiography which was published in the 'Pharmaceutical Journal' shortly after his decease:

"In 1840 the Microscopical Society was formed, and my friend Frederick Bell and I joined it on the foundation. I invested 10*l.* in a microscope, and began work investigating and mounting objects with great ardour. This instrument did not long please me, and I got the basis of a better and more complete one, with which, in 1845, I made the remarkable discovery of the existence of Xanthidia and Polythalamia in the grey chalk of Folkestone, a bed below the common white chalk. It is well known that the white chalk is chiefly made up of the débris of the shells of Foraminifera—a fact first brought to my notice by the late Dr. Pereira—and that the layers of flints, supposed with strong reason by Dr. J. S. Bowerbank to be the fossilized condition of ancient sponges, contained a great variety of the organisms called Xanthidia and Polythalamia. I looked carefully in the chalk itself for the Xanthidia without success, but found them freely distributed in some portions of the grey chalk in which flint in layers, like those of the white chalk, do not exist, but in which occur occasional masses of chert, showing abundant evidence of the structure of sponges without, so far as I could detect, any evidence of Xanthidia. The Polythalamia in this grey chalk were in a very remarkable condition, showing what appeared to be the investing membrane of the shells and the bodies in a truly fossilized but not silicified condition. The species seemed to be identical with those found in the white chalk. Another curious fact seemed to me to be brought out, namely, the mode of distribution of the silica in these two kinds of chalk. In the white chalk it chiefly exists in the form of layers of flints; in the other it is distributed in minute grains or crystals. I also discovered in a fucus (?) prepared as an article of diet in China or Japan, an abundance of a beautiful siliceous organism occasionally found in Ichaboe guano, and which, in a notice read to the Society, I named *Arachnoidiscus Japonicus*, forming the basis of a genus of great beauty, and deservedly popular amongst amateur observers with the microscope."

"The distribution and exchange of these and other objects of interest with members of the Society led to the most friendly intercourse with many excellent men, to whom I am greatly indebted for kindly feeling and for hints in the art of observing and mounting objects, thousands of which I have had intense pleasure in distributing. It is almost invidious to name one good friend without naming others, but it would be unjust not to mention Mr. Bowerbank's open weekly evening meetings at Islington, where anyone with a desire for knowledge was ever welcomed, with or without personal introduction. Frederick Bell and I went thus, were kindly welcomed, and had our first lesson in observing with a good microscope. So began what little skill I possess in the use of an instrument which for full thirty years has afforded me an inexhaustible source of elevating and unalloyed pleasure. My microscope led to a most friendly intercourse with the late Dr. Pereira, and some of the illustrations in the edition of his 'Materia Medica,' in progress and unfinished at his decease, were made from objects prepared and mounted by myself."

Mr. Deane died suddenly at Dover on April 4, 1874, where he had been detained a day or two by stress of weather, on his way to visit his son in Hungary. Walking from his hotel to the boat, he was arrested by a sudden pain in the region of the heart, and in a few minutes expired.

Mr. John Williams was born October 19, 1797, in London, and educated at the Charterhouse. At the age of twenty-two he became a schoolmaster, and held the mastership of the parochial schools of Spitalfields for several years. In 1822 he became a member of the Mathematical Society, of which he was afterwards Secretary. At this time he paid much attention to microscopical pursuits, in connection with Dr. Bowerbank, Mr. Page, and others. When the Mathematical Society was merged in the Royal Astronomical Society, he became Assistant-Secretary of the latter, a position which he held up to the time of his decease. On the formation of the Microscopical Society he held the same place in it, and served it with zeal and success until the period when the Royal Astronomical Society required the whole of his time, and he was reluctantly compelled to relinquish his position with the Microscopical. In token of respect he was recommended by the President and Council as an Honorary Fellow of the Royal Microscopical Society, and was unanimously elected in November, 1867.

He constructed more than one microscope out of odds and ends, which he put together with much skill and ingenuity. His earliest instrument was furnished with globule objectives, made by fusing small pieces of glass in thin rings of platinum wire, a process very liable to failure, but by which he succeeded in producing many that were very perfect of their kind. His most elaborate microscope was made with cardboard tubes and brass screw adjustments. It was furnished with an ingenious stage movement, believed to have been suggested by Mr. Page. This instrument, when supplied with objectives by Ross and others, contrasted favourably in point of utility with constructions of a more costly description.

Amongst the papers he contributed to this Society will be found one on the Martin Microscope, and another on the occurrence of a Parasitic Rotifer in Volvox. He also devised a simple modification of the collecting stick. He possessed an interesting collection of ancient microscopes, which he was skilful in displaying at gatherings of the Society.

He was a man of considerable literary attainments in classical and oriental languages, and succeeded in becoming an eminent authority in Chinese, although he did not commence studying it until he was fifty years of age. In 1871 he published 'Observations on Comets by the Chinese.' He was a good mathematician, and other multifarious occupations never lessened his interest in the progress of astronomy.

His knowledge of antiquities was very considerable, and he possessed a remarkable collection of sulphur casts of coins and medals made with his own hands. One of his last acts was to attend the Oriental Conference held in London last year, to which he had intended to contribute a paper, but the fatigue of moving with the Astronomical Society from Somerset to Burlington House prevented its completion. He died of heart disease on December 3, 1874, in the seventy-eighth year of his age, surviving his wife, to whom he was deeply attached, after a union of fifty-two years, by only twenty-three days. He was buried at Highgate Cemetery.

Mr. Thomas William Burr was born July 10, 1821, and educated at a private school at Margate. In early youth he evinced a love of chemistry, which he studied for many years. At the age of sixteen he was articled to a solicitor, and at twenty-one he was admitted to practice. In 1845 he married Mary Anne, second daughter of the late Robert Greenwood, solicitor, formerly of Lancaster, who entered warmly into his scientific pursuits. At the age of twenty-five he took up the study of astronomy, which became from that time his favourite pursuit. In May, 1853, he was elected a Fellow of the Royal Astronomical Society, and in January, 1854, he became a member of the Microscopical Society. He also joined the Chemical Society, and soon after its formation the Quekett Club, of which he became a Vice-President. His connection with these Societies continued to the time of his death. As a council member of the Royal Astronomical and Royal Microscopical Societies he was highly esteemed, and rendered important services to the latter in gratuitously undertaking the legal business connected with obtaining its Royal Charter.

Mr. Burr was an ardent supporter of scientific education, which he promoted by many excellent lectures, and by papers in the 'Intellectual Observer,' and other periodicals. For several years he had an observatory at Highbury, in which was mounted one of the finest equatorial telescopes by the late Andrew Ross. He exerted himself usefully in promoting a taste for microscopical pursuits, and was always ready to help young students over their difficulties by placing his scientific knowledge and experience at their disposal. He died on May 22, 1874, at St. John's Park, Upper Holloway.

Frederick Henry Leaf, son of William Leaf, head of the well-known firm William Leaf and Sons, was one of the founders of the Old Change Microscopical Society, which gave a valuable stimulus to the cultivation of science by men of business in their leisure hours, and has acquired an honourable position amongst popular scientific bodies. He was elected in November, 1866, and died on October 23, 1874, at the early age of forty-seven.

R. W. Skeffington Lutwidge, M.A., was an acquaintance and cotemporary of your President at Cambridge. He was subsequently called to the Bar, but his private means relieved him of the necessity of practising his profession, and for many years he occupied the responsible position of a Commissioner in Lunacy. He died on May 28, 1873, in consequence of a blow on the forehead from a lunatic, whom he was officially visiting. He was well known as a dilettante in science, was in possession of excellent instruments for the observation of both the most minute and the most remote objects in nature, and took much pleasure in promoting scientific inquiry.

Sir Thomas Brograve Proctor-Beauchamp, of Langley Park, Norfolk, who died on October 7, 1874, aged sixty, held for some years a commission in the Royal Horse Guards. He was also a magistrate and deputy-lieutenant for that county. He succeeded to his father's title and estates in 1861.

John Armstrong Purefoy Colles was born in Dublin on September 15, 1834. Until he began his medical studies in the year 1850, he had only such advantages as could be offered by a home education; but at a very early age his mind was made up as to his profession, and at the age of sixteen he became the apprentice of his cousin, William Colles, M.B., a surgeon of well-known skill and benevolence in Dublin, under whom he began to work as a student of medicine in the Royal College of Surgeons, Ireland, and at Steevens' Hospital. Early in his studentship he obtained a botanical premium in money, which he devoted to the purchase of his first microscope, the constant companion of his after-years of Indian life.

In 1854 he took out his diploma as Licentiate of the Royal College of Surgeons, Ireland; and as he was then too young for the competitive examination for the Indian medical service, he obtained a commission as assistant-surgeon in the Tipperary Light Infantry. He remained in the regiment until it was disbanded in 1856, and after he left it, being still under age for India, he passed the time in reading for the degree of Doctor of Medicine, which he took out at St. Andrews in 1857, and he was appointed Demonstrator of Anatomy at the College of Surgeons in the winter of the same year. All this time he worked steadily for the Indian examination, and went in for the competitive examination for the Indian medical service held in London in January, 1858, and, to his own unbounded astonishment, he obtained the *first place* among forty candidates.

After ten years of service in India, he returned to Ireland on furlough, with the same undying energy and thirst for knowledge and self-improvement which had marked his course as a student, "the same desperate fellow for work he always was," one of his old friends remarked. Not content to rest or take holiday after ten years of constant work in a climate more or less trying to every

European constitution, he read diligently for a Fellowship in his old school, the College of Surgeons, which he obtained in 1869, and became also a Licentiate of the Dublin College of Physicians. He daily gave a portion of his time to one or other of the Dublin hospitals, and was constant in his attendance at the meetings of the surgical and pathological societies, besides carrying on a close pursuit of microscopic study.

He returned to India in December, 1870, and after two more years of active, happy, and useful life, during the latter portion of which he officiated as Professor of Descriptive and Surgical Anatomy at the Medical College of Calcutta, he died of malaria fever, the effect of the climate in Calcutta, at the age of thirty-eight years and five months, on February 8, 1873, at Dinapore.

Charles Morgan Topping was born May 23, 1800, in the parish of St. Bride's, Fleet Street, London. In 1840 he adopted the profession of preparing and mounting objects for the microscope, in which he acquired great skill, so that his slides were very generally sought for, and occupy an important place in most collections. Mr. Topping was remarkably successful in preparing minute injections; some of his preparations injected with chromate of lead are amongst the finest that have been produced. In recognition of his services to microscopical pursuits, he was elected an Associate of this Society on February 10, 1846. He died on September 5, 1874.

Finally, in concluding this, his second biennial tenure of the office, your President desires to express his most cordial thanks to the officers of the Society for the kind assistance and support he has on all occasions received from them; and to the Society generally for the courtesy and consideration that have ever been manifested towards him.

## II.—*Studies in the Natural History of the Urates.*

By W. M. Ord, M.B. Lond., Senior Assistant-Physician to St. Thomas's Hospital.

(*Read before the* Royal Microscopical Society, *January* 6, 1875.)

Plate XCIV.

I. *Urate of Soda.*—Uric acid, combined in various proportions with soda, and probably, as the experiments now to be related indicate, with other substances, presents itself to observation in three forms at least as a pathological phenomenon. Urate of soda is crystalline in tophaceous concretions and in cartilage, spherical in certain somewhat rare conditions of the urine, amorphous in cartilage, tophi, and urine.

In a slice of the cartilage from a gouty great-toe joint the urate occurs in long needles usually bent, and pointed at both ends. These are sometimes scattered, sometimes in parallel bundles, sometimes in radiating tufts, sometimes woven and matted irregularly. Dr. Garrod,* Dr. Roberts, Drs. Cornil and Ranvier, and others, have figured this form of deposit, and as it agrees with the form in which neutral urate is artificially deposited, also figured by Dr. Roberts,† Dr. Beale,‡ and Drs. Ultzmann and Hofmann,§ I do not propose here to put forward any fresh drawing of a so-well-recognized form.

Mixed with the crystals, masses and clouds of fine granular matter having a brownish tint by transmitted light may be observed. These agree with the amorphous form of urate of soda,

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EXPLANATION OF PLATE XCIV.

Fig. 1.—Urate of soda, deposited in liquor sodæ.
„ 4.—Biurate (?) of soda, in gelatin.
„ 5.—Uric acid in gelatin.
„ 6 *a.*—Urate of soda, with uric acid, in gelatin.
„ 6 *b.*—Ditto.
„ 6 *c.*—Ditto.
„ 9.—Uric acid and urate of soda, in water.
„ 17.—Uric acid and urate of soda in presence of concentrated solution of chloride of sodium.
„ 18.—Urate of soda and uric acid with concentrated solution of chloride of ammonium.
„ 19.—Deposit from solution of urate of ammonia after rapid evaporation—*a*, first day; *b*, second day.
„ 20.—Urate of soda in concentrated solution of chloride of sodium.
„ 21.—Urate of soda in concentrated solution of chloride of potassium.

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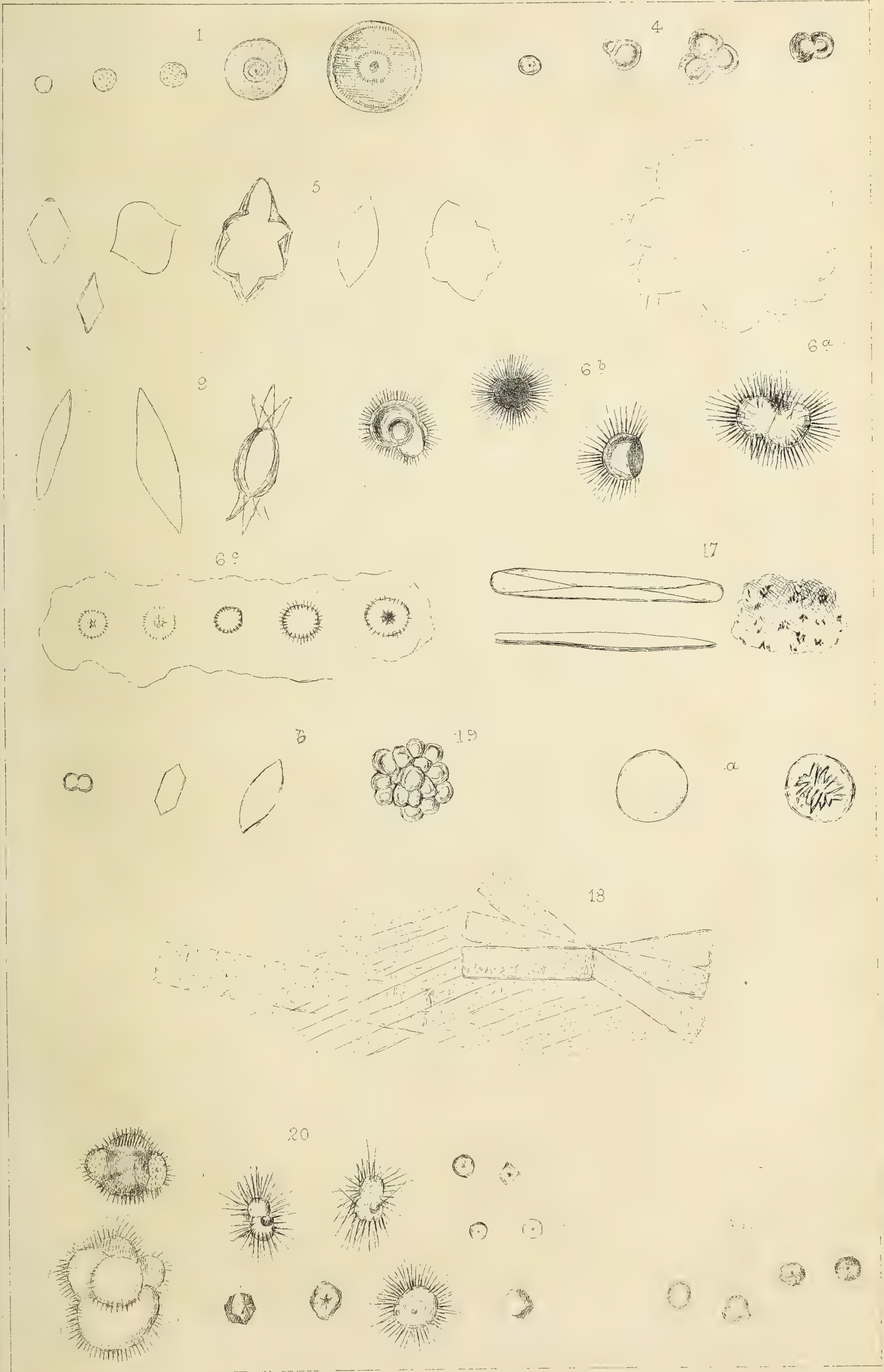
* 'Med. Chir. Trans.,' vol. xiii., 1848, p. 97.
† 'Urinary and Renal Diseases,' p. 71.
‡ 'Urinary Deposits,' pl. xviii., figs. 98 and 100.
§ 'Atlas der Physiologischen und Pathologischen Harnsedimente,' Taf. viii.




W. West & Co. lith.

Figures of Urates to illustrate Dr. Ord's Paper.

and are, like the crystals, slowly extracted by water. The amorphous form is well represented in the coloured figure No. 2, Taf. viii., in Ultzmann and Hofmann's Atlas.

A spherical form of what is generally supposed to be urate of soda has been represented by Dr. Golding Bird, Dr. Thudichum, Dr. Roberts, Dr. Beale, and Drs. Ultzmann and Hofmann. It consists of small, very perfectly-outlined spheres, which, as Dr. Beale describes them, are of great lustre, and which have usually spikelets of uric acid deposited on their surface. They are said to be found chiefly in the urine of children, and of persons in a feverish state (Dr. Beale); in the long-retained urine of children in fever (Dr. Roberts); in the urine of persons labouring under fever who were treated with carbonate of soda (Dr. Golding Bird); in the urine of children suffering from petechial typhus not treated by carbonate of soda, from measles, and from scarlatina (Dr. Thudichum). Drs. Ultzmann and Hofmann give, besides these, figures of large, somewhat irregular crystalline dumb-bells from the urine of a child suffering from intestinal catarrh.

Dr. Thudichum * states that if a solution of urate of soda be allowed to evaporate spontaneously, the salt is deposited in simple spherical masses and granules. Dr. Beale † gives twice a drawing of large spheres of urate of soda with halo of fine radiating needles, obtained by concentrating healthy urine; and also a drawing of some very large spheres from a case of remittent fever (sent by Dr. Kennion).

It may be assumed that the needles are to be regarded as crystals, though their appearance of flexibility, their remarkable tenuity, and the absence from them of indications of angularity of section are departures from the typical qualities of the crystal. They are to my mind crystals with definite colloidal affinities.

Assuming that the needles are crystals, they occur where, *primâ facie*, crystals ought not to be looked for. Crystallizable matters of low solubility deposited in a colloid such as the substance of cartilage, deposited moreover at the comparatively high temperature of the body, might, according to our experience, be expected to affect a spheroidal form. The departure from the rule is so startling as to make it probable that when the conditions are fully known much addition to our knowledge of the working of the rule may be hoped for.

Conversely, the same substance which forms crystals in the presence of colloids, forms spheres where the influence of colloids is less manifest or is absent. Here again extended observation of conditions is necessary.

I will now relate a series of experiments affecting the habits

* 'Pathology of the Urine,' p. 91.

† 'Kidney Diseases, Urinary Deposits,' &c., 1868.

of urate of soda under various conditions, and endeavour to draw therefrom inferences in explanation of the aberrations above noted.

1. Uric acid from the urine of the boa, white in colour, crystallized in long rectangular plates, was dissolved with the aid of heat in liquor sodæ (Br. Ph.). The acid was added in small quantities until no more was dissolved. The liquid was filtered and left to crystallize. The urate was deposited entirely in spheres. The spheres were sharply outlined, brownish, very regular; in diameter from $\frac{1}{800}$ inch downwards; some were clear in their interior, some granular; sometimes marked with concentric zones of lighter and darker colour; sometimes gathered in conglobate masses.

Fig. 1. Smallest; clear; $\frac{1}{2000}$ inch.
Next, granular, $\frac{1}{2000}$ to $\frac{1}{1000}$ inch.
Larger, zonate, $\frac{1}{1000}$ to $\frac{1}{800}$ inch or more.

These forms did not affect polarized light.

2. The precipitate from the above was collected on a filter, washed with cold water, and afterwards dissolved in boiling water. The solution was neutral. On cooling, long, curved, pointed needles were deposited. They were partly matted, were partly in not very regular radiating masses, and resembled very closely the needles of gouty deposit in cartilage.

3. A weak solution of gelatin, which formed only a very thin scarcely coherent jelly on cooling, was prepared. It was clarified by albumen, but not further purified. In this a portion of the precipitate of No. 2 was boiled, the quantity of precipitate being in excess of the solvent powers of the liquid. The liquor was gradually cooled to 40° Fahr., and then formed a soft jelly. The urate was deposited in a fine molecular condition. The molecules were gathered into irregularly spherical masses, which were soft and friable on the least pressure. There were no firm, separable spheres, and there were no crystalline or other structural forms.

4. To a solution prepared and treated as in 3, acetic acid was cautiously added, before cooling, till a faint acid reaction was obtained. The next day there was a large deposit of brilliant, highly-refractile spheres with smooth surface, some homogeneous, some with faint, radiant, internal disturbance. These spheres were of about twice the diameter of blood-corpuscles, and were mixed with molecular deposit as in 3. See Fig. 4.

5. A similar solution treated with large excess of acetic acid. The forms were now decidedly the forms of uric acid as known to be deposited in gelatin. They included large halberds, diamonds, pointed ovals, and small rhombohedra. See Fig. 5.

The foregoing experiments tending to show that with acid urates spherical forms were assumed, further investigation was made in this direction. And with very interesting result,

6. Pure urate of soda and pure uric acid were added in equal quantities to a weak non-gelatinizing solution of gelatin. Mixture boiled and filtered. In the filtrate there appeared at the end of twenty-four hours (temp. 45°) forms as in No. 4. At the end of forty-eight hours, the temperature having meanwhile fallen to 40°, the spheres were found covered with a dense deposit of needles (Fig. 6). The precipitate included three groups of forms: *a*, large irregular masses, looking like black velvet; *b*, dark brown spheres, with a halo of densely-packed fine bristles; *c*, curious pale spheres, looking very much like pus-globules in partial decomposition. They were composed of a thin crust of tiny radiating rods and a cavity within containing a few granules not having any obvious regularity of arrangement. All these forms were imbedded in a gelatinous matrix distinctly defined from the original solution. This matrix may have been hardened gelatin, or a compound of urate or uric acid and gelatin, or a colloidal form of uric acid or urate. And the deposit of needles may have been either a further crystallization or a metamorphosis. The fact that where they could be seen the spheres round which the bristles were arranged were imperfect on one side and reduced to crescents is important. See Fig. 6, *b*.

7. The whole mixture of No. 6 was boiled for some time, but without perfect solution of the precipitate. It was filtered, and the solution was divided into two equal portions, *a* and *b*.

To *a*, a solution of 20 grains of chloride of sodium in half an ounce of distilled water was added. The next day (temp. 45°) there was a brownish granular deposit resolved under high powers of the microscope (300 diam.) into fine matted needles and molecular matter. To *b*, acetic acid in excess was added. Result, next day (temp. 45°), long slender wheatsheaves; circular radiant tufts of needles; large, very perfect rhombohedra; and very large foliaceous and pennate crystals.

8. To fresh urate, of alkaline reaction, uric acid added in decided excess. This, after boiling in distilled water and filtering, deposited at 45° dense tufts of needles. The solution was only faintly acid.

9. Neutral urate of soda with equal quantity of uric acid boiled in distilled water. Solution acid. Filtrate deposited delicate navicular crystals, single, in bunches and in stars.

10. To boiling solution of urate of soda with slight excess of uric acid an equal quantity of boiling distilled water added. Solution cooled by next day to 38°. A small deposit, chiefly on the surface, of thick navicular crystals with central hollow; no other forms.

11. Mixture in 10 boiled, evaporated to $\frac{1}{3}$, and then divided into two portions; one rendered alkaline by four drops of liq. sodæ, the other left acid. Cooled to 38°. The first day produced soft much conglobate spheres, in semi-gelatinous matrix, in both solu-

tions ; the spheres being larger in the alkaline solution. Both solutions were left in a temperature ranging between 50° and 60° for a week, and at the end of that time the whole deposit was found transformed into needles.

A parallel series of observations was next made with albumen.

12. To a clear filtered solution of urate of soda in boiling water, one-third bulk of egg albumen was added when the temperature had fallen to 120°. On complete cooling, tufts of the ordinary form of needle were deposited.

13. Slightly alkaline urate was digested in distilled water at 100° Fahr., and one-third bulk of egg albumen added. No crystalline or formed precipitate, but formless tracts of molecular matter.

14. Alkaline urate boiled in distilled water and filtered. Half bulk of egg albumen added to hot solution and the coagulated mixture cooled. Needles both in fluid and coagulum, with much molecular matter in coagulum.

15. The mixture in 14 was now boiled for some time, in order that the coagulum might be fully saturated with the urate solution. On cooling, the flakes of albumen were found to have absorbed most of the urate. They contained large brown tracts of molecular matter, with faint indications of the presence of very small delicate needles. The solution contained no needles.

16. The mixture was again heated, and a small excess of acetic acid added. Now there occurred granular deposit in the albumen, and a free formation of small thick rhombohedra on the surface of the flakes. On again heating and adding more acetic acid, rhombohedra and bright spheroids were formed.

The influence of chlorides was now examined. Dr. Bence Jones in the year 1844 showed that the presence of chloride of sodium in solution with urate of ammonia prevented the formation of crystals and determined the occurrence of the molecular urate; and that the solubility of urate of ammonia was doubled by the presence of chloride of sodium.

17. To a solution of urate with free uric acid as in 10, a strong, nearly saturated solution of chloride of sodium was added in equal bulk. On cooling, the liquid was found almost filled with a gelatinous precipitate, which did not subside. It had just the appearance of freshly precipitated gelatinous silica. Under the microscope the gelatinous matter was found in well-defined masses, quite distinct from the liquid; it had a mottling of bright points and obscurely indicated acicular crystals, which were often gathered into rounded groups. The crystals were irregularly interlaced, and gave rise to an appearance of polygonal cellular structure. Imbedded in the gelatinous matter were numerous lengthened navicellæ, and chisel-ended three-sided prisms. See Fig. 7.

18. A similar experiment to 17 was made, with the substitution

of chloride of ammonium for chloride of sodium. A light semi-gelatinous substance was at once perceived to be formed in the liquid. This was boiled for several minutes, but was not dissolved. On cooling, it was found to consist of a brownish matrix, richly moleculate, and having imbedded numerous long, flat, quadrangular plates like those of uric acid in sugary solutions. See Fig. 18.

19. To solution of alkaline urate of soda at 100° Fahr., a hot concentrated solution of chloride of ammonium was added. A white gelatinous precipitate fell at once. It was amorphous and molecular both when formed and after cooling.

20. To solution of alkaline urate at 100°, a hot concentrated solution of chloride of sodium was added. No precipitate occurred at the time; but next day a gelatinous mass was found filling two-thirds of the liquid. It contained (*a*) small refractile spheres, almost homogeneous, with a tiny central cavity; these were about as large as lymph-corpuscles, and were very like the spheres of carbonate of lime in gelatin; they did not affect polarized light; (*b*) large brown spheres variously aggregated; granular, not fibrous; not refractile; denser at the surface than at the centre; covered with radiating needles, just like cilia round an infusorium; these with the rest of the forms were imbedded in a gelatinous stuff which was apparently condensed among the cilia, so that the cilia corresponded to an outer sphere of partly condensed matter aggregated round the dark spheres; (*c*) rhombohedra, clear, thick, colourless, separate; with forms intermediate between them and the spheres (*a*). See Fig. 20.

21. To a solution of alkaline urate at 100°, a hot concentrated solution of chloride of potassium was added. A precipitate fell at once. It was, under the microscope, a mixture of semi-transparent, waxy-looking stuff, with bright granules and dark solid spheres in large coalesced masses. The spheres had central points (cavities) and indications of radial striation, but there were no needles. See Fig. 21.

22. A hot but not very strong solution of phosphate of soda and a hot alkaline solution of urate of soda being mixed, no precipitate occurred till the mixture was cooled. The whole mixture then became a firm jelly, which did not liquefy or subside after a portion had been scooped out, but remained with sharp edges after twenty-four hours. Under the microscope it consisted of soft compressible spheres and a structureless transparent gelatinous matrix.

23. A very strong solution of phosphate of soda was mixed with equal bulk of solution of urate of soda at 100°. A precipitate speedily appeared. At the end of four hours this was found to contain large, beautiful, homogeneous, yellow spheres, presenting a black cross on pale white ground in the polariscope. The next day only half the spheres remained as at first, the other half being

opaque and granular, or radiatingly fibrous, or expanded into tufts of fine needles. And at the end of a week the spheres were altogether replaced by tufts of needles.

## II. *Urate of Ammonia.*

1. Uric acid was digested in liq. ammoniæ at 150° for two hours; left for twenty-four hours at temperature of laboratory; then water being added the mixture was boiled, and filtered. The salt was deposited in tufts of fine needles.

2. The filtrate was evaporated somewhat rapidly to a small bulk. It was then discoloured, having a pinkish brown tint, and had exchanged its former alkaline for a very decided acid reaction. On cooling, it deposited dark brown spheres with strongly-marked radiating crystalline tendency within, and an outer layer of dense, homogeneous, non-crystalline character. They looked as though in a state of tension. On examining the deposit next day I found the spheres all broken up into delicate six-sided plates, rhombs, and diamonds, with masses of small, bright, coalescing spheres, mixed with small dumb-bells and octohedra of oxalate of lime. There was apparently a mixture of uric acid, acid urate, and perhaps neutral urate. See Fig. 19, *a*, *b*.

3. With albumen used in many ways I obtained urate of ammonia always in the finely molecular form.

4. Gelatin had the same effect.

5. A strong hot solution of chloride of ammonium being added to a hot solution of urate of ammonia, a large, soft, light precipitate was formed at once. It consisted of coarse granules which did not sink or run together, being held apart by a gelatinous deposit containing very fine needles irregularly arranged. The granules were spherular, dumb-bell-shaped and irregular. The deposit had not altered when examined after several days. The solution was faintly acid.

6. Hot strong solutions of urate of ammonia and chloride of sodium were mixed. The solution was still clear while hot, and was faintly acid. Next day a bulky gelatinous precipitate had fallen, and filled three-fourths of the fluid. The precipitate was composed of bright spheres of various sizes, and bright spheroidal granules imbedded in a clear gelatinous stuff. The spheres were in appearance a little denser than white blood-corpuscles, and were a little more refractile. They looked soft, were easily altered in shape without being broken up by compression, and appeared to consist of fine molecules. The next day they were all altered, being replaced by dense masses of tufts of needles, and small, bright, firm, homogeneous spheres.

This precipitate was thrown on a filter, and washed with distilled water. Under the process of washing it lost its gelatinous character and became a tough pasty mass. A portion of this was incinerated on platinum foil, and the fused residue was dissolved in distilled water. It did not yield the least trace of chlorine, but consisted apparently only of soda with a little carbonate. It occurred to me that chlorine might have been sublimed with ammonia, and I now took some of the precipitate formed by mixing hot solutions of urate of soda and chloride of sodium. This was thrown on a filter, and washed thrice. A portion of this was incinerated; the rest was then well washed and again incinerated.

The first incineration of the possible double soda salt yielded free indications of chlorine.

The salt was then compressed for some time between blotting paper, and was afterwards incinerated. There were again good indications of chlorine; so that the spheres were possibly a combination of urate of soda and chloride of sodium. But the combination if it really existed was destroyed by prolonged washing with distilled water; the salt left yielding no chlorine reaction.

As far as may be judged at the present stage of the inquiry, two forms at least must be added to the three forms of urate of soda already observed. And I am inclined to arrange the five forms thus obtained in the following order, according to their several degrees of departure from the colloidal or quasi-living state, to the crystalline or not-living state:

1. Gelatinous colloid.
2. Molecular urate.
3. Spherules of first kind, soft, and tending after a time to crystallize.
4. Needles.
5. Spheroids of second kind; composed, as I believe, of matter originally crystalline, but subdued by colloid crowd to colloid form.

i. The gelatinous colloid form has been observed both in solutions containing only urate with uric acid, and in solutions of urate and chlorides or phosphates. It appears to correspond with the gelatinous form in which uric acid is deposited from alkaline solutions after the addition of acids, and, like that gelatinous form, has an impulse to crystallization. The gelatinous form of uric acid is called a "hydrate" by Prout and others. I am disposed to call it simply the colloid form, leaving the question of any chemical distinction between a colloid and a crystalline form, such as would consist in hydration or non-hydration, an open question: though as it seems to me the alterations of molecular arrangement must be larger and more comprehensive than hydrations.

ii. The urate assumes the molecular form where as a crystalloid it should take the spherical. The molecules may be either small spheroids or small crystals. They show the mark of two several influences—the influence of a recognized colloid such as gelatin in one case, and in the other the influence of the colloidal forms of their own substance, from which they are departing in different degrees towards crystallinity.

iii. The large soft spherules, breaking up after a time into needles, appear to be magnified spherical molecules. They occur under two conditions—in acid urates, of soda and ammonia, and in the combinations of hot saturated solutions of urates with chlorides or phosphates of alkalies. In both cases they are transitional between the gelatinous colloid and the needle. In the second case it is open to considerable doubt whether they are merely urates having their molecular arrangement altered by the presence of strong saline solutions, or whether they are urates in combination with chlorides or phosphates. The medium of solution being the same in both cases, I incline to the latter view. But this part of the inquiry, full of suggestion and analogy, requires a great deal of experimental testing. It may be noted that these spheres closely resemble Dr. Garrod's drawings of urate of soda from pigeon's urine (Med. Chir. Tr., 1848).

iv. The needle, though a crystalline form, is not by any means the true or perfect crystalline form of urate of soda. The true form is a short six-sided prism. The needle of urate of soda occurs where uric acid would be found in spheres, and urate of ammonia in molecules. But it also occurs where uric acid would be in crystals—that is to say where no colloid save colloidal modifications of itself exists.

In the cartilage the long, bent, rounded, pointed needle certainly recalls the rhombohedron of uric acid in urine, and may be fairly supposed to represent some corresponding change to the change from flat rectangular plate to rhombohedron with rounded angles.

But the influence of alkali in altering the behaviour of uric acid has yet to be understood. First, what do the light spheres of urate in strong liq. sodæ mean? Do they mean that a strong solution of a crystalloid exerts some of the power of a colloid upon crystalline polarity? Or do they mean that the liquor sodæ, being able in excess and with the aid of heat to break up uric acid, exercises some of the molecule-disturbing power possessed by colloids beyond and above the influence of its density? Second, why are the needles formed in weaker alkaline solutions? Further observation is needed to explain this; but it is not unlikely that these needles are also the outcome of transient sphere formations; and after crystallization the alkali may hold the uric acid in a crystalline state.

v. The bright compact spheres formed by acid urate in gelatin approach closely to the urinary forms recorded by several observers. They are in appearance like the spheres of crystalloids such as carbonate of lime, sulphate of baryta, iodide of mercury, and oxalate of copper, all of which when small are bright and homogeneous.

Acid urates therefore assume shapes indicating that their form and polarities revert towards uric acid. In particular where uric acid is used in large excess of soda or ammonia the crystals deposited from watery solutions are always navicular plates of great beauty and delicacy, and sometimes of great size, making the fluid in which they are suspended sparkle with bright flakes.

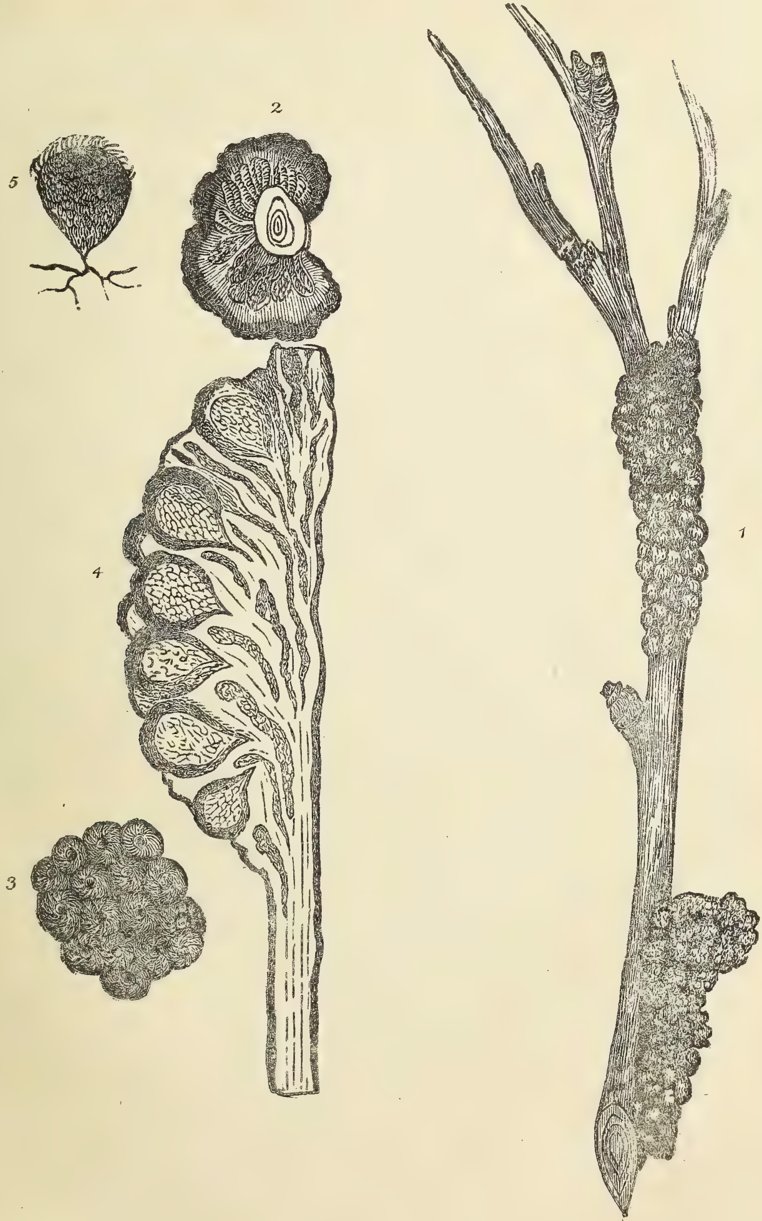
## III.—*Certain Fungi Parasitic on Plants.* By Thomas Taylor, Microscopist of the United States' Department of Agriculture, Washington, D.C., U.S.A.

Plates XCV., XCVI., and XCVII.

### (1) Black-Knot.

Entomologists and botanists are now generally agreed that the black-knot of cherry and plum trees is produced by a fungus, but they have failed thus far to define sufficiently its internal and external structure. Schweinitz, the American botanist, who died in 1834, seems to have been the first who suggested its fungoid origin, and he named it *Sphæria morbosa.* During the past year the Hon. M. P. Wilder, of Boston, forwarded a specimen of the black-knot on a plum-tree branch, which I used as the basis of my experiments. The ordinary methods of investigating the black-knot by placing opaque sections of it under the microscope gave results so unsatisfactory that I determined to employ my usual methods of rendering such bodies translucent and soft, by means of acids and alkalies. In this way the higher powers of the microscope may be brought to bear effectively on the fungus, its mycelium, flocci, and spores, if present. The immediate use of strong mineral acids and caustic alkalies on suspected fungoid bodies has this advantage, that these prevent the possibility of the production of fungus growth by fermentation during the investigation. Portions of the black-knot were subjected to the action of nitro-muriatic acid (concentrated) during several days, after which the acids were neutralized with ammonia. This process rendered the outer surface of the sphæria translucent, and I then examined them under low and high powers of the microscope. Portions were also well washed in pure water, to free them from acid, and then submitted to the action of caustic potash. For the purpose of distinguishing colourless spores, mycelium, starch, and cellulose from one another, a solution of iodine, containing a small portion of nitric acid, was applied in excess to the specimens, and afterwards washed in pure alcohol. This solution stains starch blue, fungoid cells a light amber, infusorial matter a dark amber, while cellulose remains generally colourless. When viewed under a power of 100 to 600 diameters, these substances are clearly distinguished from one another. I also submitted the dry leaves of the twigs to the same processes and examined their transparent cellulose forms carefully, and observed some indications of fungoid forms within the cells of the leaves.

Fig. 1, Plate XCV., represents the general appearance of the black-knot of the plum; 2, a cross section; 3, an enlarged view of it, showing indentations on the external upper surface of the sphæria; 4, a longitudinal section of the black-knot and branch




Black-knot.
(Sphæria morbosa, *Schweinitz*)

W. West & Co. imp.


Black-knot.
(Sphæria morbosa. *Schweinitz*)

W. West & Co imp.

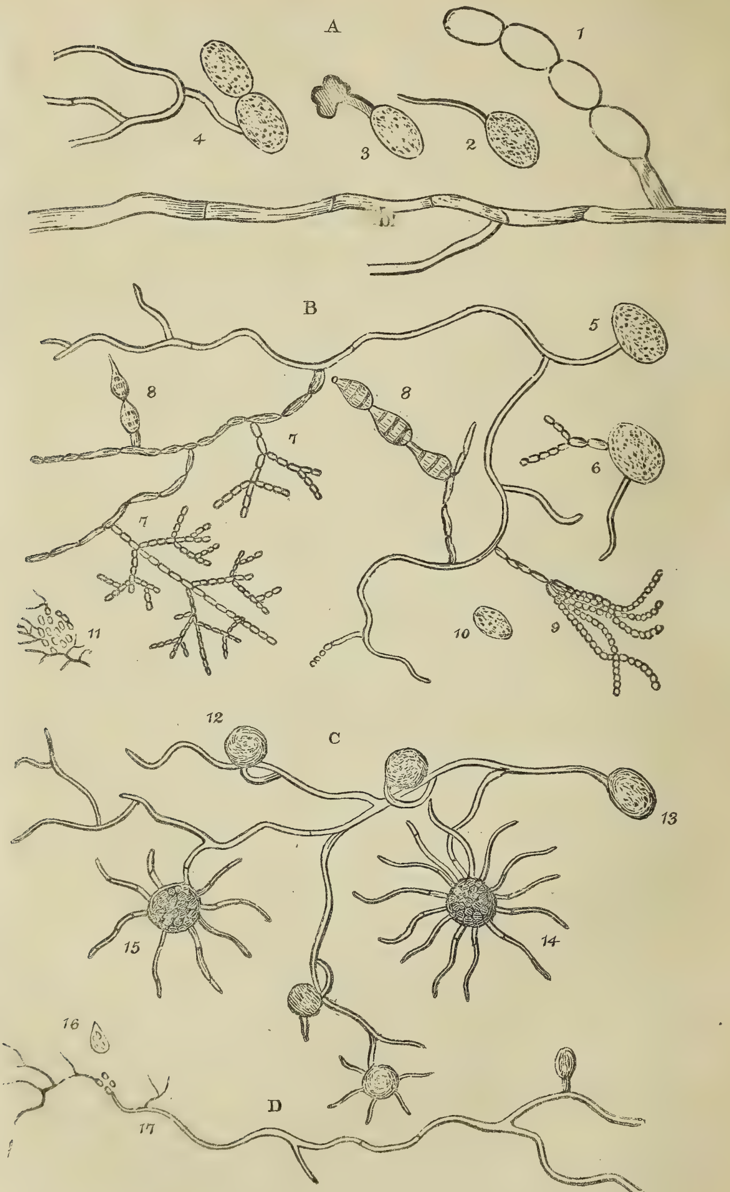
of a plum tree, in which sections of the sphæria (*perithecia*) are exhibited highly magnified, while the woody fibre is represented of the natural size, as seen by the naked eye; in its advanced stages its woody structure appears as if it had been broken up into shreds and its interstices partly filled with a very porous bark-like substance, which is again interspersed with a very fine transparent thread-like mycelium; 5, a typical representation of a perithecium; 6 *a*, Plate XCVI., a very highly magnified translucent view of a perithecium partly covered with branches (flocci). Its surface is cellular, as exhibited, and of a dark amber colour. The flocci is jointed and branched, and resembles very much black-orange mycelium. I have found floating in the gum solution, when examining the respective parts of black-knot, several small forms resembling *Cladosporium*, having very short stalks, and of the colour of the perithecium and its flocci. This led me to renew my experiments. I placed on slips of glass crushed portions of perithecia and their flocci, and exposed them within glass jars containing about an ounce of water, securing the contents with ground stoppers. I then subjected them to a temperature of about 75° Fahr. for a period of fifteen days. After many trials of this character I obtained several examples of Cladosporium forms growing on the flocci, as suspected by Peck. I endeavoured to secure the specimens under glass with a gum solution in the usual way, but the moment the fungus was wetted with the gum water, the Cladosporium forms separated from the flocci. I have since frequently endeavoured to restore all the conditions necessary to produce the visible combination of the flocci with these forms, but have thus far failed, although it is common to find them floating on the glass slides. When a branch, such as that represented by Fig. 1, Plate XCV., is bleached by the alternate action of nitric acid and chlorinated soda, the parts covered with the fungus are freed from it: I find it exists mostly on the surface of the excrescences. A microscopical examination of the mass will show that it is almost wholly composed of woody tissue, vascular bundles, &c., and seems void of earthy matters, and is very delicate in texture. That portion of the branch which appears unaffected by the fungus remains as firm as if it had not been treated with chemicals. It would seem that this fungus produces an irritation on the surface of the branches, from which masses of perfectly formed cellular structure then burst out, preventing the growth of consolidated wood. It is generally supposed that when black-knot encircles a branch it dies from compression, whereas black-knot, so called, consists of exudations of organized pure cellulose, on the surface of which black-knot fungus principally grows. The substance of the exudation is closely approximated to starch and sugar. This may account for its having been so frequently made the abode of insect life, and has led to the belief that it was produced by them.

In answer to a communication of mine, Professor C. H. Peck, botanist, of Albany, New York, informs me that he has found sacs (asci) filled with spores within the perithecia of black-knot, and has furnished me with a sketch of the sac as seen by him (see U, Plate XCVI.). T represents a highly magnified view of the true spore of this form of *Sphæria*. After several fruitless dissections, in search of the sporidia, I found them ultimately floating among the crushed cells on the glass slide. They are almost colourless, with a slight tinge of green, and are smaller than the spores of the flocci. Although I have not seen these spores in sacs, I think it likely that the drawing U is correct, and I can testify to the accuracy of that of the spore T.

In the growth of black-knot the perithecia crowd each other so much, that their original shape is changed in a variety of ways, but in the main I have frequently found well-defined forms, as represented.

Having recently received from a gentleman of New Jersey, Mr. Abram McMurtrie, some excellent specimens of black-knot taken from plum and cherry trees of different ages, I resumed my investigations of that disease with very satisfactory results. A portion of the fungus being removed from a specimen of the black-knot which had grown on a plum tree about seven years old, and being submitted to an examination by the microscope, at a very low power, exhibited forms of fruit (perithecia) as seen at 8, Plate XCVI. When viewed in section by a higher power, it appears as at 9; and in top view as at 10, showing an indentation in each perithecium. When they are imperfect they appear as at 11.

If a perfect specimen, as seen at 9 or 10, is submitted to the action of nitro-muriatic acid for about thirty minutes, a slight decomposition of the acid takes place, indicating that the resinous or oily matter of the perithecium becomes oxidized. These strong mineral acids have no destructive action on the organic structure of the perithecium, although they have the property of bleaching it in some degree, thus rendering it translucent, and making its cellular structure visible. If ammonia is added in drops to the specimens, after having been treated with acids, their albuminoids become pliable. This process is especially valuable when applied to matured and dry specimens; 6, Plate XCVI., represents a very highly magnified specimen of a perithecium, a part of which is in section and represents the internal arrangement of the asci and sporidia in them. From my recent experiments on black-knot I am now able to demonstrate its structure. If a perfect perithecium which has been treated with acid and ammonia, as previously described, is gently bruised on a microscopic glass slide, by any of the well-known

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Erysiphe Tuckeri.

modes, the asci containing the true sporidia will escape, and frequently the sporidia will be seen in profusion on the glass. I have counted as many as ten sporidia in one ascus. When the perithecium is very pliable, and the interior mass of asci well matured, it may be removed entirely by pressure, as represented at A. A power of about 600 diameters is necessary to see it properly.

An ascus measures about $\frac{1}{1000}$th of an inch in diameter, and is about seven times its diameter in length.

If an ascus is treated with an alcoholic solution of iodine, containing a few drops of nitric aci.., its nitrogenous matter becomes stained of a dark amber while the sporidia retain their natural colour. The asci will frequently exhibit, when treated with acids and alkalies, an expanded membrane of very delicate texture and quite transparent, as exhibited at B and C, Plate XCVI.

The true cause of this disease is unknown at present. My future investigations will be principally confined to its mode of propagation. Investigations of this character lose much of their value when they are confined to the microscope and laboratory. Districts affected with the dreaded pest should be visited, and the roots of the trees and their branches examined, that the investigator may become acquainted with all the stages of growth of the fungus, and thus ascertain how the disease is propagated.

### (2) The fungus Erysiphe Tuckeri.

On the 15th of May last, one of the foreign grape-vines of the experimental grapery of the Department of Agriculture was found to be affected with the fungus called *Oidium Tuckeri.* It first appeared on the leaves, then on the green branches, and finally on the fruit.

I determined to take advantage of its presence to make further investigations in reference to its habits. I secured on a glass slide a few of its Oidium spores, placed them in a clean glass jar containing a little water, excluded the atmosphere by a ground-glass stopper, and subjected the jar to a temperature of about 75° Fahr. during the investigations. On the second day the spores were examined, when it was found that many of them had germinated.

1, group A, Plate XCVII., represents the Oidium. I think that the spores in this case are thrown out from the peduncle * in the same manner as soap-bubbles from a pipe. I have never seen a case of an Oidium spore having a small spore attached to it as if in the act of reproducing a facsimile of itself, as is so frequently observed in the spores of the common yeast-plant (*Torula cerevisiæ*). The Oidium spores germinated and threw out branches as shown in the drawings 2, 3, and 4. The protruding branch of spore 2 differs in

* The stem or stalk that supports the flower and fruit of a plant.

form from that of 3. The branched state of 4 illustrates the changes which take place in 2. I have observed many spores germinating like 3, upon the functions of which I have been unable to decide. After exposure for a few days, more new forms of fungi appeared on the branches of the mycelium of the Oidium. (See 5 and its ramifications, group B.) Nos. 6 and 7 next appeared, followed by 8 and 9; 10 represents a highly magnified spore of *Penicillium glaucum*, 9; 11, the spores of 7 germinating, which resemble *Penicillium Armeniacum*, Berk. The flask-shaped spores, 8 (*Antennaria tenuis*, Ness.), are generally the last to appear. They belong to a genus of *Torulacei*, remarkable for their close resemblance to a Florence flask.*

My object was to ascertain what changes, if any, would take place during the germination of the spores. I therefore varied my experiments in numerous ways, and am satisfied that the forms 6, 7, 8, and 9 have no relation to the Oidium under experiment, but are distinct fermenting plants, living on and consuming the mycelium and spores of the Oidium, preventing the further healthy growth of the vine fungus. The facts observed have an important bearing on the cultivation of foreign grape-vines when grown in moist hot-houses; for since it has been shown that parasitic fungi are nourished by the spores and mycelium of the Oidium of the vine, and grow profusely on them, the vine itself will become affected by the growth of the fungi over its leaves, green branches, and fruit. I have frequently transferred to varnished glass slides the same class of spores direct from a leaf which had been kept unusually moist while growing. These will doubtless hasten the death of the plant on which they grow. The evidence is conclusive that when the flowers of sulphur have been applied early to mildewed vines, they have been saved, and that later applications have been unavailing. This may arise from the fact that the other forms of fungi, such as I have pointed out, may assist in the destructive work. These experiments have been repeated often under varied conditions, with an unvarying similarity of results. A slip of glass was varnished, and, when nearly dry, a vine-leaf covered with the Oidium was pressed on it, so that many of the spores adhered to the varnish. When the slip was introduced into a moist jar at the temperature mentioned, the spores adhering to the varnish germinated, as shown at B. When placed in an atmosphere containing turpentine, benzine, or carbolic acid, they failed to germinate, and the distorted forms of the Oidium were clearly seen, showing the destructive action of these substances on fungus germs.

I next placed the dust of roll sulphur on Oidium spores, and

* The 'Micrographic Dictionary' says of this genus that "no British representatives of this genus appear to have been recorded hitherto" (p. 29, vol. i., second edition, 1860).

also the dust of the flowers of sulphur on a second lot, each set being secured on glass slides, an inch and a half wide by six inches long.

These slides were subjected to moisture and heat, as before, in separate jars. After the usual exposure it was observed that the same fungus forms of group B appeared on the germinating Oidium.

These results were not expected, as it has been generally supposed that sulphur is a perfect preventive of fungoid growth. This led me to test the effectiveness of sulphur for that purpose. I placed in an eight-ounce jar four ounces of pure water, one ounce of green peach-leaves, and two ounces of the flowers of sulphur, and subjected the whole to a temperature of 75° to 80° Fahr. In three days fermentation commenced in full force, giving off a strong odour of sulphuretted hydrogen. In the course of ten days the leaves were completely destroyed by the fermentation, demonstrating that, if the flowers of sulphur are anti-fungoid, the beneficial results of its application have not been due, as has been supposed, to its chemical qualities, but probably to its absorption of moisture.

These experiments also go to show that the vine fungus is a true parasite, and that it will not fruit when removed from the plant on which it grows. A peculiar condition of the atmosphere may also be necessary. The Oidium form of the fungus is not supposed by mycologists to be a true mould, but merely a condition of a species of *Erysiphe*. Group B represents a theoretical view of its supposed condition; 12, 13, and 14 its stages of fruiting. Figs. 14 and 15 are filled with little sacs (asci) containing sporidia, which germinate. Fig. 16, group D, represents one of them, and 17 a branch of mycelium growing from them, on which grows the Oidium.

It is stated on good authority that the fruit of this fungus has not been seen on the vine in Europe. In the fall and summer of 1871, and also 1872, I found specimens of its perfect fruit in great profusion on the foreign vine of the grapery of the Department. During the last two years, 1873 and 1874, not a single specimen of fruit could be found. Late in the fall of 1872 Mr. William Saunders, superintendent of the experimental gardens, had all the branches of the foreign vines in the grapery painted with a mixture of clay and carbolic acid, for the purpose of destroying the fruit of the vine fungus. Future observations may show that such treatment will prevent, in a measure, the ravages of the vine fungus. It has long been observed that very dry seasons are favourable to the growth of the *Erysiphe* fungus. Although a hundred foreign vines were exposed to the Oidium in the same grapery, very few were affected by it during the last season; and it is observed that the mildew is confined to certain varieties. The black Hamburgh, for example, was not affected at all by it, although growing side by side with mildewed vines. The green wood is always more injured by the Oidium than the ripe; consequently, as some varieties of vines ripen sooner than others under the same conditions, so the green branches of the later varieties will probably be more affected than those of the early. It was shown by my paper on the fungus of the American grape-vine, in the Annual Report of the Department for 1871, that the early spring leaves of American grape-vines are not affected by the mildew (*Peronospora viticola*) during the summer months, under ordinary conditions, although the leaves that sprout in summer, particularly during rainy weather, when sappy and of a very light-green colour, are very liable to be affected with the mildew, particularly some varieties.

In the fall of 1872 I selected several vine leaves from the foreign Department grapery, having on their surface patches of mildew intermixed with perithecia of the *Erysiphe Tuckeri.* Having removed portions of them, I placed them on glass slides, and secured them in position with gum water, over which I placed a thin glass disk. While viewing them under a power of about 100 diameters, I applied pressure on the disk in order to burst the perithecia. I used great care in my manipulation, but failed to get sporangia out of them. I then laid the leaves aside until November, 1874. In consideration of recent successful experiments on perithecia of black-knot fungus, I resumed my experiments on those of the foreign grape-vine mentioned. I removed a small portion of the leaves procured in 1872, containing the perithecia, placed it in a capsule and poured over it concentrated ammonia with the view of softening its albuminoid matter. To another portion I added nitro-muriatic acid, and neutralized the acid by ammonia. This latter method has the advantage of bleaching the perithecium, which is naturally opaque, but when partially bleached is of a translucent Vandyke-brown colour. Under either treatment the perithecia become soft and pliable, and the proper degree of pressure may be given during the operation while viewing them under the microscope. In this way I have succeeded in bursting them and forcing out their sporangia in perfect form. I had previously failed in this experiment, probably for the reason that the sporangia had not matured sufficiently, and in consequence of the thinness of their cell-walls they burst with slight pressure, and a grumous mass was all that I obtained. The sporangia of some of the forms of *Microsphæria* are easily removed, and seem to bear more pressure without breaking the cell-walls of the sporangia than those of the vine, judging from my experience thus far.

During the last four years I have examined many hundreds of specimens of the Oidium form of the vine fungus, but in no case have I seen connected with them pycnidia, forms of a cell described and illustrated by Professor Amicé and Doctor Plomley, of Europe, and represented by them as connected in some way with the Oidium. I am certain, however, that I have found in great profusion, during the summer and fall of both 1871 and 1872, on the vines in the foreign Department grapery, the true fruit or perithecia of *Erysiphe Tuckeri*. The Rev. M. J. Berkley says:

"It is true that the real sporangia of the vine mildew have not yet been observed. . . . . We do not doubt, therefore, that at some future period the true sporangia may be found; and we trust that the little parasite which has been of such unlooked-for importance may still preserve the specific name originally assigned to it, in honour of the meritorious cultivator who first observed it. . . . . It may, therefore, be named *Erysiphe Tuckeri*, and the name of *Oidium Tuckeri* should be rejected."

When Professor Planchon visited this Department last year, I prepared for him a microscopic slide containing specimens of the perithecia of *Erysiphe Tuckeri*, taken from a foreign vine of the Department grapery.

Should the climatic condition of the summer and fall of 1875 prove favourable for further investigation in this direction, I may be enabled to define more clearly the habits of *Erysiphe Tuckeri*, on a knowledge of which depends the proper remedy to be applied for its destruction and the consequent protection of the vine.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*Bacteria in Organic Tissues protected from Air.*—M. Servel recently read an interesting paper on this subject before the French Academy of Sciences. He said that his experiments were suggested by some ineffectual attempts he made to harden large fragments of cerebral substance with chromic acid. If the tissue be not treated in thin slices, the central parts of the piece, not being reached by the acid, undergo putrefaction. In the five experiments now to be noticed he used a solution of chromic acid, containing one part in 100. The first two experiments were on guinea-pigs (in October, 1874). The live animals were decapitated so that the head fell at once into the chromic acid bath. In both cases the results corresponded to those with the cerebral substance. Examined six days after immersion, the outer parts of the head were hard and preserved; but the central parts were in manifest corruption; under the microscope, the cerebral pulp presented a large number of bacteria of all sizes. Feeling, however, that in these experiments the absence of air-germs was not sufficiently demonstrated, as the deep parts of the nasal fossæ or the buccal cavity might possibly have retained them notwithstanding the immersion, M. Servel repeated the experiment with the liver or kidney of dogs, killed for this purpose by femoral bleeding. To eliminate sources of error, and especially entrance of air by the wound, he placed a ligature at the level of the hilum of the liver and the kidney to be experimented on; then he completely removed these organs, preserving their envelope of connective tissue throughout its extent. The threads of the ligature were used to suspend the organs and keep them from contact with the sides of the vessel containing the solution. This experiment, repeated three times (in October and November) on two hunting dogs and a shepherd's dog, gave the following results, after five days' immersion (the average surrounding temperature being fifteen degrees [Centigrade]). The liver and kidney were more voluminous than in the fresh state, elastic to pressure. The surface was hardened throughout, and gave the peculiar odour of organs immersed in chromic acid. On section, there was emanation of fetid odours. Under the microscope, the outer layer was found entire; the central parts were full of bacteria, showing characteristic movements; some, in the liver, were large, some enlarged at one end (*Bacterium capitatum*); in the kidney they were fewer, thinner, and more mixed with cells still intact. The solution of chromic acid at once arrested the movement of the bacteria. Hence M. Servel concludes: 1. That MM. Bechamp and Estor's demonstration of the production and evolution of bacteria in organic tissues protected from air-germs is quite exact; 2. That the effect produced by preservative agents is the death of microzymes or molecular elements surviving in the organs.

*Professor Williamson's Deep-sea Researches.*—Professor Williamson, F.R.S., is known to have conducted some important inquiries into the marine Tertiary deposits, and therefore it is somewhat unfair in the recent discussion of the 'Challenger's' researches to leave him out of sight. He evidently thinks so himself, as the following extract from a letter in a late number of 'Nature' will show:

"When Prof. Wyville Thomson published his recent volume giving the results of the deep-sea researches conducted by himself and his colleagues, Dr. Carpenter, Mr. Jeffreys, and others, he also gave a sketch of the history of the subject; but he made no mention of my memoir on the Microscopic Organisms of the Levant Mud, published in 1847 in the Transactions of the Literary and Philosophical Society of Manchester, though this memoir had been referred to from time to time by Dr. Carpenter, Messrs. Parker and Rupert Jones, and others, and was, next to Ehrenberg's discovery of the microscopic structure of chalk, the starting-point of all these deep-sea investigations. It was the first to call attention to the existence of foraminiferous deposits in the sea, and to insist upon the organic origin of *all* limestones except a few fresh-water Travertins, in opposition to the theory of chemical deposits that had previously been advocated in the works of Phillips and other geologists. Dr. Wyville Thomson in a recent article points out that extensive areas of the deep-sea bottom are now occupied by a reddish earth, and he has arrived at the conclusion that this earth is a residue left after all the calcareous Globigerinæ and other such elements have been removed by the solvent action of carbonic acid accumulated in these deep waters. In my memoir I arrived at the same conclusion from the study of the marine Tertiary deposits, containing Diatomaceæ, of Bermuda, Virginia, and elsewhere. I may perhaps be permitted to republish the following extracts from that memoir, since it is not now readily accessible to all the numerous naturalists who are interested in this question:

"'In the recent deposit of the Levant we have generally an admixture of calcareous and silicious organisms. In some localities the latter are more sparingly distributed than in others; in a few instances they are almost entirely absent. The same admixture occurs in the recent sands from the West Indies. The soft calcareous mud from the bottom of the lagoons of the Coral Islands contains a considerable number of similar silicious forms, and corresponding results have been obtained in most of the marine sediments from various parts of the globe, examined by M. Ehrenberg.

"'On the other hand, the infusorial deposits of Bermuda and Virginia are altogether silicious. Not one calcareous organism exists. The silicious forms comprehend the majority of those which I have described from the Levant, many of them being not only *similar*, but specifically identical, and the manner in which they are grouped together in these distant localities indicates something more than mere accident. Indeed, we want nothing but the calcareous structures to render these Miocene strata perfectly analogous to those now in process of formation, both in the Mediterranean and in the West Indian seas. Are these silicious deposits, so void of any calcareous organisms, still in the condition in which they were originally accumulated? or were they once of a mixed character, like those of the Levant, having been subsequently submitted to some chemical action which has removed all the calcareous forms, leaving only the silicious structures to constitute the permanent stratum? I am disposed to adopt the latter opinion, for several reasons.'

"After showing the resemblance between the residue left after treating certain substances with nitric acid, and the diatomaceous deposits, I proceed to say:

"'Such deposits, in these present conditions, stand out as anomalies in the existing order of oceanic phenomena, and have nothing resembling them except the local fresh-water accumulations which occur in various places. Between these, however, no real analogy exists. It must not be forgotten that the Virginian deposit can be traced for above two hundred miles; and, being marine, would doubtless be mixed up with such marine products as were likely to occur along so extended a line. The only recorded instance with which I am acquainted, that exhibits the slightest resemblance, is furnished by M. Ehrenberg, in his examination of materials brought home from the South Pole by Dr. Hooker. Some pancake ice, obtained in lat. 78° 10′, long. 162° W., when melted, furnished seventy-nine species of organisms, of which only four were calcareous Polythalamia, the remainder being all silicious. But even this example, remarkable as it is, does not supply us with any real parallelism. The deposits in question have never yet exhibited a single example of a calcareous organism.'

"After referring to the European greensands, I continue:

"'Nature furnishes us with an agent quite equal to the production of such effects as we are at present acquainted with. This is carbonic acid gas in solution in water. Mr. Lyell has already availed himself of the instrument to account for the subtraction of calcareous matter from imbedded shells, as well as for some of the changes that have taken place in the structure and composition of stratified rocks. . . . It is easy to conceive that whilst these strata were in a less consolidated state than at present, they might be charged with water containing carbonic acid gas. This would act as a solvent of the organic atom of lime until the acid was neutralized.' . . . .

"After venturing upon these conclusions in 1847, not as mere speculative guesses, but as the deliberate result of a long series of investigations carefully worked out, I need scarcely say how intense was the interest with which I read Dr. Wyville Thomson's observations, which so thoroughly sustain and confirm the accuracy of mine. My conclusions were wholly derived from the microscopic observations of earths and rock specimens which I compared with the few examples of foraminiferous ooze with which I was then familiar. The 'Challenger' researches now show us how extensively the conditions described in my memoir have prevailed; a fact which could not have been ascertained before the machinery for deep-sea exploration attained to its present perfection. But having arrived at them in a decided or definite manner when the materials for doing so were much more scanty than they now are, and when no one except myself and the late Prof. Bailey of West Point were giving much attention to the subject, I think I am justified in wishing the fact to be placed on record."

# NOTES AND MEMORANDA.

**Angular Aperture of no Importance!!!**—An American gentleman, who certainly speaks, if with no other favourable quality, at least with firmness and decisiveness, has read a paper on the above question before a recent meeting (Jan. 5, 1875) of the Memphis Microscopical Society. The following extracts from his communication will be read with some little surprise: "Now, gentlemen, it is with some diffidence, but with no lack of firmness, that I assure you there are a few of us who have been hard workers at the tube, who do not believe this doctrine of penetration, and did not believe it ten years ago. To my mind a good object-glass, whether of low or wide aperture, should give intense definition on *one* focal plane and *one only;* any variation from this (penetration or what not) will be at a sacrifice of the intensity of the definition. The modern objectives of to-day (1874) as furnished by our countryman, Mr. Tolles, having air angles of 180°, and balsam angles of, say, 85° to 95°, are instruments in every respect far removed from the objectives of ten years ago; these glasses admitting both the central and oblique pencils almost perfectly corrected, and thoroughly under the control of the eminent optician who has just introduced these new 'four systems.'—hence they work equally well either by central, moderately oblique, or very oblique light, and are equally serviceable for the purposes of histologist or diatomist. Now for the proof. Select any object (only be sure and not select a diatom, for Dr. Beale says that such look confused when received with low-angled glasses): suppose you take a blood-corpuscle or a specimen of striated muscular fibre, or anything you may elect; view this, using central light with the new four-system Tolles' $\frac{1}{10}$th, recently purchased by your secretary, Mr. Dod, first with A, afterward with B, and other still higher eye-pieces, thus carrying the amplification up to 7000 diameters or more, and note what you see. Now remove the Tolles', substitute the best low-angle objective that can be obtained, repeating the previous experiment. Assuming that both objectives are manipulated so as to obtain maximum performance of each, I confidently predict that the Tolles' $\frac{1}{10}$th will vastly excel any low-angled objective extant. The view of your object, as seen with the Tolles' $\frac{1}{10}$th, under an amplification of seven to eight thousand diameters, will be sharply defined and well illuminated, while with the low-angle glass you will do well to see the object thus amplified at all. I shall be greatly pleased to have the Society try this and similar experiments, and feel sure that the results obtained will surely explode the current idea that wide-angle glasses are of no use to the histologist. At a future time I shall offer further remarks, and will give in detail a few experiments of mine which perhaps some of your members will be sufficiently interested in to repeat."

## CORRESPONDENCE.

### The Formulæ of the "Museum" $\frac{1}{10}$th.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *January* 21, 1875.

Sir,—You have published a diagram of a $\frac{1}{10}$th inch objective described by Dr. Woodward in the number of your Journal for November, 1873, p. 214. The diagram appears in connection with a computation of the angular aperture of the objective by Prof. R. Keith given in this Journal for September, 1874, p. 124.

In your Journal for June, 1872, p. 273, I find that Mr. Wenham says this,—"If Mr. Tolles will give us the correct radii, diameters, distances, &c., of any one combination that has been actually constructed and transmitting a full aperture in balsam, and show us the passage of the rays through to prove the result, then I will strike my colours; or if he will furnish the drawing, the rays shall be traced through for him."

The same suggestion substantially was made to Dr. Woodward, as quoted by Dr. W. in his last article, this Journal for September, 1874, p. 125.

In view of these invitations and proffers on the part of Mr. Wenham, I have furnished the formula on which the "Museum" $\frac{1}{10}$th in., measured and computed, was constructed. It was carefully *recorded* from actual *measurements* of the curves, diameters, and distances.

There can only be a question of *care* and *accuracy* in taking these dimensions. In support thereto I will state that it has been my custom for a decade or two to record such dimensions for my own use and convenience, in the case of all objectives of any considerable merits constructed under my direction. I also send you the dimensions of the *next* $\frac{1}{10}$th made on the "Museum" $\frac{1}{10}$th in. formula, and which was fully tested at the Museum by Dr. Woodward, and endorsed as similar to the "Museum" $\frac{1}{10}$th in. in its traits.

For convenience of comparison I give the formula in the same tabulated form adopted by Prof. Keith. There are but slight variations as to the back and middle systems, while the *front* is identical.

| | *a.* | *b.* | *c.* | *d.* | *e.* | *f.* | *g.* |
|---|---|---|---|---|---|---|---|
| Refractive index .. | 1·525 | 1·620 | 1·525 | 1·620 | 1·525 | 1·654 | 1·525 |
| Radius of 1st surface | 0·265 | 0·200 | ∞ | 0·200 | 0·100 | 0·180 | 0·033 |
| " 2nd " | 0·200 | ∞ | 0·200 | 0·510 | 0·180 | 0·500 | ∞ |
| Thickness at centre | 0·050 | 0·020 | 0·040 | 0·027 | 0·067 | 0·010 | 0·035 |
| Diameter .. .. .. | 0·205 | 0·205 | 0·205 | 0·205 | 0·167 | 0·167 | 0·066 |

Yours respectfully,

Robt. B. Tolles.

## Mode of Measuring Mr. Tolles' Objective.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *January* 26, 1875.

Sir,—The 'Journal' for January is just to hand. I see Mr. Wenham is heard from again, but not to the point. He quotes from a late editorial in the 'Naturalist,'* and saddles a *quotation* from *my* statement, upon the editor, who will not want the burden. I am the offender all the while; the editor was only telling of it. "Some one" lies not concealed.

And now to the point. Andrew Ross observed the "negative" aberration of the covering glass to be the cause of indefinition in his, as otherwise used, well-defining object-glasses. Andrew Ross invented "adjustment for cover" by closing the systems to a point giving balancing amount of "*positive*" aberration as the remedy.

Well, the wonder has come to pass that Mr. Wenham *closes* the systems in measuring angle, and persists in *ignoring* the aberration. He refuses to put in 'twixt slit and lens a *compensating* cover, and on the other hand does not give any results of measure at open point, or contact of lens with slit, nor, which is only fair, measurements at *approximations* to open point, *uncovered*, with water contact.

If he would but do what is now just indicated, he would get an angle much above "112°," say nothing about 180°, for the nonce, but a good *approach* to that baleful number nevertheless.

Yours respectfully,

R. B. Tolles.

### *Note by Mr. Wenham.*

I really cannot continue this aperture question with Mr. Tolles on the position and passage of rays, for when argument at length resolves itself into mere repetitions, it is a sign that it is well-nigh exhausted. I repeat (as I have stated before) that I did try the $\frac{1}{6}$th of Mr. Tolles with several thicknesses of glass in front, and whether these were superadded in water contact or not, the aperture, or ultimate emergent pencil, was alike with all. There has been much quibbling about differences at open and closed points. This reminds me that I have omitted to mention that the aperture of Mr. Tolles' $\frac{1}{6}$th at *open point* was 108°, closed 112°—a difference of 4° only. I have never argued that the slit cut off any degrees of true aperture, for it will admit rays up to 180°. My argument was that in the $\frac{1}{6}$th Mr. Tolles' angles do not exist, but that the maximum in air was 112°, and in balsam 68°; and whether the focal point lays on the front lens, or cover, or much beyond either, or is wholly immersed in fluid, it will still admit all rays from that point, and there I always place it.

However the case may be in America, it is generally believed here that enormous errors have been promulgated, in the measurement of

* *Not* "anonymous," as he makes it appear.

apertures, by mistaking or including *angle of diffusion* for angle of aperture, and Mr. Tolles wearies us by wishing to claim special exemption for himself. If he feels aggrieved by my counter-statements of his alleged apertures, or attributes a malicious motive to me in making them, why not object to my sole evidence? and ask for a formal trial by others, with the slit in any way that he may suggest. I have no desire to interfere further, and as I wish the question off my hands, I will aid the trial by the loan of any arrangement that is needed. I append this, that Mr. Tolles may not have to wait till I am "heard from again."

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## A Self-centering Turn-table.

*To the Editor of the 'Monthly Microscopical Journal.'*

13, William Street, New York, *Feb.* 1, 1875.

Dear Sir,—Herewith I enclose two drawings illustrating an invention of mine for the improvement of the turn-table now in use with microscopists. The particular purpose of the device is to obtain uniform centering of slides, without measurement or trouble of any kind. The drawings are made to a scale of one-half of the actual measurements of the table which I have, and which I lately exhibited to the American Microscopical Society of this city, of which Society I am the secretary.

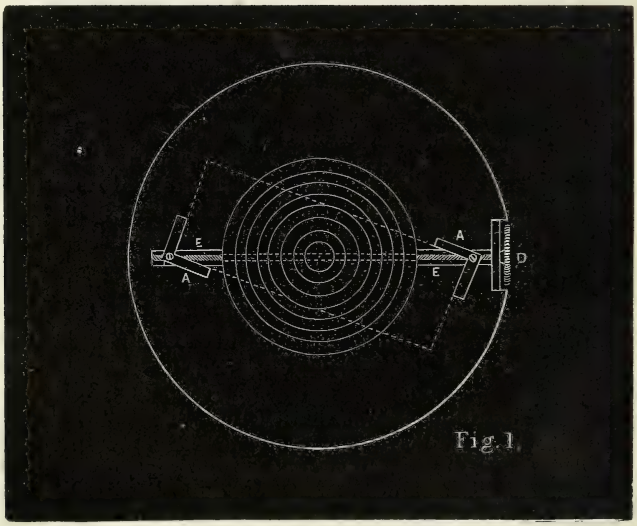


Fig. 1 is the upper face of the table, and Fig. 2 is a perpendicular section through the line of the milled-head and spindle.

A A are right-angled clutches or clamps, set loosely upon the lugs B B by screws, allowing free rotatory movement upon the screws, so that the clutches may conform easily, and without assistance, to various sizes and forms of slides. The lugs B B are fitted to and move in the slots E E, and are carried upon the right and left screw C C, which is turned by the milled head D, thus approximating or separating the clutches A A, simultaneously and at the same rate, towards or from the exact centre of the table.

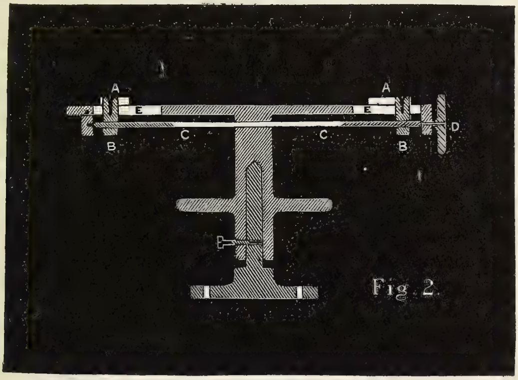

Fig. 2.

The rest of the apparatus is like the ordinary turn-table, except perhaps as to size. Upon the scale to which these figures are drawn, the table, which is 4 inches in diameter, will take any slide from about $3\frac{1}{8}$ by $1\frac{1}{8}$ inches, down to about 2 inches square.

It will be readily seen that the point of revolution of the slide *must* be the place where its two diagonals cross, no matter what its size or shape may be. If the slide is perfectly rectangular, this point will, of course, be the exact centre as to both width and length. If the angles vary a little from right angles, this point will be *very nearly* the centre,—practically nearly enough.

This form of table, therefore, not only secures uniformity in the original placing of cells and objects upon slides, but also ensures perfect concentricity in finishing. It is almost always necessary to apply several different kinds or coats of cement or varnish upon the same slide, and every preparer of objects knows what uncertainty there is of centering a slide a number of times alike upon the turn-table now in common use. By this arrangement, however, there can be no difficulty in always replacing a slide upon the table in precisely the same position.

If this contrivance should meet with general favour, and be adopted by all preparers of microscopic specimens, it would become, to objects, what "the London Society's screw" is to objectives,—a universal standard. For if Möller and Wheeler, and other mounters, use this table, anyone else who has one can always centre their slides just as they did in the first instance, and thus refinishing, which all microscopists have to do sometimes, becomes an operation of the greatest ease.

As the matter now stands, measuring with a rule is more trouble than most persons are willing to take; and so, microscopists usually trust to the eye for centering their specimens. As a consequence, those prepared by even some of the most celebrated workers are often as much as an eighth of an inch away from a true centre. This is not merely a defect, artistically, but is a source of considerable annoyance in many ways, practically.

Believing that my invention will prove to be of no little value to the great body of microscopical workers, I trust that you will allow me to present it to them, through the medium of your excellent Journal.

Respectfully yours,

C. F. Cox.

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## Loan Collection of Scientific Instruments and Microscopy.

*To the Editor of the 'Monthly Microscopical Journal.'*

*February* 20, 1875.

Sir,—Can you tell me why, at the meeting at South Kensington on Saturday, the Royal Microscopical Society was totally unrepresented? Surely the Society has not declined a position on a body which has to do with "discussing the advisability of bringing together a loan collection of scientific apparatus"? If so, I should like to know the reason; for it seems to me that the Microscopical Society has in its collection the finest illustrations of the progress made in the optical construction of the microscope, from the time of Leuwenhoek to that of Wenham, that the world possesses. It does seem strange, then, that a meeting which included representatives of every other possible branch of physical, natural, and applied science, should nevertheless have had no member who in the slightest degree "stood up" for microscopy *par excellence.*

I am, Sir, yours, &c.,

Camera Lucida.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.—Anniversary Meeting.

King's College, *February* 3, 1875.

Charles Brooke, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the Fellows were voted to the donors.

Before proceeding to ballot for the election of Officers and Council for the ensuing year, the President requested the meeting to elect two gentlemen to act as Scrutineers.

Mr. Gay—proposed by Mr. Curties, and seconded by Mr. Coppock—and Mr. Bevington—proposed by Mr. Hinton, and seconded by Mr. Moginie—were then elected Scrutineers in the usual way.

The Secretary read the Treasurer's Annual Statement of Accounts, which had been duly audited; also the Annual Report of the Council upon the proceedings of the past year, and the present position of the Society.

The Secretary announced that the Assistant-Secretary exhibited in the room a most interesting slide of Foraminifera from the river Dee, near Chester, mounted and kindly lent for the occasion by Mr. J. D. Liddall, of Chester. This slide contained no less than sixty-six species, separated from each other by black cells, each of which was numbered. There was also exhibited by the Society a slide, sent by Mr. Martin, of the anthers of a butterfly orchis on the proboscis of a butterfly, illustrative of Darwin on the fertilization of orchids. Attention was also called to several numbers of a new and important work upon Diatomaceæ, which had been kindly sent for the inspection of the Fellows, by Mr. Hardwicke. The title of the work was the 'Atlas of the Diatomaceæ,' and it was edited by Adolph Schmidt, assisted by Messrs. Gründler, Grunow, Janesch, Weisflog, and Witt.

The Scrutineers having handed in the result of the ballot,

The President announced that the following gentlemen had been duly elected as Officers and Council of the Society for the ensuing year:

*President.*—*Henry Clifton Sorby, F.R.S.

*Vice-Presidents.*—Robert Braithwaite, M.D., F.L.S.; *Charles Brooke, M.A., F.R.S.; John Millar, L.R.C.P., F.L.S.; *William B. Carpenter, M.D., F.R.S.

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

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

*Council.*—Frank Crisp, LL.B., B.A.; John E. Ingpen, Esq.; Samuel J. McIntire, Esq.; Henry Lee, F.L.S.; William T. Loy, Esq.; Henry Lawson, M.D.; *John Matthews, M.D.; *George Shadbolt, Esq.; Charles Tyler, F.L.S.; *Frederic H. Ward, M.R.C.S.; *Francis H. Wenham, C.E.; *Charles F. White, Esq.

* Those with an asterisk before their names are new members.

Mr. Charles Brooke expressed his regret that his successor in office was prevented by urgent engagement from being present on that occasion. He was too well known by his important contributions to microscopical science to need any recommendation or introduction from him; he would therefore only say that he hoped they would have the pleasure of seeing Mr. Sorby as the occupant of that chair the next time they met together.

Mr. Brooke then proceeded to deliver the President's Annual Address, in which he reviewed the position of the Society and its work during the past year, and gave a short *résumé* of the progress of microscopy during that period. The Address, which was listened to with great attention, was concluded by brief obituary notices of Fellows deceased during the year. (The Address will be found printed at p. 97.)

Dr. R. Braithwaite proposed a cordial vote of thanks to the President for the Address, which he felt sure all must have heard with so much pleasure, and also for the efficiency and courtesy with which he —the President—had performed the duties of his position. He also moved that the Address, together with the reports, be printed and circulated in the usual way.

Dr. G. W. Royston-Pigott thought that all who were present must feel greatly indebted to the President for his admirable Address; he had great pleasure in seconding the vote of thanks just proposed, and he joined most cordially in wishing Mr. Brooke every pleasure in his future course.

The motion having been put to the meeting by Dr. Braithwaite, was unanimously carried by acclamation.

Mr. Brooke, in responding to the vote, said he felt personally very much indebted to the Fellows for the kind manner in which his observations had been received. Though he left that chair, he should always take an active interest in their Society, and should endeavour to promote its welfare as long as his life lasted.

The meeting was then adjourned to March 3.

## *Annual Report.*

The Society's books and collections are generally in good condition, but the Council have still to regret the want of the space necessary for their better arrangement and utilization. The subjoined lists will show the additions from presentation and purchase during the past year.

### Books Presented during the Year.

Transactions of the Northumberland and Durham Natural History Society. Vol. V. Part 1.
Transactions of the Linnean Society.
A Manual of Botanic Terms. By M. C. Cooke.
Flora of Middlesex. By Dr. Trimen and W. T. Thiselton Dyer.
Preparation and Mounting Microscopic Objects. By Thomas Davis. Second Edition.
Our Reptiles. By M. C. Cooke.
Popular Science Review. Vol. XIII.

The Toner Lectures. By Dr. J. J. Woodward.
Marvels of Pond Life. 2nd Edition. By H. J. Slack.
The Protoplasmic Theory of Life. By Dr. Drysdale.

### Books Purchased.

Annals of Natural History. 2 Vols.
Quarterly Journal of Microscopical Science. Vol. XXII.
Monograph of British Annelids. Part 2. By Dr. McIntosh.
Monograph of the British Spongiadæ. By Dr. Bowerbank. Vol. III.
Proceedings of the Royal Society. 2 Vols.
Fungi Britannici, cent. vij. By M. C. Cooke.
A History of British Quadrupeds. By Thomas Bell, &c. Second Edition.

### Apparatus and Slides Presented during the Year.

A Simple Microscope and Set of Apparatus. By Tully. }
A Single ditto. By Dolland. } Dr. J. E. Gray, F.R.S.
An Ancient Microscope and some Spectacles used by the late Robert Brown. }
A Mechanical Finger. W. T. Suffolk, Esq.
Ross's instrument for measuring thin glass. Dr. Millar.
10 Slides.

Seventeen Fellows, one Corresponding, and one Honorary Fellow, have been elected during the past year.

Ten Fellows and one Associate have died:

Sir Thomas William Brograve Proctor-Beauchamp, Bart., elected June, 1867, died Oct. 7, 1874.
*Thomas William Burr, elected Jan., 1854, died May 22, 1874.
‡John Armstrong Purefoy Colles, M.D., Bengal Army, elected June, 1869, died at Dinapore, Feb. 8, 1873.
**Henry Deane, elected Jan., 1840, died April 4, 1874.
†Frederick Henry Leaf, elected Nov., 1866, died Oct. 23, 1874.
*Edwin Lankester, M.D., LL.D., F.R.S., elected Jan., 1865, died Oct. 30, 1874.
*Robert Lloyd, elected Feb., 1860, died June 22, 1874.
‡Arthur Waller, elected March, 1868, died Dec. 1, 1874.
**John Williams, elected Jan., 1840, died Dec. 3, 1874.
*†R. W. S. Lulwidge, M.A., elected April, 1854, died May 28, 1873.
*Charles M. Topping, elected an Associate Feb. 1846, died Sept. 3, 1874.

### Papers Read before the Royal Microscopical Society during the past Year.

Contributions towards a Knowledge of the Appendicularia. By Alfred Sanders.
Note on the Verification of Structure by the Movements of Compressed Fluids. By Dr. Royston-Pigott.
On Bog Mosses. By Dr. Braithwaite.
The Scales of Lepisma as seen with Reflected and Transmitted Light. By Dr. J. Anthony.
Notes on a Curious Proboscis of an unknown Moth. By S. J. McIntire.
An Instrument for excluding Extraneous Rays, in measuring Apertures of Microscope Object-glasses. By F. H. Wenham.
On certain Beaded Silica Films Artificially Formed. By H. J. Slack.
The Suctorial Organs of the Blow-fly. By Dr. J. Anthony.
On the Use of Black Shadow Markings and on a Black Shadow Illuminator. By Dr. Royston-Pigott.
Continued Researches into the Life History of the Monads. By W. H. Dallinger and Dr. J. Drysdale.
On some Microscopic Leaf Fungi from the Himalayas. By Dr. J. Fleming.
Synopsis of the Principal Facts elicited from a series of Microscopical Researches upon the Nervous Tissues. By Dr. H. D. Schmidt.

On some New Diatoms. By F. Kitton.
On the Development of the Smaller Blood-vessels in the Human Embryo. By Dr. H. D. Schmidt.
On some Male Rotifers. By C. T. Hudson, LL.D. This paper describes males of *Lacinularia socialis*, *Floscularia Campanulata*, and of a new Asplanchna previously unknown, and of *Notommata Brachionus* not previously distinguished.
Studies in the Natural History of the Common Urates. By Dr. W. M. Ord.
On the Invisibility of Minute Refracting Bodies caused by Excess of Aperture, and upon the Development of Black Aperture Test-Bands and Diffraction Rings. By Dr. Royston-Pigott.

John Ware Stephenson in Account with the Royal Microscopical Society.

| *Dr.* | £ | s. | d. | *Cr.* | £ | s. | d. |
|---|---|---|---|---|---|---|---|
| 1874. | | | | 1874. | | | |
| To Balance brought from 31st Dec., 1873 .. .. | 145 | 17 | 9 | By Cash paid for Journal .. | 240 | 0 | 0 |
| „ One Year's Dividend on 1082*l.* 0*s.* 11*d.* 3 per cent. Consolidated Bank Annuities .. .. .. | 32 | 2 | 6 | „ Rent and Attendance at King's College .. .. | 58 | 0 | 8 |
| „ One Composition Subscription .. .. .. | 21 | 0 | 0 | „ Reporter .. .. .. .. .. | 9 | 9 | 0 |
| „ Annual Subscriptions, &c. | 426 | 10 | 0 | „ Mr. Reeves' Salary .. | 80 | 0 | 0 |
| „ Screw-tools sold .. .. | 0 | 1 | 6 | „ Ditto for Commission .. | 11 | 4 | 0 |
| „ Journals sold .. .. .. | 1 | 18 | 0 | „ Subscription to Ray Society for 1874 .. .. | 1 | 1 | 0 |
| | | | | „ Fire Insurance .. .. | 1 | 4 | 0 |
| | | | | „ Stationery and Printing | 10 | 3 | 0 |
| | | | | „ Books purchased .. .. | 5 | 14 | 5 |
| | | | | „ Cash paid for 22*l.* 12*s.* 5*d.* Stock in the 3 per cent. Consols .. .. .. .. | 21 | 0 | 0 |
| | | | | „ Repairing Microscope .. | 1 | 2 | 0 |
| | | | | „ Petty Cash .. .. .. | 35 | 0 | 0 |
| | | | | „ Stamped Cheques .. .. | 0 | 4 | 2 |
| | | | | „ Balance remaining 31st Dec., 1874 .. .. .. | 153 | 7 | 6 |
| | £627 | 9 | 9 | | £627 | 9 | 9 |

Feb. 1, 1875.

Examined and found correct,
E. W. JONES,
B. DAYDON JACKSON.

Donations to the Library and Cabinet since Jan. 6, 1875 :—

| | From |
|---|---|
| Nature. Weekly .. .. .. .. .. .. .. .. .. .. | *The Editor.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts Journal. Weekly .. .. .. .. .. .. | *Society.* |
| Quarterly Journal of the Geological Society, No. 120 .. | *Ditto.* |
| The Clergy Directory, 1875 .. .. .. .. .. .. .. .. | *J. W. Stephenson, Esq.* |
| One Slide .. .. .. .. .. .. .. .. .. .. .. .. | *J. Martin, Esq.* |

Thomas William Cowan, Esq., and S. Hackett Roberts, Esq., were elected Fellows of the Society.

Walter W. Reeves,
*Assistant-Secretary.*

## Medical Microscopical Society.

The Annual General Meeting of this Society was held on Friday, January 15, at the Royal Westminster Ophthalmic Hospital, Jabez Hogg, Esq., the retiring President, in the chair.

From the report of the Committee it appeared that the Society was in a flourishing condition; the number of members being 135. The number of papers read during the past year was sixteen, besides several minor communications, all of which were followed by brisk discussions. Above 100 specimens were exhibited during the year, and eighteen were presented to the Society. A present was also announced of a microscope for use in the *exchange* of specimens; a system which is found to work well, and offers great facilities for obtaining a large collection of good preparations. The Treasurer's Report showed a balance of 15*l.* 10*s.*

The following officers were elected:

*President.*—Dr. J. F. Payne.

*Vice-Presidents.*—Mr. Jabez Hogg; Mr. W. B. Kesteven; Mr. H. Power; Dr. U. Pritchard.

*Treasurer.*—Mr. T. C. White.

*Hon. Secretaries.*—Mr. C. H. Golding Bird; Mr. J. W. Groves.

*Committee.*—St. Bartholomew's, Mr. J. A. Omerod; Charing Cross, Dr. M. Bruce; St. George's, Mr. E. C. Baber; Guy's, Mr. F. E. Durham; King's, Mr. H. S. Atkinson; London, Mr. J. Needham; St. Mary's, Mr. Geo. Giles; Middlesex, Dr. S. Coupland; St. Thomas's, Dr. W. S. Greenfield; University College, Mr. E. A. Schäfer; Westminster, Dr. W. H. Allchin; General Profession, Dr. Foulerton.

The retiring President then read an Address, which was followed by a vote of thanks to the various officers, and the proceedings terminated.

## Quekett Microscopical Club.

Ordinary Meeting, January 22, 1875.—Dr. Matthews, F.R.M.S., President, in the chair.

Mr. T. Charters White, F.R.M.S., read a paper on "The Aquarium as a field of Microscopical Research."

The author commenced his paper by regretting that the aquarium was not more largely employed as an aid to the observation of aquatic life; and after pointing out the various life histories that might be worked out and the developmental phases of which accurately recorded, proceeded to describe the simple arrangements necessary to the successful maintenance of a marine aquarium. The most convenient form of aquarium for microscopical observation was of an oblong shape, 36 inches long, 18 wide, and 9 in depth, and constructed of slate, with the exception of the front, which was of stout plate glass: a false bottom of slate, inclined from the front upwards and backwards at an angle of about 15°, so as to afford a varying depth of water, the water below it being kept cool and in the dark, and consequently at rest, while that above was exposed to the light, and actively ministering to the growth of the animal and vegetable life contained in it: roughly disposed on the false bottom, and cemented there by Portland cement, irregular pieces of sandstone rock might be fixed, as they were useful as shelter to the live stock, and by increasing the superficial area of the bottom adding considerably to the aërating capacity of the vegetation which will ultimately clothe it. If the observer constructs an aquarium for himself, great care must be taken that the rockwork and cement are thoroughly soaked for a fortnight or even longer, that all the soluble salts may be eliminated from them, the water being frequently changed: sea sand (or in default of that silver sand), repeatedly washed till the water is clear when stirred up, is to be placed in the tank. The sea water may now be added; this may be either natural or artificial, the preference being given to the former, but in localities where it is not easily procurable that made from Mr. Gosse's formula may be used:

| | | |
|---|---|---|
| Common salt, $3\frac{1}{2}$ ounces .. .. | } avoirdupois. | |
| Epsom salts, $\frac{1}{4}$ ounce .. .. .. | | |
| Chloride of magnesium, 200 grains | } troy. | |
| Chloride of potassium, 40 „ | | |

This may be dissolved in a little less than four quarts of fresh water, and a specific gravity bubble of 1026 ought to sink slowly in this fluid if it is of the right density. The tank being filled with this, some handfuls of freshly-gathered seaweeds, especially *Ulva latissima*, may be washed in it, but not left permanently in, and the tank exposed to the sunlight for about a fortnight, when it will be found teeming with the germs of vegetable life and in a suitable condition for the support of its animal occupants. The use of the specific gravity bubble in regulating the density of the water was shown, and a new method of aërating the water devised by the author described, by which the use of syringes advocated by writers on Aquaria was superseded. The utility of the aquarium to the microscopist was then adverted to, and its advantages pointed out in the facilities it afforded for the observation of the conjugation and multiplication of Diatomaceæ, the development of the Foraminifera, the growth of the germs of marine Algæ, and the various transitional stages in the life history of the Polyzoa. The paper concluded with some practical suggestions relative to the selection of suitable occupants for the tank, and upon those which would be likely to interest the practical microscopist.

Mr. Ingpen described a portable microscope, designed and made by Mr. Moginie especially for use with low powers. The eye-piece was very large, and gave a fine field of view. Its lenses were made to slide instead of screwing, so that they could be very readily cleaned. The general pattern of the instrument was somewhat similar to the portable microscope of the same maker, but the stage was moved by rackwork instead of the body, which was fixed to the stand, thus adding greatly to the steadiness of the whole. The rackwork was sufficiently delicate for use with a $\frac{1}{2}$ or even $\frac{1}{4}$ inch power. Notwithstanding its large size, the instrument was exceedingly portable, and its wide range and great steadiness rendered it very serviceable for many purposes, particularly in botanical and aquarium work.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

APRIL 1, 1875.

## DEATH OF MR. HARDWICKE.

It is our painful, painful duty to record the death of our publisher, Mr. Robert Hardwicke, which occurred on the morning of Monday, 8th March, at the age of 52 years, after an illness which lasted but for ten short days. Thus was he cut off nearly in his prime, at a time too when his business relations were almost at their best. Of his friends, it is not too much to say, that those who knew him longest knew him best, and have only to record their extreme sorrow for his loss. For assuredly there were none who were more thoroughly kind, genial, and considerate in all their dealings. Never before in the course of our experience have we met with one, with whom we have never within the period of ten long years had a single bitter word. All his dealings were kindly, none were severe. And though we feel that the fewest words are best when all are vain, we cannot help expressing our bitter sorrow at his death. For we have not the least hesitation in saying that we have lost a good, sincere, and earnest friend.

## I.—*Some Remarks on Bucephalus polymorphus*, by Mr. John Badcock, F.R.M.S.; *together with Translations from Papers of Von Baer, Lacaze-Duthiers, and Alf. Giard, on B. polymorphus and Haimeanus*, by Henry J. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *March* 3, 1875.)

Plate XCVIII.

In October last Mr. John Badcock exhibited to this Society a specimen of *Bucephalus polymorphus*,* concerning which he supplied the following particulars. He states that "he found the animal in his aquarium at the latter end of June, 1874. A fresh-water mussel had been placed in it a few days before, and remained

* The representations of the creature by Von Baer are not sufficient for an *absolute* identification; but there is little doubt of it.

there during the summer. Bucephalus was developed in great numbers a few days after its first appearance, and exhibited a beautiful sight of transparent creatures flying like eagles through the water, the wing-like appendages spreading out to an enormous length, and constantly in motion, with a general upward tendency. They never attached themselves to any object, but always swam freely, and were neither seen to feed upon other creatures, or to be the objects of attack. The top of the central organ exhibited, under the microscope, a small slit or sucker, and a ventral contractile sucker. The slightest pressure bursts the two bags, as well as the side appendages; while the central organ, thus set free, retains an independent vitality for a considerable time, and preserves its structure, although the other parts resolve themselves into a shapeless mass of globules.

"The animal, as a whole, is extremely brittle, and often breaks in pieces, the lateral organs usually coming off. When removed with a dipping tube, and placed in a small bottle of water, a little shaking easily breaks them up, unless the bottle is quite full.

"They are so transparent that small objects, such as the cilia of paramecia or vorticella, can be readily seen through them.

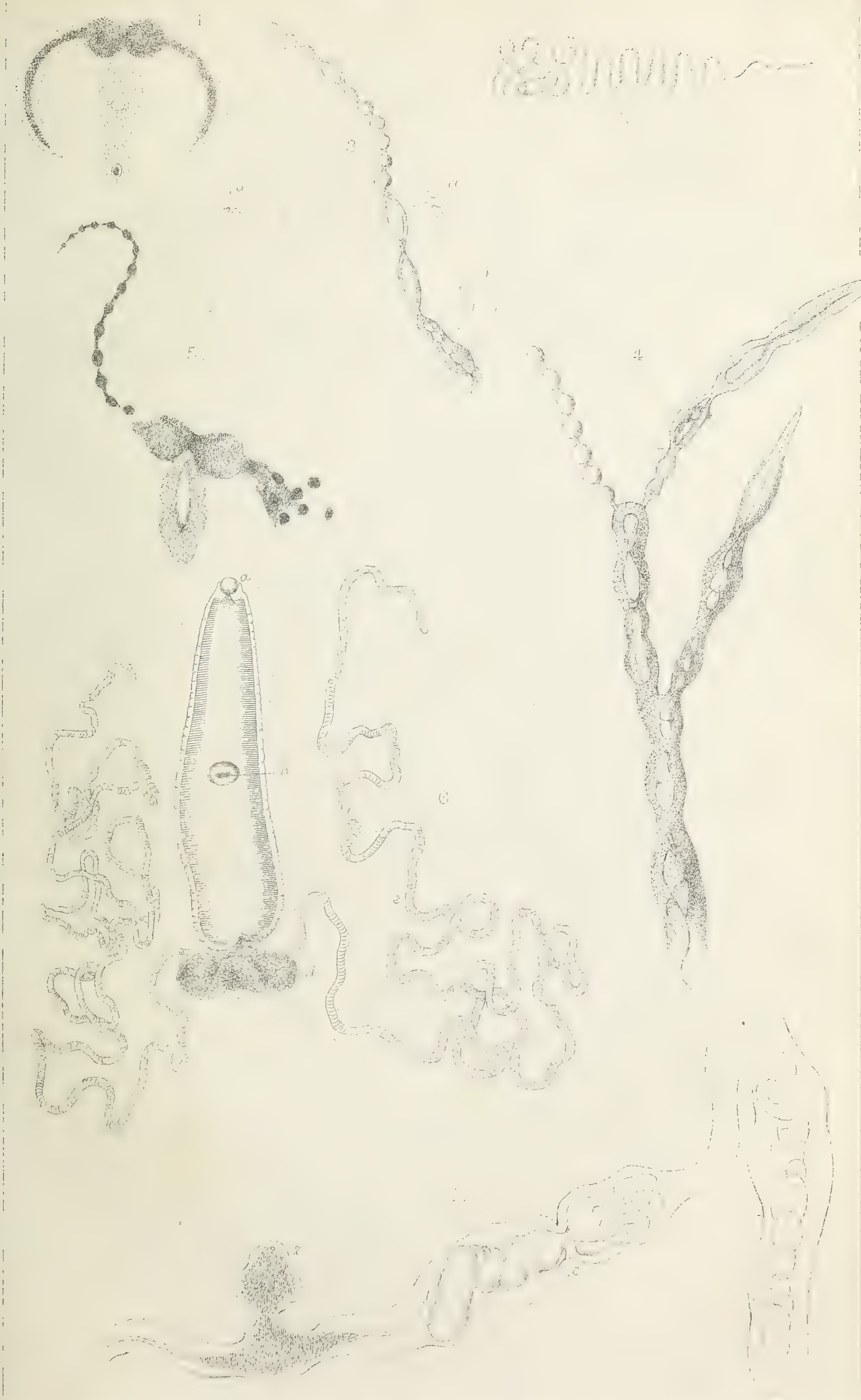
"The side appendages are capable of wonderful extension, being often thrown out to four or five times their normal length, with a quick active motion quite unlike the deliberation with which the hydra moves its tentacles.

"During four months no other change than those mentioned was observed in the form of these creatures, and they all vanished on the setting in of cold weather."

Mr. Badcock showed these creatures to several well-known naturalists, to many of whom it was quite new; and no one of those who recognized it as *Bucephalus polymorphus* was well acquainted with it, which shows that it has not been sufficiently noticed.

From a cursory view of the animal, it was evident that many specimens would be required to make out its internal structure. When not compressed, no internal organs were visible during the short time it was exhibited to the Society, and, as Mr. Badcock reports, pressure easily destroys it.

The rough sketches appended by Mr. Badcock to his note leave little doubt that the creature is the *Bucephalus polymorphus* of Von Baer, described by that author in the 'Nova Acta' of the Leopold-Caroline Academy of Bonn, in the vol. for 1826, p. 570, from which Figs. 1 to 5, Pl. XCVIII. are taken. Fig. 1 is like the specimen shown to this Society, except that the latter did not exhibit the same obvious distinction between the integuments of the lateral appendages and their contents; on the contrary, they seemed hollow, and without solid contents. Von Baer states in his paper that he found the animals in mussels, and in 1825 was able to trace their develop-



W. West & Co. lith.

Bucephalus polymorphus & Haimeanus.

ment. Chiefly in the liver and region of the kidneys, he discovered a number of white "threads," similar to what he had noticed two years earlier. They were easily removed by a needle, and he was at first doubtful whether they were worms, or lymphatic vessels. Fig. 2 represents one of these thread-like objects in its natural size, "seen on the dark ground of the kidney, and lying just under its enveloping membrane." He found it exceedingly difficult, if not impossible, to dissect out one of these hair-like bodies when buried in the substance of the organ. Fig. 3 shows an instance in which he was successful, × 20. Further search led to the discovery of dichotomously-branched threads, "which called to mind the lower vegetable forms," and which he named *Schleimfäden*, protoplasm-threads. Fig. 3 exhibits a stage of development, the simple thread being constricted at various points. Fig. 4, the same in more developed condition. "The elongated swellings are further developed than the round ones." The small objects to the right of Fig. 3 are the germ-cells the swollen parts contain. Fig. 4 represents several forms branching from one stem. "In the most developed parts the naked eye is able to see that these are no longer germ-cells, but developed animals."

"The shape of the creature," says Von Baer, "which lives in the swellings of the threads is most wonderful. I twice saw it," he exclaims, "under strong magnification, and it seemed as if the head of an ox was before me, with the horns directed towards me."

"The horns, or rather arms, were formed of soft matter, curved and conical, and their bases were seated upon two pudding-like bodies (*Wülsten*). Their tips were rounded, and at times showed a fine projecting transparent point, which seemed capable of being thrust in and out. The 'horns' at times curled like those of a goat, and fell off." At other times their contents separated into distinct masses, as in Fig. 5. In some cases the arms took the aspect of chaplets of pearls, and swam swiftly away. In others, after separation, the little balls into which the contents were resolved fell out. How long the "pearl chaplets" lived, Von Baer could not discover; but they did not die quickly, and he noticed their lively motions for a good half hour.

The older the creature was the larger and darker he found the two pudding-like swellings, but they were usually lighter than the horns. At times they looked as if full of germs.

"The body is less changeable in shape than the other parts. It is flat, now lancet-shaped, now slightly drawn in in the middle, and sometimes strongly indented on one side. The connection with the two round bodies was at times very narrow, at others broader; but this may have been the effect of position rather than of structure . . . . a mouth opening is usually invisible." When seen it appeared as in Fig. 1. "In the middle of the body there appeared, but not often, though more so than in the case of the mouth, a dark round seam, which must be regarded as a sucker." This organ was more often noticed as an ellipse. He observes that the development of single animals was easier to trace than that of the threads from which it must be distinguished, and he gives a series of sketches showing how the germs in Fig. 3 first became notched at one end; how this increasing, forms a semi-lunar tail, the base of which thickens partially, constricts, and forms the two round swellings. The origin of the threads he did not succeed in tracing, but the paper contains many suggestions, comparisons with arthronema, &c.

Another Bucephalus, *B. Haimeanus*, was described by Lacaze-Duthiers, in the 'Annales des Sciences Naturelles,' 4$^{me}$ Serie, Tome 1. He found the abdominal glands of oysters and cockles completely invaded by the sporocysts of this parasite. "Removed from the organs," he says, "they unroll themselves to white filaments of great fragility, so that it is difficult, almost impossible, to obtain them entire, and to examine their extremities. Their length is considerable, some being many centimetres. These long filaments, primitively cylindrical, are tubular. They become more or less moniliform in chaplets, through the contractions by which they are animated. The most perfectly developed animal found in the sporocysts presents, when first developed, the form of a flattened cone. At the top is seen a mouth surrounded with a cup-shaped sucker, and its base folds and filaments of variable lengths. The body is finely striated perpendicular to its axis. . . . . The mass is most transparent in the middle. We see in it a general cavity which must be considered as a single non-ramified digestive cavity, terminating with a *cul de sac* at the base, and communicating with the mouth at its summit. I observed nothing in its interior. The mouth is simple, without any hook. Above it is a conduit, contracted like an œsophagus, and communicating with the central cavity. In front it is surrounded with an expanded disk when the animal is elongated, and by a cup when the contractions are very strong. The walls of the body, from outside to in, are composed of three layers; the outer one smooth and non-striated, the middle one becoming annulate under contraction, and the inner one, which may be called parenchymatous, being the thickest." In this layer vesicles and granules were observed. "The extremity of the body which corresponds with the base of the cone bears a lamellar appendage, and two long filaments." These filaments are described as very contractile and extensile. The development of *B. Haimeanus* from a round germ through various gradations, is described and figured much like Von Baer's account of a similar development in *B. polymorphus*.

M. Lacaze-Duthiers exclaims, "What idea can we form of the sporocysts? It seems natural to regard them as the mothers of the numerous brood they contain. Their whole body is transformed into a veritable matrix, or chamber of incubation. But these mothers, born fecund, do not arrive at a form which terminates or begins a series of changes: they are themselves larvæ. According to the observations of Siebold, they are only a part of an embryon; and Steenstrup noticed in mussels larvæ resembling parameciæ, which after losing their ciliated epithelium transformed themselves into a germinative tube. This, then, is another form to add to those already so numerous of this same helminth."

M. Lacaze-Duthiers found these sporocysts subject to lively contractions, while those of *B. polymorphus* described by Von Baer were rigid. "Another remarkable fact is their budding . . . . when a bud is only slightly protuberant it contains nothing but a little brown granular matter." He adds that "the oysters of Mahon, and the cockles of the Lake of Thun, near Cette, were found to be sterile, these helminths occupying the conduits of the genital glands, and even of the intertubular spaces." Fig. 6 represents the sporocyst, copied from Lacaze-Duthiers, and Fig. 7 the complete animal.

Further light is thrown upon *B. Haimeanus* by a note of M. Alf. Giard in 'Comptes Rendus,' Aug. 17, 1874. He mentions its discovery by Claparède* on the coast of Normandy, fishing with fine nets. His specimens differed a little from those of Lacaze-Duthiers, chiefly in the form of the lamellar appendages, but not sufficiently so to require the formation of a new species. M. Giard found it encysted in the liver, genital glands, and other organs of the garfish (*Belone vulgaris*). His anatomical investigations were not completed, but he agrees with Claparède in rejecting the opinion of Lacaze-Duthiers, that the general cavity is a digestive one. He inquires, "What becomes of the encysted Bucephalus? Does it reach maturity in the body of the garfish, or undertake a fresh migration? and is that migration active or passive? This remains to be discovered." He states that Claparède found the *Cercaria Haimeana* several times fixed on *Sarsia* and *Oceania*, and he himself found an adult trematode in the intestinal cavity of *Cydippe pileus*, but with Claparède he regards such appearances as accidental. According to Siebold, *B. polymorphus* transforms itself into *Gasterostomum fimbriatum* in the digestive tube of the perch (*Perca fluviatilis, Lucio-perca*), and it is also found encysted in the carp. "It is therefore reasonable

* Claparède, Beobachtungen über Anatomie, &c., au der Kuste von Normandie, 1863.

to suppose that *B. Haimeanus* encysted in *Belone vulgaris* metamorphoses itself into some species of *Gasterostomum* in the intestine of some big fish which feeds upon the Belone."

Specimens of *Bucephalus Haimeanus* were exhibited to the Society by Mr. W. F. Woods, at the Scientific Evening held in December, as "living organisms from the cockle."

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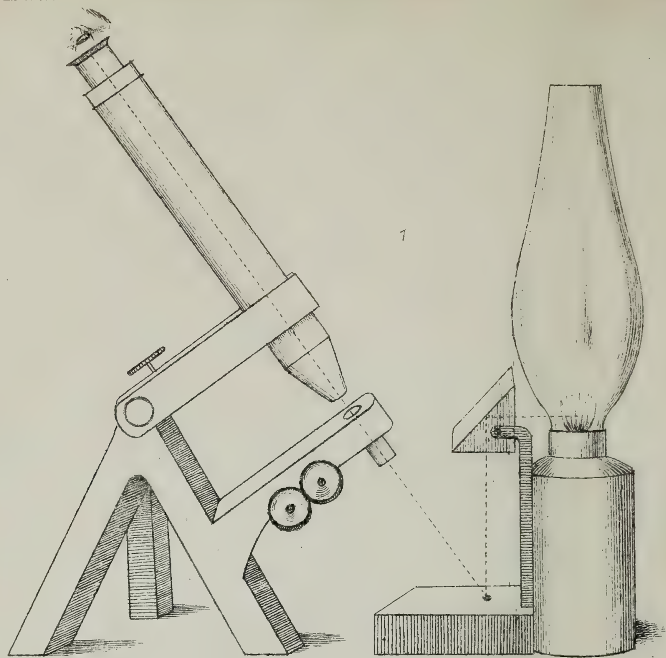
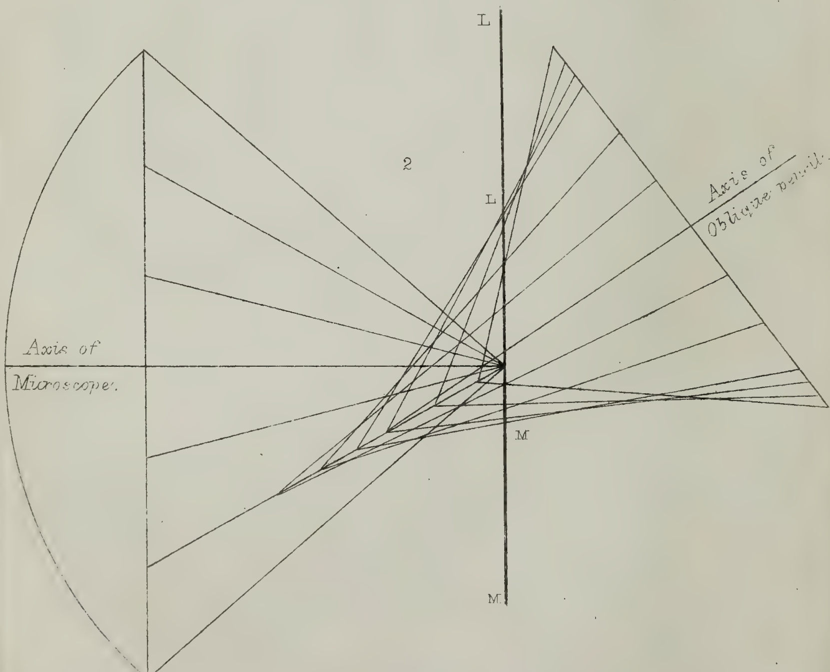
## EXPLANATION OF PLATE XCVIII.

Figs. 1 to 5 after Von Baer, 6 and 7 after Lacaze-Duthiers.

| | | | |
|---|---|---|---|
| Fig. 1.— | *Bucephalus* | *polymorphus*; | 1 *a*, n. s. |
| „ 2.— | „ | „ | sporocyst, n. s. |
| „ 3 & 4.— | „ | „ | different stages of ditto × 20; 3 *a* and *b*, germs from sporocyst, *b* beginning to develop. |
| „ 5.— | „ | „ | showing change in lateral appendages. |
| „ 6.— | „ | *Haimeanus*. | |
| „ 7.— | „ | „ | sporocyst: a young bud. |

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## II.—*On the Principle of testing Object-glasses by Miniatures of Illuminated Objects examined under the Microscope, especially of Sun-lit Mercurial Globules; and on the Development of Eidola or False Images.* By Dr. Royston-Pigott, M.A., F.R.S., F.R.A.S., F.C.P.S., formerly Fellow of St. Peter's College, Cambridge.

(*Read before the* Royal Microscopical Society, *March* 3, 1875.)

Plates XCIX., C., and CI.

The method of examining miniatures as a means of testing objectives, as fully described in the 'Transactions of the Royal Society,' offers some intrinsic advantages not attainable by the examination of objects placed immediately upon the stage. For want of this method, it has hitherto been impracticable to develope the splendid interference phenomena of sun-lit mercurial globules, which can only be perfectly seen by the means of magnified miniatures.

Comparison of the enlarged miniature with its original is so facile and self-testing, that the merest tyro can at once declare whether the image shown when highly magnified agrees with the thing itself. For instance, if he detects a kind of white smoke or fog or coloured haloes about the bright points of the miniature, he knows that there must be error somewhere. On the contrary, all doubt as to quality and goodness of definition is completely barred out, when the enlarged image emerges sharp and clear and brilliant in all its details. This can only happen when all the rays, whether *white* or *coloured,* emanating from each single point, are made to converge again into one and the same point in the image. The white rays, if achromatic, but not aplanatic, create a white mist: the coloured rays, prismatic haloes.

And this brings me to the standard principle of perfect definition, viz. that the image of a given point shall not be a confusion of images of that point, but one single point. But to attain this

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DESCRIPTION OF PLATES XCIX., C., AND CI.

Fig. 1.—Shows the arrangement for illuminating artificially a mercurial globule by means of a prism: the miniature is formed in the focal plane of the observing object-glass.

„ 2.—L M the focal plane of vision: aberrating rays intersect in this plane and produce the diffraction phenomena which assume their peculiar forms according to obliquity, kind of correction, and quality of the glasses.

„ 3–14.—Show the elegant and delicate varieties of diffractions, in different stages of obliquity and correction.

„ 15, 16, 17.—Display of the conic sections into which the phenomena arrange themselves.

„ 18.—Apertures forming luminous disks and eidola out of focus.

„ 19.—An exceedingly minute brilliant disk out of focus.

„ 20, 21.—Wire gauze and its eidola.

perfection, how much genius, patience, and manipulative skill, knowledge of the various kinds of glass and curvatures required, the makers of such perfection can alone declare. The continual abandonment of old forms and combinations, formerly thought the *ne plus ultra* of art, and the adoption of new, sufficiently vindicate a former declaration that perfect corrections had not then been attained; but that the future is full of hope is shown by recent improvements still made in the face of such refined difficulties.

In my first paper, Dec. 1869, I stated the residuary aberration in the best glasses might be reckoned at the 50,000th of an inch. This small matter seems insignificant; yet in defining a single bead the sixty or even the thirty thousandth of an inch, such an error cannot be despised.

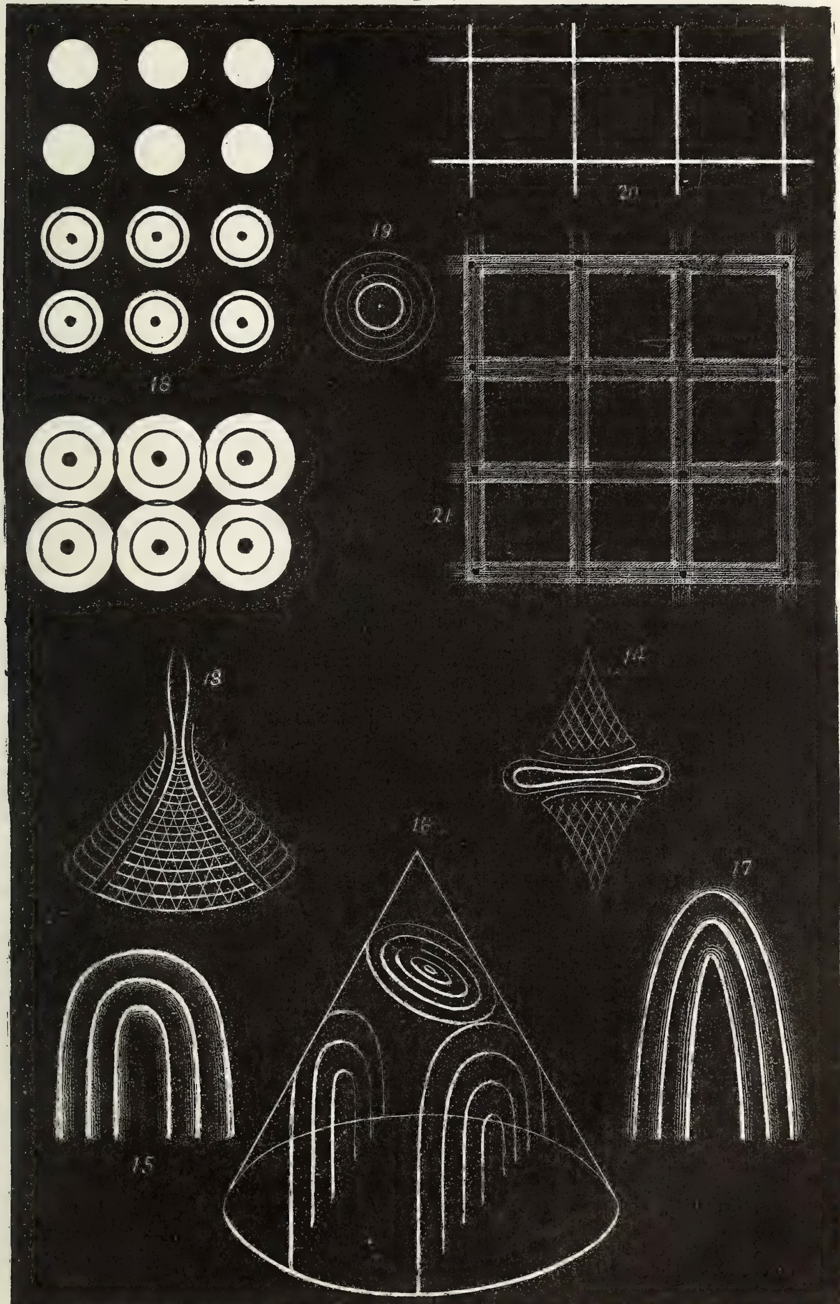
In the older glasses, the effect of this aberration (often much larger than this) was to cause a series of beads to run into each other. I have ever found, as the definition became more and more refined, these objects appeared smaller and smaller with the same magnifying powers. Our Honorary Secretary, Mr. Slack, noticed recently that in Powell and Lealand's new construction (not yet issued) the beading of the *Angulatum* appeared under a power of 4000 with an eighth much smaller than usual: *in short, more "widely divided."*

A bad glass, it may now be stated, unduly enlarges a point: if it be black, it is diluted as it were into a larger and paler area than the truth. In the same way a fine line, which is an assemblage of points, appears thickened and blurred: I have even here noticed dark diffraction rings * surrounding dark points.

If the point be brilliant, it has special developments as to its appearances as the glasses depart more and more from perfection. And these developments take their own peculiar forms according as the point is itself an origin of light—a reflector or a transmitter of light. They also vary with the directness or indirectness of illumination, and with the position of the line of vision.

If a globule of mercury of exceeding minuteness is placed upon the stage, illuminated as best we may, and then be viewed with a $\frac{1}{16}$th, only a very imperfect picture of the illuminating flame is exhibited by the globule, in consequence of the great inconvenience of the closeness of the objective to the stage and the limited range

* These may often be seen in gold leaf placed between two glasses and illuminated by sunlight by transmitted light. The beautiful malachite green of the film of gold will be seen perforated in many places with apertures of any degree of minuteness, each displaying diffraction rings; the best objectives will show an intensely black border round each aperture, and as the focussing is changed, over the general surface dark points will be seen to swell and shrink, and dark diffraction rings may be noticed correspondingly to expand and contract with great regularity. The behaviour of a good glass may be strikingly contrasted with that of an inferior quality when tried upon this gold-leaf test, which I strongly recommend, especially when transmitting solar light.

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of illumination. Those who may have tried this plan will best appreciate the difficulty and trouble attending the experiment.

The method of miniatures is entirely free from this embarrassment.

An object-glass of very fine quality, and in an inverted position, is screwed into the sub-stage.

On black velvet are scattered, from a syringe containing mercury, a number of mercurial globules; then, by means of a Reade's prism, a brilliant light is thrown vertically downwards upon them. The object-glass to be tested is now employed in the usual way, screwed to the nose of the microscope. The two objectives are brought to a central position, so that their axes coincide, and the instrument is then adjusted to form miniatures of the globules for examination. The most beautiful effects are seen under sunlight; and most of those which I shall have to describe are so produced.

These miniatures will develope appearances of marvellous beauty and variety, according to the following circumstances of their employment:

(1) Coincidence of the axes of the observing and miniature-making objective.

(2) Particular corrections, either spherical or chromatic, of either.

(3) (Which is included in the last), the distances at which the original globules are placed from their miniature images.

It is also worth remarking that the aperture of the miniature-making objective should be at least as wide as that of the objective to be tested, and of the finest quality obtainable, and furnished with a screw-collar. Also the choice of powers is a matter of taste and experience. It requires an excellent pair of objectives for diminishing and enlarging, in order to successfully develope the beautiful phenomena caused by the clashing as it were of the waves of light, commonly called *interferences*. As a rule, the observing objective should not be more than double the power of the diminishing objective used to form the miniature.

Some of the most beautiful effects have been produced by a most excellent $1\frac{1}{2}$-inch Ross for making the miniatures, and then observed with a $\frac{1}{5}$th Wray and a B eye-piece. Occasionally both objectives are used of the same power.

If it be remembered that at 10 inches an eighth diminishes an object 80 times, a power of 800 applied by the microscope will develope too much residuary error: 500 will present a more eligible image for examination.

Various objects may be used for miniatures. A small, brilliantly illuminated opened watch, in motion, is rather a surprising test. Fine glasses show the working parts of the watch, balance, &c., almost as plainly as the naked eye, under a power of 1000 diameters. Inferior glasses spread the miniature over with a *white* smoke if achromatic, or it appears as through thin horn; but coloured lights play about if the achromatism is at fault.

It may be interesting to quote the 'Philosophical Transactions' here: that if the distance of the image from the original be 100 inches, the miniature, with an eighth, will be 800 times smaller.*

Again, the sparkle of light on the miniature of a small thermometer as well as the visibility of the divisions on the ivory scale afford highly instructive lessons on residuary spherical or chromatic aberration. At a distance of 100 inches, if the thermometer scale is small, say 30 degrees to the inch, the miniature formed by an eighth 800 times as small will be the 24,000th of an inch; while the *divisions* on the miniature exceed the delicacy of Nobert's minutest lines. At 100 inches the image or miniature formed is in an over-corrected state, and the observing objective should probably be placed at "uncovered," or at least considerably over-corrected. If the miniature be corrected by closing the collar as much as possible, a similar effect should be introduced into the observing objective. Indeed, plying both collars at once will soon discover to the experimenter the peculiar correlations of the observing and miniature-making glasses.

I strongly recommend, however, what I may call the fundamental experiment,—a disk of intense light as small as possible; miniatured from a distance sufficiently great to develope the test diffraction rings.

The appearance of one thick, broad, dull ring surrounding a planetary disk, I may say, was the first real insight I obtained into the real state of the best glasses I could procure. I then worked in a darkened room and with a shaded lamp; illuminating a minute aperture or pairs of apertures in blackened brass plate. These apertures were about the $\frac{1}{100}$th of an inch in diameter. But they were found afterwards to be a poor and insignificant substitute for sun miniatures, at this season so difficult of attainment.

The experimenter on these methods will occasionally be surprised at the very curious results obtained by viewing the same miniature with equal magnifying powers used differently.

For instance:

*Experiment.* A miniature landscape was formed by a small convexo-plane lens $\frac{1}{30}$th focal length and a lineal aperture of $\frac{3}{100}$ths, 0·03″, placed on the stage, the tube of the microscope being directed

* The formula calculated by the writer is there given:

$$f = \frac{d}{m + 2 + \frac{1}{m}},$$

from which it follows if $m$ be large,

$$m = \frac{d}{f} - 2,$$

where $m$ is the minimizing power and $d$ the distance between object and miniature.

to an open window disclosing a lovely foreground. Eighth O.G. A low eye-piece: power 400.

*Result.* Landscape dark and hazy. *The deficiency of light was most remarkable.* The same power was now got from a half-inch and D eye-piece.

*New result.* An exquisite picture brilliantly lit up, even the glittering foliage twinkling in the sunbeams, and the garden details were marvellously displayed. This difference is truly surprising. *Increased light and superb definition with diminished aperture and same power.*

To those who have not yet tried this method, and are desirous of becoming acquainted with the peculiar effects of over or under correcting their glasses as far as their construction is capable, I may be permitted to recommend the following commencement with these experiments:

A. *Experiment for spherical aberration.* Remove the front glass of the observing objective. Examine the miniatures of the illuminated globules.

B. Remove the internal glasses and replace the front only.

C. Replace all the glasses and close up the screw-collar for the mark "covered" if there be one.

D. Open the collar for "uncovered" position. At the last point for the first time the miniatures will begin to more nearly resemble their original.

The various changes of the appearances of the miniature, to the student of this department of optics, form a most interesting study.

These phenomena, varying from a mild brilliance to a gorgeous splendour, according to the quality of the light, and its mode of exhibition, may be greatly varied by the *reverse process,* viz. changing the lenses of the *miniature*-forming glasses, just as done with the observing. Some of old Pritchard's exquisitely minute lenses mounted by him give very charming miniatures. But these form no test of the quality of the objective in use, because their aperture is extremely limited.

I now may be allowed to describe what has not before appeared before the Society—the singularly beautiful phenomena displayed by *miniatures of sun-lit mercurial globules.*

It is well known that the surface of minute globules of mercury becomes more nearly spherical as they diminish in weight. The law of the curvature of these surfaces, dependent upon the specific attraction of mercury, has been investigated by Professor Bashforth, though not yet published; and according to this law they seem incapable of forming a perfect image by reflexion. But under direct illumination (not that wild bull's-eye side illumination of the globule hitherto employed) a minute spectrum of the sun may be descried. The use of a good prism as a reflector gives a pure beam greatly superior to a quicksilvered plane mirror. On one occasion the subtle delicacy of the interference-waves of light was thus revealed, for the whole of a series of magnificent phenomena was wholly obliterated by substituting a plane ordinary mirror for the prism.*

In the 'Proceedings of the Royal Society,' the appearances of a minute miniature of the solar disk, republished in this Journal, are so fully described, as dependent upon the quality and condition of the objectives employed, that I may be excused the trouble of recapitulation here. I proceed therefore to treat of the miniatures of illuminated globules of mercury. These may be dismissed with the general statement that the symmetry, beauty, and fineness of the diffraction rings are severe tests of the objective. And finally:

These rings are either wholly above or wholly below the focal point with a bright haze on the other side; or else equal and similar on both sides of the focal point; or altogether ill-defined.

The experimenter will soon ascertain for himself these various appearances. I therefore pass on to the effects of *obliquity.*

If the miniature-forming objective be slightly inclined, so that its axis has a few degrees of obliquity, a new order of effects display themselves of extraordinary beauty and complexity. Sun-lit globules suffice; but of course a minute focal disk of the sun in miniature gives more brilliant effects.

The conditions of over or under correction are beautifully seen, and the best possible adjustments still give very peculiar forms, some of which are shown in the accompanying drawings.

The most extraordinary of all is the curiously winged butterfly form: the cometary, double vase, and conical diffractions are well worth development. These very beautiful solar phenomena, as shown by a miniature of the illuminated globule, vary their exquisite forms according to the quality of the glasses, and according as they are over or under corrected, and according to

* The same destruction of beauty and symmetry occurs with a badly constructed screw-collar, whose movements decentre the component lenses: a very little error in this mechanical adjustment produces huge derangement. Better is a glass of lower aperture without such source of error, than a large aperture and loose adjustment. Indeed, a trifling difference of thickness in the glass cover introduces much milder error than the shake of the adjusting screw causing central displacements. Besides, if the glass be a little thicker than before, a slight extension of the draw-tube will compensate for thickness with extreme nicety. Lengthening the tube "over corrects," whilst a thicker glass introduces "under correction." In the case of telescopes with *fixed glasses*, I have never been able to persuade an optician to clean old glasses and reset them: it is almost always a total failure: nothing is more delicate and difficult than exactly centring the lenses of a telescope, an instrument which seldom magnifies more than 500 times. How much more difficult then must be a *movable* construction of the many lenses of a microscopic object-glass, which is used for much higher powers!

the obliquity at which the miniature is formed in the field of view by inclining the sub-stage. The surpassing beauty and perfection of these figures thus obtained by the magnified miniature of the mercurial solar star, render it probable that the obliquely illumined mercurial globule, viewed *directly* in close proximity to the front of the object-glass of the microscope, and placed upon the stage, is a very imperfect test; and the methods here described are submitted as possessing very superior delicacy and convenience. The perfection of the diffraction lines cannot be displayed at all by the old method.

*Eidola.* With such instances of wonderful variation of the spectra formed by the miniature of a sun-lit mercurial globule, we may well suspect that innumerable diffraction images may be developed:

(1) By obliquity of illumination.
(2) By erroneous correction of the glasses.
(3) By erroneous focussing.

I. An absolute knowledge of structure cannot be probably obtained by obliquity of illumination. Those structures which can only be thus seen are liable to distortion and misrepresentation.

*Example* (1). Display very fine gauze by oblique light and a low power a little out of focus, a complex structure is seen bearing very little resemblance to the reality.

*Example* (2). Treat the transparently-mounted eyes of an insect in the same way. The eidolic forms of delusion are endless. The diffraction effects are the most exaggerated when the illumination is most oblique.

II. *Example* (1). Illuminate perforated metal from behind in a darkened room; view an exquisite miniature of these perforations. The holes will appear enlarged; black dots take the place of apertures; haloes join haloes, and the images are altogether disguised and transformed according to the correction of the observing objective and the focal plane of vision employed.

*Example* (2). Fine copper wire gauze thus treated loses its apparent solidity. The meshes appear chequered with black dots, sharply defined, and the wires appear translucent. Thus in an unknown structure these false eidola might readily be mistaken for the true images. In this way a variety of forms having no reality start occasionally into almost tangible form and existence.

III. Erroneous focussing.

Microscopists are not always agreed in viewing an unknown object which is the correct focal plane. And it is just possible that different observers with the same instrument and adjustment view a different focal picture.

The most peculiar difficulty of all is the existence of two sets of false images, viz. the one above and the other below the focal reality.

This is seen beautifully in miniaturing (by the method now advocated) two distinct structures placed the one behind the other. In the case of the perforated metal the false images or *eidola* were made to exist in front of the true image. If therefore another structure were placed behind the first, the eidolic images of the one, as it were, will be mixed and confused with the real image of the other, and *vice versâ*.

The earnest microscopist, remembering the results he has obtained from miniatures of the sun-lit or lamp-lit globule, that the false image lies wholly above or below the best focal point according as the object-glass is over or under corrected, can have no difficulty in perceiving that also when viewing a duller object, a false image will similarly lie wholly above or below the real according to the corrections.

A corollary may be drawn from these principles, viz. that in duplex structures *the upper may be best seen by throwing the false image wholly below*, for then the eidola of the lower structure does not mingle with the true image of the upper. And conversely the lower structure is best seen when the false images are wholly above, for then the eidola of the upper does not mingle with the true image of the lower. In such cases under-correction best displays the upper and over-correction the details of the lower structure immediately subjacent.

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I now search among the Quadrata diatoms, and with a half-inch by Wray, of three lenses, I just discover a waviness like the early stage of Podura definition, of somewhat irregular pattern, two of these objects lying exactly over each other at right angles. Another half-inch by the same maker, with a C eye-piece, sharply defines these dark waves, and upon entering with a D the peculiar structure of these markings becomes delicately visible. A Ross $\frac{1}{4}$, 1851, resolves them; but the 1870 water-lens of Powell and Lealand's $\frac{1}{8}$th now exhibits a more remarkable pattern than the Angulata. On applying the finest powers of definition I possess, these markings disappear almost entirely, and the view is wholly confined to the upper surface; then alone clearly beaded.

I have just seen a beautiful example of illusion. On examining this evening a fine specimen of Möller's *Angulata pleurosigma*, I succeeded in finding* several instances where two diatoms overlay each other at a favourable angle. On carefully illuminating with a small achromatic pencil of direct rays, with a blue shade, I dis-

* With Powell and Lealand's dry eighth and C eye-piece about 800 diameters.

cerned very beautiful well-defined dark bars nearly parallel, with four rows of beads between each. The pattern is exquisite, and *varies with a change of the screw-collar*, but they look quite as real as the other structure. Here is a remarkable case of eidola. The images of the lower structure mix with those of the upper. These false images are best seen when the objects are overlaid at a slight obliquity. If they cross each other at a large angle, then these eidola change to large bright beading twice the natural size.

The transformation of appearances under the microscope by the application of a series of powers in order of power and merit, is a subject now worthy of special interest and development. For instance, the various plates and engravings of objects still extant, accumulated within the last fifty years, would upon patient investigation yield instructive results as to the causes of these optical misrepresentations, for such, with the extraordinary advances made in the goodness of modern glasses, we are now compelled to pronounce them.

I have the pleasure of recording here that just about four years ago I had the honour of first exhibiting to Mr. Slack many examples of eidola at my house.

The diagrams accompanying this paper represent various forms of interference phenomena developed by sun-lit mercurial globules, miniatured either by a 1½-inch Ross or a ¼ Ross. The sub-stage of the microscope employed has several movements, made by the writer about ten years ago, which enable the operator to manage the miniature as easily as an object on the stage, and also to give great obliquity. The examination of oblique pencils coming from the miniature, as to their conditions of achromatism or aplanatism, whether well or ill corrected, is thus rendered particularly easy and interesting. The figures given represent some of the many forms developed by different conditions of correction and obliquity.

I should state that the Figures 3–14 were copied from sun-lit globules at my house by Mr. Hollick, of 135, Tufnell Park Road, Holloway, N.

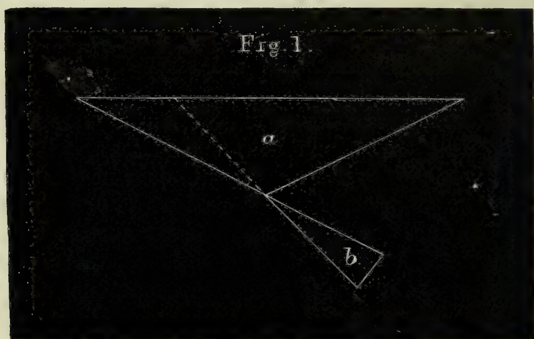
## III.—*On a Method of obtaining Oblique Vision of Surface Structure, under the Highest Powers of the Microscope.*

By F. H. Wenham.

(*Described before the* Royal Microscopical Society, *March* 3, 1875.)

If we take a thin semi-transparent object, such as a slice of melon, for the purpose of examining its structure, instinct directs us to hold it aslant to the light, and also to view it in an oblique direction. Experience has taught us that if we oppose the object to very strong rays, and look straight through it, the minutiæ of configuration will be obliterated by a flood of light. It is in this way that we see objects under the microscope, as their mounting lies square with the axis of the object-glass.

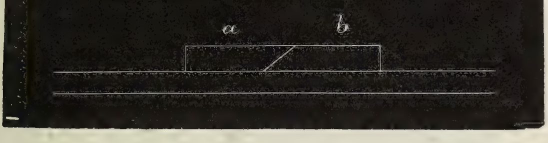
For resolving the striated structure of the most difficult tests, extreme obliquity of the illuminating pencil is requisite, and numerous adaptations to the microscope have been invented for this special purpose. Let angle *a*, Fig. 1, represent the aperture of the




object-glass, *b* the angle of the illuminating pencil for an object in the focus. It is evident that the portion of the object-glass brought most strongly into action under these conditions, is that opposite to the direction of illumination; the near extreme will be in comparative darkness, and it is questionable whether this gives any material aid, by collecting mere radiations necessarily feeble, from surface configurations. This favours the hypothesis that, besides oblique light, *oblique vision* is an important condition for discovering the closest striations on microscope tests, and that their development does not probably depend so much upon a large aperture as the degree of obliquity at which they are viewed. Oblique vision is thus obtained by the extreme marginal and *most imperfect* image-forming rays of the object-glass. The best or central ones come so little into action, that they even mar the result; for, if obscured by a central stop, the exterior light is not overpowered, and greater distinctness in the appearance of the structure is the consequence. The closeness of the high powers to the covering glass will not permit the slides to be tilted, to an extent sufficient to cause any appreciable difference in the appearance of the object.

The following arrangement will produce an extreme obliquity of vision obtained with the axial pencil of the object-glass. *a*, Fig. 2,

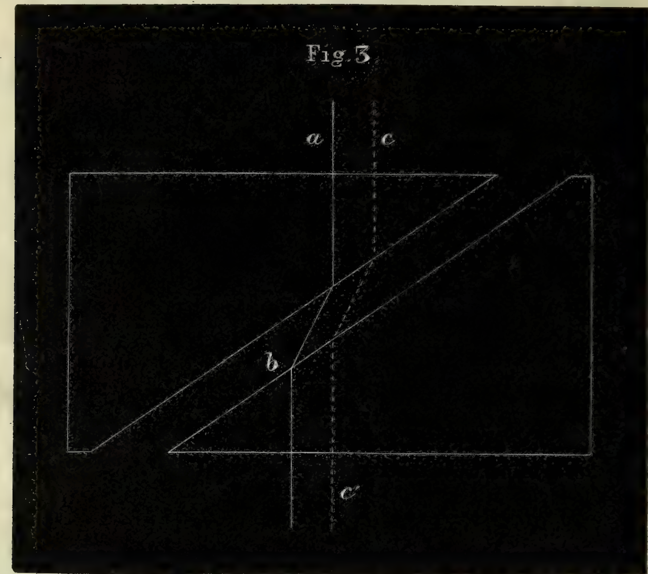
Fig. 2.




is a slip of glass about $\frac{4}{10}$ wide, ground and polished off to an angle. Objects to be mounted, such as diatoms or lepidopterous scales, are scraped up with the knife-edge, so as to be distributed thereon along the sloping plane. Those situated near the edge may be viewed with the highest powers, as the glass is of course thinner here than any cover. The thickness of the remainder of the prismatic slip is of no consequence, and it may be of the same gauge as an ordinary thin slide. The slip is tacked on to a $3 \times 1$ slide with a dot of balsam or cement. Another similar slip, *b*, is then pressed endways against it, so as to lay the objects flat between the two inclines. The lower prism is necessary; for without it, a deal of offensive colour enters the object-glass from the decomposition of the transmitted light. This is recomposed or neutralized by the under prism, which also greatly increases the obliquity of the illuminating ray by refracting this to the same angle as that of vision, from the deflection of the axial ray of the object-glass.

The conditions are as follows. *a*, Fig. 3, is the central ray of the object-glass. This ray, after leaving the upper inclined surface, is refracted in the direction *b*. If the surface is inclined near 40°, the upper side of the object will be viewed from a small elevation of between one and two degrees, according to the refraction of the glass; conversely, an illuminating ray thrown up axially with the microscope will strike on the object at the same degree; both the visual and illuminating angles will therefore be similar, as represented by line *c*, *c'*.

The degree of inclination of the facets of the prisms for dry objects should be less than 40°; for on holding before a flame a slide having this angle, and tilting it slightly, the width of the junction of the prisms appears as a dark band impervious to light—the effect of total reflexion. About 35° is therefore more suitable for objects mounted dry. If balsam is run between the inclines, of course total reflexion is eliminated, and refraction nearly so, and we then only see the object at an angle *the same as the incline;* therefore for objects in balsam 45° would be preferable.




In using slides with these prismatic or inclined mounts, it must be borne in mind that nearly every object lies under a different thickness of glass, according to the distance from the keen edge; therefore, having selected a suitable one, the objective is to be carefully adjusted to give proper definition, and a thickness of glass over the scale may probably be found that will best suit its correction. For dry mountings, the object-glass must be used dry, as water would run in and spoil the object. If this is in balsam, of course the immersion system can be employed.

In the dry slide, a direct illuminator is used—such as the ordinary achromatic condenser; for, as shown by the diagram, axial light becomes that of excessive obliquity on reaching the object. At first sight, it might be urged that colour would arise from this extreme refraction or deviation of the axial ray of the object-glass; but such is not the case, because *distance* must be an element in chromatic dispersion or separation; and as the object is on the refracting surface, the outline is as free from colour as if seen through the usual covering glass. Nor is there any difference in the appearance, whether the object is adherent to the face of either the upper or lower prism.*

* As there is no chromatic appearance, dense flint glass might be used for the prisms, as this medium having a higher refractive power, more acute angles would give the required obliquity of vision.

These prismatic mounting slips may be cheaply and easily made by any ordinary glass-grinder, in the following manner: Thin well-polished sheet glass (such as is used for microscope slides) is cut into pieces, three-quarters of an inch long by four-tenths wide. Eight or ten of these are cemented together with hard Canada balsam, and their step-like projecting edges adjusted against a bevel, set at the desired angle, say 35°, as shown by Fig. 4. They must

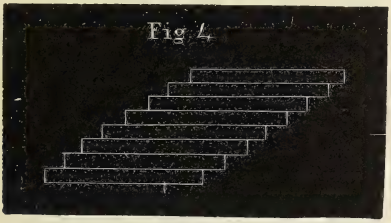


Fig. 4.

be pressed in very close contact, as it is important to have the edges worked fine and clean, and any thickness of balsam stratum between them will prevent this, for during the work close edges mutually protect each other. Having got a sufficient number of blocks together in this way, any quantity of them are cemented on to a runner or metal plate, as in Fig. 5, with the usual cement of

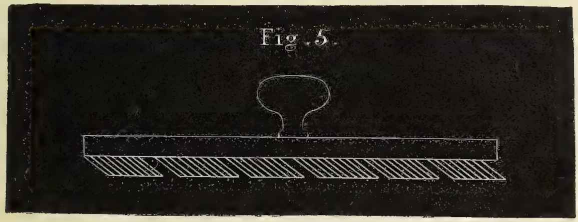


Fig. 5.

pitch and wood ashes. They are then ground and smoothed on a flat metal lap, till all the steps are gone and keen edges shown, and are then finally polished. Thus hundreds may be made at a time.

Discussion as to the effects of this suggestion of mounting for oblique vision, and its probable value, is at present premature. The author has done but little more than enunciate the optical principle. It may, however, be stated that the object selected should either lie in a parallel direction with the faces of the incline, or at right angles to it. If it occupies an intermediate position, rather a curious confusion of cross striation results. Oblique mounting affords a very marked means of discriminating whether the ribbings are on the upper or lower side of the scale, two of which adjoining sometimes showing the series in one very distinctly, and in the other confused, simply because they are relatively inverted.

One of the first objects observed was the scales of the common *Lepisma saccharina* (the active "window fish" of our childhood). The late Richard Beck announced that the markings on the lower surface of this scale consisted entirely of fan-like radiations starting from the quill—a structure by some considered illusory; but with axial vision thus refracted obliquely, the accuracy of this careful observer was confirmed, as these radiating lines were shown very distinctly, as if graven in black and white.

## IV.—*On the Connection between Fluorescence and Absorption.*

By H. C. Sorby, F.R.S., &c., President R.M.S.

Though the relation between fluorescence and absorption has already attracted a good deal of attention, there are some important facts which appear to be often imperfectly known. I have been surprised to find that even some of those who have paid considerable attention to such subjects have so far misunderstood the question as to suppose that the light of the fluorescence consists of rays which are as it were reflected by the solution, and do not penetrate through it, so that the spectrum of the fluorescence would show a bright band in the same place as some dark band seen in the spectrum of the transmitted light. This is certainly an error. A much nearer approximation to the truth is the conclusion arrived at by Lubarsch, who has recently published a paper on this subject in Poggendorff's 'Annalen,'* of which a good outline is given in 'Der Naturforscher,' 1875, Feb. 6th, p. 52. It is the appearance of this paper which has induced me to describe some of my own observations. He gives a table showing the connection between the fluorescence and absorption of eight different substances, all of which show that the spectrum of the light of fluorescence extends some distance on the red end side of the principal absorption band in the spectrum of transmitted light, commencing very nearly at that part which corresponds to the centre of the absorption band, or maximum point of absorption. He expresses this law by saying that "the spectrum of fluorescence of a substance can never contain rays which are more refrangible than those which are most readily absorbed by a very dilute solution." To express this in wave-length phraseology, I should say that if a fluorescent solution gave by transmitted light a spectrum having a well-marked absorption band, whose centre corresponded to light of some particular wave-length, as, for example, 600 millionths of a millimeter, the spectrum of the light of fluorescence would extend from nearly that point towards the red end, so as to include a band of light of wave-lengths varying from 600 to some other greater length, according to each particular substance. This is certainly such a common fact, that for a long time I was disposed to believe it to be general, but I have now had the opportunity of examining a greater number of fluorescent substances, and find that there are very decided exceptions. It appears to me important to know what is the true state of the case; since, as I shall show, the study of the spectra of fluorescence may often be of great value in deciding whether any solution contains a number of different substances in solution, or only one.

In studying the spectra of fluorescent solutions, I often find it

* Vol. cliii., p. 420.

convenient to examine them in the small glass cells used in all my chromatological inquiries, cut from barometer tubes, having a length of $\frac{1}{2}$ inch and an internal diameter of $\frac{1}{8}$th inch. The spectrum of the light of fluorescence depends a good deal on the kind of illumination. Of course transmitted light must be entirely excluded, and the solution illuminated by strong light thrown perpendicular to the line of vision. For this purpose I use a bull's-eye condenser, which for the study of some substances is better if made of quartz, but for very many may be of glass. I then concentrate direct sunlight on the uppermost part of the solution in the cell, so as to get very bright fluorescence, and at the same time to avoid letting the light of fluorescence pass through any such material thickness of the solution that it would lose any portion by simple absorption. The effect of this may easily be seen by concentrating the sunlight on the lower part of the cell, and allowance made for any slight action due to this cause. Having observed the spectrum of the light of fluorescence, it is easy to examine that of transmitted light, by removing the condenser, screening off the side light, and reflecting light up through the solution by means of the usual mirror below the stage of the microscope.

Whilst thus describing the general phenomena of fluorescence, it would, I think, be well to allude to what looks like it, but is entirely spurious. There are some cases in which we appear to have a perfectly clear coloured solution with strong fluorescence; and yet the substance is not really dissolved, but disseminated through the liquid in such extremely minute particles that they do not subside, and can be seen only by high magnifying powers, moving about tumultuously in currents due to slight alterations in temperature. On illuminating such a liquid by concentrated sunlight, passed through a solution of some salt of didymium, the spectrum shows all the bands due to this substance; whereas, if the liquid were quite clear and possessed genuine fluorescence, no trace of the didymium bands would be seen. The existence of some true fluorescence along with much spurious, due to minute disseminated particles, may be recognized by the presence of some of the didymium bands, and the comparative absence of others from those parts of the spectrum where the light of true fluorescence exists.

By adopting these methods of observation, I have found that in the case of some substances the light of fluorescence does contain rays of greater refrangibility than those most readily absorbed by a dilute solution, and extends from the red end a little beyond the centre of the main absorption band. This is, however, only when the illumination is strong and near the upper surface, and, if weaker or lower down, so as to increase the effect of absorption, the spectrum of the light of fluorescence extends only to the centre of the main absorption band, or may even stop considerably short of the centre, and be bounded by the limit of absorption towards the red end. We may thus easily explain why there is a slight difference in the case of different substances; since, if the intensity of absorption be greater in proportion to the intensity of fluorescence, all other circumstances being equal, the limit of the light of fluorescence will lie more towards the red end than the centre of the main absorption band, than if the absorption be relatively weaker. The result is that in the case of the substances I have examined, the average limit of the light of fluorescence lies a very little on the red side of the centre of the main absorption band, which is in exact agreement with the observations of Lubarsch, t.ough there is a very decided variation in different cases not shown by the substances named in his published list.

The knowledge of this law is very important in studying such mixtures of different colouring matters as are obtained in examining animals and plants. For example, when the superficial membranous coloured layer of some such species of fungi as *Russula nitida* and *vesca* is digested in alcohol, a purple fluorescent solution is obtained, which gives a spectrum with a well-defined absorption band, having its centre at wave-length 554 millionths of a millimeter, whilst the spectrum of fluorescence extends far beyond that towards the blue end, up to wave-length 440. This led me to conclude that the coloured solution was a mixture of a purple non-fluorescent substance with one that is pale yellow and fluorescent; and after trying various methods, I at length succeeded in separating two substances of exactly that character. If it had not been for such theoretical considerations, I might never have suspected that there was any such mixture. Another admirable illustration is furnished by the beautiful purple fluorescent solution obtained by keeping *Oscillatoriæ* in water, described by me in this Journal, vol. iii., p. 229. This gives a spectrum with two well-marked absorption bands, having their centres at wave-lengths 620 and 569. The spectrum of fluorescence shows two bright bands, having their centres at 647 and 580, and their limits towards the blue end at 632 and 571, which bright bands are thus manifestly related to the absorption bands, as though they were due to two independent fluorescent substances mixed together. Now by heating this solution to the point of the coagulation of albumen, a pink substance is deposited, and the clear solution gives a spectrum with only the band at 620, thus proving that the original solution is a mixture—a conclusion borne out by many other facts. In a similar manner I was led to find that the higher classes of plants contain a mixture of two different kinds of chlorophyll (blue chlorophyll and yellow chlorophyll), by observing that the spectrum of the light of fluorescence of the solution showed two narrow red bands related to two absorption bands, and that on exposure to the sun one of these bands rapidly disappeared, on account of the unaltered yellow chlorophyll being decomposed by sunlight far more rapidly than the product of the action of acids on blue chlorophyll.*

Though a knowledge of this connection between fluorescence and absorption may thus often serve to indicate whether any coloured fluorescent solution is or is not a mixture, yet I must say that I do not think it furnishes conclusive evidence; since I have found a few cases in which what appears to be a single substance gives a spectrum with *two* or more bright bands of fluorescence. The most decided case is that of the green colouring matter found in the annelid *Bonellia viridis*, described by me in a paper in the current number of the 'Quarterly Journal of Microscopical Science' (April, 1875). This substance, which I have called *bonelleine*, is green and has strong fluorescence, and the spectrum of the light of fluorescence of a solution in alcohol shows two bright bands,—one red and the other green,—whose centres are at wave-lengths 643 and 588, and their limits towards the blue end at 632 and 582, corresponding to two well-marked absorption bands, having their centres at 636 and 584, the latter being much less intense than the former. One might therefore suppose that it was a mixture, but on adding an acid the spectrum is materially changed, by the removal of some bands and the development of others, so that there is only one well-marked absorption band at wave-length 614, and a single red band of fluorescence, having the limit towards the blue end at 612, as though it were a single substance. Independent of the two bands of fluorescence, there is nothing to lead us to suppose that this colouring matter is a mixture.

Similar facts are met with in the case of the product of the action of acids on blue chlorophyll, but the limit of the band of fluorescence in the green is a considerable distance towards the red end from the connected absorption band, which agrees with the general law, when, as in this case, the fluorescence related to the band in the green is comparatively weak. The yellow substance obtained from some Aphides, which I have named *aphidiluteine*, gives fluorescence with three bright bands, as described in my paper in the 'Quarterly Journal of Microscopical Science,' xi., 1871, p. 352.

My conclusions therefore are, that though, on the average, the law adopted by Lubarsch is approximately correct, yet there are important differences in individual cases, depending on the relative intensity of the absorption and fluorescence, and in some instances it is necessary to modify the law very materially, so that it may express the connection between the fluorescence and more than one dominant absorption band.

* 'Proceedings of the Royal Society,' vol. xxi., p. 453.

## NEW BOOKS, WITH SHORT NOTICES.

*The Micrographic Dictionary.* Third Edition. Edited by J. W. Griffith, M.D., Professor Martin Duncan, M.B., F.R.S.; assisted by the Rev. M. J. Berkeley, M.A., F.L.S., and T. Rupert Jones, F.R.S. Parts XVII., XVIII., XIX., and XX. London: Van Voorst, 1874.

(Second Notice.)

We much regret that our remarks on the subject of the above work were "crushed out" of the last number of this Journal, as it necessarily makes our present observations more brief. We shall now run shortly over the Nos. XVII., XVIII., XIX., and XX.,—the last two being merely one number in reality, containing the remainder of the plates, and the various portions, as the preface and title-page, which are usually reserved for the final portion of a work which comes out in serial parts. And in the first place we must observe, that it seems to us the authors and their assistants have taken more care of the articles as they have approached the completion of the work, so that our criticism is less marked than it was in our review of the earlier parts. *Imprimis*, we have noticed several short paragraphs on the subject of Herr Haëckel's work. These have been all new, and though brief, yet they are clear and to the point. Such are, for example, the several words *Protista*, *Protogenes*, *Protohydra*, *Protomonas*, *Protomyxa*. Protozoa is not lengthy, nor is it bad, but we think the writer has been much to blame in dismissing the important subject of bibliography with the words "works on Comparative Anatomy." He must have known very well that the great majority of works on the subject referred to, contain nothing in the shape of observations upon the Protozoa; and it would not have occupied further space than he has occupied with the misleading reference he has given, to have referred to the best, and indeed the only book on the subject, the excellent 'Manual of Protozoa,' by Professor J. Reay Greene. Another point on which we have to take the authors to task is their reference to the 'Microscopical Journal.' Such a term might have been employed with indifference in the older editions, when there was but one periodical devoted to microscopy. But now it is absolutely inexcusable. For how is anyone to know whether the 'Monthly' or the 'Quarterly' is the Journal referred to? It is exclusively the fault of one of the editors, for the other, when he cites either Journal, is always careful to distinguish between the two periodicals.

As we said in our former notice, the Fungi and Algæ are excellently done, and so we must say of the last three numbers of the work. As a type of this we may take the article on *Puccinia*, which gives an excellent account of these peculiar fungi. *Herepathite*, too, is a good paper, describing at length this peculiar salt and its polarizing properties. *Raphides* is not so good a paragraph as we should have expected. Much work has been done on this point in our own country of late, and we should have expected that the 'Micrographic Dictionary' would certainly have taken notice of it. *Rhizopoda* is a short but nevertheless a good contribution, and Rock Structure is also very fully given. However, we think that the writer might have given less space to his article on the latter subject, and withal have composed a fuller paper. For instance, we think he should have avoided the quotations which he has made, and he should have read Forbes' paper to which he has referred. By doing so he would have been able to attempt something better in the shape of classification than he has given. *Rotation* or cyclosis is a good paragraph; but *Rotatoria* is certainly too short a contribution for the 'Micrographic Dictionary.' We had expected a tolerably long paper on the wheel-animalcules, but we are sadly disappointed, and we cannot urge a reason for this treatment of a class which is perhaps more than any other essentially the microscopist's. Assuredly it is a mistake. *Salivary glands* is fairly done, and the notice contains allusion to Pflüger's remarkable discovery (?) of the nerves, which run, so to speak, into the very secreting cells of the gland. *Sarcina*, too, is a good contribution, but we hardly agree with the writer in his supposition that when it is formed in any quantity in the stomach, it is perfectly harmless. Is it not the case that many instances of gastric catarrh arise solely from the presence of this alga in the stomach? The *Scales* of insects is, we think, but poorly done, and Mr. McIntire's work is not at all fully given. *Shell* is fairly done, and so is *Silica;* in this instance the writer has evidently considered carefully the researches both of Messrs. Slack and Sorby. *Spermatozoa* and *Sphœroplea* are excellently given, and will well repay perusal. The articles on *Spiral structures* in plants; *Spores; Staining tissues; Stomata; Vaucheria; Volvox*, and *Vorticella*, are all good; they are, as a rule, modern, concise, and to the point. But, on the other hand, Teeth is a paper by no means so fully dealt with as it merits; indeed, much of this subject is totally left out. But our most severe remarks have to be made upon one, and only one contribution, that on the spinal cord. This is an eminently bad paper, which by no means gives even a reflex of the splendid work achieved, both at home and abroad, on this important subject. It is really the only exceedingly badly executed portion of the entire work; but that it by no means indicates the great advance that has been made in this subject is but too evident. It were better to say nothing of the cord than to leave it and the brain in the condition in which they have been disposed of by whoever has had charge of this part of the Dictionary. And now we have to speak of the paper on *Test objects*, which, though it precedes some of the others, we have left to the last. On the whole, we look on this contribution as a very good one, for the remarks are terse and to the purpose; and there is a good deal of practical information on the subject of objectives (though not on immersion ones), which will prove immensely useful to the student who is commencing to work with the microscope. Still we must observe that the writer appears to us to state opinions in regard to the optical quality of penetration which are not universally held. Of course we may have misinterpreted his observations, but so far as we have been able to gather from his remarks (and in regard to angular apertures we thoroughly agree with him) we do not see the force of his opinions on the subject of penetration. To us it appears to be an optical defect, but one which is to a certain extent needed in anatomical work, or indeed in any observation where one wishes for more than the mere *superficial* detail.

However, it will, we hope, be seen that the 'Micrographic Dictionary' is by no means the very imperfect work which it was represented to be by some critics; and though we could have wished for an improvement in the plates and in some of the articles, still we are bound to offer our best thanks to the authors for what we consider on the whole a successful re-issue of a very elaborate work on the wide field of microscopical research.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*How does the Amœba Swallow its Food?*—According to the recent researches of Professor Leidy (in 'Silliman's American Journal,' Feb., 1875), some important facts have been arrived at. This observer remarked that he had supposed that the Amœba swallows food by this becoming adherent to the body, and then enveloped, much as insects become caught and involved in syrup or other viscid substances. He had repeatedly observed a large Amœba, which he supposes to be *A. princeps*, creep into the interstices of a mass of mud and appear on the other side without a particle adherent. On one occasion he had accidentally noticed an Amœba with an active flagellate infusorium, a Urocentrum, included between two of its finger-like pseudopods. It so happened that the ends of these were in contact with a confervous filament, and the glasses above and below, between which the Amœba was examined, effectually prevented the Urocentrum from escaping. The condition of imprisonment of the latter was so peculiar that he was led to watch it. The ends of the two pseudopods of the Amœba gradually approached, came into contact, and then actually became fused, a thing which he had never before observed with the pseudopods of an Amœba. The Urocentrum continued to move actively back and forth, endeavouring to escape. At the next moment a delicate film of the ectosarc proceeded from the body of the Amœba, above and below, and gradually extended outwardly so as to convert the circle of the pseudopods into a complete sac enclosing the Urocentrum. Another of these creatures was noticed within the Amœba, which appeared to have been enclosed in the same manner. This observation would make it appear that the food of the Amœba ordinarily does not simply adhere to the body, and then sink into its substance, but rather, after becoming adherent or covered by the pseudopods or body, is then enclosed by the active extension of a film of ectosarc around it.

*The Microscopic Anatomy of Sponges.—Infusoria* has been very fully given with abundant illustrations in the February number of the 'American Naturalist,' by one of the editors, Mr. A. S. Packard, jun.

To these pages we would commend all who desire to know something of our recent knowledge of these important groups of animals. We can only state the results arrived at, which are as follow in the case of the Infusoria. The writer says: "There are, then, two modes of development among the Infusoria (Ciliata): 1. By fission. 2. By production of internal ciliated embryo arising from eggs. We have, then, for the first time among the Protozoa, if the observations of Balbiani be correct (though this is denied by good observers), truly sexual animals, producing true eggs and spermatic particles. The same animal reproduces both by fission and by the production of ciliated embryos. Most of them before producing embryos undergo fission. This is comparable to the alternation of generations among the Hydroids, Aphides, &c." In the same manner he gives the following summing up as regards the stages of the life-history of sponges: 1. Fertilization of a true egg by genuine spermatozoa; both eggs and sperm-cells arising from the inner germ-layer. 2. Total segmentation of the yolk, or protoplasmic contents of the egg. 3. A ciliated embryo. 4. A free-swimming "planula"-like larva, with two germ-layers, not, however, originating as in the true planula of the acalephs. The planula becomes sessile, spicules are developed in the hinder end of the body, afterwards a gastro-vascular cavity appears, constituting the—5. Gastrula stage. 6. A mouth and side openings appear and the true sponge characters are assumed.

*The Power of Motion which Diatoms possess.*—In a recent paper before the Academy of Natural Science of Philadelphia ('Proceedings,' p. 113), Professor Leidy made some remarks on the moving power of Diatoms, Desmids, and other Algæ. While the cause of motion remains unknown, some of the uses are obvious. The power is considerable, and enables these minute organisms, when mingled with mud, readily to extricate themselves and rise to the surface, where they may receive the influence of light and air. In examining the surface mud of a shallow rain-water pool in a recent excavation in brick-clay, he found little else but an abundance of minute diatoms. He was not sufficiently familiar with the diatoms to name the species, but it resembled *Navicula radiosa.* The little diatoms were very active, gliding hither and thither, and knocking the quartz sand grains about. Noticing the latter, he made some comparative measurements, and found that the Naviculæ would move grains of sand as much as twenty-five times their own superficial area, and probably fifty times their own bulk and weight, or perhaps more.

*How Pigment-cells are influenced by the Nerves.*—We are glad to see that M. Pouchet, who published one of his first papers on the subject in these pages, has been awarded the Montyon prize by the French Academy for his researches. The following abstract of the Commission's report appears in the 'Medical Record,' Jan. 13, 1875:

The memoir is in two parts, one purely anatomical, the other physiological. The former contains new facts, but it is the latter that has chiefly engaged the attention of the Commission.

From this point of view the work is almost without precedent.

It was widely believed, indeed, that the skin of certain fishes took the colour of the bottom on which they lived; but exaggeration often deprived statements to this effect of their value.

In 1830 certain experiments on the subject, by Stark, were described in the 'Edinburgh New Philosophical Journal.' Putting fishes in vessels enclosed in dark or in bright cloth, he perceived that the colour of the animals changed in the same direction, becoming darker or brighter; but he abstained from giving an opinion as to the internal conditions of production of this phenomenon.

It has been shown by physiologists, further, that the colour of the frog's skin may be modified from various causes, section or stimulation of the nerves, various conditions of habitat in water or air, &c.; but it was pretty generally agreed that these changes might be explained by disturbances in the circulation, due to the various modes of treatment, and bringing about in their turn a change in the state of dilatation or of contraction of the pigmentary cells. The special feature of M. Pouchet's experiments is that they show the pigmentary cells or chromoblasts to be in direct and immediate dependence on the nervous system; so that they must be added to the list of anatomical elements, in which nervous excitation is transformed into mechanical work. The nerves determine contractility of the chromoblasts, as well as that of striated fibres of voluntary muscles and fibre-cells of the muscles of vegetative life.

The author first verifies the fact that certain kinds of fish, such as young turbots, placed successively in water on bright and dark bottoms, show very rapid changes of colour, or tone, produced by dilatation or contraction of the chromoblasts charged with dark pigment, more especially those having the *rôle* of changing to brown, or abating more or less the proper colouration of neighbouring parts. As there are also, however, contractile cells charged with coloured pigments varying from red to yellow, it may happen that, by the state of relative contraction of these different elements, the shade of the animal may be modified in a certain measure.

If in most of the species presenting these changes it is difficult to make out the influences which cause them, there are other species in which the determining conditions may be ascertained with ease. Let a turbot, measuring only twelve to fifteen centimeters rest for some minutes over a light bottom, such as one of sand, and it becomes pale in unison with the sand; let it rest, on the other hand, over a rocky bottom, and it grows brown like that. One has only to contrast two animals placed under such conditions, to ascertain that the brightness of their colouration corresponds exactly to that presented by the colour of the two bottoms. We may thus produce indefinitely, in the same animal, a considerable change of colour, which does not require more than twenty to forty minutes for its production, and is sometimes much more rapid.

M. Pouchet calls this power which the animal has, its chromatic function. It is subject, within variable limits (according to species), to the influence of the nervous system. The colour of several species of fish was observed to change when they were irritated, or on simple sight of an external object. And since the changes depend on the greater or less absorption of light by the bottom, they must be regarded as true reflex acts, having their centre in the brain, and their starting-point in retinal impressions. The fundamental experiment in M. Pouchet's work is that in which he suppresses the chromatic function by removing the ocular globe, or simply cutting the optic nerve. The blinded animal loses its power of changing colour according to the bottom.

Having thus first established that the dilatation or contraction of the chromoblasts does not depend on local conditions produced in these elements at that point in the organism which they may occupy (as was previously thought), but is determined at a distance, by antecedent change of the elements of the central nervous system, it remained to find out by what route this transmission takes place, from the brain to the pigmentary cells of the periphery.

The author made various sections of nerves, and he has demonstrated that the spinal cord is not the nervous conductor, nor yet the lateral nerve, to which it seemed natural to attribute a *rôle* in this function. The trigeminus, on the other hand, has a direct action. Turbots taken from off a brown bottom, and, after section of the trigeminus, placed in a basin with sandy bottom, grow pale over their whole surface, except the face, which remains shaded, as if covered with a mask. Section of the spinal nerves gives results no less distinct. It confirms what has been said about the negative *rôle* of the cord. For the section of spinal nerves to influence the chromatic function, it is necessary that it be made below the point where they receive the thread of the great sympathetic. The result is a transverse dark band marking the region under the influence of mixed nerves receiving the cut sympathetic fibres.

It is, then, the great sympathetic which governs the chromatic function. It forms the route of transmission for the influence going from the brain to the cutaneous chromoblasts. The disposition of this nerve in fishes, lying as it does in the same osseous canal with the principal artery and the principal vein of the body, does not allow of the section being made with advantage directly, as grave disorders ensue, which spoil the experiment. But the result of section of the mixed nerves, as above, sufficiently attests the influence in question.

The author has not confined himself to fishes; he has shown that the chromatic function also exists in some articulata; more particularly in *Palemon serratus.* It may be demonstrated in the way that has been indicated for fish. Removal of the eyes, also, suppresses the function; at least till these organs are regenerated. But M. Pouchet did not succeed in finding what route the nervous influence took in crustacea, from the cerebral ganglions.

M. Pouchet's observations establish, it will be seen, a series of new facts, which have, moreover, a remarkable character of generality. They open up an unexplored region, by revealing a series of reflex actions, of which the retina is the starting-point, and which irradiate over the whole system.

These researches were prosecuted at Concarneau, in the laboratory founded by M. Coste, which has already yielded various important scientific results.

*Effects of Concentration on the Movements of White Blood-corpuscles.*—A very interesting paper recording experiments on the blood of the frog and newt, is that of Herr R. Thoma, which appears in Virchow's 'Archiv.'* It has been abstracted in the 'Medical Record' of December 30, 1874, by Dr. W. Stirling. Herr Thoma divides his experimental inquiries into three sections:

1. *Influence of the Concentration of the Surrounding Fluid on the Amœboid Movements of the Colourless Blood-corpuscles removed from the Body.*—Blood of the frog, placed in a gas-chamber, and from which water was removed by the passage through it of a stream of air, showed that, in the portion of blood poor in water, the number of round, motionless, colourless corpuscles surpassed considerably the number of those showing changes of form. In blood in which the quantity of water was increased, the greater number of the colourless corpuscles showed the branched forms, such as are produced by the flowing movement of protoplasm. These corpuscles which adhere to the cover-glass are more spread out, show clearly three or four nuclei, and bear more richly branched processes, oftener contain vacuoles, and show more lively changes of form than those floating free in the fluid. This is, without doubt, due to the action of the surface, and is produced by strong adhesion of the body of the cell to the surface of the glass. This property also belongs to a series of other solid bodies, and also to the intima of the vessels. The white blood-corpuscles become more sluggish in their changes of form with increase in the concentration of the fluid, and the greater number change into rounded cells, which sometimes have fine processes on their surface. This is not due to death of the corpuscles, for on increase in the quantity of water they again become lively in their movements, and resume the properties of freshly-drawn white blood-corpuscles. These observations were made in the blood of *Rana temporaria* and *esculenta*; but the same is also true for that of *Salamandra maculosa* and *Triton cristatus*, and also for that of warm-blooded animals; at least for the guinea-pig and dog. Under the influence of water, the contents of the white corpuscles may be increased four times, and this can only be regarded as an imbibition phenomenon.

2. *Experiments on Colourless Corpuscles circulating in the Blood*, produced by injection of water into the circulation of the frog.—Besides unchanged colourless corpuscles, there are a large number which show forms such as can be produced in blood under the influence of water outside the body. Those which lie upon the walls of the vessels exhibit very lively changes of form. In an opposite experiment, frogs were exposed for several days to evaporation. Microscopic observation showed that in the tongue, under these conditions, no amœboid movements were to be observed in the corpuscles circulating in the blood, and also in those touching the walls of the vessels; and the injection of a 3 per cent. solution of common salt into the veins showed that increase of the quantity of salts acted quite in a similar

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manner to the regular concentration of the blood by the evaporation of water from the surface of the skin.

3. *Experiments on the Wandering Cells in Living Tissues.*—The question was, whether colourless corpuscles which have wandered outside the vessels are influenced in a similar manner by differences in concentration of the tissue-fluids. The cells which have wandered out into the tissue show the lively amœboid changes of form and place, whilst, by infusion of a 3 per cent. salt solution and evaporation from the skin, the amœboid movements of the wandering cells becomes lower and very soon cease altogether. The same was observed with a 1·5 per cent. solution, the colourless corpuscles becoming round and shining, and changes of place could no longer be observed of them; whilst with a 0·5 per cent. solution the changes both of form and place were very lively. Irrigation of the frog's tongue with salt solution of various strengths also produced important changes in the calibre of the blood-vessels and therefore on the rapidity of the blood-current. Under irrigation by a 0·5 per cent. solution, a very plentiful out-wandering, specially from the small veins, takes place, while in the same organ with a 1·5 per cent. solution, the wandering out of the colourless corpuscles is completely suppressed. This solution acts first on the blood-vessels, producing a pronounced dilatation of the arteries, and therewith an acceleration of the blood-current in the arteries, capillaries and veins, as Wharton Jones had already proved, and which, as H. Weber, F. Schuler, Buchheim, Vierordt, &c., showed, depends essentially on the diffusion of the blood-plasma with the salt solution. The acceleration of the blood-current is so considerable, that the venous current takes on part of the characteristics of the arterial one. Specially, the marginal position of the colourless corpuscles disappears. The second effect of the 1·5 per cent. solution of common salt is its influence on the changes of form and place of the colourless corpuscles.

## NOTES AND MEMORANDA.

**Notes on Recent Objectives.**—The following observations are by Dr. J. A. Thacker, the editor of the Cincinnati 'Medical News' (January, 1875), and have a certain interest for our readers: "Dr. J. G. Richardson, Microscopist to the Pennsylvania Hospital, read a paper before the Biological and Microscopical Section of the Academy of Natural Sciences, entitled 'Notes on the Performance of Two One-Fiftieth Objectives.' One of the glasses was an immersion, by Tolles, and the other a dry one, by Powell and Lealand. Whether the object of the comparison was to determine the relative merits of the work of the makers is not stated; but if it were, certainly the mode adopted was very improper. An immersion lens should be compared with an immersion, a dry one with a dry one. It is as unfair to compare a wet with a dry objective, as it is to compare an eighth with a quarter. The drop of water gives an immersion lens a larger angle of aperture and more light, besides other advantages, and it has therefore, other things being equal, greater resolving power. The Doctor says that his skill, or unskilfulness, as a microscopist, were all constant factors, so that the superior performance seemed certainly due to the superior qualities of the higher power lens. Now, certainly, no greater fallacy could be than such an hypothesis. In unskilfulness *chance* is constantly occurring to deceive, and cannot be provided against. We think we can almost say, without exaggeration, that in the majority of instances an unskilful microscopist will select the poorer of two objectives as the one *he can do the most with.* And this is not very strange when we come to consider; for the finer a lens is, the more perfect must be the conditions for it to perform well, and the greater is the skill required in its management. A tyro, who is filled with admiration with the conduct of a French commercial objective that a microscopist would regard of no value whatever, would probably not be able to see anything with a fine Powell and Lealand $\frac{1}{16}$th, so perfect must be all the conditions in order for it to disclose its exquisite powers. Dr. R., in comparing the resolving powers of the two $\frac{1}{50}$ths, found that the transverse striæ of *Surirella gemma* were easily shown, but that the finer longitudinal striæ were not distinctly visible by gaslight. On our part, we never find any difficulty in bringing into view the *transverse striæ*, with any ordinarily good quarter or even half inch, and we have no trouble in showing, by lamplight, the longitudinal lines with our Powell and Lealand's $\frac{1}{16}$th, Verick's No. 10, Gundlach's German No. 7, Seibert and Krafft's $\frac{1}{6}$th, &c., &c. 'Under the employment of monochromatic sunlight,' the Doctor goes on to say, 'these faint markings, which Frey says are "only to be mastered with much pains," are clearly visible, even under Wales' $\frac{1}{25}$th.' Now, we are not the owner of a Wales' $\frac{1}{25}$th, but we are of an eighth by him, and from its performance we do not think that with a little skill in its management there would be any difficulty in the $\frac{1}{30}$th mastering the longitudinal striæ without the aid of monochromatic sunlight. Not only does Seibert and Krafft's $\frac{1}{30}$th bring out these faint lines without the aid of the blue cell, but even their $\frac{1}{6}$th does it with proper amplification. In testing with the Podura scales, the result of his comparative trials with central light was that the definition of the note-of-exclamation-marks afforded by the Tolles' $\frac{1}{50}$th immersion was somewhat superior to that given by the Wales' immersion $\frac{1}{25}$th, and the Powell and Lealand's dry $\frac{1}{50}$th, although the advantage over the latter was very slight. We do not think that the Doctor's judgment in this matter would have much weight with microscopists. The failure to define the longitudinal lines of the *Surirella gemma* with the glasses of such eminent makers as Tolles, and Powell and Lealand, implies such a want of skill in manipulating very fine lenses of high power, as to render an opinion valueless in regard to any comparative merit. In the use of the microscope in the study of pathology, Dr. Richardson has undoubtedly done a great deal of work, and, in that department, has used the instrument to advantage, as the results of his labours have shown; but, to judge from his paper, as a manipulator in '*advanced microscopy*,' he has not yet attained to a very high standard. Nor do we think that his knowledge of the capacity of glasses of different powers is sufficient for him to fully appreciate the comparative merits of high and low powers. In a recent article in the 'Monthly Microscopical Journal' he undertook to prove that high powers, as a $\frac{1}{25}$th and upwards, were valuable in that by their means the differences in the size of the red blood-corpuscles of man and the lower animals, by the greatly increased amplification rendering their comparison easy, could be so clearly made out that there need be no difficulty in discriminating one from the other. Now, as has been pointed out in previous numbers of the 'Medical News,' all the advantages of increased magnitude can be obtained by means of amplifiers, in any lens of sufficient power to bring an object into view. To be more definite; with a $\frac{1}{6}$th, or at the most with a $\frac{1}{10}$th, with a high angle of aperture, the utmost limit of defining power is attained, and, having reached that, it is a matter of indifference at which end of the microscopic tube amplification is made. A $\frac{1}{25}$th, with an A eye-piece, magnifies 1250 diameters; a $\frac{1}{10}$th, with deeper eye-pieces and other means of amplifying, can be made to magnify several thousand diameters, without material loss of sharpness of outline. Where, then, is the advantage of the latter over the former? In fact, it has been proven that the capability of the highest powers is less than those that are lower. For instance, Dr. Woodward has proven that a Powell and Lealand's $\frac{1}{16}$th will show more than their $\frac{1}{50}$th."

**The Advantages of High and Low Powers—An American Difficulty.**—The editor of the Philadelphia 'Medical Times' thinks that a committee of several microscopists should be appointed to settle the matter in dispute between Dr. J. G. Richardson and Prof. Hunt, of the Woman's Medical College, as to the comparative merits of the former's $\frac{1}{50}$th and the latter's $\frac{1}{10}$th—or the advantages of high over low powers.

## CORRESPONDENCE.

### Observation of Test Diatoms.

*To the Editor of the 'Monthly Microscopical Journal.'*

London, *February* 8, 1875.

Sir,—It may interest students of the diatoms most difficult to resolve, to note a few of these objects as recently shown to me by Mr. W. J. Hickie, and I shall confine myself to a brief announcement of the principal results.

1. *Stauroneis spicula;* with a $\frac{1}{24}$th immersion very little could be made out, but with a $\frac{1}{40}$th immersion objective the lines were distinctly observable. This seemed to be a very delicate and useful test.

The object was very flat, and I conceive that it would require very careful manipulation to display it.

2. In *Navicula crassinervis* the lines were distinct when viewed under a $\frac{1}{24}$th immersion objective.

3. Two specimens of *Frustulia Saxonica* were shown with a $\frac{1}{24}$th immersion glass, in one of which the lines appeared somewhat coarse, while in the other (mounted by Rodig) the lines were remarkably fine.

4. A select slide of *Surirella gemma* displayed fine markings under the $\frac{1}{24}$th objective, while with another slide of the same (taking the objects just as they came) under the same objective, the lines were less finely seen, though tolerably distinguishable. All the above objects were viewed by very oblique light.

Some diatoms were then shown to me by straight candle-light, which I had never before seen so displayed. Under a $\frac{1}{8}$th dry objective the following were noteworthy:

1. *Navicula cuspidata*, dry; two different slides by Lind, and another slide of the same in balsam, by Topping.
2. *Nitzschia sigmoidea*, mounted dry by Norman.
3. *Hyalodiscus subtilis*, mounted in balsam by Topping.
4. *Navicula rhomboides*, dry, by Norman.

The above four diatoms were distinctly seen, to my surprise and pleasure, by a foreign $\frac{1}{8}$th. The same objective was brought to bear on *Papilio janira* (known to the readers and friends of Dr. Frey), but here it proved a comparative failure with direct light, while under oblique light the cross markings were clearly seen.

I will not take up additional space with further details on the above, or on less remarkable exhibitions of less difficult diatoms.

Your humble servant,

J. R. Leifchild.

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# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *March* 3, 1875.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The President, having been introduced to the meeting by his predecessor in office (Mr. Chas. Brooke), said that in taking the chair for the first time as President of that Society, he begged that the Fellows would allow him to express how much he felt the honour which they had done to him. He had heard it said that there could be no greater honour than being elected President of an important scientific Society, since those on whom the election depended were of all men the best qualified to judge of the fitness of him who was chosen for the office. It would be affectation on his part if he were to say that he had not adequately devoted himself to the microscope, since he had made it his constant companion for something like twenty years; but at the same time he had applied it in such a different manner, and to such different subjects from those which were usually brought before the Society, that he felt himself in many respects very imperfectly qualified to preside at their meetings. His great aim had always been to occupy himself with those branches of research which had been neglected by others, and this had necessarily occupied so much of his time that he feared he was very imperfectly acquainted with much of what had been done by other observers. For his own part, he was inclined to believe that the development of new lines of inquiry and the application of new methods would conduce as much or more than anything else to advance science; but still, anyone devoting himself to such subjects had necessarily to contend against many difficulties. He might say that in no respect did the fact that the Fellows had chosen him for their President give him more satisfaction than in its proving that the Society approved of new methods and of new kinds of investigation, and that in consideration of that, they were willing to overlook many deficiencies and shortcomings in other respects.

The minutes of the preceding meeting were read and confirmed.

A list of donations to the Society since the last meeting was read by the Secretary, and the thanks of the Fellows were voted to the donors.

Mr. H. J. Slack said that most of the Fellows saw a short time ago a curious living organism which was exhibited there by Mr. Badcock, and which was thought at the time might be the same as *Bucephalus polymorphus.* It was thought that it might have come from a fresh-water mussel. As some considerable interest attached to it, he thought it might be desirable to publish in connection with Mr. Badcock's observations extracts from Von Baer's paper and also some descriptions of similar organisms from the 'Annales des Sciences Naturelles,' and from 'Comptes Rendus.' Mr. Slack then gave a *résumé* of the extracts to which he referred (and which will be found printed at p. 141), illustrating the subject by drawings on the board. In reply to an inquiry from Mr. Beck as to the size of the creature, Mr. Slack said that it was quite small, but still in its free-swimming state visible to the eye. Drawings—which will be published with the paper—showing the natural size and also enlargements $\times$ 20, were handed to Mr. Beck for inspection.

Mr. Badcock said that for some considerable time after he had een observing them, he could not find out that anyone else had described any similar creature, until Professor Huxley suggested that it might possibly be the Bucephalus of Von Baer. But having seen Von Baer's remarks and drawings, and having carefully compared them with his own observations, he thought it impossible to identify them as the same from the description given. The mode in which Von Baer proceeded to dissect them out showed them to be tough and not easily injured, but his own specimens were so excessively brittle that it was most difficult even to take them up. They had been shown to numbers of scientific men who had not recognized their identity, and he would simply point out that Professor Huxley himself had since doubted whether the creature was the *Bucephalus polymorphus* of Von Baer, from certain appearances and observations which had been described to him, and also from the circumstance of its being found free swimming. The resemblance to an ox he could not at all make out.

Mr. Slack suggested that Von Baer only used a magnification of 20 diameters. It was a mistake to suppose the creatures he found were tough.

Mr. Badcock said that his own impression was that the creature might belong to the same class of animals as that of Von Baer, but might be a different species.

Mr. Slack thought it most probable that higher powers might be required in order to determine the species.

The thanks of the meeting were then voted to Mr. Slack and to Mr. Badcock for their communications.

A paper by Dr. G. W. Royston-Pigott, "On the Principle of testing Object-glasses by the Coloured Images produced by Reflexion from a Globule of Mercury; and on Eidola," was read by the Secretary. The subject was illustrated by numerous diagrams, which were explained by Dr. Pigott as the reading of the paper proceeded.

The President said that looking at the subject from an *a priori* point of view, he thought it might be very useful when applied to the study of forms of minute bodies which were well known, in order to see what kinds of false images were produced, so that it might be by analogy inferred what was the real nature of other bodies which were only known to us possibly by such kinds of images.

Mr. Browning said he had followed the paper with considerable interest, but he had been rather puzzled by the statement that when a prism was used good images were obtained, but when a mirror was made use of they became confused. Dr. Pigott had, however, explained this to him by saying that the mirror was a glass one having the back surface silvered.

Dr. Pigott had often seen with a silvered glass mirror of this kind as many as half-a-dozen images of the flame of a candle, so that when this was used as a reflector, there were perhaps five or six images thrown one over the other in such a way as to obliterate the sharpness of outline.

Mr. Elphinstone inquired if Dr. Pigott had seen in an early number of the 'Quarterly Journal of Mathematics' an article by Mr. Munro, "On the Final Interference of Light," which bore upon these questions. It was well known that if they put two things marked in the same way one behind the other, and then looked through them, they would immediately get patterns, as in the case of two pieces of wire gauze, and it had often occurred to him that there were things seen in this way which, though described as real, were in fact only false images of things which showed two sets of lines, one behind the other. If they went to a pond and threw in two stones, and looked at the rings obliquely where they crossed each other, they would see a series of hyperbolas. No doubt the things they should see would be two series of circles, the one well defined and the other ill defined, and crossed by a series of hyperbolas. He thought there were often very erroneous appearances produced by seeing two series of markings one behind the other.

Mr. Beck said that it was for that very reason that they had such difficulty in making out the nature of such fine structural markings as those on the diatoms, which were probably not simple structures, for nature did not deal in simple structures; so that when looking at the structure of the Podura scale, it was not easy to say what it was they really did see; and he thought it was not so much by examining these objects themselves as by taking similar structures of a simple kind and arguing from analogies, that they would be able to form a correct idea of what really existed. The reference to a wire-gauze blind reminded him that if light were passed through it upon a sheet of white paper, by varying the distance they could get the effects of all the markings of the Diatomaceæ, but it was clear that none of them really existed. He must express his dissent from the statement that they did not learn anything from oblique light. He should quite agree with the opinion that if they were to settle questions of structure merely from what they could make out by means of oblique light, they would fall into a series of errors; but oblique light was of great value in many respects, particularly when sections had to be examined. They had not been told the size of the globules of mercury employed by Dr. Pigott; this was, however, not of great importance, as the real object in view was to produce an artificial star, the most minute point of light with which they were acquainted.

Dr. Pigott said that Mr. Beck had very properly remarked that he had not said anything about the size of the globules. His practice was to pour out a small quantity of the metal, and then to keep sweeping the globules away until he got them sufficiently small for the purpose. Globules about $\frac{1}{10}$th of an inch were small enough to form a miniature, to be reduced by the objective employed.

The thanks of the Society were voted to Dr. Pigott for his paper.

Mr. Wenham then explained to the meeting a new method of viewing objects at extreme angles, and illustrated the manner of mounting and observing them by means of drawings on the black-board. Several mounted specimens were also placed in the hands of the Fellows for inspection. (Mr. Wenham's observations will be found printed at p. 156).

The thanks of the meeting were unanimously voted to Mr. Wenham for his communication.

The President remarked that all observers were well aware that illumination was half the battle in microscopy. In many cases he had no doubt but that Mr. Wenham's method would be a very valuable aid to investigation.

Mr. Browning suggested that as the glass must be ground to a perfect knife-edge, the greatest care must be taken of the edges, as they would certainly break if touched.

Mr. Wenham said he did not think there was much difficulty about that; the edges could be butted together, and would be pretty safe; but of course they must be quite sharp, or the appearance of the jagged edge would be very unsightly. Any glass-grinder would be able to prepare them. Those he had brought to the meeting were made of the ordinary slip glass.

Mr. Slack thought it might be termed an oblique unilateral illumination.

Mr. Wenham said it was the only plan by which the object could be obliquely viewed by the central portion of the object-glass. At the suggestion of the President, Mr. Wenham then drew a more enlarged diagram, showing clearly the method of mounting and the position of the object upon the slide.

The President announced that the Council was endeavouring to arrange for a scientific meeting to be held on Wednesday, April 14—subsequently changed to 21st; also that at the next ordinary meeting of the Society he hoped to be able to read a paper "On some New Contrivances for the Study of Spectra, and for applying the mode of Spectrum Analysis to the Microscope."

The Secretary, Mr. Stewart, called the special attention of the Fellows to some extremely beautiful preparations of Polycistinæ exhibited in the room by Mr. Stephenson. Some of them were new, and some were very delicately mounted, preserving the spines in a very perfect manner.

Donations to the Library since February 3, 1875:

| | From |
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| Nature. Weekly .. .. .. .. .. .. .. .. .. .. .. .. .. | *The Editor.* |
| Athenæum. Weekly .. .. .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Society of Arts Journal. Weekly .. .. .. .. .. .. .. .. .. | *Society.* |
| Journal of the Linnean Society. No. 78 .. .. .. .. .. .. | *Ditto.* |
| The Cincinnati Medical News. | |
| Quarterly Journal of the Geological Society. No. 121 .. .. .. | *Ditto.* |
| Transactions of the Natural History Society of Northumberland and Durham. Vol. VI. .. .. .. .. .. .. .. .. .. .. .. | *Ditto.* |
| Bulletin de la Société Botanique de France .. .. .. .. .. | *Ditto.* |
| Microscopical Notes regarding the Fungi found present in Opium Blight. By Dr. D. D. Cunningham .. .. .. .. .. .. .. | *Author.* |
| A Report on the Microscopical and Physiological Researches into the Nature of the Agent producing Cholera. By Drs. Lewis and Cunningham .. .. .. .. .. .. .. .. .. .. .. | *Authors.* |
| The Pathological Significance of Nematode Hæmatozoa. By Dr. T. R. Lewis .. .. .. .. .. .. .. .. .. .. .. .. .. | *Author.* |
| Proceedings of the Literary Society of Liverpool. 1874 .. .. .. | *Society.* |
| A Monograph of the British Spongiadæ. Vol. III. By Dr. Bowerbank | *Author.* |

Charles Dexter Barker, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
*Assist.-Secretary.*

## MICROSCOPICAL SOCIETY OF VICTORIA, N.S.W.

The first annual conversazione of the members of this Microscopical Society was held on the evening of October 29, 1874, in the Royal Society's hall. There were about 200 ladies and gentlemen present. His Excellency the Governor, accompanied by Major Pitt, private secretary; Archdeacon Stoke, of New Zealand; and Mr. Samuel Johnson, arrived at the hall shortly before eight o'clock, and the proceedings were at once commenced.

There were a great number of microscopes exhibited, some of them being of great power. Among the principal objects of interest shown during the evening were the following: Diatoms, or minute fossilized vegetable deposits, showing beautiful markings on their surface; foraminifera, or small marine animals—those found on Australian and New Zealand shores are very beautiful; infusoria, animalcules generally prevalent in infusions of organic matter; rotifera, having cilia on their bodies, giving the appearance of wheels in action; polyzoa, animals for the most part microscopic in their dimensions, found in most pools, streams, and on the shores, very interesting objects for anatomical examination, as well as for observation in the living state—some supposed new species will be exhibited; insect dissections, which form a large and interesting branch of microscopy, plant cells and seeds; circulation in foot of frog and water worms, and the ciliary motion in the gills of the mussel; one of the greatest curiosities was the cocoon of the leech; crystals shown with the polariscope; microphotographs, of which wonderful results have been effected; wool-classifying apparatus. Some very fine microscopes of great magnifying power were also exhibited by Messrs. F. F. Balliere and Co., of 104, Collins Street East. There were also some handsome cases of Australian and exotic beetles, which were a source of great attraction during the evening.

Mr. Ralph, the President of the Society, having taken the chair, the Secretary (Dr. Robertson) read the following report:

The committee of the Microscopical Society of Victoria, in presenting their first annual report, are pleased to be able to give such good accounts of the state of the Society, both in its working and financial aspects. The Society was formed twelve months ago, and held its inaugural meeting in this hall on the 10th October, 1873. At that time it consisted of thirteen members. The number has now increased to thirty-six. The subscriptions amount to 54*l.* 12*s.*, and the expenses to 23*l.* 19*s.* 10*d.*, thus leaving a balance in hand of 30*l.* 12*s.* 2*d.* The committee desire in the first place to express their thanks to the council of the Royal Society for the kindness and liberality in granting the free use of this hall, both at the inaugural and this their first annual soirée; also to acknowledge with thanks the following presentations to the Society in their order: Microscopical drawings, by Dr. Sturt; a collection of Victorian insects, by Mr. Wooster, of Narree Warreen; vol. vi. 'Flora Australiensis,' by Bentham and Mueller, from the Government; a number of valuable books as a loan to form the nucleus of a library of micrographical works, by Mr. Sydney Gibbons; some deep-sea soundings from H.M.S. 'Challenger,' by Professor Wyville Thomson and Mr. Murray; some deep-sea soundings from the neighbourhood of King's Island, by Mr. S. S. Crispo, of H.M.C.S. Victoria; also some beautiful stalactites from the same gentleman. Besides these, numerous objects, mounted and unmounted, have at various times been presented, and others promised, from various parts of the colonies. During the past twelve months eleven meetings have been held, and the papers read and objects exhibited have been generally of great interest. At the inaugural meeting our late President, Mr. W. H. Archer, gave a very able address, which the leading papers of the colony thought fit to publish *in extenso*, and which was reprinted in the 'Monthly Microscopical Journal' of England. Among the principal subjects at the subsequent meetings was a very interesting paper by Mr. T. S. Ralph, our new President, "On the Fungus affecting the Rye-grass," which proved to be *Isaria graminiperda*, and this was borne out by Mr. Sydney Gibbons, who had investigated the same parasite. Mr. Archer brought forward some living specimens of *Hydra viridis*, and a fresh-water polyzoon, found in a pool on the banks of the Yarra, which he considered a new species allied to the *Fredericella*. Some entozoa found in sheep and rabbits on a run near the Werribee, and which had proved very fatal to those animals, were kindly sent to the Society by Dr. Youl. Mr. Ralph and Dr. Wigg undertook to examine and report upon the same, and they were found to be a species of tapeworm—filaria and hydatids. Mr. Sydney Gibbons reported on some deposit found near Bacchus Marsh, consisting for the most part of diatoms. The microspectroscope—the advantages derivable from the use of it in microscopic researches, especially in plant chemistry, toxicological, and medico-legal inquiries, were pointed out by Professor Ellery, and the committee trust at some future date to hear more upon this subject from such an able exponent. Mr. Gibbons read a paper "On a Mode of Detecting Sewage Contamination in Water," and mentioned an interesting fact as a proof of the purity of the Yan Yean, that the fungi found in impure water would not live in Yan Yean. A *résumé* of works on natural history having reference to this colony was given by Mr. Archer, and will be found of great value to those who may wish to refer for information in their microscopical and natural history studies. Mr. Gibbons gave a practical lecture "On the Method of Applying Re-agents to Objects *in situ* under the Microscope," and showed some ingenious contrivances for collecting and preparing objects. Dr. Sturt also added some others. A paper "On the Mouth of the Yarra as a Collecting Pound," by Dr. Sturt, proved of great interest, and it is the intention of some members of the Society to form fishing and dredging excursions in and around the bay; and in connection with this subject the committee refer with pleasure to the visit paid by some members of the Society to H.M.S. 'Challenger,' when, through the kindness and courtesy shown by the officers of the ship, both naval and civil, they were enabled to inspect the whole apparatus used in connection with the trawling and dredging in deep seas. Mr. Murray has kindly promised, at the end of the voyage, to forward dredgings from various parts of the world to this Society. Mr. S. Gibbons took up the subject now under discussion so much in the microscopical world, and read a *précis* of a paper "On the Transformation of Monads, with some Observations on Heterogenesis." Mr. Robert Scott drew attention to the necessity for "standard gauges for microscopical apparatus," and suggested that this Society should take up the subject and report to the Royal Microscopical Society of London, with a view of obtaining their co-operation for the adoption of such measures as it deemed desirable. Dr. Sturt read a paper on a "Peat-ash Deposit found near Sebastopol," composed almost entirely of diatoms; also an extract from 'Nature' on "Microscopic Examination of Air," followed by one on "Lithofracteur." A wool-classifying apparatus, invented by Messrs. Hesse and Rummell, was brought under the notice of the Society, and is one of the many instances of the microscope being brought into use in the mercantile world. Among the objects exhibited may be mentioned—Victorian foraminifera and diatoms, by Mr. Barnard; microphotographs, by Mr. Noone; new fungi, by Mr. Ralph; spicules of isis and a medusa, by Dr. Sturt; cuticle of synapta and the echinococcus, by Mr. Gibbons; some *Entozoa folliculorum* from a pig's snout, by Mr. Girdlestone: a tetrarhynchus from a flathead, by Mr. R. Robertson; and an apus, by Dr. Johnson. Communications have been received from London, Philadelphia, Queensland, Western Australia, and Tasmania. One of the council of the Royal Microscopical Society of London—Mr. Frank Crisp—has written, wishing to become a member of this Society. But the most interesting communication was read by the Hon. Secretary at the last meeting of the year, from Mr. C. Maplestone, of Maryborough, relating to the "results of his observations with the microscope in Victoria," and the committee would recommend a perusal of it to those interested in the microscopical and natural history of this colony, as showing how much work has been and still remains to be done; and with a view to increasing the knowledge and usefulness of the microscope, especially among the younger investigators, it has been decided to set apart special evenings during the year for its study and manipulation, in the hope that students may be induced to join, and thus strengthen the committee in their endeavours to maintain the present successful position of the Microscopical Society of Victoria.

On the motion of the Chairman, seconded by Mr. Sydney Gibbons, the report was adopted.

The President then read a paper "On Observations and Experiments with the Microscope on the Nature and Character of the Blood." He stated that as the time was limited he could not enter very largely into the subject, and must therefore deal briefly with it. He pointed out that in the higher animals the colour of the blood was red, but in the lower forms of life it was colourless, with a few granules diffused through it. When we come to look at it in the fish and reptile, and onwards in the bird and mammal, we find that it becomes of a redder colour. This colour was due to the immense number of particles of which the blood is composed—each particle being in reality very slightly coloured of a yellowish-red tint. The blood-corpuscles are of very minute dimensions, so that 3300 of the red will lie in a row, and occupy only 1 inch. A rough calculation gives about 10,000,000 of these in a large thimbleful of blood, and we can very soon lose ourselves in trying to calculate what a number must be in a human body, which is known to contain over 100 oz. of this wonderful fluid. He then pointed out what was supposed to occur in the history of the blood, and stated his opinion that the red corpuscles of the blood originated from the white ones.

Mr. Sydney Gibbons gave an interesting description of a number of marine crustacea that had been obtained from the hull of the 'Cerberus' when she was in dock.

Mr. Bosisto read an interesting paper "On the *Hirudo Australis*," or Australian leech, pointing out that the true medicinal leech of Australia ranks equally in its capabilities for drawing blood without causing inflammatory wounds with that of the "Sanguisuga," or medicinal leech of northern Europe. Two varieties exist here, the "bright olive yellow" and the "dark olive green." The former represents in character and value the "spotted," and the latter the "green," varieties of Europe.

Mr. Ellery delivered some very interesting remarks on the relations of light to colour. He pointed out that in the white light all colours were mixed, and unless this necessary mixture took place there could be no colour. He instanced this by showing a fine bouquet of roses with a sodium light. The whole of the colours at once vanished, and everything appeared of a ghastly grey.

Dr. Wigg read a paper "On some Points in the Microscopical Examination of Handwriting," in which he referred at some length to evidence he had been recently called upon to give in a court of law.

The last paper read was one by Dr. Sturt "On a Medusa found in the Sandridge Lagoon."

## Memphis Microscopical Society.

[There is considerable carelessness exhibited in making out these reports for the 'M. M. J.' The present one reaches us without any date whatsoever; we fancy that it should be dated December, 1874.]

The Society met at the usual hour. Dr. J. A. Thacker, of Cincinnati; Prof. Chas. E. West, of Brooklyn, New York; C. Leo Mees, of Columbus, Ohio; Edward Moulton, of Wooster, Massachusetts, and B. F. Quimby, of Philadelphia, were elected corresponding members. The Secretary announced the receipt of one dozen beautiful slides of entire insect preparations, donated by T. W. Starr, of Philadelphia: also one half-dozen slides of crystals, for polariscope, from Dr. A. F. Holt, of Cambridge, Massachusetts. Two slides of diatoms were also received from Charles Stoddard, of Boston; and Mr. A. F. Dod, of Memphis, contributed two elegantly prepared slides—one of microscopic shells from Barbadoes, and the other scales from the common fern. The preparations of Mr. Starr were much admired for their perfection of finish and artistic style of mounting. The slides were accompanied by a paper, giving in detail the donor's method of working in this species of mounting. It was read by the Secretary, to the great delight of the members, most of whom acknowledged that it was far superior to their own *modus operandi*.

A resolution of thanks to the donors of the various preparations was passed, after which the Secretary read a number of letters addressed to the Society. Among these we may mention one from

John Pierce, Secretary of the Providence (Rhode Island) Microscopical Club, announcing that a box of slides would soon be sent for exchange; acknowledgments of election to corresponding membership from Dr. J. J. Woodward, of the Army Medical Museum at Washington; Dr. R. H. Ward, editor of the department of microscopy of the 'American Naturalist'; H. C. Clay, Shrevepoint, Louisiana; also a letter from Dr. Harrison, Secretary of the Biological Section of the Maryland Academy of Sciences, mentioning the fact that their association would be glad to exchange materials or slides at any time, and that they proposed soon to forward specimens of their handiwork for the inspection of the Memphis Club.

The Secretary explained that published report of last proceedings made it appear that Mr. G. E. Smith's observations on the new $\frac{1}{10}$th were confirmatory of Mr. Morehouse's; whereas Mr. S. is the undoubted originator and demonstrator of the idea that a first-class $\frac{1}{50}$th could be equalled and indeed excelled by a $\frac{1}{10}$th.

The Secretary explained the workings of the Postal Micro-cabinet Club as detailed in a letter from F. B. Kingdon, Secretary of the Margate (England) Microscopical Society. Mr. Kingdon's letter also extended the hearty good wishes of the English society, and expressed the hope that the two societies might be of material aid to each other. Mr. J. A. Omberg read an interesting paper on Crystallography, which called out a hearty vote of thanks from the Society and a motion to publish. A lively discussion ensued on certain points sprung by Mr. Omberg's essay, after which the Society adjourned to the next regular meeting, first Thursday in January.

## Quekett Microscopical Club.

Ordinary Meeting, February 26, 1875.—Dr. Matthews, F.R.M.S., President, in the chair.

Notice was given that the permission of the Council of University College had been granted to hold the annual soirée of the club on Friday, the 16th of April.

A letter from Colonel Horsley to Mr. Curties was read, describing a possibly new species of *Vaginicola* (somewhat resembling *V. decumbens*), and drawings of it by Mr. Fullagar were exhibited.

The President announced that Mr. F. Crisp had made a donation to the club of a sum of 20*l.* annually for five years, and read a report of the committee as to the best method of applying it. This was considered to be by awarding presents of books or scientific instruments to members who might distinguish themselves in microscopical work.

Mr. B. T. Lowne gave an introductory lecture on the Histology of the Eye, in which he treated of its structure and functions, giving a general description of its various parts, and their relation to each other. The histology of the separate portions was to form the subject of a future lecture.

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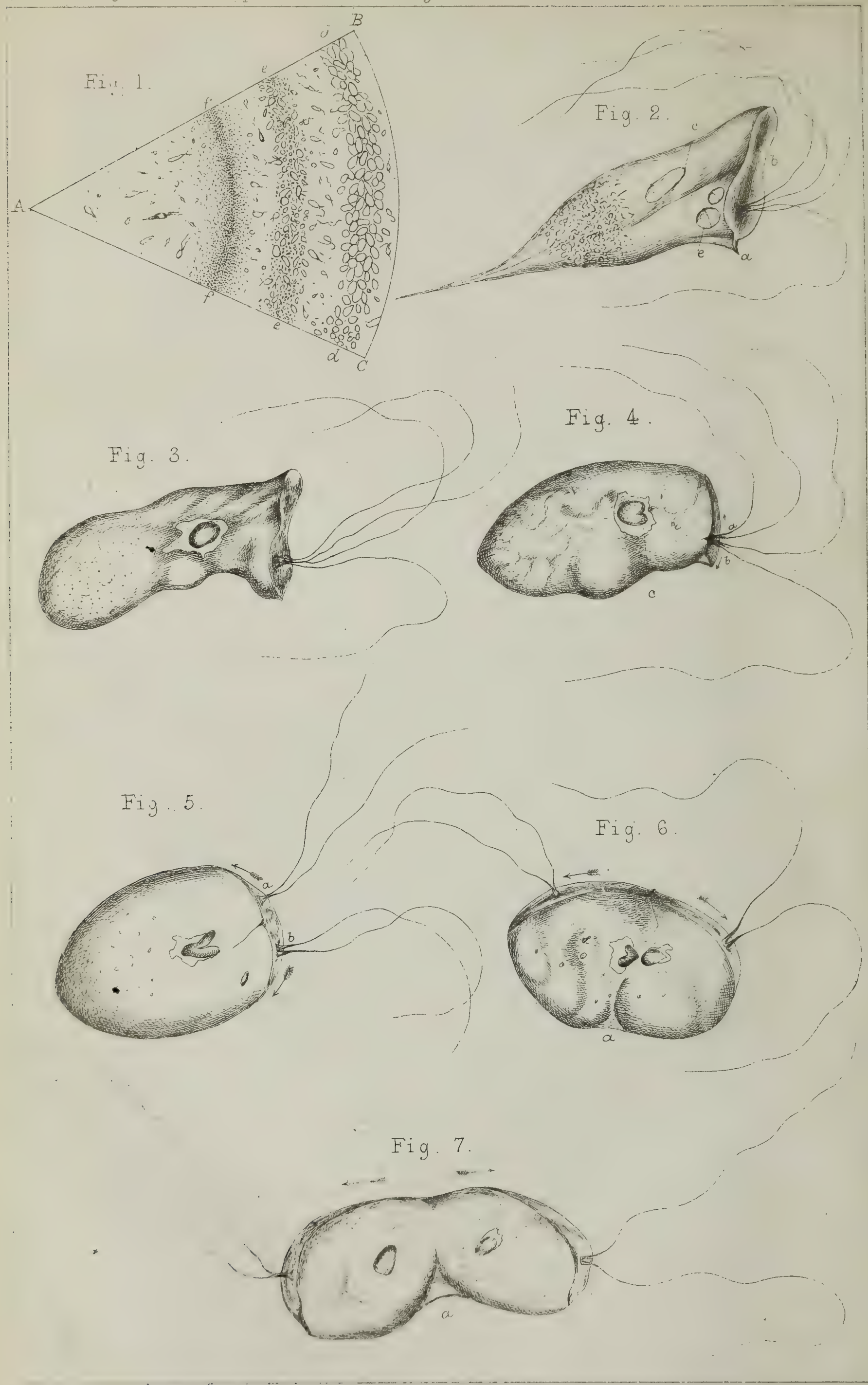
Erratum.

Page 10, line 15, *for* "found," *read* "formed."




W West & C° lith.

Life history of a monad.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

MAY 1, 1875.

## I.—*Further Researches into the Life History of the Monads.* By W. H. Dallinger, F.R.M.S., and J. Drysdale, M.D., F.R.M.S.

(*Read before the* Royal Microscopical Society, *April* 7, 1875.)

Plates CII., CIII., and CIV.

This paper will complete the present series of researches—commenced some four years since, and terminated at the close of last year—on the developmental history of monads. At the close of

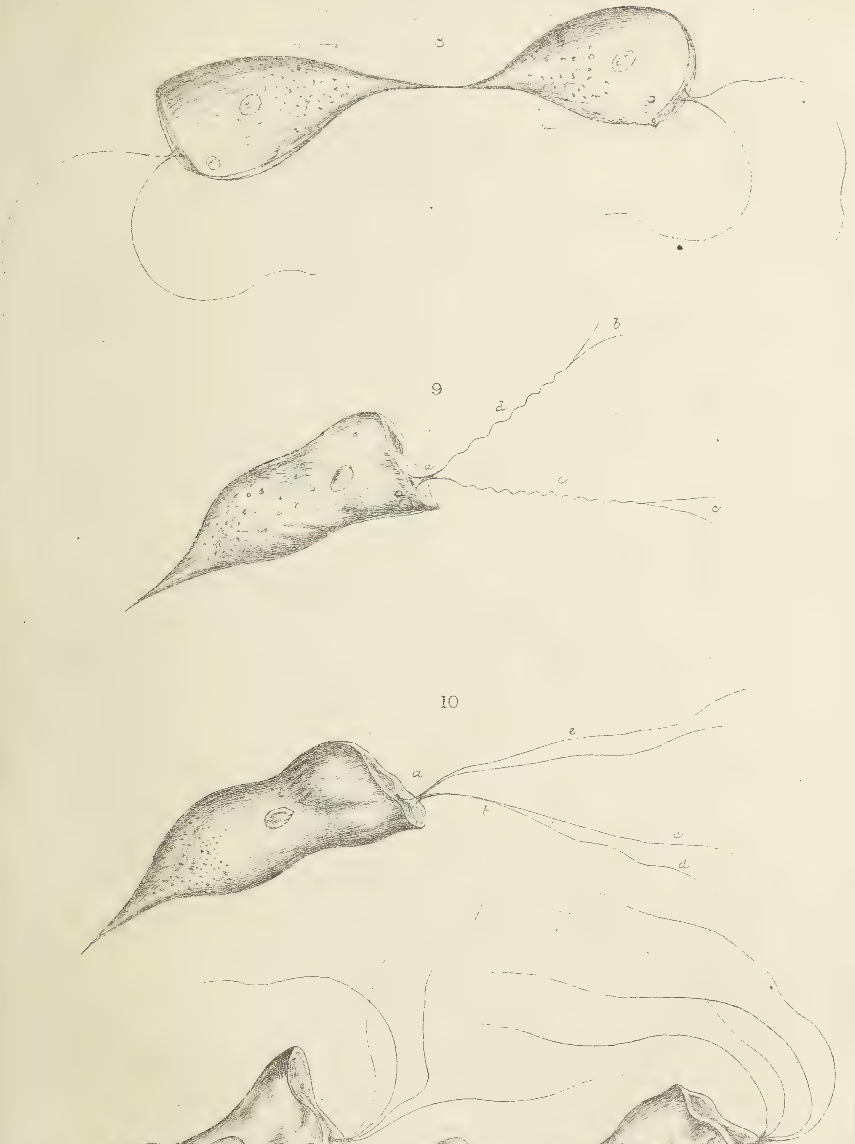
DESCRIPTION OF PLATES CII., CIII., AND CIV.

Fig. 1.—Zonal arrangement of monads as seen in moist developing cell—a portion only of the field.
„ 2.—A typical calycine.
„ 3.—The semi-amœboid condition preceding fission.
„ 4.—The first stage of fission, where the base of the flagella has split into two, and the sarcode is opening in the direction of the arrows, carrying a *pair* of flagella, *a*, *b*, in each direction; and the nuclear body is also dividing.
„ 5.—Fission continued. The sarcode is still moving in the direction of the arrows. The "nucleus" is more parted.
„ 6.—Fission continued. The body of sarcode is now "bean shaped"; the motion is still in the direction of the arrows. The "nucleus" is wholly divided; and the first symptom of division of sarcode is seen at *a*.
„ 7.—Fission continued. The flagella are opposite each other; distinct constriction is visible at *a*, and each part is pulling in the direction of the arrows.
„ 8.—Fission continued. The aspect immediately before partition.
„ 9.—The halves produced by fission have only a pair of flagella instead of four. This figure shows how the pair are divided into two, each by attachment at the ends *b*, *c*, and rapid vibration along the whole length, causing splitting at the ends *b*, *c*.
„ 10.—The flagella split by vibration.
„ 11, 12.—The amœboid state which precedes sexual union.
„ 13.—The same in an extreme state, as often seen.
„ 14.—The blending of two in this condition.
„ 15.—The sac resulting from the perfect fusing of all the parts of each with the other.
„ 16.—This sac in its completely developed state, gently opening and pouring out sporules.
„ 17, 18, 19, 20.—The spores developing in different stages until they reach the parent form.

All the figures are magnified 2600 diam., except 9, 10, 15, which are magnified 5000 diam. and reduced. Figs. 17, 18, 19, 20, are each × 5000.

the work some general remarks may be admissible, and to make these we shall have to refer to the monads severally described; but as these are unnamed, and reference is therefore difficult, we will venture to distinguish them by the names by which for working purposes we designated them in our diaries. The first was the *cercomonad*, described in the tenth volume of the 'M. M. J.,' at p. 53. The next we called the "springing" monad, from its peculiar habit of coiling and uncoiling one of its flagella with a darting motion, not unlike the vorticella, carrying the body with it. This is described at p. 245, ibid. The third we designate the "hooked monad," from the presence of a persistent hook-like flagellum, described and figured in vol. xi., p. 7. The fourth we call the "*uniflagellate*" monad, being possessed of only one flagellum, and that at the anterior part of the body. It is described at p. 69, ibid. The next is the "biflagellate or acorn monad," being possessed of two anterior flagella, and at almost all stages of its development has the posterior end of its oval sarcode shaped like the cup of an acorn. This is described in vol. xii., p. 261. And the one we are now about to describe we name the "calycine" monad, from what will be seen is its peculiar calyx-like form. It has been before stated that the acorn-like form was the one which first arrested our attention; but we were unable to study its complete development either in this or the following summer. It was almost the only species that existed in the maceration for nearly three months; scarcely anything indeed existed with it save *Bacterium termo*. But at the end of the time named it was rapidly superseded by the form which we are now about to describe; and most of the other monads we have described appeared simultaneously with it.

At the end of the first year we had accumulated a large number of individual observations, the correctness of which was confirmed by subsequent work, but the connection, correlation, and interpretation of which at the time entirely baffled us. To the practised worker with high powers it is well known that it is very much more difficult to *discover* an obscure or delicate phenomenon than to see it again after once the actual discovery has been made. A minute striation or an exquisitely attenuated flagellum may cost immense labour and perseverance to *find*, but once found it is easy to see it again and again afterwards. And curious as it appears to us now, many of the processes with which we are now familiar and can easily discover, *then* eluded us; so that even into the second year of working it would not have been at all out of harmony with the *facts*, as we then saw them, to have inferred that the biflagellate monad, the springing monad, the hooked monad, and the calycine, described below, were all connected in one cycle of generation, or at least in some way related to each

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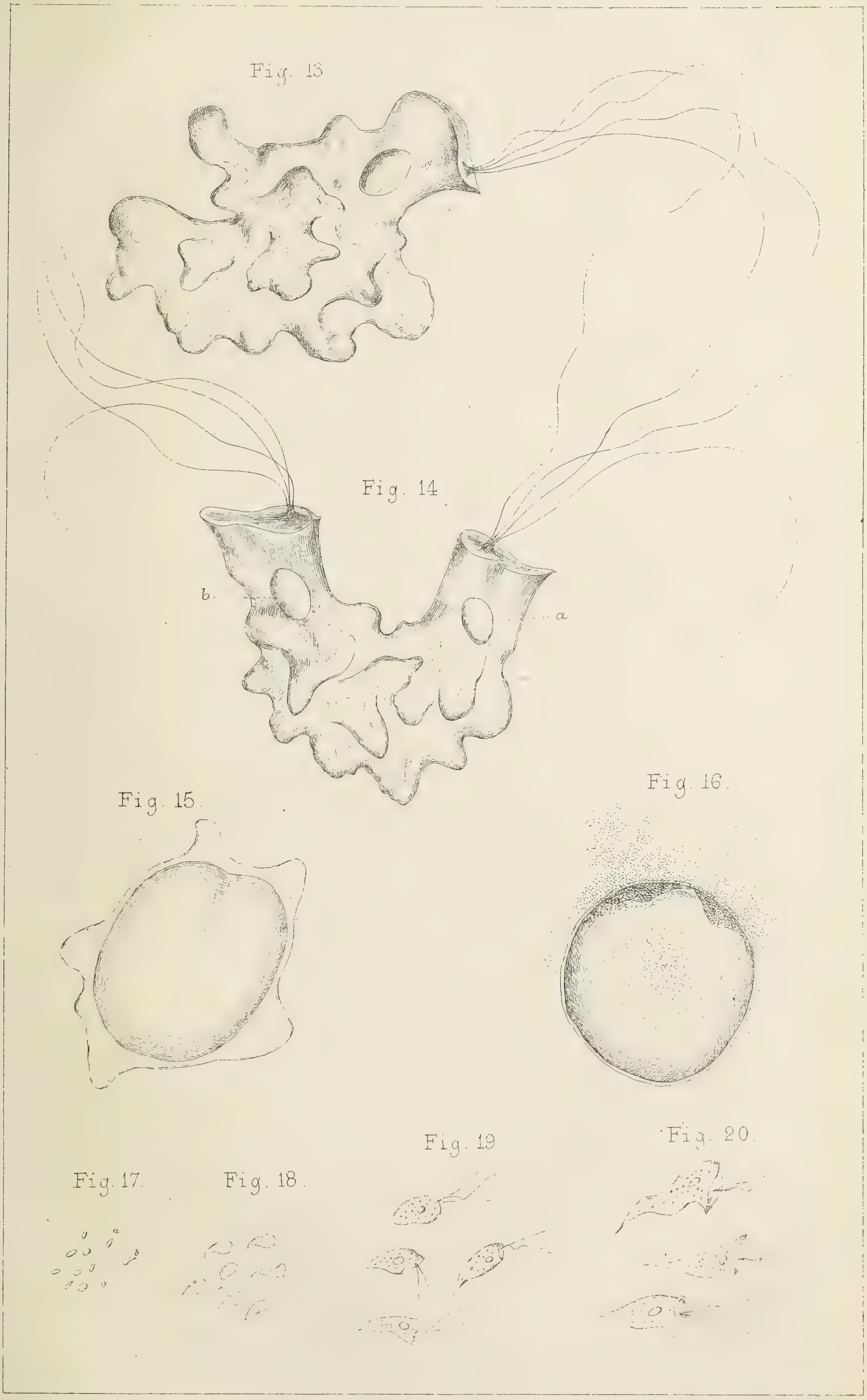
Life history of a monad.




W. West & Co. lith.

Life history of a monad.

other. Indeed, from the fact of their being all in the same fluid, and developing in the same field at one time, it appeared perfectly natural—if it had been permissible—to *assume* from the then known facts that the springing form was a younger condition of the biflagellate, and the calycine a higher development of the latter. So strongly did we at one time believe the facts to point in this direction, that when we had followed a calycine form to the state of encystment, to be presently described, we did not hesitate to express the conviction in our diary that in a few hours the field would be full of the young of the springing monad!—a prophecy which the event completely falsified, administering by that means a salutary caution, and opening up one of the many pathways made known to us by our failures, and leading to the truth. Indeed, it became perfectly clear after continued work that we could never conclude that the contents of our continuously moist field, at any given time, were necessarily sprung from the most conspicuous forms predominant some little time before; and therefore until the complete life cycle of any given group of forms is known, nothing can be certainly affirmed concerning them.

It may be interesting to allude to the peculiar phenomena presented by the monads when they had been for some hours in our moist cell. We take an example when our maceration was most prolific in the variety of forms present; some hours after the "dip" had been put into the chamber and under examination, the monads ranged themselves in *zones;* and these zones were persistent, however many days this drop might be preserved in unevaporated condition. The illustration is taken from our diaries in 1873; it is seen in Fig. 1, which is a portion of the field after it had remained under examination in the moist chamber four days. B C represents the margin of the cover-glass—the field being seen with a comparatively low power—*d d* is an outermost zone, in this instance, of the "springing monad"; these were in intense activity, constantly changing places with a rapid spring, but always preserving the zone in its integrity. At a distance from this zone about equal to its own width came another, composed largely of the "biflagellate" form, and an immense number of the young of this, and probably of other forms also, intensely active, shown at *e e*; and finally within this, a zone of what with comparatively low powers would be taken merely for bacteria, but which under the scrutiny of the highest powers proves itself to be composed by no means in the larger proportion of these, but to consist of still and active germ-like bodies, the development of which with care and persistence may be followed. This is drawn at *f f*. Swimming about freely between and within the zones were stragglers from all of them, with many other forms in no way connected with them. This was, with variations merely as to which monad or group occupied the several positions, almost the universal condition of the field. Sometimes the zones were not complete, and sometimes there were two, or even one, instead of three; but this largely depended upon the abundance, variety, and condition of the forms which might be taken up in any given "dip."

Now it would be an extremely difficult matter to prove that any arrangement approximate to this existed in a large macerating mass; but we have nevertheless repeatedly observed that required forms were found in incomparably greater abundance in one part of the fluid than another. Be this, however, as it may, this zonal arrangement was a remarkable feature. What aggregated them together in this way it would be impossible to say satisfactorily; probably the desire for oxygen may have drawn them towards the edge of the cover-glass; but this will not account for their *sifting out* as it were into separate groups; and it leaves unexplained how it was that creatures of higher organization, as ordinary paramœcia and forms of kerona, could live to the very end, in the greatest activity and apparent health, in the centre of the field, that is at A, Fig. 1, and its immediate neighbourhood. This at least we learned from it, that the earlier stages of many different forms were present, the adults of which *may not at all be represented in the field at the time.* There are also, doubtless, myriads of other forms not recognizable, and we have given reason to believe * others still which to our best appliances are invisible. It is further in harmony with biological facts, and the evidence afforded by our inquiries, that many of these must await some change of circumstances before they could develop and come to maturity. The flagellate monads, for example, as a whole did not appear for some weeks after the various forms of bacteria; and the calycine form did not in any case appear for some months after the "springing," "hooked," and "biflagellate" monads. We do not attempt to explain this; but we do not hesitate to believe that the reason is discoverable; and it will in all probability be found to depend on very simple causes, such as the season and temperature, which is without question a real cause, the preparation of the pabulum by the bacteria and successive monads, and the chance addition of germs to the macerating mass, either through the air or otherwise. The two first reasons apply on the supposition that the fluid does contain the germs in a dormant condition until the circumstances become favourable. The third is a fact; for as we have already pointed out, the "biflagellate" monad was wholly absent one year, although its follower the "calycine" came in spite of its absence. In the same way we have observed the absence of the *Spirillum volutans* and other forms. It is true that this never

* 'M. M. J.,' vol. ix., 1874, p. 71.

applies to *Bacterium termo* and *Penicillium glaucum;* but in the present state of our knowledge we may be allowed to infer that the sporules of these are simply ubiquitous.

These simple reasons may quite sufficiently account for the successive appearance of different forms of infusoria in organic infusions, without having recourse to the startling hypothesis of heterogenesis. The reported observations of Dr. Bastian, Dr. Gros, and some few others, alleging the origination of one known form from parents of an entirely different nature, such as the development of nematoids from spores of vaucheria,* of diatoms, pediastrieæ and other algæ from euglenæ,† and so forth, are instances which we presume could only be accepted scientifically by tracing the whole process step by step, repeatedly, until every stage in the process of mutation was actually discovered and described. The possibility of misinterpretation is great. Indeed, we distrust all observations founded on successive "dips" in a quickly changing organic infusion, and in fact put no faith in observations of this sort not conducted on the plan of keeping the same drop under continuous observation during all alleged transformations. As far as our observations upon these lowly forms go, we are bound to say that not the slightest countenance is given to this doctrine of heterogenesis. On the contrary, the life cycle of a monad is as rigidly circumscribed within defined limits as that of a mollusc or a bird. There is no indication of any unusual or more intense methods of specific mutation than those resulting from the secular processes involved in the Darwinian law, which is held to furnish the only legitimate theory of the origin of species.

In the facts pointed out in the previous pages we also see an explanation of the *sameness* which Billroth attributes to the organisms of the septic processes: he remarks frequently on the recurrence of so small a number or variety of generic forms in all putrefactive processes. He says, for example: "The notion is widely prevalent that the minute organisms generally met with in the putrefaction of the juices of the human body form an impassable labyrinth, and belong partly to the animal and partly to the vegetable kingdom; and also that in infusions of various organic tissues a countless multitude of so-called infusoria are found, with endless variety of form and species. But if anyone sets about looking into the wonders of the creation he is very soon undeceived; instead of the expected variety, he finds a monotony of forms which soon extinguishes all interest of a merely superficial nature, for he may search various putrefying fluids for *weeks* without finding anything but what he was already familiar with in the first days of his microscopical studies, as vibriones, bacteria, and

* 'Beginnings of Life,' p. 531. † Ibid., p. 447.

micrococci."* This is undoubtedly true of the *earlier stages* of the septic processes, but certainly not of the later. The first stages of a maceration are all that Billroth describes, without question; but if the animal matter exist in sufficient quantity, and the process be allowed to continue—not for the "weeks" of which this observer speaks, but for *months*—then an immense variety of flagellate forms arise, often wholly extinguishing almost every trace of the bacteria and their congeners, and still the putrefactive process is carried on with great vigour. Hence we are wholly disinclined to believe that the bacteria are the only, or even (in the end) the chief organic agents of putrefaction; for most certainly in the later stages of the disintegration of dead organic matter the most active agents are a large variety of flagellate monads.

We do not profess to decide what is the true nature of the monads we have studied; that is, to decide whether they be vegetable or animal. We nevertheless strongly believe in their animal nature. But if this be so, they afford another illustration of the inefficiency of the distinction between the animal and vegetable kingdom, which assumes that animals can only assimilate organic compounds; while vegetables can elaborate their protoplasm from those which are inorganic. We made a series of experiments on the transplantation of known forms to Cohn's "nutritive fluid,"† which contains no albuminous matter, but only mineral salts and tartarate of ammonia. The result was that we found that not only the bacteria, but the flagellate monads lived, throve, and multiplied in it, although supplied with no other pabulum. If it be affirmed that this is a proof of their vegetable nature, we can only say that the same must be said of the *kerona* of Ehrenberg and Dujardin, which flourished side by side with the monads, with this nutritive fluid as the sole source of pabulum. And both alike lived and multiplied in the dark.

In reference to the mode of locomotion among the monads, it may be remembered that what appeared like an *organ* of locomotion—an arrangement by which the action of the flagella appeared to be originated and controlled—was seen in the "biflagellate" form.‡ In every instance where there was only one flagellum, or where the two arise and move from the same point, their insertion in the body-sarcode was always in front; so that the flagellum or flagella had a pulling motion like that of the paddle of an ancient coracle; never the pushing motion from the stern like the sculling or rowing of a modern boat. This evidently

* 'Untersuchungen über die vegetation-formen von Coccobacteria Septica,' &c. Von Theodore Billroth. Page 3. Berlin. 1874.

† This fluid is composed of the following ingredients, viz. phosphate of potash, sulphate of magnesia, triple basic phosphate of lime, tartarate of ammonia, and distilled water.

‡ 'M. M. J.,' vol. xii., p. 264.

arises from the complete flexibility of the flagella, by which a propelling motion plainly could not be applied.

We now proceed to the description of the form immediately before us. For working purposes we have called it the "calycine." Its general appearance is accurately shown in Fig. 2. Its length varies from the $\frac{1}{900}$th to the $\frac{1}{1000}$th of an inch, its breadth in the broad part being a little more than a third of this; but it is compressed from side to side, and its width edgewise is not more than an eighth of its length. The form as shown in the drawing is very regularly preserved. The front of the body-sarcode is obliquely flattened, and at its lower part this gives rise to four flagella. They take their origin in a stalk which is short and almost at the point of contact with the body-sarcode divides into four pyramidal parts, out of which the flagella come. Just under the place where the flagella originate, at the flattened end of the sarcode, a spout-like projection occurs, as at *a*, Fig. 2. A long furrow goes obliquely down from this spout towards the pointed tail at the opposite end; and another depression occurs in the middle of the form, also lengthwise, giving the hourglass shape to the flattened extremity *b*. The sarcode is at times loose in texture, and the monad takes as a consequence unusual shapes, one of them being remarkably like a Brazil nut, and others still more distorted. The flagella are extremely fine, and are so rapid in their movement that their number was only certainly made out after nearly a week of watching; but as the forms became more inert we were enabled to discover their number accurately. A large nuclear body was always present in about the same position—*c*, Fig. 2—and two large "eye spots," with the strange rhythmical opening and shutting to which we have referred in other monads.* The position and relative size of these are shown at *e*; the diametrical line in each disk is the line of juncture, and from this both halves opened and then closed again with a snap. Its mode of locomotion was a graceful gliding through the water, the flagella moving so often and so rapidly as to render their detection impossible when the monad is at its swiftest. They could roll over on their long axis, and change the direction of their motion with lightning-like rapidity, and however crowded the field, not the slightest approximation to collision occurred.

A number of vacuoles are sometimes scattered over the sarcode, and at times the body becomes distinctly granular, the sarcode being slightly distended; and we have seen this granular matter quickly discharged, the body being left transparent and retracted. From the analogy of the "biflagellate" we were led to presume that this was one of the developmental phases, but the presumption was not confirmed by observation, and we simply record the fact.

The first process in the life history of this monad is, as usual,

* 'M. M. J.,' vol. x., p. 248; also vols. xi. and xii.

fission, and the more carefully this process is studied, the more remarkable it appears; for we have here not a uniform homogeneous rod of sarcode like the bacterium which we can easily imagine by mere growth to elongate and divide by transverse fission, as it is said to do, but we have a creature of distinctly marked shape, a certain amount of structure and differentiation of parts, of which each part appears to generate a counterpart of itself. How long *this* process may be going on we cannot exactly say, but the time taken in *visible* separation is from five to sixty minutes; but several times one-half of a fission has been followed, and whatever internal processes may have been at work must have been completed in from twenty to thirty minutes, for after that time fission has again ensued. But in every case the division results in two individuals equal to each other in shape and apparent size; a little less in bulk than the original monad, but in every sense as perfect.

The process is as follows. An actively swimming form becomes soft and plastic; the posterior end loses its sharpness, and becomes blunt and rounded. At the same time a semi-amœboid state ensues all over the sarcode, causing singular projections in every part of the body. In this state the nucleus-like body becomes very developed, and often is surrounded by what appears like something flowing from its substance. The "calycine" in this condition is drawn at Fig. 3. This may be repeated several times, after which a comparatively pear-shaped form is taken, the flagella being at the broad end: during the whole of this time the sarcode is in vivid internal motion—a kind of self-acting kneading process. At this time the root of the flagella is seen distinctly to split, dividing them into two separate pairs; and at the same moment a motion is set up which pulls the divided pairs of flagella asunder, making the interval of sarcode to grow constantly greater between them. This is seen in Fig. 4, where *a*, *b* show the complete division of the thick quadruple base of the flagella, and the arrows show the direction in which each pair is pulled. At the same time the nucleus shows marked symptoms of constriction, as is shown at *c*. From this time there is a tendency to take a rounder form, while the separate pairs of flagella rapidly diverge, as shown in Fig. 5; the flagella still moving in the direction of the arrows, and the nucleus-like body still more completely divided. The opening between the two pairs of flagella now rapidly increases, and the mass of sarcode becomes bean-shaped, as in Fig. 6, the nuclear disk having completely divided into two; at the same time an internal indication of constriction is given at *a*; and very shortly the two pairs of flagella have reached a position exactly opposite each other; the constriction has become very decided, as seen in *a*, Fig. 7, and the parts now evidently separating pull *against* each other, as seen by the arrows in the same figure. From this time the constriction rapidly deepens, the two halves become more normal in shape, until the moment when they are about to divide; they are drawn at Fig. 8, the nucleus-like bodies taking their normal place in each, and often the "eye-spot" making its appearance. Still pulling in the direction of the arrows, complete fission is effected, and each half is provided with a sharp "tail." Much of this, it is needless to say, was only made out after weeks, and in some instances months, of continuous labour, and only then with the highest powers. The general method of fission, indeed, was made out with the $\frac{1}{16}$th, with eye-piece giving 1200 diameters; but the complete *detail* was only successfully compassed by the $\frac{1}{25}$th and $\frac{1}{50}$th of Powell and Lealand, with diameters ranging from 2500 to 5000.

But even now the whole difficulty of fission in this monad was not overcome, for, as we have seen, it is normally provided with *four* flagella; but at fission these divide into two *pairs*, so that each half of the original monad, although complete in all other respects, has only *two* instead of *four* flagella. Yet in a very few minutes the free halves were seen to have acquired the full complement. At first, and for a long time, an inquiry into their mode of acquisition seemed hopeless; but we were at length rewarded by seeing the manner in which it happened. The newly fission-formed "calycine," after darting about rapidly but irregularly for some few seconds, attaches itself to the floor, or to some comparatively fixed object, by the free ends of the flagella, and remaining almost motionless itself, a rapid vibratory action is set up for nearly the whole length of the flagella, as seen in *d*, *e*, Fig. 9. Very speedily the ends *split*, as seen at *b* and *c*. This splitting is carried further and further towards the base, as seen in *b*, Fig. 10, where *c*, *d* have opened out nearly to the end until at length it opens completely, as seen in the same figure at *a*, *e*. The whole of this process was complete in 130 seconds after the pair of flagella became fixed and vibratory, and was seen perfectly with the supplementary stage and small condenser made by Powell and Lealand for developing the markings on *Amphipleura pellucida*. But it was also seen with the usual condenser and the $\frac{1}{50}$th.

The semi-amœboid condition preceding fission appears common to all these monad forms before any remarkable vital change. In the instance before us it was impossible to predicate whether this condition in any "calycine" in the field would issue in fission, or another vital process in its life history, to which we must now refer. Certainly the more frequent phenomenon was mere self-division; but long-continued observation showed in this case, as in others, that, although most frequent, it was far from being most important.

The fact that the semi-amœboid condition is common to both great transformations in this monad, and the one we are about to describe is very much the least frequent, enhances the difficulty of finding instances of the phenomenon. But if the amœboid forms be patiently watched here and there over two or three days of watching, some of those which we have described as in a self-kneading process internally will be seen to retain the form of the flagellate end more perfectly than others, while the opposite end of the sarcode will become much more truly amœboid. Now if this be carefully watched, the "ventral disk" (as we have called it for working purposes), or nuclear body, will be seen to grow unusually large and prominent. The "eye spots" will be seen to be in rapid rhythmical action, and soon—in the course of two or three hours—the posterior end of the sarcode will be strongly amœba-like, pseudopodia being protruded with a more constant and rapid motion than is usually seen in the amœba. A "calycine" in this condition is drawn at Fig. 11, and another very near the same field at the same time is seen at Fig. 12. It will be seen that the flagellate end of both is only slightly changed, and the large size of the nucleus-like body will be observed. Now in this condition they swim more and more slowly for some hours, until in some cases they cease to swim entirely, moving exactly as the amœba does, by the extrusion of pseudopodia. Indeed it could not be distinguished from an amœba but for the persistence of the shape of the flagellate end of the body and the slow waving of the flagella. Its aspect in this condition is drawn at Fig. 13. And one remarkable peculiarity of this condition is the great voracity of the creature. The "field" in its neighbourhood is rapidly cleared of dead and living bacteria, simply devoured by it. It is probable that this capacity for absorbing nutriment, which must give large advantage in the struggle for existence, explains the amœboid condition so common at what will be seen to be such an important period in the development of the monads.

In some instances it does not become so utterly amœba-like as this, but swims very slowly; but in either case, whether by swimming or creeping, if it meet another in a similar state the *amœboid parts touch*, and instantly blend into each other. This is shown in Fig. 14. In this condition the blended creatures may swim again with great freedom and ease, both sets of flagella acting apparently in concert. But now blending of the entire mass goes on, and in the course of thirteen hours the two "eye-spots" blend and cease to act; the two sets of flagella unite and fuse into mere sarcode, and the two nucleus-like masses, *a*, *b*, flow into one, until in about eighteen hours all trace of form is gone, and a somewhat irregular, distended sac is all that remains. This is drawn at Fig. 15; and in the course of another six hours this sac becomes round, and will be seen, if carefully watched, to pour out in all directions, without any violent splitting or breaking up, innumerable masses of little bodies, just visible to a magnifying power of 1800 diameters, and well defined as exquisitely minute oval bodies, highly refractive, with a power of 2500 diameters ($\frac{1}{50}$). This is drawn at Fig. 16.

The future of these minute bodies was carefully followed with the $\frac{1}{50}$th, and large numbers developed under examination, the development being distinctly traced in all its stages. First the minute and just perceptible specks appeared to swell—to grow larger in all directions, but they were perfectly inactive. This continued for from two to three hours, when some of them began to have a beaked appearance, as shown at *a*, *b*, Fig. 17. Growth was now very much more rapid, and at the end of two hours more they had assumed the shapes shown at Fig. 18. Between this time and the end of the next hour, in some way which we entirely failed to elucidate, flagella were acquired; in some cases two, but in the majority three, were made out, but never more at this stage. They now became rapidly motile, and of course the difficulties of noticing minute development increased; but their appearance thirty-five minutes after the acquisition of the flagella is drawn at Fig. 19, the nucleus-like body having definitely appeared. From this time they grew rapidly, and in many the four flagella could be seen; and at the end of the ninth hour after emission they had taken the parent form, and in all save size were the well-known "calycine," which had so long occupied our attention. Their aspect in this stage is drawn in Fig. 20.

The complete life history of this monad is therefore,—development from a germ or sporule of extreme minuteness, and on the attainment of maturity multiplication by fission, constantly and for an indefinite time; but the vital power is at intervals renewed by the blending of the genetic elements, effected by the union of two, when both are in an amœboid condition, from which a still sac results, in which germs or sporules are formed, which eventually escape, and again originate the life cycle.

We now proceeded to make the usual experiments on temperature, and its effect on the adult and the sporule. Our method has already been minutely described,* and in this case, as in all the others, was strictly followed. The result showed that the sporules of the "calycine" can resist a temperature of 250° Fahr. (121° C.), but no higher. We may quote one example in illustration. Six slides were taken, the contents of which were fully ascertained to be what was needed. They were heated in the usual way up to 250° Fahr., and allowed gently to cool. The contents were then examined with our best powers, first dry and then moist, but no trace of motion—even Brownian—was visible in any one instance. But in twenty-two hours from the time of heating three of the slides had a number of "calycine" forms in a very advanced stage, which had been watched from their origin in still gelatinous points,

* 'M. M. J.,' vol. x., p. 57.

as seen on former occasions, to the earliest stage of motion, and on to the acquisition of flagella. One other slide contained only a very few, which did not fully develop, and the other two, so far as this form was concerned, were barren.

This may be taken as a typical example. But we had ascertained on this and many former occasions that a temperature of 150° Fahr. (65°·5 C.), destroys utterly all the adult forms, as well as those which had reached any stage of development which might clearly be distinguished from the sporule.

In reviewing the whole series, then, it is plain that rapidity of increase and multiplication is the prominent feature in the vital phenomena of the monads. Everything subserves this end; but it also appears that the methods by which this prolificness is secured are dependent upon the recurrent blending of the genetic elements from which each species arises.

It may be well to compare the results of the whole.

In the cercomonad division by fission was the constant phenomenon. But this was the division simply of one into two. The result of the blending of the sexual elements was the production of spores in inconceivable quantities and immeasurably minute.* These survived exposure to a temperature of 260° Fahr. (178° C.).

In the "springing monad" the methods of increase were in a general sense the same.† In the "hooked monad" the increase by fission resembled broadly all the preceding; but it differed remarkably in the fact that the product of the genetic blending was not sporules, but minute *living* forms resembling the parents.‡

But the "uniflagellate" monad multiplied with enormous rapidity; not by mere bi-fissipartition, but by *multiple fission,* as many as from thirty to sixty being the product of *each* fission.§ And this form, after the union of the sexual elements, poured out innumerable myriads of sporules, so minute that at first they could not be seen by our highest powers, but it was merely perceived that a mass of something glairy was flowing out of the broken sac, and these were afterwards watched unceasingly, and seen in their early stages of development.

Now of these three forms, the two which poured out sporules were enabled *by* their sporules to survive a temperature of 148°·88 C. (300° Fahr.), but the form which developed *living* young only feebly survived a temperature of 82°·22 C. (180° Fahr.).

The "biflagellate" monad is characterized by multiple fission, and in addition a kind of parthenogenetic budding, aiding immensely in the rapidity of increase, and also the emission of minute sporules genetically produced; and these germs can survive a temperature of 121° C. (250° Fahr.), which is exactly the tem-

* 'M. M. J.,' vol. x., p. 53.
† Ibid., p. 245.
‡ Ibid., vol. xi., p. 7.
§ Ibid., p. 69.

perature resisted, with no injury to the developmental power, by the sporules of the monad now described.

Thus it will be seen that in no instance was the continuance of the species maintained without the introduction of a sexual process, a blending of what are shown in the sequel to be genetic elements, and thus going farther to suggest caution as to the supposition that *any* organism can be perpetuated by the mere self-division of single individuals.

Our heating experiments have uniformly proved the fact that the spores resulting from sexual generation have a power of resistance to heat over the adult which is greater in the proportion of 11 to 6 on the average, and this appears to us to be the very essence of the question of Biogenesis *versus* Abiogenesis. In some, at least, of the septic organisms spores are demonstrably produced, and these spores can resist a temperature nearly double that of the adults on the average; and that which some can resist is 88° Fahr. above the boiling point of water. All this is in general harmony with the admirable experiments of Dr. W. Roberts,* as well as with the later ones of Huitzinga,† who could not destroy the bacteria or their germs by boiling under a heat of 230° Fahr. continued for half an hour.

* 'Phil. Trans.,' 1874. † 'Academy,' March 13, 1875.

# II.—*On New and Improved Microscope Spectrum Apparatus, and on its Application to various branches of Research.*

By H. C. Sorby, F.R.S., &c., Pres. R.M.S.

(*Read before the* Royal Microscopical Society, *April* 7, 1875.)

## 1. *Description of Apparatus.*

The general construction of the spectrum eye-piece which I carried out some years ago in conjunction with Mr. Browning, is so well known that I will not occupy time in describing it, but confine myself to what is new.

Having lately felt the need of a complete yet compact instrument to take with me from home, I was anxious to arrange so that I could use it as a binocular microscope, and at once examine the spectrum of any object by merely inserting a slit into one of the eye-pieces, like a micrometer, and then placing the compound direct prisms over the ordinary eye lens. As usually made, this does not allow convenient space for focal adjustment of the spectrum, and is of too short focal length for good definition. Moreover, being a single simple plano-convex lens, the spherical aberration is objectionable; but by inserting a plano-concave lens of 7 inches negative focus under the prisms, the definition is all that could be desired, and the focal length is at the same time increased to what is practically the most convenient. In order to be able to compare together two spectra side by side, a small reflecting prism is fixed over half the slit, with another slit in front of it, which can be made narrower or wider so as to regulate the amount of illumination; since otherwise the spectrum of the light merely reflected by the prism would be much too bright for comparison with that of the magnified object on the stage. A small side stage to hold objects for comparison is permanently fixed on the main body of the instrument, in such a way as not to interfere with its use as a simple binocular microscope.

All these arrangements have turned out very satisfactory, being not only compact and convenient, but also as good in all other respects as is compatible with the eye-piece spectrum method. I, however, have found that for the study of the spectrum of solutions in small glass cells or test tubes, or that of any objects of sufficient size like portions of leaves or feathers, or in fact of anything not less than $\frac{1}{10}$th of an inch in diameter, it is far more convenient to use the binocular spectrum apparatus described in my paper in the 'Proceedings of the Royal Society,'* since we then view the two spectra with both eyes under more similar conditions, and can use two of the small glass cells at the same time, and illuminate both with one lamp with far greater ease than when one is on the stage and the

* 1867, vol. xv., p. 443.

other at the side of the eye-piece. The two spectra can also be readily made equally bright, which is of the very greatest importance where the instrument is used for comparative quantitative analyses.* The illumination of the object placed in front of the small reflecting prism can easily be regulated by means of a large bull's-eye condenser, and that of the object on the stage by using a plano-convex condensing lens of about ·6 inch diameter and of about the same focal length placed below the stage. With the ordinary diaphragm having several holes of various sizes, or with an iris diaphragm, on reducing the amount of light we also limit the width of the spectrum. In order to avoid this it is requisite to reduce the light by using a *narrow* but *not shorter* beam, and for this purpose I have found a diaphragm with a volute opening extremely convenient, since by turning it round the amount of light can be regulated to a great nicety without in any way limiting the length of the spectrum.

## 2. *Measurement of Bands.*

In many of my former publications I have described the manner in which I have used as a scale of measurement the dark bands in the spectrum of the light passing through a plate of quartz between two Nicol's prisms, the thickness of the plate being such that the whole visible spectrum contains twelve dark bands, and the Fraunhofer's line D is half-way between the bands 3 and 4, counting from the red end. With the binocular instrument the bands seen in any spectrum could be measured by means of an ordinary ruled micrometer in the eye-piece, but it is not only very difficult to see lines over the blue, but the least movement of the apparatus would throw all out of adjustment. Mr. Browning's method of measurement, described in this Journal (vol. iii., p. 68), though extremely useful for certain purposes, cannot be conveniently adapted to the binocular instrument, and even when employed in the manner proposed by him, has unfortunately several serious defects for expeditious practical working. Slight movements throw it out of adjustment, and it is very inconvenient to have to measure in all cases from the sodium line, and to have to read off from the small graduated circle; but a still greater objection is that, if the bright dot be arranged over the centre of an absorption band when the slit is narrow, it is no longer over the centre when the slit is made wider, and such an alteration in width is often very desirable. For these reasons I make use of Mr. Browning's plan only for particular purposes, and for general work still employ the interference scale, since no movement of the instrument can alter the relative position of the bands in the scale

* For illustrations of such analyses I refer to my paper in the 'Proceedings of the Royal Society,' 1873, vol. xxi., p. 442.

and in any spectrum under examination, both being affected in the same manner and to the same amount; and, moreover, the position of a band in any part of the spectrum is seen at once, without having to measure from it to some more remote fixed point. Another great advantage is that the measurements given by the interference bands agree far more closely than any others with the law of the position of absorption bands, and thus the observer is able at once to draw several important conclusions from the actual measurements, without previous reduction to another scale.

### 3. *Wave-length Method.*

Until quite recently, in common with nearly all writers on absorption spectra, I have given the position of the bands in reference to an arbitrary scale; but I feel quite convinced that for the future we ought all to adopt the plan now employed by many in the case of luminous spectra, and express everything in terms of wave-lengths. For the purposes of the spectrum microscope, it appears to me that the most convenient units are millionths of a millimeter. It is just possible in some cases with great care to measure to that degree of accuracy, but not to a smaller quantity, and therefore it seems undesirable to use four figures when three will express all that can be certainly determined. In order to be able to make use of this system I have with great care constructed a table, giving the wave-lengths of every $\frac{1}{8}$th division of my quartz interference scale, so that, after having measured the position of any part of the spectrum by means of this scale, I can by the use of the table at once express everything in terms of millionths of a millimeter of wave-length. I propose to publish this table and give with it the means of correction, in case anyone should have a quartz scale not accurately corresponding to my own, so that the error would be of no practical importance, and each observer could express the measurements made by his own scale in accurate wave-lengths. Not only will this, as I trust, lead to the use of an uniform system, but measurements thus expressed, having a definite relation to the most important physical character of the light of the different parts of the spectrum, and not being in any way arbitrary, there is *a priori* a far greater probability that a comparison of the results will lead to the discovery of true general laws. Since I have adopted this system I have been led to look on the whole subject from an entirely new point of view, and to perceive the importance of many phenomena which previously did not appear to have any well-marked significance. On the present occasion I do not think that I could do better than describe a number of facts which seem to point out that the wave-length system is that likely to lead to the most perfect development of the spectrum method of research.

4. *Relation between the Absorption Bands in different Solutions.*

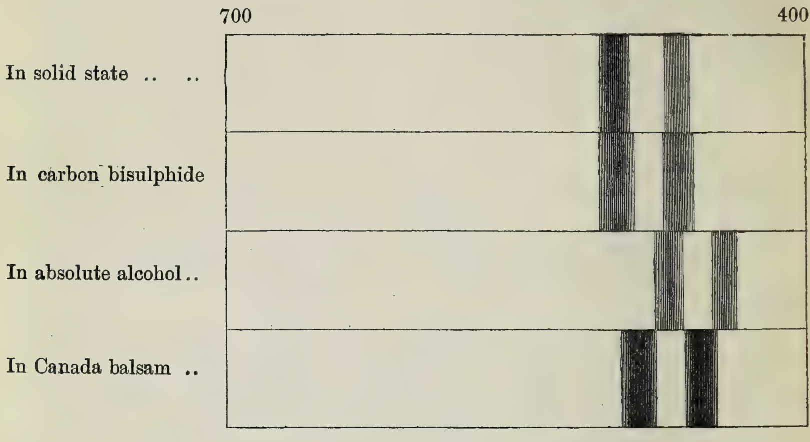
Much still remains to be learned before we can form a satisfactory opinion as to the full significance of the absorption bands seen in the spectrum of any substance. It is quite clear that they must indicate that its ultimate particles are in some way or other so related to waves of light of particular length that their force is expended either in raising the temperature of the solution or in causing chemical change, whereas waves of other lengths pass through comparatively unimpeded, as though the bands were in some way or other connected with the *size* of the particles. The general character of the spectrum—whether having one or more bands, which are narrow or broad, dark or faint—must, I think, depend on the *shape* or *constitution* of the particles, and therefore it seeems probable that the spectrum as a whole furnishes us with evidence both as to the character and magnitude of the ultimate molecules.

It may perhaps be premature to conclude that it is a law subject to no important correction, that when the spectrum of a single substance contains a number of well-marked absorption bands they are all related to one another in a perfectly definite manner, but as far as I am able to judge from my present knowledge, there is a far more uniform connection between the wave-lengths of their centres than between any other condition. At all events, in many spectra having a series of bands whose centres are at wave-lengths $a$, $b$, $c$, and $d$, there is the same ratio between each consecutive two, so that $\frac{a}{b} = \frac{b}{c} = \frac{c}{d}$. Possibly this may be a true general law, subject to modifications depending on particular conditions. Another important point is that, in the case of substances giving two or more well-marked bands, though the position of these bands may vary very considerably in the spectrum of the solid substance and in that of its solutions in different solvents, that is to say, though the *actual wave-lengths* of the centre of the bands may vary with the conditions in which the substance occurs, the *ratio* between the wave-lengths of the bands remains almost if not quite constant, the discrepancies being no greater than may be due to errors of observation. As a good example, I will refer to yellow xanthophyll.

| Condition. | Centre of the two Bands. | | Ratio. |
|---|---|---|---|
| In free state and solid .. .. .. .. | 501 | 469 | 1 : ·936 |
| Dissolved in carbon bisulphide .. .. | 498 | 467 | 1 : ·937 |
| Dissolved in absolute alcohol .. .. | 471 | 442 | 1 : ·938 |
| Combined with Canada balsam .. .. | 488 | 457 | 1 : ·936 |

The position of these bands and also their general character will be better understood by means of Fig. 1.

Fig. 1.—Spectra of Yellow Xanthophyll.



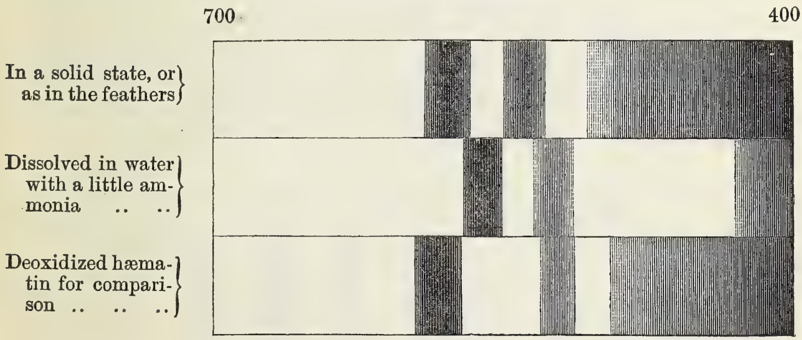

In this and all the following drawings I do not give the spectra as they are seen with a *prism*, but as they would appear in an *interference* spectrum, in which the dispersion is in direct proportion to wave-lengths. This of course makes the red end much broader and the blue end much narrower than in the spectra usually observed. For convenience of reference I give the wave-lengths of a few Fraunhofer lines:

A 760 D 589 F 486 G 430

The change in the position of the bands often makes it easy to ascertain the condition in which a substance occurs in a living organism. For example, when yellow xanthophyll is dissolved in even a very small quantity of oil, the bands are at wave-lengths 483 and 453, whereas, when it is in a free state, they are at 501 and 469, and these differences show that in the petals of some yellow flowers like *Chelidonium majus* it exists in the free state, though in the majority of cases it is dissolved in an oil. The spectra also show that in some flowers it is associated with wax or hard fat, in which it dissolves when warmed, but is set free on becoming cold and crystalline.

The colouring matter of the red feathers of the Plantain eaters, described by Professor Church, and named by him *turacine,* is also an excellent illustration of the connection between the spectra of the solid substance and its solution, as will be seen from Fig. 2 and the following table. Along with them I give the spectrum of deoxidized hæmatin, to show how spectra of very similar character may be completely distinguished by a great difference in the ratio between the wave-lengths, whilst the spectra of the same substance in two different conditions may vary far more in general appearance, and yet the ratio be the same.

Fig. 2.—Spectra of Turacine.



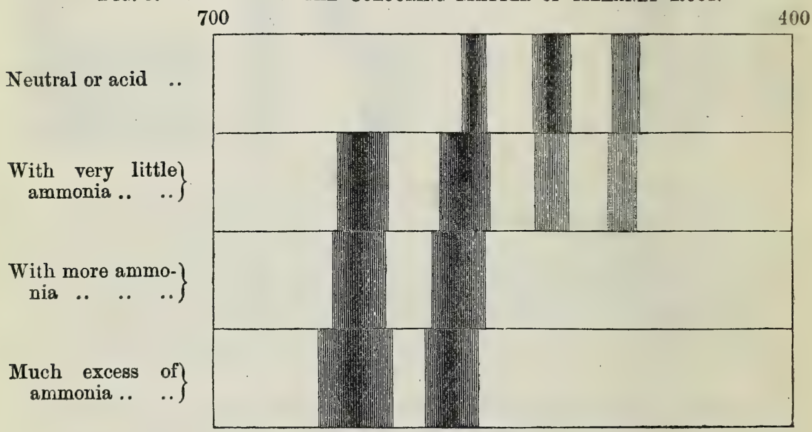

| Condition. | Centre of Bands. | | Ratio. |
|---|---|---|---|
| In a solid state, or as in the feathers .. | 582 | 541 | 1 : ·9294 |
| Dissolved in water with a little ammonia | 564 | 524 | 1 : ·9290 |
| Deoxidized hæmatin for comparison .. | 586 | 522 | 1 : ·8908 |

One band of the deoxidized hæmatin is thus nearly in the same place as one of the solid turacine, and another nearly in that of the other of the solution.

As will be seen from the above figures, there is not only a change in the position of the bands, but also in their intensity and width, according to the nature of the solvent, and these changes may produce a certain amount of departure from the general law. The cause of this will be more apparent when we have considered the effect produced by the presence of free acids and alkalies.

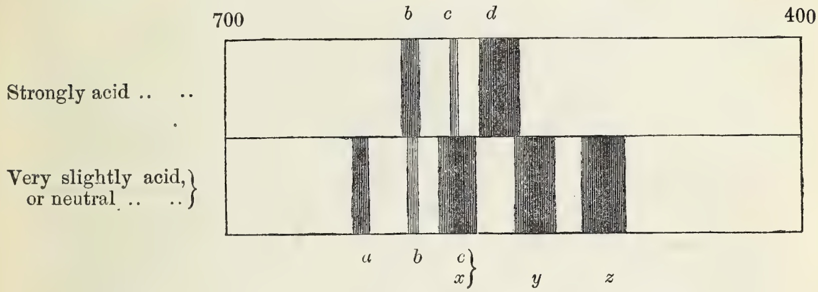
Many vegetable colouring-matters give spectra with a single broad absorption band, whose position and character vary considerably, according as the solution is made acid or alkaline. Usually acids raise the centre of the band towards the blue end, whilst alkalies lower it towards the red end. The red colouring matter of Alkanet root (*Auchusa tinctoria*) is a good example of another kind of change, when the spectrum shows several bands. When dissolved in alcohol containing some water, the spectrum shows three absorption bands, and on adding an extremely small quantity of ammonia these bands are slightly and unequally depressed towards the red end, so that the interval is a little increased, and a new band is developed in the red, at a wave-length interval equal to that between the others. The addition of a little more ammonia entirely removes the two bands which lie towards the blue end, so that the spectrum contains only two at nearly the same wave-length ratio as when the solution is neutral; but on adding large excess of ammonia they are unequally depressed towards the red end, the interval becomes less, and the colouring matter is rapidly decomposed, the change in the wave-length ratio being perhaps in some way connected with the decomposition. All these changes will be more easily understood by means of Fig. 3.

Fig. 3.—Spectra of the Colouring Matter of Alkanet Root.




Another excellent illustration of the effect of acids is furnished by the alcoholic solution of the red colouring matter found in the shells of birds' eggs, for which I have proposed the name *oorhodeine.* When neutral, or containing only some such weak acid as acetic, the spectrum exhibits five bands, whereas when the solution contains excess of some such strong acid as hydrochloric, there are only three bands, and these two spectra have such a different character that at first sight they would not be thought to be related to one another. However, on more careful examination it will be found that they have two bands in common, whose centres are of nearly the same wave-length, but the band which is very faint in the case of the neutral solution is dark in that of the acid, and that which is dark in the neutral is faint in the acid. The three other bands are destroyed by the strong acid, and a new very dark band developed. All these facts will be more easily understood by means of Fig. 4.

FIG. 4.—SPECTRA OF OORHODEINE DISSOLVED IN ABSOLUTE ALCOHOL.




On discussing the wave-lengths of the centre of the bands in the case of the neutral solution I have found that they constitute two different series of three bands each, having the central band in common, those in each of the two series having the same interval ratio, but the intervals between the two series different, and related to one another in some such ratio as 5 to 8. Perhaps my meaning will be more easily understood by representing the wave-lengths by letters, and the relative ratios of the intervals by numbers, as follows:

$$\text{Neutral} \ldots \ldots \ldots \ldots \left\{ \begin{array}{ccccccccc} & 5 & & 5 & & & & & \\ a & & b & & c & & & & \\ & & & & x & & y & & z \\ & & & & & 8 & & 8 & \end{array} \right.$$

$$\text{Strongly acid} \ldots \ldots \left\{ \begin{array}{ccccc} b & & c & & d \\ & 5 & & 5 & \end{array} \right.$$

It will thus be seen that a strong acid entirely destroys the series at the wider interval ($x\,y\,z$), removes the band ($a$) at the red end of the narrower series ($a\,b\,c$), and develops the new band ($d$) at an equal interval towards the blue end.

I could give many other illustrations of similar facts, but these few will, I trust, suffice to show that this wave-length method of study enables us to establish simple relations in cases where little or no connection could be recognized if any arbitrary scale were adopted, and such being the case I do not hesitate to claim for it precedence of all others.

### 5. *Relation between the Bands in different closely connected Compounds.*

So far I have considered only cases where a substance is examined merely in different physical states, but now proceed to those in which the physical state is the *same*, but the substance itself *chemically* modified. Hæmoglobin is a very instructive example. As is so well known, the oxidized modification gives a spectrum with two well-marked absorption bands, and on passing through the solution a stream of carbonic oxide, the loosely combined oxygen is replaced, and the spectrum shows two absorption bands, very similar to the original, rather nearer to the blue end. On the contrary, when the oxygen is replaced by passing nitric oxide through an ammoniacal solution, though we still see two such bands, they lie nearer to the red end. In all three cases, however, the ratio between the wave-lengths of the centre of the two bands is, very nearly, if not absolutely, the same. The changes, therefore, are analogous to what would occur if we were to strike two strings on a harp, and then strike them again, raising or lowering their pitch by means of the pedals, the effect of the substitution of the oxygen by carbonic oxide being as it were to make the bands more sharp, whilst nitric oxide makes them more flat. The removal of the oxygen produces a total change, and is thus unlike mere substitution.

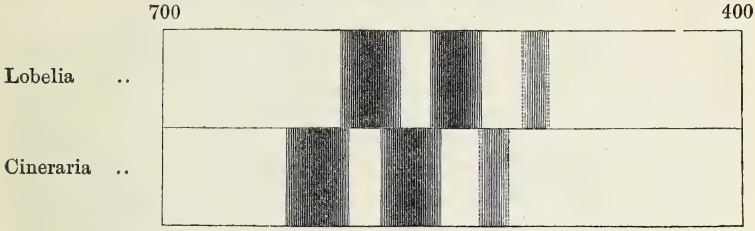
### 6. *Probable Evidence of Chemical Relationships afforded by Spectra.*

Such, then, being the relation between the spectra of compounds which are known to be related in a very simple manner, and can be changed one into the other, it becomes a question of much interest to consider whether when we meet with spectra having similar relations the substances may not be in some way connected, although it may be impossible to convert one into the other.

I find that by different processes two different modifications of hæmatin may be prepared, which give very similar spectra, but the absorption bands are in very different positions, though at the same wave-length ratio. On deoxidizing both by means of a little of the sulphate of protoxide of iron and ammonia, both are altered and give spectra similarly related, but by deoxidizing with sulphide of ammonium both are changed into the same substance and give the same spectrum. Here, then, we see that these two kinds of hæmatin must be connected in some simple manner, and by a strong deoxidizing reagent can be both changed into the same substance. There are, however, cases in which two substances give spectra having the above-named characters, and on adding various reagents both are altered in the same manner, but yet, so far, it is not known that one can be changed into the other, or both into some common product. Various plants furnish illustrations of this fact, but I do not know any more decided than the purple colouring matter of *Lobelia speciosa* as compared with the pink colour of

*Cineraria.* The spectra in a neutral aqueous solution are shown in Fig. 5.

Fig. 5.—Spectra of Lobelia and Cineraria.




| | Centres of Bands. | Ratio. |
|---|---|---|
| Lobelia .. .. .. .. | 619 573 529 | 1 : ·9257 |
| Cineraria .. .. .. | 594 550 509 | 1 : ·9258 |

It will thus be seen that the ratios are all practically identical, but the actual wave-lengths very different; and judging from what is known in the above-described cases, it appears to me very reasonable to suppose that there is some simple but unknown molecular or chemical connection between these two different colouring matters. The spectra do not differ more than is often seen in the *same* substance dissolved in different liquids; and if it were possible for such a substance to be in some way more permanently associated with two different compounds which acted in this manner, so that when thus combined they were not decomposed or dissociated when dissolved, all the observed facts would be explained in a very simple manner. It would be premature to look upon this as anything more than a plausible hypothesis, but it would explain many facts met with in studying the colouring matter of animals and plants.

Another striking example of two spectra thus related to one another is seen when we compare the spectra of the oorhodeine of birds' eggs with those of the product of the decomposition of hæmoglobin by strong sulphuric acid, discovered by Thudicum and named by him *cruentine.* I have already described the spectra of oorhodeine, and given them in Fig. 4. Now those of acid and neutral cruentine are exactly the same in every respect, but the bands in both cases all lie somewhat nearer to the blue end. The spectra may indeed be looked upon as the same complicated chord of musical notes, and as differing from one another merely by a slight difference in pitch, the spectra of cruentine being the same as those of oorhodeine, only as it were a little sharper.

### 7. *Conclusion.*

Having only recently adopted the above-described wave-length system, I have not yet been able to apply it sufficiently to feel confident that the kind of general laws now briefly indicated are of universal application. Before any such conclusion would be justifiable, far more examples must be carefully studied, with apparatus specially designed for the investigation of the mutual relations of the absorption bands, and this inquiry must be accompanied with appropriate chemical and physical experiments. This is what I propose to do, as soon as circumstances permit, since I feel that what is now known is little more than sufficient to indicate the line of research that ought to be followed, and to show that the wave-length method is incomparably better than any other. We can scarcely believe that spectra are not subject to definite laws; and, though it may prove difficult to ascertain all these laws in terms of the most important physical character of the light of different parts of the spectrum, yet we may be certain that no such general relationships could be detected by discussing the measurements given by an arbitrary scale, the value of the divisions of which in different parts depends upon the irregular dispersion of the prisms, and have no simple and direct connection with any physical peculiarity of the light itself.

*Note.*—The figures of spectra given in this paper being drawn in relation to wave-lengths, as if seen in an interference spectrum, the red end will appear to be abnormally expanded and the blue end contracted to anyone accustomed only to spectra seen in prism spectroscopes.

## III.—*Some Remarks upon Sphæria (Gibbera) morbosa (Schw.).*

By Charles B. Plowright.

The March number of the Journal contains a paper by Mr. Thomas Taylor, Microscopist of the United States Department of Agriculture, upon "Certain Fungi Parasitic upon Plants," a subject of great interest not only to mycologists, but also to the scientific public at large. The two species treated of are the *Sphæria morbosa,* Schweinitz, which infests the living branches of plum and cherry trees in the United States, causing them to be covered by unsightly and destructive swellings, and ultimately causing the death of the affected branch. The other fungus (*Erysiphe Tuckeri*) is the parasite which, in some of its earlier stages, produces the *too* well known vine disease. Of it in the ascigerous condition we have seen no specimens, and therefore offer no remarks, but would only suggest that Fückel, in his 'Symbolæ Mycologicæ,' places it in the genus *Sphærotheca,* as a variety of *S. Castagnei,* and that it would be desirable in future observations to bear this in mind, with a view of ascertaining the correctness or otherwise of Fückel's view.

With regard to the *Sphæria morbosa,* the perusal of Mr. Taylor's paper has suggested the following considerations, which may be of interest to your readers. In the Quekett Journal for October, 1872, are "Some Notes on the Black Knot," by Mr. C. H. Peck, in which are detailed a series of observations on the life history of this plant. Mr. Peck finds the mycelium of it may be detected in the month of November by the swellings its presence produces in the bark of the young twigs. As this swelling increases, cracks appear in the cuticle which expose the inner bark. If a portion of the inner bark thus exposed be now examined, "slender jointed filaments may be seen, which have insinuated themselves between the bark cells." During winter these swellings remain quiescent, but in spring they enlarge, and by the end of May patches having a dark-green velvet-like surface make their appearance upon them. On examination, these dark-green patches prove to be an assemblage of cladosporoid threads, bearing oval triseptate conidia. In course of time the perithecia appear in and amongst the conidia, but it is not until after the lapse of a considerable time —several months—that perfect ascigerous fructification is produced, namely, during the following winter and spring.

Mr. Taylor informs us that the asci measure about $\frac{1}{1000}$th inch in diameter by $\frac{7}{1000}$th inch in length, and that he has "counted as many as ten sporidia in one ascus." See Plate XCVI., where figures C and U represent asci containing each ten sporidia.

Now the very fact of a Sphæria having asci containing ten sporidia would render it a very interesting species, independently of its manner of growth, &c. As far as our personal knowledge goes, we cannot recall any species having this character. The vast majority of these plants having octosporous asci; in a few instances, such as *Dothidea tetraspora*, B. and Br.; *Valsa tetratrupha*, B. and Br.; *Valsa salicella*, Fr., &c., the asci are tetrasporous, while, upon the other hand, some species of the genus *Hypocrea*, such as *H. rufa*, *gelatinosa*, *delicatula*, &c., have sixteen, and in a few instances furnished by the genera *Torrubia* and *Diatrypella*, and by *Sphæria ditopa*, Fr., &c., the number of sporidia is unlimited.

Being fortunately in the possession of two specimens of *Sphæria morbosa*, one communicated by Mr. Peck, and the other by Mr. Gerard, we examined them with a view of testing the accuracy of Mr. Taylor's observations. One of our specimens exhibits the conidial stage described by Mr. Peck *ut ante*, but in it the asci are barren. The other specimen, from Sandlake, N.Y., contains asci, paraphyses, and sporidia. The asci are not of uniform size, but those we examined measured ·0005 inch by ·003 inch. The sporidia measure ·001 by ·0003 inch, are hyaline, with a pale-yellow tinge, flask-shaped, uniseptate, biseriate, and very much crowded in the asci, so as to make it a matter of considerable difficulty to count them; but whenever we succeeded in so doing, we always found the number to be eight.

*Sphæria morbosa* bears a considerable resemblance to *Cucurbitaria cupularis*, Fr.,* but its parasitic habit, upon the bark of living branches, clearly indicates its affinity with such plants as *Gibbera vaccini*, Fr., in which genus we think it should be placed.

TERRINGTON ST. CLEMENT'S.

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## IV.—*The Amœban, Actinophryan, and Difflugian Rhizopods.*

By G. C. WALLICH, M.D., F.L.S., &c.

IN last November's number of the 'Monthly Microscopical Journal,' and the number for this month (April), notices have appeared regarding alleged new discoveries and observations on certain Rhizopods published by Professor Leidy in recent issues of 'Silliman's Journal.' As I derive my present information on this head altogether from these notices and one other journal to which I shall have occasion immediately to refer, it is not my wish at present to enter further into the questions raised than I am about to do. Enough has been written, however, to warrant the conclusion that

* 'Sphæriacei Britannici,' No. 57.

the alleged discoveries and observations of Professor Leidy were most of them, if not all, forestalled by me some twelve or thirteen years ago; although I will at once do Professor Leidy the justice to declare my implicit conviction that he published his recent papers in entire ignorance of my having handled the same subjects so long ago before him.

It is, moreover, necessary that the matter should be placed in a proper light, inasmuch as one or both of the notices in the Microscopical Society's Journal have apparently been made the groundwork of a somewhat similar notice which appeared in the 'Academy' of March 13, a copy of which was sent me by a friend in order to draw my attention to what was going on. The commentator in that journal wrote in a most friendly spirit, briefly citing "some remarkable observations made by Dr. Wallich in 1863, which were published in the 'Annals and Magazine of Natural History' for that year, adding that I had at the time furnished him with living specimens in confirmation of my researches. Unfortunately, and no doubt most unintentionally, this friendly critic stopped a little short of the mark, for he did not state, as he certainly would have been justified in doing, that I had been the first to discover and *name* the very Rhizopod *Amœba villosa*, which, so far as can be gathered from the brief notices referred to, constituted one of the principal subjects of Professor Leidy's investigations and descriptions. But I had also seen in the November number of the 'Monthly Microscopical Journal' an account, somewhat more in detail, of certain observations by Professor Leidy on *Difflugian* Rhizopods supposed by him to be new, and of which he had published a description in another recent number of 'Silliman's Journal.' Now, unless I am very greatly mistaken, these very *Difflugiæ* will be found to have been fully described, and, what is more, carefully figured by me in my monograph of the entire family which appeared in the Annals for March, 1864.

I am quite sure it is only necessary for me to refer Professor Leidy and readers of American scientific journals to the series of observations contributed by me in 1863 and 1864 to the 'Annals and Magazine of Natural History,' and to some later papers which appeared in the 'Monthly Microscopical Journal' in 1869, to elicit an ungrudging acknowledgment that some, at least, of the observations on the *Amœban* and *Difflugian* Rhizopods referred to as having so recently been put forth in 'Silliman's Journal' had long since been recorded by me; although Professor Leidy and I, unfortunately, entertain very different opinions on the interpretation of characters said to involve generic and specific distinctions.

It is not my intention, and I am glad to be able to believe that, in the present instance, it is quite needless for me to enter into any lengthened discussion on the subject. I have too much faith in

American honesty for that, and can affirm that American journals have more than once done me justice where others have not chosen to do so. I shall therefore conclude this statement by simply furnishing a detailed list of the papers published by me, with the dates of publication, &c., to which allusion has been made, and by stating at the same time, for the information of those who take sufficient interest in the questions treated to induce them to reinvestigate the matter for themselves, that the figures which accompany my papers were, without exception, drawn by me from typical specimens then under observation with the aid of the microscope; that I will guarantee their accuracy; and that, in every instance in which the process was possible, the original specimens were preserved and consigned to slides which may be consulted in the cabinet and compared with drawings, &c., which I had the honour of presenting to the Royal Microscopical Society in 1865.

The subjoined is the list of papers:

1. On an Undescribed Indigenous Form of *Amœba*. With a Plate. (pp. 4.) 'Annals and Mag. Nat. Hist.,' April, 1863.

2. Further Observations on an Undescribed Indigenous *Amœba*. With a Plate. (pp. 6.) 'Annals and Mag. Nat. Hist.,' May, 1863.

3. Further Observations on *Amœba villosa* (*Wall*), and other Indigenous Rhizopods. With a Plate. (pp. 20.) 'Annals and Mag. Nat. Hist.,' June, 1863.

4. Note. Do Diatoms Live on the Sea Bottom at Great Depths? (p. 1.) 'Annals and Mag. Nat. Hist.,' Aug. 1863.

5. On the Value of the Distinctive Characters in *Amœba*. (pp. 41.) 'Annals and Mag. Nat. Hist.,' Aug. 1863.

6. Further Observations on the Distinctive Characters and the Reproductive Phenomena of the *Amœban Rhizopods*. (pp. 9.) 'Annals and Mag. Nat. Hist.,' Nov. 1863.

7. Further Observations on the Distinctive Characters, Habits, and Reproductive Phenomena of the *Amœbæ*. (pp. 21.) 'Annals and Mag. Nat. Hist.,' Dec. 1863. With a Plate.

8. On the Process of Mineral Deposit in the Rhizopods and Sponges, as affording Distinctive Characters. (pp. 10.) 'Annals and Mag. Nat. Hist.,' Jan. 1864.

9. On the Extent and some of the Principal Causes of Structural Variation amongst the *Difflugian Rhizopods*. (pp. 30.) 'Annals and Mag. Nat. Hist.,' Mar. 1864. With 2 Plates.

10. Observations on the *Thalassicollidæ*. (pp. 6.) 'Annals and Mag. Nat. Hist.,' Feb. 1869.

11. On the Structure and Affinities of the *Polycystina*. Read before R.M.S., May 10, 1865. Published in 'Monthly Micros. Journ.,' July, 1865. (pp. 28.)

12. On the Vital Functions of the Deep-Sea Protozoa. (pp. 10.) 'Monthly Micros. Journ.,' Jan. 1869.

13. On some Undescribed Testaceous Rhizopods from the North Atlantic Deposits. (pp. 7.) 'Monthly Micros. Journ.,' Feb. 1869.

14. On the Rhizipods as embodying the Primordial Type of Animal Life. (pp. 7.) 'Monthly Micros. Journ.,' April, 1869.

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## V.—*Note on the Diagnosis of Blood Stains.*

By Jos. G. Richardson, M.D., Microscopist to the Pennsylvania Hospital, Philadelphia, U.S.A.

One or two points in my paper "On the Value of High Powers in the Diagnosis of Blood Stains"* having been somewhat sharply, although courteously criticized in these pages by my friend Dr. J. J. Woodward, U. S. Army of Washington, D.C., I wish to add a few words in explanation.

Dr. Woodward states that he writes to point out that it is "never in the power of the microscopist to affirm truthfully on the strength of microscopical investigation, that a given stain is positively composed of human blood, and could not have been derived from any animal but man." With this proposition I fully agree, contending, however, that whilst it is literally true, it is *not the whole truth,* because, as may often happen in medico-legal practice, *when evidence other than microscopical* narrows down the conditions of the case to the question, Is this stain human blood or that of an ox, pig, or sheep? the microscopist can from fair specimens of blood spots as ordinarily produced, affirm truthfully that the "given stain is positively composed of human blood," should it really be so. This second statement I believe Dr. W. will admit, as an additional part of the truth, and if not I undertake to convince him (as I have some other candid doubters among my friends), by incontestable evidence.

Our real difference then is not mainly upon matters of fact, but on a matter of opinion, respecting the *just prominence* which should be given to the circumstance that "the blood-corpuscles of a few mammals approach so nearly in size to those of man as to render their distinction doubtful," a fact, be it observed, *which I thus in these words explicitly mention,* on p. 153 of my essay in this Journal for September, 1869,† of which my paper above referred to is avowedly a continuation.

Now, whilst I honour Dr. Woodward for the discharge of what

* See the 'American Journal of the Medical Sciences,' July, 1874, and this Journal for September of the same year.

† And also 'Handbook of Medical Microscopy,' p. 288. Philadelphia. 1871.

he conscientiously believes to be his duty, it seems to me, with all due deference to his superior experience, that he has in the first place a little undervalued the caution and prudence of our numerous medical brethren, who possess microscopes without considering themselves experts, and second that he has overlooked a most important factor in the calculation which we both, perhaps equally, sought to make, of how to secure for humanity by our researches the maximum advantage with the minimum amount of injury. This factor I conceive to be the keen sharp-witted lawyer to be found not only in every city, but in every county town throughout English-speaking countries, who whilst studying *during* a trial my essay, if it were brought forward to support the baseless pretensions of an unqualified microscopist, claiming to distinguish human from dog's or monkey's blood, would infallibly discover, that not one syllable of its carefully worded statements could be construed into warranting such a groundless assumption.

Hence, trusting to this powerful and omnipresent element, for the protection of two or three innocent persons, who might possibly be in danger through my agency of conviction for manslaughter, I felt whilst writing both my first paper and its continuation, that should I more than indicate the animals which render our conclusions doubtful, my work would be rendered really prejudicial to the interests of society. Indeed it was, I think, fairly to be anticipated, that if I should emphasize and reiterate the fact that science alone could not detect the falsehood of a criminal's story, if he cunningly asserted that suspicious stains were made by the blood of a dog, not only would I frequently obstruct the course of justice, but some jealous critics would utterly condemn my investigations, and compare me to a locksmith winning a wide reputation among the "dangerous classes," by an essay most minutely teaching thieves *the truth*, in regard to the secrets of opening bank vaults and fireproof safes; or to a toxicologist publishing a treatise, setting forth *most faithfully* the method by which poisoners may best destroy their victims, with the least danger of detection in their crimes.

It should be remembered also, that in all cases a really innocent person, wrongly accused of murder on the ground of blood stains upon his clothing, &c., actually proceeding from that "constant" (yet rarely slaughtered) "companion of man," the dog; or from some of our unusual associates, such as seals, otters, guinea-pigs, &c., needs neither Dr. Woodward nor Dr. Richardson to prompt him to tell (and try to corroborate the assertion) when first arrested, the *true* origin of the suspicious blood spots. And if the story which he relates after legal consultation and advice is not *the truth*, I would enter my protest against that *pseudo* philanthropy, too fashionable at the present day, which shuts its ear to the righteous appeal of our brother's blood, when, as in the days of Cain, it crieth unto us from the ground, and yet listens with a half maudlin sympathy to the pitiful tale of a guilty criminal appalled at the prospect of just punishment for his sin.

Such then were the arguments which influenced me to publish my researches in a guarded manner; but now, since Dr. Woodward has bravely taken all responsibility for harm that may ensue upon himself, I am glad, for the benefit of the *few* rash microscopists and dull-brained lawyers our countries produce, to state explicitly my full corroboration of the Doctor's assertions, and declare that as far as I am aware there is at present (1875) no method known to science for discriminating, microscopically or otherwise, the dried blood of a human being from that of a dog, monkey, rabbit, musk rat, elephant, lion, whale, seal, or in fact any animal whose corpuscles measure more than $\frac{1}{4000}$ of an inch in diameter. Hence, therefore, until further discoveries are made, a microscopist's best efforts at revealing crime can only serve the cause of right and justice in those cases where the criminal's attorneys, in spite of being *forewarned*, and consequently forearmed, are unable to prepare or suborn testimony to show that one of the creatures just enumerated has been killed in such a way as to produce blood stains, which are likely to be confounded with those from the murdered victim. Of course, however, a *change in the prisoner's story*, so as to attribute the blood spots to a dog, monkey, &c., *after* consultation with shrewd lawyers for the defence, or scientific friends, as in the case mentioned below, *must have great weight with the jury*, and go far to *put them on their guard against the crafty trick* attempted upon their intelligence.

That I was led to avoid reiterating and emphasizing this failure of our science by no unfounded apprehension of the evil likely to arise from dinning such knowledge into the ears of rogues, is proved by the fact that after my testimony was delivered in the Lambee trial at Franklin, Venango Co., Pa., the prisoner's "keen, sharp-witted lawyer" brought two female witnesses into court who testified that on a certain occasion about the time of the murder, when the defendant's boots (on which were the suspected blood spots) were standing in the corner of a particular room, they were engaged in clipping the ears of a terrier dog, and just as they got one ear done the baby cried, and they were obliged to let go the dog, which ran round the apartment shaking its head, and thus sprinkled the boots with its blood. Further to substantiate this tale, a dog with one ear clipped was exhibited to the jury, and sworn to as the very one from which the blood was shed. Fortunately, however, it so happened that I had examined one or two spots upon the prisoner's pantaloons, finding them to be human blood in contradistinction to pheasant's blood, *as he first explained* *them to be;* and since the contrivers of this dog story apparently forgot that the pantaloons were not standing up in the boots, and consequently had no chance to become sprinkled along with them, their ingenious theory failed to gain credence with the jury, who brought in a verdict of guilty of murder in the first degree.

I venture to predict that from Dr. Woodward's paper and this note to my own essays will spring, as from the dragon's teeth of ancient fable, a host of bloody dog tales to account for suspicious stains on the clothing, &c., of murderers, until even attorneys for the defence become themselves ashamed to put forward this worn-out plea.

Sometimes, as in a recent case wherein I was engaged,* the large amount of blood might enable us to expose some ingenious falsehood, attributing the tell-tale spots to one of the smaller animals, as, for instance, the rat, mouse, rabbit, or even lapdog.

The other criticism of Dr. Woodward to which I wish to advert is his remark that he suspects I have underrated the amount of contraction which the dried and remoistened corpuscles undergo, estimated by Carl Schmidt at about 48 per cent. of their diameter. Numerous experiments made to settle this point lead me to remark that I stand ready to *prove* the greater accuracy of my measurements of the least deformed corpuscles examined by my method in the thin films of BLOOD STAINS, but not in masses of dried blood clot. When the blood forms a stratum of some thickness, its corpuscles during desiccation generally become crenated, and thereby diminish in diameter to two-thirds or less of their original size. It seems probable that some at least of the measurements of Carl Schmidt and others have been made upon red disks in this contracted state.

I wish to insist most emphatically that all my statements in regard to the diagnosis of blood stains are applicable to "*stains*" *only, and not to masses* of dried blood clot.

In this conviction I reply to the chief point made by my critic in the London 'Medical Record,' Sept. 9, 1874, that bearing in mind this possibility of the disks diminishing in size by crenation, I would—in the extraordinary and, I believe, as yet unreported case where a man might be convicted if a given stain were pronounced horse's blood, and acquitted if it were human blood instead of the contrary—positively decline to say it was the blood of a horse, even if the corpuscles ranged from $\frac{1}{4000}$ to $\frac{1}{5000}$ of an inch in diameter.

Two questions very properly suggested or urged by the learned counsel in the Lindsay case above referred to, during cross-

* Trial of Owen Lindsay for the murder of F. A. Colvin. See Syracuse 'Daily Standard,' Jan. 30, 1875.

examination, I have made the subjects of repeated experiment, the results of which may be useful to future observers.

First, as to the action of freezing upon the red disks, I find that drops of blood from my finger exposed upon pine wood for twelve hours to a temperature of about 15° F. so as to be frozen into solid lumps, and then thawed and dried in a moderately warm room, present their corpuscles as distinct and uninjured as do ordinary blood stains.

Second, similar drops of blood dried in about fifteen minutes by mere exposure in my office upon a hemlock chip, and also upon a fragment of oak bark, such as is used for tanning leather, likewise exhibited the corpuscles with exactly the same characters, usually seen in those from common blood stains on paper or muslin; and I therefore conclude that the amount of tannic acid taken up by the serum from the bark, and *a fortiori* from any kind of wood, under analogous circumstances is insufficient to alter these red blood disks.

## NEW BOOKS, WITH SHORT NOTICES.

*The Pathological Significance of Nematode Hæmatozoa.* By T. R. Lewis, M.B., Staff Surgeon H.M. British Forces. Calcutta: Office of Superintendent of Government Printing, 1874.—The Government does well to attach to its Sanitary Commissioner in the Government of India so painstaking and industrious an observer as Dr. T. R. Lewis. For assuredly he has done much more excellent work than many who have had similar opportunities. Some couple of years ago we received the first part of Dr. Lewis's essay on a Hæmatozoon inhabiting human blood, which was a most valuable monograph on the subject it dealt with. Now we have the second part of this memoir, in which the author completes the evidence he has already offered, and conclusively shows that the peculiar disease in which the most marked symptom is chyliferous urine, is caused by the presence in the blood of numbers of the particular entozoon which he has described and figured so minutely in this essay. And here we may refer the reader who is interested on the subject to Mr. F. H. Welch's able paper on the subject of "Filariæ in general, with an account of the species in the Dog and in Man," a paper of great importance, which appeared in this Journal for October, 1873. Dr. Lewis in his present essay has gone fully into the anatomy of the worm and into the history of its origin, as well as that of its position in the human and the dog's bodies. From his researches on these points he appears to be led to the conclusion that though the two parasites resemble each other, and though their habitat is precisely similar, yet they are perfectly distinct. And this distinction appears in great part to depend on the possession by the human form of a distinct sheath, which is totally absent from the dog's nematode. The disease appears to be perfectly common in the Indian dogs, at least one-third of the animals examined by Dr. Lewis being found infected. He describes the following as the pathological appearance found in the afflicted animals.

"1. The most striking feature is the existence of fibrous-looking tumours, varying from the size of a pea to that of a filbert or walnut, along the walls of the thoracic aorta and œsophagus, both tubes being affected, or only one. 2 Minute nodules in the substance of the walls of the thoracic aorta, from the size of a duck-shot to that of split peas. They can be felt as tubercles, and usually project somewhat on the outer surface of the vessel; a depression or slight extravasation of blood, corresponding to the nodule, being visible on the inner surface of the aorta, and frequently a slight abrasion of the lining membrane. 3. A pitted or sacculated appearance of various portions of the interior of the thoracic aorta with thinning of its walls at some parts; the lining membrane roughened at the spots affected; the roughening, however, is not of an atheromatous character, but due to the membrane being thrown into delicate rugæ, as if from contraction of the middle and outer coat. 4. Enlargement and softening of some glandular body adjoining the vessels at the base of the heart."

The structure of the worms is most minutely gone into, and in a set of three plates and various woodcuts the author minutely illustrates the subject. With reference to the question whether these worms are associated with the so-called chyluria and elephantoid conditions Dr. Lewis makes the following observations.

"It might be desired that I should express briefly (1) the chief reasons for the belief that chyluria and the elephantoid state of the tissues, referred to on a previous page, are associated with the presence of a microscopic hæmatozoon; and (2) in what manner, such connection being satisfactorily established, this fact can aid us in offering an explanation of the evidence we possess that the disease is due to mechanical interruption to the flow of the nutritive fluid in the capillaries and lymphatics:

"1. With regard to the first clause, it may be sufficient to state that detailed histories of a considerable number of individuals affected in this manner have been published by me, and that in all the *Filaria sanguinis hominis* have been detected. I have now traced the *Filaria* to the blood *direct* in eleven, and detected them in one or other of the various tissues and secretions of the body in more than thirty individuals. The history of one of these persons could not be ascertained, but all the others were known to suffer or to have suffered from chyluria, elephantiasis, or some such closely allied pathological condition.

"2. With reference to the second clause, our knowledge is not so exact, and almost all the inferences have to be drawn from observations made in connection with the hæmatozoon described in previous pages as occurring in pariah dogs. Judging from what may be seen in these, and from data which the only post-mortem examinations which I know to have been made of individuals affected with this parasite, I think that the interference with the flow of fluid in the lymphatic capillaries and smaller blood-vessels may not unreasonably be attributed to one or other of the following causes: *a.* To tumours, produced by encysted mature entozoa along the course of the blood-vessels and lymphatics, impeding the flow of fluid in them by pressure either directly or indirectly by interfering with the functions of the nerves supplied to the part. *b.* To the active migration of the immature, or rather partially matured parasite; the act of perforating the tissues—nervous or vascular—producing more or less permanent lesions. *c.* To the activity of the liberated embryos in the capillaries causing the rupture of the delicate walls of these channels in which *possibly* ova may have accumulated owing to their size, or an aggregation of active embryos taken place, either accidentally or by the parent having migrated to the capillary termination of a blood-vessel, and there given birth to a brood of microscopic blood-worms. Once the walls of the capillaries have given way the embryos pass into the adjacent lymph channels, the boundaries of which are so extremely delicate as practically to offer no impediment to the further progress of such active organisms. Should the lymphatic spaces be situated in intimate relation with a secreting surface, the escape of the minute *filariæ*, as well as the escape of fluid from the lymphatics with the ordinary secretion of the part, would seem to be a natural consequence."

It will be seen from the foregoing quotations what this author endeavours to prove in the pages of his essay. So far as we can see, he has abundantly shown, if not the connection of elephantiasis, at least the undoubted relation of chyluria to the presence of these parasitic nematodes.

## PROGRESS OF MICROSCOPICAL SCIENCE.

*The Fecundation of Certain Fungi.*—The 'Academy' (March 13) [which by the way is a thoroughly able paper] has an interesting note on M. von Tieghem's researches on the above subject. This savant has recently brought before the French Academy some interesting experiments on the fecundation of certain fungi (*Basidiomycetes*), confirming the statements of M. Reess, to which he refers, and throwing fresh light on the interesting question of sexuality in these lower organisms. M. Reess made his observations on the common dung fungus *Coprinus stercorarius*, and M. von Tieghem selected for his *Coprinus ephemeroides*. Placing a spore of this little agaric in a decoction of dung, and confining it in a cell, under the microscope, he found it soon germinated, producing a branched cellular mycelium, anastomosing, not only from branch to branch, but from cell to cell, along each branch; the branches being about 0·003 mm. in diameter. In most cases the mycelium tubes produced, in the course of five or six days, tufts of narrow rods (*baguettes*), springing, sometimes to the number of twenty, from the tip of a short lateral branch. Each of these rods divided itself into two smaller ones (*bâtonnets*). The upper one detached itself and fell away; the lower one grew at its base and divided again. When this had gone on two or three times, the basilar joint fell off, and there remained only a pedicel and a great number of small white rods lying by it. These were 0·004 mm. to 0·005 mm. long and 0·0015 mm. wide, and often having a brilliant granule at each end. When these rods were sown in the dung decoction they did not germinate. In another set of similar experiments, no rods appeared, but about the seventh or eighth day—that is to say, when the little rods in the contemporary experiments had separated from the stems, certain lateral branches swelled at their summits, forming large vesicles, separated by partitions from the pedicels bearing them. Sometimes these vesicles, which contained a dense protoplasm and usually exhibited three vacuoles, grew in loose tufts. M. von Tieghem, having thus obtained the little rods and the vesicles in separate growing cells, brought them together, and saw the "rods" attach themselves to the vesicles, and empty into them their contents. The vesicles thus fecundated lost their vacuoles, formed two internal divisions, and transformed themselves into large tubes composed of three superimposed barrel-shaped cells. The basilar cells, which were the longest and narrowest, soon pushed out curved lateral branches, and were followed by the median cells. The branches, which were multicellular and ramose, pressed against each other and formed a little white tubercle, the beginning of the fruit.

*The "Membrana Nuclei" in the Seeds of Cycads.*—At the meeting of the Linnean Society, on March 4, Professor Thiselton Dyer read a brief note on the structure of the so-called "membrana nuclei" in the seeds of Cycads. Heinzel had described this as a cellular structure, the cells of which had thick walls penetrated by ramifying tubes. There is reason, however, for believing that the membrane only represents the wall of a single cell, and is, in fact, probably the greatly enlarged primary embryo-sac. What Heinzel had taken for tubes seemed really to be solid. They are arranged all over the membrane after the fashion of what carpet manufacturers call "moss-pattern." They are possibly the débris of the thickened walls of the cells of the nucleus which had been destroyed by the enlargement of the primary embryo-sac. In the discussion which ensued a remarkable diversity of opinion was displayed among the microscopists present, as to whether the reagent magenta exhibits the largest amount of its characteristic reaction on the cellulose wall of the cell, or on its protoplasmic cell-contents.

*Where do the White Corpuscles get through the Blood-vessels?*—This question is answered by M. L. Purves in a recent number of a Utrecht Journal, which has been abstracted in the 'Medical Record' lately by Dr. W. Stirling. It states that M. Purves, in order to investigate the place where the white blood-corpuscles pass through the wall of the vessel in Cohnheim's experiment on inflammation, injected a solution of silver into the vessels of a frog prepared after the manner of Cohnheim. The colourless corpuscles, without exception, wander out between the boundaries of the epithelioid cells. They never pass through the substance or through the nucleus of an epithelioid cell. According to the author, the red corpuscles only pass out by those channels which have been previously made for them by the colourless corpuscles. The author found no stomata of any kind on the epithelium of the vessels.

*Natural History of the Diatomaceæ.*—Dr. M. C. Cooke states in 'Grevillea' (March, 1875), that Dr. Edwards has sent him a copy of the chapter from the Reports of the Geological Survey on the above subject, which is written in a popular style for general readers, and extends over nearly 100 quarto pages. The sections into which it is divided are: 1. Introduction. 2. Movements of the Diatomaceæ. 3. Mode of growth of the Diatomaceæ. 4. Reproduction of the Diatomaceæ. 5. Modes of occurrence and uses to man of the Diatomaceæ. 6. The Diatomaceæ and Geology. 7. Directions for collecting, preserving, and transporting specimens of Diatomaceæ. 8. How to prepare specimens of Diatomaceæ for examination and study by means of the microscope. This enumeration of the sections will give an idea of the scope of the "History," which will doubtless be of eminent service in the direction for which it is intended. "Unfortunately the general public know but little, and care less, about the lower Cryptogamia, except for Algæ grouped as pretty objects for the drawing room, or ornate diatoms arranged in groups to please soirée hunters, or stewed mushrooms, and Perigord pies."

## NOTES AND MEMORANDA.

**The Belgian Microscopical Society.**—This Society, founded last year on the model of the Royal Microscopical Society, is rapidly growing into importance, and bids fair to perform its part in microscopical research. It has, we are informed, just conferred, through its President, Professor Miller, the honorary Fellowship of the Society upon Mr. Jabez Hogg.

**The Compound Microscope in the Examination of Patients.**—Dr. H. G. Piffard has devised a simple contrivance by means of which the binocular microscope can be employed in the ordinary "out-patient room," for the examination of the skin of patients suffering from skin affections. The inventor's remarks in the last number of the 'Archives of Dermatology' are, as to the subject of the aberration of lenses, utterly unimportant. But his idea of employing the binocular is a good one. He says: "The objectives which I employ are a 6″, 2″, and 1″ of Grunow, a 4″ and $\frac{1}{2}$″ of Ross. The $\frac{1}{2}$″ is made with taper front, specially constructed for use with reflected light. The advantages of this arrangement over the single lens, are enlargement of the field of view, absence of spherical and chromatic aberrations, convenient distance of the observer's eye from the object observed, ten times the amplification practically attainable with the simple microscope, and lastly, the very great advantage of true stereoscopic vision. With the instrument described any portion of the integument from the scalp to the sole of the feet can be conveniently examined, and a prolonged examination can be made without fatigue to the observer. The ordinary diffused light of a bright day affords ample illumination with all the objectives except the $\frac{1}{2}$″. For this we need direct sunlight. If the examination be made at night or in a dark place, the light from a Tobold or other good illuminator, concentrated upon the object with a mirror or bull's-eye condenser, will answer every purpose."

# CORRESPONDENCE.

## An Explanation from Mr. Tolles.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *February* 13, 1875.

Sir,—In your last issue Mr. Wenham grievedly says: "That discussion with Mr. Tolles is useless, is proved by his article, page 21 of this Journal for January, 1875, where, in order to show that I am wrong, he first tries the slit without and then with water contact, in the last case measuring not *the internal cone*, or immersion angle, but the increased emergent angle from the under surface of the slide," i.e. *the air* angle of the *immersion* objective. Just what and *all* I was talking about.

Now drolly enough he says, in closing his commentary, that even if this, the focal point, "falls exactly on the under surface of an intervening plate of glass in water contact, it will still cut off stray rays within the glass, and give the true *air* angle, as one of final emergence."

*Very well said.* Now let Mr. Wenham interpose the thin plate of glass, viz. "cover" so near to 0·013 of an inch thick as the $\frac{1}{6}$th sent Mr. Crisp will work through with *water contact* at "closed" adjustment of the objective and then putting the light down through the microscope tube accordingly as he says is the correct way, then *measure* the emergent pencil and report "the true *air* angle as one of final emergence."

This will be very fair, and I pointedly invite it in accordance, if you please, with your own suggestion appended to my article of August last, 'M. M. J.,' p. 65.

I repeat,—what seems unnecessary to say again,—I talk here (as in my last) of the emergent *air* angle of that *immersion* objective only,—and I promise him more than 112°!

But again,—"ONE HUNDRED AND EIGHTY DEGREES!" In quoting from Mr. Wenham's article, p. 113 this Journal, March, 1874, I, in my reply, alluded to the above-quoted inscription as being given by Mr. Wenham in "small caps," and Mr. Wenham thought I meant his "stops" over the $\frac{1}{6}$th.—My fault.

I suppose they are "screaming capitals" more descriptively. All this about 180° I abate for the present. *If* it should ever happen to be proven to Mr. Wenham that *more* than 82° of corrected "balsam" angle existed in an objective, say 90°, I presume *he* would admit that such an objective *must have* an *air* angle "up to 180°."

But now let all that be apart. Mr. Wenham admits the thin plate of glass which before he did not use, and, water contact. Let us know then (and thus) if the "true air angle" of "final emergence" is 112° only, or a "rational and wholesome angle" somewhat above that.

Yours respectfully,

R. B. Tolles.

*Note by Mr. Wenham.*

Anyone taking the trouble to interpret the above curious letter will admit that controversy should cease concerning apertures peculiar to Mr. Tolles. I have tried the $\frac{1}{6}$th as in paragraph 3rd with lenses closed, and the focus on the front of a thickness of glass in water contact. As might have been expected, the aperture is the same, whether the glass is there or not—a practical exemplification of the first rule in optics. I have done with the $\frac{1}{6}$th, which is here accessible for trial by others if asked for. I am confident that the apertures will be found as I have stated and that the extra immersion angles claimed have no more real existence than the 180° engraved on the object-glass to the delectation of such as are ready to believe, in spite of the focal distance and small diameter of front lens.

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## Angular Aperture.

*To the Editor of the 'Monthly Microscopical Journal.'*

Boston, *March* 11, 1875.

Sir,—In your Journal for March current, p. 131, Mr. Wenham says, "I repeat (as I have stated before) that I did try the $\frac{1}{6}$th of Mr. Tolles with several thicknesses of glass in front, and whether these were superadded in water-contact or not, the aperture or ultimate emergent pencil was alike with all." The clause, "as I have stated before," is a mistake. I deny, with challenge, that he has ever so stated before in the pages of your Journal.

This is what he *has* said, 'Monthly Microscopical Journal' for November, 1874, p. 223: "In measuring varying angles of aperture by the usual method, we take them at all points of the adjusting collar, and do not place in front a thickness of glass suitable for that correction, because with a parallel plate of glass there is no perceptible difference. The angle at the crossing point of the rays is the same whether it is there or not. I stipulate that the edges of the stop shall be in the crossing point. If anyone thinks proper to introduce an intervening plate of glass, *serving no purpose*, he must focus through it, so as still to get the stop in the focal plane."

Here is strong implication at least that he did not use "cover" to fill up the interspace. I suggest that he try it, *or* tell us what happened when he *did* try it. He has not reported accurately.

Let him use cover, the thickest the $\frac{1}{6}$th will focus through at "closed" the edges of the slit thus "in the crossing point" of the "corrected" rays, and I will bide the result. Because I *know* the state of the case from irrefragable proof, in the first place, in my own hands, and I naturally expect he will get like results. Something more than 112°!

Yours respectfully,

R. B. Tolles.

*Note by Mr. Wenham.*

Though I have received an intimation from the Editor that the discussion concerning the aperture of Mr. Tolles' "180°" $\frac{1}{6}$th is closed, I have asked for the insertion of the above that he may not complain of injustice, that the last item of his defence has been suppressed. What is the character of that defence? The science of the question having been exhausted, it has degenerated into a search for inconsistencies in my writings, with the view of imputing to me false statements concerning things measured and observed. Had Mr. Tolles quoted a few more lines to the end of my sentence, I there stated that the aperture was *found to be the same* if parallel plates were interposed. I have a vivid recollection of selecting a thickness of glass that the lens would just focus through. I now take from my note-book as follows: "Maximum distance of dry focus ·013. Will penetrate a cover ·018 thick (at adjustment on its under surface)." With the slit in focus in each case this plate did not increase the aperture. I decline to argue on a principle so obvious as this. The date in my note-book is January 17, 1874. So for near fifteen months argument concerning this $\frac{1}{6}$th has dragged out its weary length, by Mr. Tolles requiring me to answer his "challenges" in defence of the wonderful apertures engraved thereon. I can testify that Mr. Tolles commands a high degree of manipulative skill, and deserves every success in a somewhat hard and profitless line, for his industry and persevering experiments for improving object-glasses; and the tone of his letters when left to his own diction speaks favourably for his good nature.

[The controversy as to Mr. Tolles' $\frac{1}{6}$th objective must now end.—Ed. 'M. M. J.']

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## Urinary Deposits—A Note by Dr. Ord.

*To the Editor of the 'Monthly Microscopical Journal.'*

*March* 9, 1875.

Dear Sir,—In referring to some papers by Dr. Bence Jones relating to urinary deposits, I find that beyond recording the influence of chloride of sodium in modifying the form and increasing the solubility of urate of ammonia, he, in a paper contributed to the Medico-Chirurgical Transactions in 1844, relates experiments which agree with and therefore anticipate some of the experiments related in my paper read before the Royal Microscopical Society in January last.

At page 111, he records that he heated needles of urates of ammonia for some hours at about 212°, so that decomposition took place; boiling water was then poured on and filtered whilst hot. A deposit obtained at the end of forty-eight hours consisted of globules, and globules with projecting angular crystals and crystals of uric acid. This experiment is very like my second experiment with urate of ammonia in principle and in results; though Dr. Bence Jones used the experiment for a different object from mine.

At page 113 he writes: "A large excess of needles was boiled with distilled water and filtered while hot, a little salt was added, and after some hours largish globules were deposited, and no needles."

This is clearly an anticipation of experiment 6 on urate of ammonia, and of the general principle of many others.

I think it right to draw attention to these experiments, first in justice to Dr. Bence Jones's most valuable work; second, in order to point out the remarkable agreement of these experiments with my own, although the two sets of experiments were undertaken with different objects, and certainly lead to different thought and application; third, in order to clear myself from any charge of intentional plagiarism.

I am, Sir, your obedient servant,

W. M. Ord.

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## Carpenter on the Microscope.

*To the Editor of the 'Monthly Microscopical Journal.'*

*March* 13, 1875.

Sir,—Mr. Stodder has called my attention to an error in p. 213 of the above work, where Mr. Tolles is spoken of as having made the $\frac{1}{18}$th objective, so highly commended by Dr. Woodward. This glass was made by Mr. Wales.

The binocular eye-piece ascribed to Professor H. L. Smith should have been placed to the credit of Mr. Tolles.

Your obedient servant,

Henry J. Slack.

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# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *April* 7, 1875.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

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

The Secretary said that a very important paper had been received, entitled "Further Researches into the Life History of the Monads," by the Rev. W. H. Dallinger and Dr. Drysdale, in which they continued the subject treated in their previous papers, and gave a complete life history of a new species found by them in a maceration of cod's head. Other engagements would not permit of the paper being read that evening *in extenso*, and therefore after briefly alluding to its contents, he proposed that it should be taken as read; it would then be printed in the next number of the Journal, and the Fellows would be able to read it before the next meeting, at which any discussion arising out of the subject could be taken. (The paper will be found printed at p. 185.)

The paper was then taken as read, and a vote of thanks to the authors unanimously passed.

The Secretary announced that in consequence of the large hall of the college being required for other purposes, the Society would in future hold its meetings in the library. The scientific evening would also be held in the library and adjoining suite of rooms on the 21st instant, and the Council hoped that as many as possible of the Fellows would bring their instruments and objects of interest on that occasion.

The President, H. C. Sorby, Esq., F.R.S., then read a paper "On some new contrivances for the study of the Spectra, and for applying the mode of Spectrum Analysis to the Microscope." He first exhibited and explained the apparatus used in his experiments and investigations, showing the new arrangement for employing an ordinary eye-piece in connection with a slit and prisms; and also the construction of the binocular spectrum microscope, and the method of comparing the spectra of two objects in the field of that instrument. The importance of obtaining equal illumination in both spectra was pointed out, and the means of regulating the light was also described. The measurement of the position of absorption bands by means of the quartz interference scale, the spot method, and the wave-length method, were explained, and the relative advantages of each were set forth. After some interesting observations upon the meaning of the absorption bands and the variations produced in their positions by acid or alkaline additions to the same solutions, illustrations were given by means of coloured diagrams of a few remarkable spectra which had recently engaged the attention of the author, and he intimated his intention to exhibit and further explain the apparatus and objects at the forthcoming scientific evening.

Mr. Slack having proposed a vote of thanks to the President for his paper, it was put to the meeting and carried unanimously.

In consequence of the lateness of the hour the discussion upon the President's paper was postponed until the next meeting. (The paper will be found printed *in extenso* at p. 198.)

Donations to the Library and Cabinet since March 3, 1875:

| | |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| Journal of the Quekett Club. No. 28 | *Club.* |
| Bulletin de la Société Botanique de France | *Society.* |
| Report of the East Kent Natural History Society, for 1874 | *Ditto.* |

Alfred Allen Esq., of Felsted, Essex, was elected a Fellow of the Society.

WALTER W. REEVES,
*Assistant-Secretary.*

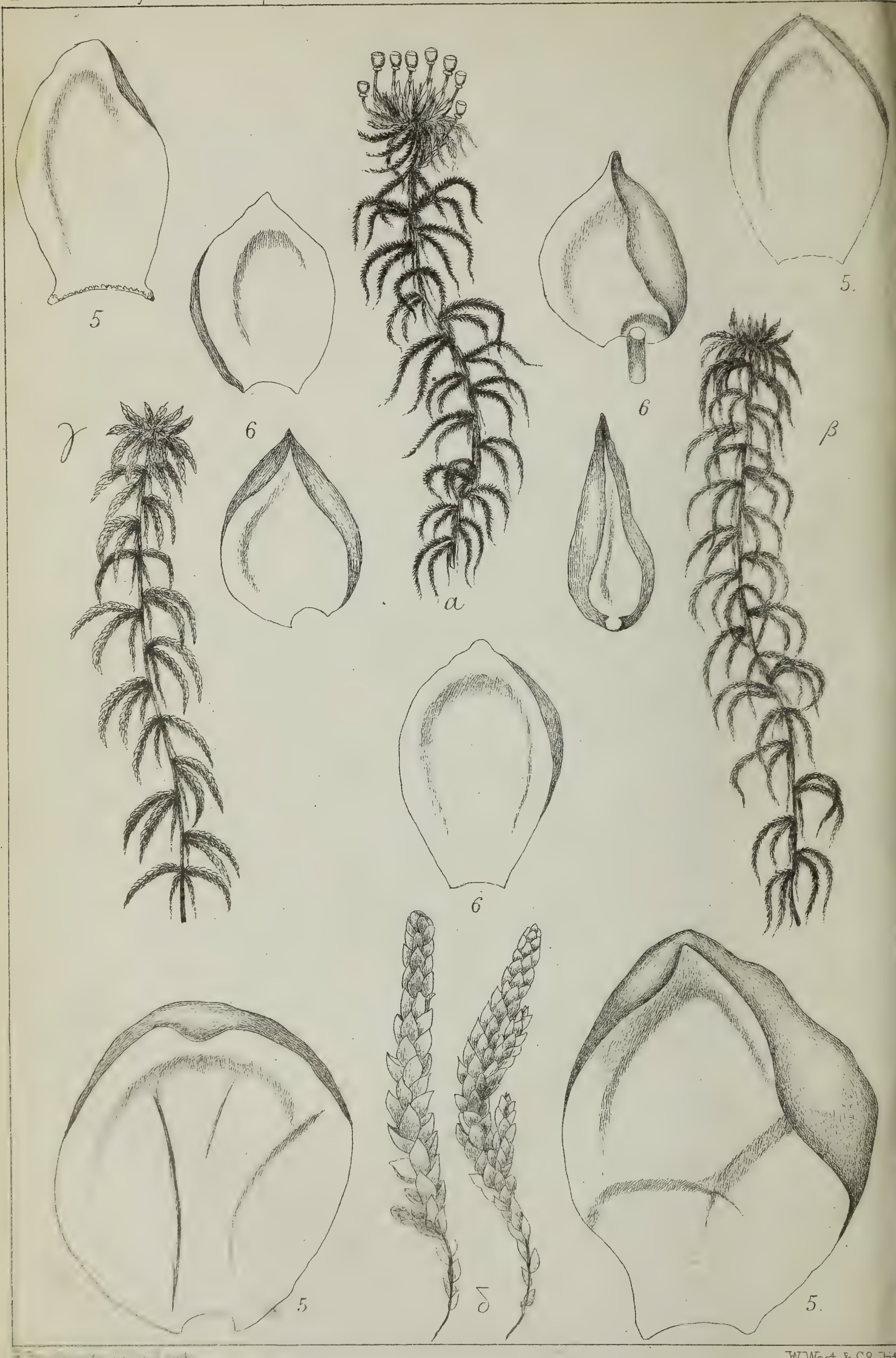
## South London Microscopical and Natural History Club.

An Ordinary Meeting of this Club was held on Tuesday evening, January 19, at the Angell Town Institution, Gresham Road, Brixton. Dr. Braithwaite occupied the chair.

A paper was on this occasion read by Mr. James Ford Wight, on "Eyes." After describing the construction of the human eye in detail, the lecturer passed to the consideration of its optical properties, and its marvellous adaptation for the purposes of vision. The characteristic differences between the eyes of the mammalia and those of insects, fishes, &c., were then pointed out; a full description being given of the compound eyes of insects, their structure, and use.

At the conclusion of this paper, a vote of thanks having been accorded to Mr. Wight, the various microscopical objects illustrative of the subject were examined by the members and visitors. These objects comprised: fibres from the crystalline lens of the eye of a cod fish; complete front of eye and membrane of eye of a lobster; optic nerve of calf; and an image of a photographic portrait, as depicted in the numerous *ocelli* of a beetle's eye. Many other objects were exhibited by the members.

The President announced a paper on "Spectrum Analysis," by William Huggins, Esq., LL.D., F.R.S., to be read at the next meeting, on February 16, on which occasion ladies would be admitted, and that the annual meeting would be held on March 16.

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Sph. laricinum Var$^{s}$.

THE

# MONTHLY MICROSCOPICAL JOURNAL.

JUNE 1, 1875.

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

(*Taken as read before the* Royal Microscopical Society, *May* 5, 1875.)

Plates CV. and CVI.

### Sphagnum laricinum Spruce.

Plate CV.

Professor Lindberg having recently investigated the forms of this species, and published the result in 'Notiser ur Sällskapets pro Fauna et Flora Fennica Förhandlingar,' xiii., p. 401, and having also liberally communicated to me specimens of each, I take this opportunity of figuring them, since they constitute a series precisely parallel to the three varieties of *Sph. subsecundum* previously illustrated.

Of the var. *β.* I have not seen British specimens, but there is little doubt it will eventually prove to be indigenous.

Var. *β. teretiusculum* Lindb.

*Sph. subsecundum* var. *β. isophyllum* Russow Torfm. p. 73, p. p. (1865).
*Sph. neglectum* var. Austin Musc. Appal. No. 27 (1870).

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### EXPLANATION OF PLATES.

Plate CV.

Sphagnum laricinum vars.

*α.*—Fertile plant of the typical form, from the Åland Islands, communicated by Professor Lindberg.
*β.*—Var. *teretiusculum.* 5.—Stem leaf. 6.—Leaves from middle and near apex of a divergent branch.
*γ.*—Var. *platyphyllum.* 5.—Stem leaf. 6.—Leaves from a divergent branch.
*δ.*—Var. *cyclophyllum.* 5.—Stem leaf. 6.—Branch leaf.

Plate CVI.

Sphagnum Pylaiei.

*a.*—From a specimen in Austin's collection.
1.—Part of stem with a divergent branch.
5.—Stem leaves. 5 *a a.*—Areolation of apex of same.
6.—Branch leaves. 6 *x.*—Section. 6 *p.*—Point of same. 6 *c.*—Cell from middle × 200.
9 *x.*—Part of section of stem. 10.—Part of a branch denuded of leaves.
*β.*—Var. *sedoides*, from Sullivant and Lesquereux's collection. *β* 5.—Stem leaves. *β* 5 *x.*—Section of same.

Branches crowded, terete, usually incurved or more or less circinate. Stem leaves large, oblong, obtuse, the apex somewhat fringed or toothed. Branch leaves short, very broad, concave. Corresponds to *Sph. subsecundum* var. *contortum.*

Hab.—Marshy places in woods. Sweden, Lapland, Finland; Dovrefjeld Mountains, Norway—near Dorpat (Russow). Closter, New Jersey (Austin).

Var. γ. *platyphyllum* (Sull.) Lindb.

*Sph. subsecundum* var. β. *isophyllum* Russow, p. p. *Sph. platyphyllum* n. sp.? vel var. *Sph. neglecti?* Sull. Mss.

Branches short, rather obtuse, with imbricated leaves. Stem leaves lingulate, with distinct auricles composed of projecting hyaline cells. Branch leaves rounded-ovate, pointed, very broad and concave. Corresponds to *Sph. subsecundum*, var. *auriculatum.*

Hab.—Marshy places in woods in subalpine districts. Sweden, Lapland, Finland, Norway, Estland (Russow). N. Wales, top of pass between Aber and Llanwrst (Prof. Lawson, 1874). New Jersey (Sullivant).

Var. δ. *cyclophyllum* (Sull. Lesq.), Lindb.

*Sph. obtusifolium* var. β. *turgidum* Hook. & Wils. in Drumm. Musc. bor.—amer. 2nd Ser. No. 17 (1841).

*Sph. cyclophyllum* Sull. & Lesq. Musc. bor.—amer. No. 5 (1856). Sull. Mosses of Un. St. p. 11 (1856). Icon. Musc. p. 13, t. 6 (1864). Austin Musc. Appal. p. 11, No. 25 (1870). *Sph. subsecundum* var. γ. *simplicissimum* Milde Bryol. Siles. p. 393 (1869)? *Sph. Drummondii* Wilson Mss. in herb. suo (Mus. Brit.).

Stems short, turgid, 1–3 in. long, quite simple or with one or more short solitary branches. Stem leaves very large, orbicular, deeply concave and cucullate, pale greenish-white. Corresponds to *Sph. subsecundum* var. *obesum.*

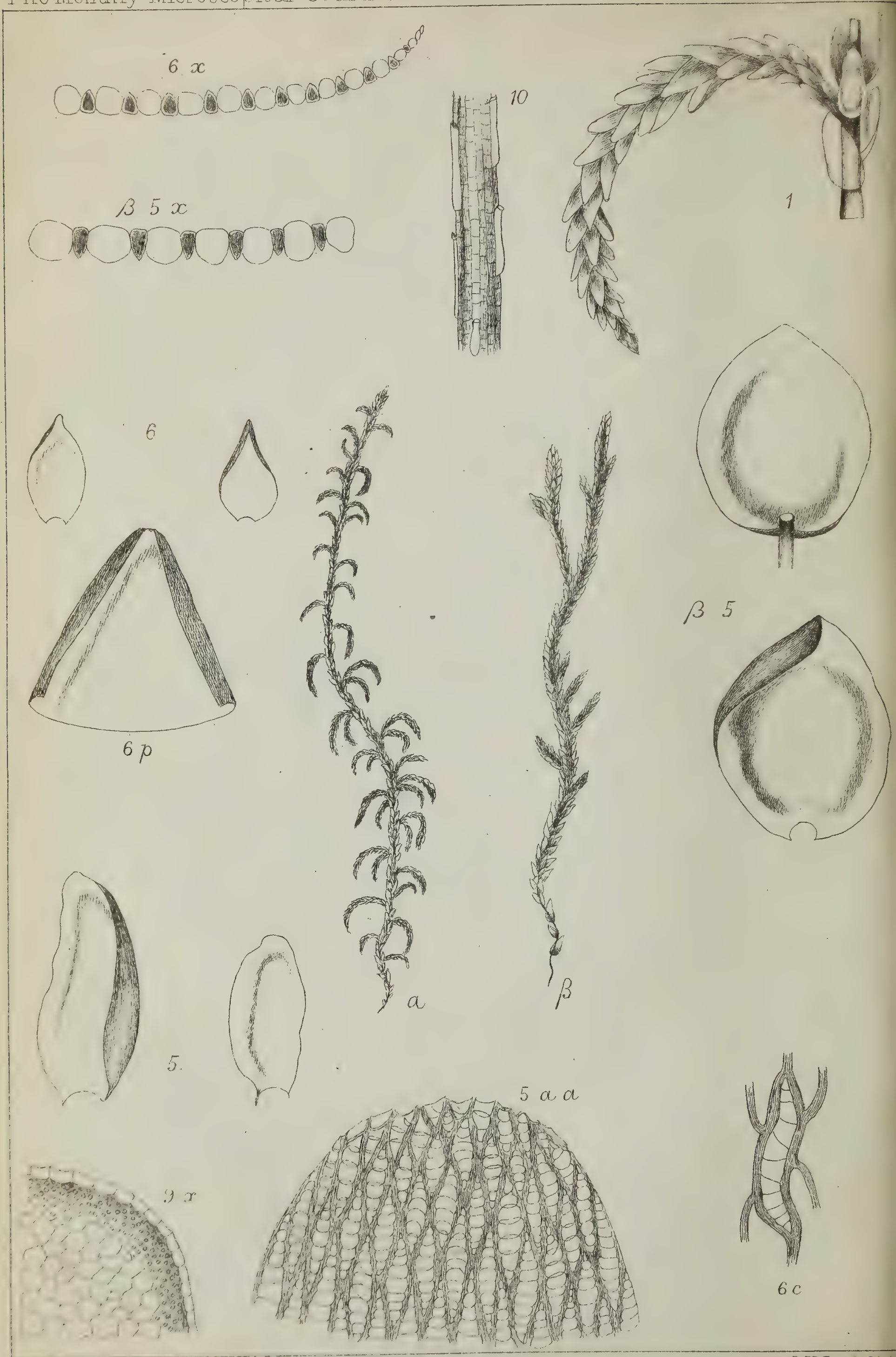
Hab.—Moist peaty places in mountain districts. Åland Islands (Reuter and Elfving). Shore of Loch Katrine, Perthshire (McKinlay). New Orleans (Drummond). Alabama (Lesquereux). New Jersey (Austin).

The var. *cyclophyllum* differs so completely in aspect from the typical form, that no one at first sight would think it could belong to the same species; yet Professor Lindberg sends specimens which distinctly show the transition between it and var. *platyphyllum*, and the sections of stem and leaf agree perfectly with those of the other forms.

A few specimens in Drummond's collection are in fruit, which is immersed in sessile lateral perichætia.

## North American Species not found in Europe.

Besides the species common to both countries, three of the family are natives of North America which have not yet been




W. West & Co. lith.

detected in Europe, and the close relationship between the mosses of the two countries has induced me to include these species also in the present monograph.

## 20. Sphagnum Pylaiei Bridel.

Bryol. Univ. I, p. 749 (1826).

Plate CVI.

Syn.—Sullivant Icon. Musc. p. 12, t. 6 (1864). Austin Musc. Appal. No. 23 (1870).

*Sph. sedoides* var. Sull. & Lesq. Musc. bor.—amer. No. 4 (1856). Sull. Mosses of Un. St. p. 12 (1856). [*Sph. cymbifolium* forma juvenilis, C. Müll. Synop. I. p. 92 (1849).

Dioicous? *olive-green, fuscous or blackish. Stem* erect, undivided, slender, 2–4 in. high, *with a single layer of small cortical cells, and a narrow reddish brown woody layer; branches all solitary or in pairs at the lower part of stem,* short, terete, obtuse, arcuato-decurved, the cortical cells small, retort-cells few, narrowly cylindric, not recurved at apex.

*Stem leaves numerous, laxly imbricated,* erect, *ovate-oblong, concave,* rounded and minutely erose at apex, *the hyaline cells fibrillose. Branch leaves laxly imbricated, very small, ovate, obtuse, the margin incurved in the upper third, entire at apex; hyaline cells with strong annular fibres, and without pores, in section circular, separated both in front and back by the chlorophyll cells, which are very thick and obtusely trigonous.*

Hab.—Peat bogs. Newfoundland (La Pylaie). Table Rock, S. Carolina (Lesquereux). Willey Mountain, New Hampshire (James). Adirondack Mountains, New York (Peck). New Jersey (Austin).

Var. *β. sedoides* (Brid.) Lindb.

*Sph. sedoides* Bridel Bry. Un. I. p. 750, et var. *β. prostratum* (1826). Sull. Musc. Alleghan. No. 208 (1845). Sull. & Lesq. Musc. bor.—amer. No. 3 (1856). Sull. Mosses of Un. St. p. 12 (1856). Ic. Musc. p. 11, t. 6 (1864). Austin Musc. Appal. No. 24 (1870).

Stem procumbent at base, 3–5 in. high, simple or with a few short, scattered branches, fragile, flaccid, dull pale green, the upper part vinous red. Leaves large, very densely imbricated, oblong-ovate, concave, obtuse, entire or eroso-denticulate, with a border of two rows of extremely narrow cells; hyaline cells elongated, with annular fibres, and very few minute pores. Branch leaves similar but smaller.

Hab.—Peat bogs. Newfoundland (La Pylaie). Wet margins of Table Rock, S. Carolina (Gray and Lesquereux). Mount Marey, New York (Torrey). Adirondack Mountains, New York (Peck).

*Sphagnum Pylaiei* and its variety have been regarded by most authors as doubtful species, partly because they have never been found in fruit; the structure of the leaves and stem is, however, so distinct that there can be no hesitation in maintaining the right of *Sph. Pylaiei*, as the most highly developed form, to the title of specific rank.

The var. *sedoides* quite resembles *Sph. laricinum* var. *cyclophyllum* and also a simple form of *Sph. subsecundum* var. *obesum* which Mr. Barnes has recently sent from Staveley; yet here also the sections of the leaves and stem will readily enable us to refer each to its proper specific type.

## II.—*On Angle of Aperture in Relation to Surface Markings and Accurate Vision.*

By Henry James Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society, *May* 5, 1875.)

The fascinating character of diatoms, and the desire to resolve their surface markings, have exercised a most important influence in this country, and on the Continent, upon the labours of opticians in the production of objectives for the microscope.

It was soon found that glasses upon the old patterns with small angles of aperture could scarcely show any objects of this description, while others, even when inferior in their corrections, if of much larger angles achieved considerable success. This stimulated the manufacture of large-angled glasses, and microscopists vied with each other in obtaining from the most enterprising makers objectives with angles so out of all reasonable proportion to their magnifying powers and focal distance from the object, that they were nearly or quite useless for general purposes of natural history and physiological research. Objectives thus became divided into two varieties, which from their persistence may be termed *species*,—one good for surface markings and nothing else, the other only capable of displaying the easier of such markings, but serviceable for general investigations, and contributing to perhaps nine-tenths of the useful discoveries made with microscopic help.

Physiologists did not attempt to conceal the scorn with which they looked at the mere displayers of diatom dots, and for the most part gave up all hope of obtaining objectives suitable to their researches, and at the same time possessing a high degree of that resolving power upon which the dot showers depended for their success. It was in fact generally supposed that resolving power and penetrating power stood in relations of irreconcilable hostility, and it was also supposed that a considerable amount of chromatic error was essential to the sort of correction best fitted for the dot work. No one could deny that up to a certain date the best dot-displaying glasses had considerable chromatic errors, and that other glasses with better chromatic corrections did not show difficult dots so well. Had it been considered that all chromatic aberration involves spherical aberration, the belief in any theoretical necessity for leaving considerable chromatic error in order to ensure sharp definition would scarcely have become so prevalent. It is obvious that the best image of an object would be formed by bringing all the light rays from it into their right places, and as chromatic errors bring some into wrong places, they must cause spherical distortion.

As a rule, English microscopists do not attempt to show delicate surface markings with glasses below $\frac{1}{4}$ths and $\frac{1}{5}$ths, unless—as in the case of certain $\frac{1}{2}$ and $\frac{4}{10}$ths, specially made, in answer to demand, by Mr. Wray and others for this purpose—they have angular apertures so large as to involve an almost complete sacrifice of penetration.

Objectives that have been in great favour have had about the following proportions of angular aperture to assumed focal lengths: $\frac{1}{2}$ inch, 90° and upwards; $\frac{4}{10}$ths, 100° and upwards; $\frac{1}{4}$ths, 140°; $\frac{1}{8}$ths, and higher powers, 170°, or more. If we allow for some exaggerations in these estimates taken from trade catalogues, we still find that angles quite inconsistent with a good working distance from the object to be viewed, and with a useful amount of penetrating power, have been imagined necessary for the exhibition of delicate surface markings.

In using an objective of large angular aperture, the extent to which that aperture is brought into action depends upon the illumination; and if only parallel rays are sent through the object, or rays of slight divergence, only a portion of the aperture is employed. This enables us to ascertain approximately with any objective how much of its capacity for receiving oblique rays is necessary to enable it to show given surface markings. Stops may also be introduced to lessen the acting angle of the objective, and Dr. Pigott has employed the Iris diaphragm, made by Messrs. Beck, specially mounted for the purpose. Whatever means are employed, it will be found that the best large-angled objectives will show most lined and dotted objects of difficulty with less than their full angles, and those most perfect in their corrections will do this in many cases with direct light on a clear day.

The old opinion upon this subject, and one still common, will be found repeated in the latest edition of the 'Micrographic Dictionary,' in the article "Test Objects," which, like many others, has not been brought down to date. It is there stated: "Now, if we examine a valve of Gyrosigma by direct light, the minute structure will be invisible, however small, or large, the angular aperture may be, or however perfect the defining power."

At the time of copying this passage the author of this paper has a microscope in his library, opposite a north window. The instrument has the sub-stage mirror turned on one side, quite out of the way, and is pointed like a telescope towards the clear sky. With Powell and Lealand's immersion $\frac{1}{8}$th, their last but one, a perfect definition is obtained of *P. hippocampus* with the A eye-piece of Ross's series. Beck's $\frac{1}{5}$th with less aperture shows it with same eye-piece rather better, because not so much drowned in light. A $\frac{1}{6}$th by Zeiss, of Jena (his D), aperture according to catalogue 72°, but by measurement of Dr. Pigott and the writer, 68°, gives admirable definition; a C ($\frac{1}{4}$), also by Zeiss, aperture 48°, suffices with B eye-piece if a little superfluous light is screened off by holding a sheet of white paper, so as to stop some rays from entering the field. With the diaphragm eye-piece, constructed by Mr. Ross at the request of the writer, many years ago, a person acquainted with the object can just see the cross markings with A eye-piece. B eye-piece with diaphragms makes them quite plain.

Powell and Lealand's $\frac{1}{8}$th will show much more difficult lined objects with direct light, but *P. hippocampus* was selected to suit a range of powers and angles of aperture.

Zeiss has worked, under the direction of Professor Abbe, of Jena, on a plan precisely opposite to that usually followed by our leading opticians at the request of their customers. He has, so to speak, minimized angles of aperture and secured great working distance and penetration, and yet obtained an amount of separating or resolving power hitherto supposed to be exclusively the property of far larger angled glasses.

The experiments mentioned should be tried on a clear day, with the microscope some way from the window, where there is a little shade; and it is well to surround the eye-piece with a screen of black cotton velvet, to keep all glare from the eye. Excess of light, whether from the sky or a lamp, affects small-angled glasses more detrimentally than larger ones; and it was through not being aware of the amount of caution required in this respect that the writer underrated the corrections and powers of the Zeiss D, when describing its merits last June in a letter to the 'M. M. J.' By lamplight an angle of about 45° with central stop, or two radial slots, is best for the C objective, and an angle of from 55° to 75° for the D when an achromatic condenser is used for light-ground illumination.

Many other diatoms are well shown by Zeiss' C and D objectives, with their small angles and ordinary illumination with sub-stage mirror or condenser; but if we take a delicate valve of *P. angulatum* that is not satisfactorily exhibited in this manner, we can instantly increase the resolving power by the employment of Mr. Wenham's dark-ground Reflex Illuminator, provided the object adheres to the slide and not to the cover. With this apparatus and C and D eye-pieces, *P. angulatum* with the terminal lines of beads where fractures occur can be sharply and elegantly displayed with the D objective. So great is the resolving power of this process, that the writer was able, at the late Scientific Evening of this Society, to show the transverse marks of *Surirella gemma* resolved into beads with this objective ($\frac{1}{6}$th), and C and D eye-pieces of Ross's series. It is not pretended that any glass yet made with so small an angle is the best for showing such objects, or can display them as well as those with larger angles. The use of such experiments is to prove that small angles can do much more in the way of resolution than has been commonly supposed, and that few objects require for their finest exhibition as large angles as are usually given to the most carefully made high powers. When a high degree of resolving power is obtained by large angles, the objectives necessarily fail in every instance in which the surfaces it is desired to examine are not very flat, or cannot be placed exactly in a horizontal plane. Were more skill exerted in the construction of smaller-angled glasses, the instances would be very few in which they would fail to show markings for which large angles are thought indispensable.

Professor Abbe, of Jena, in a paper which will be found in Schultze's 'Archiv. f. Mikroskop. Anat.,' vol. ix. (1873), is entitled "Beiträge zur Theorie des Mikroskops," observes that we should look for improvement in the direction of making objectives of 3 and 4 millimètres focus do the work now done by higher powers. He affirms that corrections cannot be well made with dry lenses exceeding 105° to 111° aperture, without a considerable reduction of working distance. This is much in accordance with the opinion of the late Richard Beck, that it was not well to give an $\frac{1}{8}$th a greater angle than 120°, and the $\frac{1}{20}$th constructed by him was of about that aperture. Immersion lenses, Dr. Abbe says, allow of good correction to an air valve of 180°, giving, according to Zeiss' catalogue, from 104° to 108° in water; and an objective that corresponds with our English $\frac{1}{25}$th has the power of working through a covering glass $\frac{1}{5}$th mm. thick.*

With regard to Zeiss' immersion systems, $\frac{1}{8}$th, $\frac{1}{15}$th, and $\frac{1}{25}$th, of about 100° aperture in water, he says: "Personally, I am convinced that even in immersion systems, for the normal requirements of science, there would be no loss, but in many respects a gain, if they were constructed with smaller angles of aperture, although we cannot suppose that practical opticians will exert themselves in this direction while, according to a universally spread opinion, such objectives would be valued as of only second rank."

It is this absurd mode of valuing objectives that is now the greatest hindrance to further progress. An optician can get great credit for giving a $\frac{1}{2}$ inch an angle too much for a $\frac{1}{25}$th, but might get no credit for constructing a quarter of moderate angle so perfect in correction as to possess the resolving power associated with a large-angled $\frac{1}{25}$th, and yet to accomplish the latter would be a feat of higher skill and of much greater usefulness.

No English optician is known to the writer as now attempting this task, and we must refer to Zeiss' productions for illustrations of what has already been accomplished. There are, however, small-

* The millimètre is equal to 0·039 of an inch, one-fifth of which is nearly $\frac{1}{125}$ inch.

angled $\frac{1}{8}$ths in existence by our great makers, of remarkable merit, and their resolving powers would probably be found very considerable if new modes of illumination were applied to them. Dr. Pigott possesses a $\frac{1}{5}$th of Andrew Ross's, angle about 68°, which with E eye-piece and 16 inches of tube can show his famous Podura beads, which are certainly fine tests, whatever disputes may continue as to the structure that causes their appearance.

In Professor Abbe's paper there is a reference to Dr. Pigott's "Aplanatic Searcher," which is condemned, not on the ground taken by certain objectors here, but for a reason that its inventor will be the first to endorse, namely, that the corrections it can make ought to be effected by the optician through an improvement in the combinations he employs.

Some years ago Dr. Carpenter showed that excess of angular aperture led to great distortion of objects seen with the binocular microscope, converting spherical bodies into ovals, &c. He stated that he "had caused Messrs. Powell and Lealand to construct for him an objective of half-an-inch focus, with an angular aperture of 40°, and found it to answer most admirably."* If we take this angle of 40°, as best suiting magnifications of from 90° to 120° or a little higher, what angle will best suit higher powers so as to avoid distortion? This is a question for which it is difficult to find the data for a mathematical calculation, but we may perhaps arrive at it approximately by well-conducted experiments.

It is often supposed that an object that requires very oblique illumination must also require a large-angled glass to view it; but the better the spherical correction and the less false light—that is, light not concerned in forming an optical image—that is admitted, the smaller seems to be the angle of aperture necessary for seeing an obliquely illuminated set of surface markings well.

Professor Abbe observes that "chromatic aberration for large angles of aperture not only depends upon the focal differences which affect the image-making light cone, by reason of the unequal passage of the different coloured rays through crown and flint glass, but also upon incurable inequalities in recomposing the coloured rays of differently refracted pencils, so that an objective achromatically corrected for direct illumination must be more or less over-corrected for oblique rays."

He makes similar observations on spherical corrections, and points out that increasing angles of aperture beyond narrow limits augments the outstanding deficit of correction and damages definition. It is probable that many favourite objectives have had their definition damaged in this way, and it is curious that large angles do not work well with the silica films described by the writer, and that with suitable illumination small angles will resolve them.

* Note to 'The Microscope,' 5th edit., p. 72.

The new $\frac{1}{8}$th by Powell and Lealand must, if fairly considered, be regarded as a convincing proof that a comparatively low power can be made to do work for which the highest that can be constructed have been supposed necessary. It likewise indicates the narrow limits within which a fine large-angled glass, working near the object, can be made to operate with advantage. If this remarkable objective is compared with an ordinary $\frac{1}{8}$th, no admiration of its merits will prevent complaint of its limited amount of penetration; but it is in fact an improved substitute for a far higher power, and in that respect deserves high praise. Few, if any, of the finest powers previously made would give a clear view with anything like the magnification which deep eye-pieces afford with this objective. By the kindness of Mr. Lettsom, who was one of the first to order and obtain one of these glasses, the writer has been able to experiment with it. The E eye-piece of Ross's series, giving a magnification of about 2000 linear, suits it as well as the lowest eye-piece suits ordinarily fine glasses; and the view that can be thus obtained of such an object as *P. angulatum* surpasses in beauty and brilliancy anything seen before. With appropriate illumination there is a marvellous stereoscopic rotundity of the beads, the interspaces are remarkably large, and the shadows are wonderfully sharp. It would obviously bear a much deeper eye-piece than E, and was exhibited to this Society with one stated by Messrs. Powell and Lealand to bring it up to 4000 ×.

Under special conditions this objective may serve the naturalist and physiologist in a remarkable manner. It has enough penetration to show the internal structure of small rotifers, and it, together with previous high powers by the same optical artists, proves that Professor Abbe is quite wrong in his dictum that no microscope can show anything beyond that which a sharp eye can detect with 800 ×. To say nothing of lined objects, Messrs. Dallinger and Drysdale have been indebted to the much higher magnification obtained by $\frac{1}{50}$ths for some of the most valuable information concerning minute germs they have laid before us.

It may be impossible to obtain such enormous magnification and resolving power as this glass will give without an angle of aperture and an approximation to the object which is incompatible with much penetration, and in using it we must compare it with $\frac{1}{25}$ths and $\frac{1}{50}$ths rather than with an ordinary $\frac{1}{8}$th. So compared, its working distance will be pronounced large, and its penetration considerable for the power. There is with it, however, a rapid, almost violent, transition from perfect performance when all its conditions are complied with, to bad performance, and no performance, if the object is not sufficiently flat, placed exactly in the best position, and illuminated in the best way.

It would be well worth trying whether the same principle of construction applied to an $\frac{1}{8}$th of some thirty degrees less aperture would not work still better, even upon surface markings, as it certainly would upon objects requiring much penetration. This suggestion is equivalent to asking whether the proportion of angle of aperture to focal length is such as to admit of the best corrections, bearing in mind Professor Abbe's remarks.

Zeiss' $\frac{1}{8}$th, not at all competing with the new Powell and Lealand glass in its specialty, shows how much more than has been expected can be done on the plan of a small angle and great working distance. This glass stands C and D eye-pieces well, and has a very remarkable amount of penetration, united to more than usual resolving powers.

To sum up the results of experiments with various powers of different constructions: In the first place it appears that opticians have been encouraged to make excessive apertures substitutes for good corrections; 2, that naturalists and physiologists have been too contented with feeble resolving powers, under the belief that any more capacity for resolution must mean less penetration: and they ought to demand more penetration than they have been accustomed to, and far greater resolving power in addition; 3, that better illumination, and specially Mr. Wenham's Reflex Illuminator, adds greatly to the resolving powers of really good small-angled glasses; 4, that microscopists, and especially this Society, should so act as to secure opticians from the unfair treatment alluded to by Professor Abbe, and cause all the honour that is deserved to be awarded to those who will bring comparatively small-angled glasses to the highest degree of perfection in resolving as well as in penetrating power.

## III.—*Measurements of the Möller Probe-Platte.*

By J. Edwards Smith, Esq., Ashtabula, O., U.S.A.

Some twelve months since my friend Professor Edward W. Morley, of Hudson, O., at my request made careful and accurate measurements of his Möller Platte (No. 258), and kindly forwarded to me the results obtained. I have cross-questioned pretty severely some of the figures as given by Professor M., by "throwing down" the images on paper with camera lucida, and comparing the various markings each with the other *graphically*, and find the Professor's results to harmonize very nicely.

In making the measurements Professor Morley used the superb Houghton and Sims' micrometer, belonging to the Hudson Equatorial. The objective was a very fine Tolles' $\frac{1}{16}$th. Monochromatic sunlight was also employed.

With pleasure I forward you the results obtained by Professor Morley, as per table annexed.

I have had occasion to examine several of these Probe-Plattes of Möller, and found them all singularly even as to the "markings." This being the case, the Platte becomes at once valuable as a *stage micrometer*, using the camera and tracing the marking on paper—hence these values would at once be obtained by consulting the table. The whole process is so plain that it is quite unnecessary for me to go through the details. It will sometimes be better to trace several of the markings from the camera, as by example given.

Take shell No. 3 (*Nav. lyra*), and throw, by camera, its markings on paper. Selecting now the lines best tabulated and tracing off the space occupied by *six* of these, the distance obtained should equal the distance similarly obtained from shell No. 1; or we may trace from No. 1 the distance occupied by six hexagons, and dividing this by six graphically, get a more correct result than could be obtained by measuring a single hexagon.

After a little practice the observer becomes expert with the camera, and will be able to deal with the finer shells. Monochromatic sunlight will now be found of advantage.

Again, the observer may subdivide the distances given by the coarser shells, and thus obtain finer comparison scales, i. e. taking the distance already obtained from the tracings of *T. favus* (No. 1), and with a fine lithographic pen divide this into three parts; each of these parts will represent the $\frac{1}{1000}$th of an English inch; all this is plain enough.

Professor Morley assures me that he can readily measure *N. crassinervis* (No. 18) of the Platte with his $\frac{1}{16}$th, *using lamplight.* When we bear in mind the high magnifying power of the H. and S. micrometer, and also the delicacy required for this kind of work, we must regard the measurement of No. 18 by lamp as little less than a feat.

MEASUREMENT OF MÖLLER PROBE-PLATTE, NO. 258.

| | Numbers counted. | | Lines in 1-1000 Eng. inch. |
|---|---|---|---|
| 1. *Triceratium favus* | 4 | centre to centre of hexagons | 3·06 |
| | 4 | .. .. .. | 3·08 |
| | 4 | .. .. .. | 3·08 |
| 2. *Pinnularia nobilis* | 14 | at edge from centre .. top | 10·8 |
| | 15 | " " .. bottom | 10·5 |
| | 18 | midway from centre to end .. top | 12·5 |
| | 18 | " " .. bottom | 12·1 |
| 3. *Navicula lyra* | 24 | from centre along axis .. top | 16·3 |
| | 25 | " " .. bottom | 17·1 |
| | 29 | short lines between marginal .. top | 17·3 |
| | 31 | " " .. bottom | 18·5 |
| 4. *Navicula lyra* | 36 | from centre along axis .. top | 25·0 |
| | 36 | " " .. bottom | 25·0 |
| | 36 | short lines .. top | 27·1 |
| | 36 | " .. bottom | 27·1 |
| 5. *Pinnularia interrupta* | 20 | along each edge from centre .. top | 26·8 |
| | 19 | " " .. bottom | 25·5 |
| 6. *S. Phœnicenteron* | 44 | along axis .. top | 33· |
| | 44 | " .. bottom | 32·7 |
| | 41 | along edge .. top | 31·1 |
| 7. *Gramm. marina* | 57 | .. .. .. | 36·3 |
| 8. *P. Balticum* | 48 | near end | 32·9 |
| | 50 | including middle | 34·3 |
| | 17 | longitudinal | 31·5 |
| 9. *P. accumenata* | 52 | including middle along axis | 42·7 |
| 10. *Nitzschia amphoryx* | 46 | " " convex edge | 42·9 |
| | 20 | " " concave edge | 45·3 |
| 11. *P. angulata* | 47 | diagonal near centre (angles of diagonals 58°) | 43·8 |
| 12. *G. subtil.* | 58 | .. .. .. | 61·7 |
| | 80 | .. .. .. | 61·2 |
| 13. *Surirella gemma* | 51 | at axis from centre | 54·8 |
| | 48 | at edge | 51·4 |
| | 37 | longitudinal | 63·5 |
| 14. *Nitzschia sigmoidea* | 63 | including middle | 63·0 |
| | 64 | " " | 63·3 |
| | 64 | " " | 63·2 |
| 15. *P. fasciola* | 46 | .. .. .. | 56·5 |
| | 42 | .. .. .. | 56·2 |
| | 40 | .. .. .. | 55·5 |
| 16. *Surirella gemma* | 16 | near middle of length | 64·2 |
| | 20 | " " | 63·0 |
| | 20 | " " | 64·4 |
| | 24 | almost half-way from middle to end | 67·3 |
| | 19 | nearer to end than middle | 70·4 |
| 17. *Cym. elliptica* | 35 | along edge from centre | 63·3 |
| | 31 | (probably was 30—doubtful) | 65·1? |
| 18. *Navicula crassinervis* | 47 | .. .. .. | 82·2 |
| | 24 | .. .. .. | 81·1 |
| | 37 | .. .. .. | 79·4 |
| 19. *Nitzschia curvula* | 50 | .. .. .. | 84·7 |
| | ? | (counted number not given J. E. S.) | 84·5 |
| 20. *Amph. pellucida* | 42 | counted three times | 92·9 |
| | 43 | " " | 92·7 |

Remarks.—It will be noticed that the figures corresponding to the last three or six shells are considerably below those formerly published; it is, however, probable that the figures above given will not be materially changed. Professor Morley's determination of *A. pellucida* as above given, agrees very well with Colonel Woodward's observations.—*A Paper read before the Memphis Microscopical Society, reported in the 'Cincinnati Medical News,' February*, 1875.

# PROGRESS OF MICROSCOPICAL SCIENCE.

*Diatomaceæ in the Carboniferous Epoch*, by Count Castracane. 'Transactions of the Academia dei nuovi Lincei.' Rome. February, 1875.—The remains of several species of Diatomaceæ were discovered in coal by the following method. A portion of the solid interior was reduced to a coarse powder, and burned in a small porcelain vessel in a glass tube, through which a stream of oxygen was passed, but the temperature was kept as low as possible, in order to avoid any fusion of the ashes. These were then heated in a mixture of nitric and hydrochloric acids with some chlorate of potash, in order to remove, as far as possible, all impurities. On carefully examining the residue thus obtained from various specimens of coal from different localities, mostly British, Diatomaceæ were invariably found, though usually only in small numbers. All agree with known living species in form and in the number of the markings, and in fact in every particular. Some specimens yielded marine, but the greater number fresh-water species. The author appears to have taken every precaution to avoid being misled by the presence of accidental impurities, and he points out the importance of the facts in connection with the origin of coal, and as showing that such low organisms have continued to exist with the same constant characters for such a vast geological period. In conclusion, it may be named that the author has kindly sent for exhibition at the meeting of the Royal Microscopical Society, two mounted specimens, showing well-marked Diatomaceæ and other interesting minute organic remains.

*On "Personal Equation" in Microscopy.*—Those who are familiar with astronomical matters will readily understand the above expression. But to the microscopist it will be almost entirely new. Mr. J. Ingpen, the Secretary of the Quekett Club, has a very important paper on this subject in the last number (March) of the 'Journal of the Quekett Club.' He points out the many varieties of this "equation," which must be taken into consideration in every instance, and he explains by numerous examples the several optical differences of observation under the heads of *colour*, *focus*, and *form*. The paper must be read itself, as it is impossible to briefly abstract it. The discussion to which it gave rise is also of much importance, the President's (Dr. Matthews) remarks being of much interest.

*Influence of Light on Development.*—This subject has been examined from time to time with the most contradictory results, and in nearly all cases it is the young of the frog that has been examined. M. Thury has been recently examining this question. He took the eggs of *Rana temporaria* and placed them all under precisely the same favourable circumstances, except that while part received light through colourless glass, another part received it through green glass. The former developed rapidly, and by the end of May had a length of four centimeters, and well-developed hind legs in most of them; while the latter were slowly developed, blackish in colour, hardly had a length of two centimeters by the end of May, and were without a trace of the hind legs. By the 10th of June the former had their fore legs, and some were changed to frogs; the latter, still black, had no trace of legs, and breathed almost exclusively by means of their gills. By the 15th of July all the former had become frogs; but those of the latter still had no legs, and by the 2nd of August they were all dead, without a trace of legs having appeared. Some of the young of the latter lot transferred to the vessel of the former on the 15th of July finished their metamorphosis. At the same time, some of the former transferred to the vessel containing the latter continued to develop, showing the influence of the first impulse in their development.

*The Eozoon Question—An American Mistake.*—In a notice of a book on the geology of New Hampshire, in 'Silliman's American Journal,' March, 1875, the writer makes the following remarks: "On the question of the animal nature of the *Eozoon*, Professor Hitchcock writes judiciously, excepting in a single remark. He observes that 'those who disbelieve the organic theory are mostly better skilled in mineralogy than biology.' But Messrs. King and Rowney, the chief contestants, are not mineralogists, but *zoologists*, and Mr. Carter, of England, another strong opponent of the 'organic theory,' is also a zoologist, and one particularly versed in the lower orders of animal life. There are probably mineralogists that doubt, as there certainly are zoologists, but we can recall no articles by any such on the subject, excepting one or two which aim to show that the limestones containing *Eozoon* are sometimes of igneous origin, an observation which, whether sustained or not, cannot be attributed to mineralogical prejudices." To this we may observe, that Professor King is essentially a geologist, and not a student of microscopic structure; whilst Professor Rowney is exclusively a chemist.

*The Poppy Fungus.*—The 'Academy,' in a late number, points out that Dr. Cunningham (whose memoir we have not received) states that this fungus, which is so destructive to the opium crop, is a near relative of the potato blight, and is named *Peronospora arborescens.* Dr. Cunningham found that soaking fine sections of the poppy leaves in carmine solution enabled the mycelium threads, which took up the colour, to be traced running between the cells, but not in any case perforating them. The *conidia*, which crop out abundantly from the fertile filaments on the under surface of the leaves, he states, "appear very rapidly to lose their power of germinating." He was unsuccessful in his search for the *oogonia* and *oospores*, supposed from analogy to exist in these fungi and spring from the mycelium in the tissues of the plant. Oospores can preserve their germinating power for months, and are conjectured to be important means of propagating the Peronospora moulds. As the *Peronospora arborescens*, or poppy mould, is common on wild poppies in this country, English microscopists may contribute to the further elucidation of its life history.

*The Dimorphic Development of the Cladocera.*—A late number (March) of 'Silliman's American Journal' points out that Dr. G. Sars has discovered a remarkable dimorphism and alternation of generation in *Leptodora hyalina*.* The development from the ordinary summer-eggs, as already described by E. P. Müller, is without metamorphosis and like that of ordinary Cladocera, the young when excluded from the egg agreeing essentially with the adult; while, according to Sars' observations, the young are excluded from the winter-eggs in a very imperfect condition, quite unlike the known young of any other Cladocera, and pass through a marked post-embryonal metamorphosis. In the earliest observed stage of the young of this form, the body is obovate, wholly without segmentation, the compound eye wanting, while there is a simple eye between the bases of the antennulæ, the swimming arms (antennæ) well developed, and the six pairs of legs represented only by minute processes projecting scarcely beyond the sides of the body; but the most remarkable feature is the presence of a pair of appendages tipped with cilia and nearly as long as the body, which are evidently homologous with the mandibular palpi of other Crustaceans, although these appendages have always been supposed to be wanting in the species of Cladocera. Two subsequent stages, gradually approaching the adult form, are described. The adults from the winter-eggs have no vestige of the mandibular palpi left, yet the simple eye—which is wholly absent in ordinary individuals developed from summer-eggs—is persistent, and thus marks a distinct generation. Three stages of the young from winter-eggs are beautifully figured upon the plate accompanying the memoir. This remarkable species has, still more recently, been made the subject of a very elaborate memoir by Professor Weismann of Freiburg,† who, however, had not observed the peculiar development of the winter-eggs.

*The 'Challenger' Soundings.*—Dr. Wyville Thomson says, in his report in the 'Proceedings of the Royal Society,' No. 156, that on the 11th of February, lat. 60° 52′ S., long. 80° 20′ E., and March 3, lat. 53° 55′ S., long. 108° 35′ E., the sounding instrument came up filled with a very fine cream-coloured paste, which scarcely effervesced with acid, and dried into a very light impalpable white powder. This, when examined under the microscope, was found to consist almost entirely of the frustules of diatoms, some of them wonderfully perfect in all the details of their ornament, and many of them broken up. The species of diatoms entering into this deposit have not yet been worked up, but they appear to be referable chiefly to the genera *Fragillaria*, *Coscinodiscus*, *Chætoceros*, *Asteromphalus*, and *Dictyocha*, with fragments of the separated rods of a singular silicious organism, with which we were unacquainted, and which made up a large proportion of the finer matter of this deposit. Mixed with the diatoms there were a few small *Globigerinæ*, some of the tests and spicules of Radiolarians, and some sand particles; but these foreign bodies were in too small proportion to affect the formation as consisting practically of diatoms alone. On the 4th of February, in lat. 52° 29′ S.,

* Om en dimorph Udvikling samt Generationsvexel hos *Leptodora*, Forhandlinger Vidensk.-Selsk., Christiania, for 1873, p. 15, and plate.

† Uber Bau und Lebenserscheinungen von *Leptodora hyalina*, Zeitschrift für wissensch. Zool., xxiv., Sept. 1874, pp. 349–418, plates 33–38.

long. 71° 36′ E., a little to the north of the Heard Islands, the tow-net, dragging a few fathoms below the surface, came up nearly filled with a pale yellow gelatinous mass. This was found to consist entirely of diatoms of the same species as that found at the bottom. By far the most abundant was the little bundle of silicious rods (pl. iii. fig. 5) fastened together loosely at one end, separating from one another at the other end, and the whole bundle loosely twisted into a spindle. The rods are hollow, and contain the characteristic endochrome of the *Diatomaceæ.* Like the "*Globigerina*-ooze," then, which it succeeds to the southward in a band apparently of no great width, the materials of this silicious deposit are derived entirely from the surface and intermediate depths. It is somewhat singular that diatoms did not appear to be in such large numbers on the surface over the Diatom-ooze as they were a little farther north. This may perhaps be accounted for by our not having struck their belt of depth with the tow-net; or it is possible that when we found it, on the 11th of February, the bottom deposit was really shifted a little to the south by the warm current, the excessively fine flocculent débris of the diatoms taking a certain time to sink. The belt of Diatom-ooze is certainly a little farther to the southward in long. 80° E. in the path of the reflux of the Agulhas current, than in long. 108° E.

*Structure of the Lobules of the Liver.*—Herr G. Asp gives a valuable paper of some length on this subject in Ludwig's 'Arbeiten' (vol. viii.), which has been lately fully abstracted in the 'Medical Record.' We merely give the following paragraph. He says, the bile-ducts, in penetrating into the lobule, lose at the same time their cylindrical epithelium and their striated investment, their walls being composed only of fusiform nucleated plates disposed in spirals. E. H. Weber has already shown that a solution of alkannine in turpentine penetrates into the interior of the cells, and the author has satisfied himself by the injections of gutta-percha dissolved in alcohol, and afterwards by the non-passage of a watery solution of Berlin blue into the cells, that there is no rupture of the cells produced by the injection, and that therefore this passage of alkannine and gutta-percha into these cells must take place by filtration. MacGillavry, as is known, injected intralobular perivascular spaces, both by injection of the lymphatics in the liver of a dog, and also by the "puncture" (*Einstich*) method. Frey and Irminger confirmed the existence of these spaces in the liver of the rabbit. E. Hering, however, denied that these spaces were the origin of the lymphatics, and did not succeed in injecting them in the liver of the rabbit. Asp has succeeded in injecting them in the rabbit, by forcing serum for a long time into the vena portæ, under a pressure of 30 to 50 millimeters of mercury (1·2 to 2 inches).

*Is Peronospora the cause of the American Onion Blight?*—According to the editor of 'Grevillea' (March), this seems to be doubtful. He says, the cause is supposed to be a species of *Peronospora*, but the rough figures [given] forbid any such conclusion. It is much more like a species of *Fusisporium.* There would appear from the description to be at least three parasites, one of which is an *Oidium*, and the other (beside the *Fusisporium?*) it is very difficult, as the writer is evidently no mycologist, to make out from either the rude figure or the vague description. The spores as figured resemble those of *Urocystis;* but if they are produced on the branches of erect threads, then they belong to quite another order, and may be some black mould (*Dematiei*). If so, the *Fusisporium* (?) or *Oidium* are much more likely to be the cause of the disease.

*The Peripheral Nervous System of Marine Nematoids.*—In a paper by M. A. Villot, in the 'Comptes Rendus' (Feb. 8), and which is abstracted in the 'Academy' (March 13), it states that the connection of the tactile papillæ of these worms, and of their eyes, with a nervous system has been hitherto obscure, and M. Villot finds that when the worms are rendered transparent by maceration in a mixture of acetic acid, alcohol, glycerine, and water, a thin, granular, highly refracting layer is seen beneath the cuticle. This was described by Dr. Charlton Bastian in 1866, who observed that it contained cellules. Each of these cellules sends a delicate thread to a papilla, and distributes lateral prolongations to adjacent papillæ. "The subcutaneous layer of these marine Nematoids contains a veritable network of ganglionic cells, which supply more filaments to the tactile and visual organs, and this peripheral network is related with the central nervous system through a plexus which traverses the muscular layer, and connects the ventral nerve with the subcutaneous layer." M. Villot alludes to a similar arrangement in sea anemones, and to his own discovery of it in Gordius, and he remarks that "this disposition of the ganglionic cells in a network (*réseau*) is certainly less rare amongst invertebrates than has been generally supposed, and probably represents the whole nervous system of the lower types."

*A Skin Disease caused by Filariæ.*—Dr. John O'Neill,* Surgeon R.N., describes "craw-craw" as a contagious skin disease, endemic among the negroes on the west coast of Africa, which in most of its symptoms closely resembles scabies. He appears to have demonstrated the fact that the irritation is due to the burrowing in the true skin of a minute filaria, one-hundredth of an inch in length, of which he gives drawings.

*The so-called Fungus-Foot of India.*—With reference to this subject, we have received a copy of a leading article from the 'Indian Medical Gazette' (Feb. 1, 1875), in which the writer endeavours to disprove Dr. Carter's view that the disease is caused by the presence of a minute fungus. The article is much too long for insertion, and it seems to lack conciseness. We, without giving an opinion as to the accuracy of the view it suggests, give the following quotation from it: "The 'pale' and the 'black' varieties of the malady are still—and we think rightly so—classed as essentially one and the same disease, but the obscurity with reference to the association of mycelial filaments with the latter and not with the former variety is as evident as ever. We are not aware that anyone who has

* 'Lancet,' Feb. 20.

had the opportunity of examining the dark, granular masses found in the one kind, has failed to satisfy himself that this substance is plentifully penetrated by mycelial threads—although, even in this variety, the tissues immediately adjoining the excavated channels containing the débris present no trace of a mould. Nor, on the other hand, are we aware that any observer has confirmed the statement that precisely similar vegetations are to be found in the roe-like particles of the 'pale' variety: on the contrary, quite an array of authorities might be referred to who have emphatically declared that no such appearances can be detected. Until this discrepancy can be definitely settled, the doctrine that the disease is due to any such vegetable parasite is quite untenable; and even were such growths indubitably demonstrated to exist in both kinds, it would still have to be shown that Godfrey was wrong in inferring, as he appears to have done some thirty years ago, that the peculiar substance was 'an accidental product in, but not forming part of, this peculiar disease of the foot.'"

*Structure and Development of the Teeth in Ophidia.*—A very valuable paper was lately read before the Royal Society by Mr. C. S. Tomes, M.A., on the above subject. The following is an abstract from the 'Proceedings of the Royal Society,' No. 157. "Contrary to the opinion expressed by Professor Owen and endorsed by Giebel and all subsequent writers, the author finds that there is no cementum upon the teeth of snakes, the tissue which has been so named proving, both from a study of its physical characters and, yet more conclusively, from its development, to be enamel. The generalization that the teeth of all reptiles consist of dentine and cement, to which is occasionally added enamel, must hence be abandoned. Without as yet pledging himself to the following opinion, the author believes that in the class of Reptiles the presence of cementum will be found associated with the implantation of the teeth in more or less complete sockets, as in the Crocodiles and Ichthyosaurs. The tooth-germs of Ophidia consist of a conical dentine-germ, resembling in all save its shape that of other animals, of an enamel organ, and of a feebly expressed capsule, derived mainly from the condensation of the surrounding connective tissue. The enamel organ consists only of a layer of enamel-cells, forming a very regular columnar epithelium, and of a few compressed cells external to this, hardly amounting to a distinct layer; the enamel organ is coextensive with the dentine-germ. There is no stellate reticulum separating the outer and inner epithelia of the enamel organ. The successional teeth are very numerous, no less than seven being often seen in a single section; and their arrangement is peculiar, and quite characteristic of the Ophidia. The tooth next in order of succession is to be found at the inner side of the base of the tooth in place, where it lies nearly horizontally; but the others stand more nearly vertically, parallel with the jaw and with the tooth in place, the youngest of the series being at the bottom. The whole row of tooth-sacs is contained within a single general connective-tissue investment, which is entered at the top by the descending process of oral epithelium, whence the enamel-germs are derived. As they attain considerable length, the forming teeth, which were at first vertical, become nearly horizontal, resuming, of course, their upright position once more when they come into place. The clue to the whole peculiarity of this arrangement is to be found in the extreme dilatation which the mouth of the snake undergoes. The general capsular investment probably serves to preserve the tooth-sacs from displacement; while, if the forming teeth remained vertical after they had attained to any considerable length, their points would be protruded through the mucous membrane when this was put upon the stretch in the swallowing of prey. Just as the author has shown in a previous communication to be the case in the Batrachia and Sauria, the hypothetical 'papillary stage' is at no time present. From the oral epithelium there extends downwards a process which, passing between and winding around the older tooth-sacs, after pursuing a tortuous course, reaches the farthest and lowest extremity of the area of tooth-development. Here its cæcal end gives origin to an enamel organ, and, while it does so, buds forth again beyond it in the form of a cæcal extremity. Thus at the bottom of this area of tooth-development there is a perpetual formation of fresh enamel organs, beneath which arise corresponding dentine organs, or papillæ, if such they can be called when arising thus far away from the surface. In essential principle, therefore, the formation of a tooth-germ is similar to that already described in mammals and other reptiles, the difference lying principally in the enormous relative length of, and the tortuous course pursued by, that inflection of the oral epithelium which serves to form the enamel organs. The attachment of the tooth to the jaw is effected by the rapid development of a coarse bone, which is not derived from the ossification of the feebly expressed tooth-capsule, but from tissues altogether external to it. Nevertheless this coarse bone of attachment adheres more closely to the tooth than to the rest of the jaw, from which, in making sections, it often breaks away. The base of the dentinal pulp assists in firmly binding the tooth to this new bone, being converted into a layer of irregular dentine. This 'bone of attachment' is almost wholly removed and renewed with the change of each tooth."

*Action of Crotalus-poison on Microscopic Life.*—A most valuable paper has been contributed to the Royal Society * by Drs. Brunton and Fayrer, on the action of crotalus-poison on animals. We give only the author's observations on microscopic life. We also quote Mr. Darwin's results, which are of great interest.

### *Influence of Cobra-poison on Ciliary Action.*

June 29, 1874.—Ciliated epithelium from the frog's mouth was treated with a solution of cobra-poison and examined under the microscope. At 1.35 P.M., when examined, the action of the cilia was vigorous. At 1.45 it was much diminished. At 1.55 it had entirely ceased. Ciliated epithelium placed under microscope; one part was

* 'Proceedings of the Royal Society,' No. 159.

treated with water, the other with the poisoned solution. At 2.10 P.M. ciliary motion vigorous in both, perhaps more so in that subjected to the poisoned solution. 2.18. Non-poisoned cilia active. Poisoned cilia very feeble. 2.20. Non-poisoned cilia still active. Poisoned cilia very feeble. 2.24. Non-poisoned cilia active. Poisoned cilia very languid. 2.30. Non-poisoned cilia still active. Poisoned cilia have entirely ceased to act. It is evident from this that the poison first stimulates and then destroys the activity of the ciliary action.

August 14.—Frog's blood placed in salt solution, ·75 per cent., at 1.25 P.M. on warm stage, and then subjected to the action of cobra-poison. At first the amœboid movements of white corpuscles went on vigorously. At 2 P.M. they had ceased, or very nearly so, in all that appeared in the field. 2.30. All movement had entirely ceased. The red corpuscles seemed more flattened, the nucleus more visible, and the edges better defined, assuming a pointed and more oval form than usual.

August 25.—Newts' blood examined under $\frac{1}{8}$th object-glass on hot stage, white corpuscles moving slowly. Cobra-poison applied, but no perceptible change observed. The following communications were received from Mr. C. Darwin on the action of some of the same cobra-poison on vegetable protoplasm:—"You will perhaps like to hear how it acted on *Drosera*. I made a solution of $\frac{1}{4}$ gr. to ℥ij of water. A minute drop on a small pin's head acted powerfully on several glands, more powerfully than the fresh poison from an adder's fang. I also immersed three leaves in 90 minims of the solution; the tentacles soon became inflated and the glands quite white, as if they had been placed in boiling water. I felt sure that the leaves were killed; but after eight hours' immersion they were placed in water, and after about forty-eight hours re-expanded, showing that they were by no means killed. The most surprising circumstance is, that, after an immersion of forty-eight hours, the protoplasm in the cells was in unusually active movement. Now, can you inform me whether this poison, if diluted, arrests the movement of vibratile cilia? I dissolved $\frac{1}{2}$ gr. [of cobra-poison] in ℥j of water, so that I was able to immerse two leaves. It acted as before, but more energetically; and I observed more clearly, this time, that the solution makes the secretion round the glands cloudy, which I have never before observed. But here comes the remarkable point; after an immersion of forty-eight hours, the protoplasm within the cells incessantly changes form, and I never saw it on any other occasion so active. Hence I cannot doubt that this poison is a stimulant to the protoplasm; and I shall be very curious to find out in your papers whether you have tried its action on the cilia and on the colourless corpuscles of the blood. If the poison does arrest their movement, it will show that there is a profound difference between the protoplasm of animals and of this plant. Therefore if you try any further experiments I hope that you will be so kind as to inform me of the results. I may add that I tried at first 1 gr. to the ℥j, as that is my standard strength for all substances. It is certainly very remarkable that the poison should act so differently on the cilia and on the pro-oplasm of *Drosera*. After the forty-eight hours' immersion, I placed the two leaves in water and they partially re-expanded. I thought that the whitened glands were perhaps killed; but those of one leaf which I tried with carbonate of ammonia absorbed it, and the protoplasm was affected in the usual manner. I am very much surprised at the action of the poison on the viscid secretion from the glands, which it coagulates into threads and bits of membrane, with much granular matter. Have you observed whether the poison affects in any marked manner mucus or other such secretions?"

### *Action of Cobra-poison on Muscle.*

June 29, 1874.—A standard solution of cobra-poison, ·03 gramme to 4·6 cubic centims. of water, was prepared. 1.25 P.M. The gastrocnemius of a frog was separated and immersed in this solution in a watch-glass; it immediately contracted considerably. 1.30. The muscle contracts with current at 11. 1.45. The muscle has lost its irritability; does not respond to the strongest current. At the same time (1.25 P.M.) the gastrocnemius from the other leg of the same frog immersed in water. Did not immediately contract like that placed in the poisoned solution. 1.30. Contracts strongly to current at 15 c. m. of Du Bois Reymond's coil, more than the poisoned muscle at 11, at the same moment. 1.45. Contracts distinctly at 11, whilst the poisoned muscle has lost all irritability. From this it is evident that the poison first stimulates the muscular fibre to contract, but rapidly afterwards destroys its irritability. The gastrocnemii of a frog were again treated in the same way as in the previous experiment, with precisely the same results.

June 28.—Made several experiments with cobra-poison on ciliated epithelium of frog's mouth, and found that it at first accelerated, then destroyed, the action of the cilia.

November.—A little cobra-poison, dissolved in water, was added to water containing some cells scraped from the mantle of a fresh-water mussel. Among these was a large ciliated cell, which, before the addition of the poison, had been moving slowly, although its cilia were moving actively. Immediately after the addition of the poison the cell began to spin round on its own axis with extraordinary rapidity. In about three or four minutes its motions began to be languid, the ciliary motion ceased, the cell itself elongated, contracted, and then slowly resumed its former shape and became perfectly motionless. Water from the interior of a fresh-water mussel, and containing two specimens of *Paramœcium* in active motion, was examined. They were rotating with great rapidity. A little cobra-poison diluted with water was added. Three minutes after the addition one was discovered with both the cilia and cell-body perfectly still. The cilia of the other were still, but the cell-body was contracted. In about half a minute more it expanded to its normal size and then remained perfectly still.

A piece taken from the mantle of a fresh-water mussel was placed on the slide and examined at the end of about half an hour. Active ciliary motion could be observed both in the fringe of the mantle itself and in several specimens of *Paramœcium*. A little dilute poison was added. At first the ciliary motion seemed increased, but in about two minutes it became slower, and in six had become very languid, and in ten minutes stopped altogether in the specimens of *Paramœcium*, but still continued in some of the cilia of the mantle. A little dilute cobra-poison was added to a piece of the mantle of a fresh-water mussel. The cilia began immediately to move much more rapidly. This was watched for some time. Ciliary motion not affected, or at all events not arrested, after more than half an hour.

December 10. — A piece of the gills of a fresh-water mussel placed under the microscope and a little cobra-poison added at 10.40 P.M. The cilia were extremely active. At 10.55 still active. 11.5. Several ciliated amœboid masses are now quiet instead of rolling over and over as they did, but the cilia on their surface are still moving. 11.15. The cilia on these Infusoria have now nearly all stopped. A few are moving slowly, whilst those on the gills are but little affected. 11.55. Cilia on the gills are still quite active. Those on the ciliated bodies still moving, rather more actively than before. 1.30. Cilia on gills have become much more sharply outlined. Many are standing still, though many still move briskly. To another specimen a strong solution of cobra-poison was added at 10.50. 1.30. Cilia still moving. A third specimen was laid in an almost syrupy solution of dried cobra-poison at 11.28. At 11.40 no effect observable. 1.30. Some have stopped, but numbers are still moving quite briskly. In this case the poison seemed not to have any action on the ciliary motion.

January 6, 1875.—At 3.40 some diluted cobra-poison added to *Vallisneria*. Circulation going on vigorously. About $\frac{1}{10}$ grain in three drops of water. 3.58. The movements are unchanged. 5 P.M. Movements going on as before. Added some solution of cobra-poison at 4 P.M. to another specimen of *Vallisneria.* 4.10. No change. 4.45. Circulation goes on vigorously. 4.55. Perhaps rather less brisk in their movements.

The results of these experiments show that cobra-virus must be regarded as, to a certain extent, a poison to protoplasm, seeing that it arrested with rapidity the movements in Infusoria. Still it cannot be regarded certainly as a very powerful one, for the cilia of the fresh-water mussel continued to move for many hours in a strong solution of cobra-poison; though in other experiments the action was apparently arrested even in weaker solutions of the poison. In the case of cilia from the frog's mouth, the results were more definite, but action was not invariably destroyed. The results of the action of the poison on the amœboid movements of the blood-corpuscles are not very definite. In the case of *Vallisneria*, the circulation in the cells went on with undiminished vigour after the application of the poison for two hours.

*Development of Teeth in Mammals, Birds, and Fishes.*—Mr. C. S. Tomes, M.A., has contributed a valuable paper to the Royal Society,* of which the following is a very brief abstract. He says: "Observa-

* 'Proceedings of the Royal Society,' No. 160.

tions upon many mammals, reptiles, and fishes lead me to the following general conclusions as to the development of teeth:

"(i.) All tooth-germs whatever consist, in the first instance, of two parts, and two alone—the dentine papilla and the enamel organ.

"(ii.) The existence of an enamel organ is wholly independent of the presence or absence of enamel upon the teeth. Examples of this have been recorded by Professor Turner and by myself among mammalia, and by myself among reptiles and fishes.

"(iii.) Nothing justifies the arbitrary division into 'papillary,' 'follicular,' and 'eruptive' stages; nor does any open primitive dental groove or fissure exist in any animal examined.

"(iv.) In all cases an active ingrowth of a process of the oral epithelium, dipping inwards into solid tissue, is the first thing distinguishable, although the formation of a dentine papilla opposite to its deepest extremity goes on *pari passu* with the development of its cæcal end into an enamel organ.

"(v.) A special capsule, or follicle, to the tooth-germ may or may not be present. When present it is, in part, a secondary development from the base of the dentine papilla; in part a mere condensation of surrounding tissue."

*Filaria in the House-fly.*—Professor Leidy (of U.S.A.) has recently found that the common house-fly is afflicted by a thread-worm, about a line in length, which takes up its abode in the proboscis of the fly. From one to three worms occurred in about one fly in five. This parasite was first discovered in the house-fly of India, by Carter, who described it under the name of *Filaria muscæ*, and suggested that it might be the source of the Guinea-worm in man.

*Mode of Development in Echinoderms.*—Mr. A. S. Packard, jun., who has published an interesting summary on the development of Radiata, in the 'American Naturalist' (April, 1875), draws the following conclusions as to the Echinoderms:

"Echinoderms as a rule, then, are reproduced alone by eggs and sperm-cells. After fertilization of the egg they pass through:

"1. Morula stage.

"2. Gastrula stage.

"3. A larval, temporary stage (Pluteus, Brachiolaria, Auricularia).

"4. The Echinoderm grows from a water tube of the larva, finally absorbing the latter, whose form is often materially changed during the process. It thus undergoes a true metamorphosis, in a degree comparable with that of some insects."

*The Colouring Matter of Birds' Eggs.*—Mr. H. C. Sorby, F.R.S., our President, read a paper before the Zoological Society of London, May 4, "On the Colouring Matter of the Shells of Birds' Eggs, as studied by the Spectrum Method," in which he showed that all their different tints are due to a variable mixture of seven well-marked colouring matters. Hitherto the greater part of these had not been found elsewhere. The principal red colouring matter was connected with the hæmoglobin of blood, and the two blue colouring matters were probably related to bile-pigments; but in both cases it was only a chemical and physical relationship, and the individual substances were quite distinct, and it seemed as though they were special secretions. There appeared to be no simple connection between the production of these various egg-pigments and the general organization of the birds, unless it were in the case of the Tinamous, in the shells of the eggs of many species of which occur an orange-red substance not met with in any other eggs, unless it were in those of some species of Cassowary.

*Thin Sections of the Traps of the Mesozoic Basin.*—Professor Frazer made the following remarks before the Academy of Sciences of Philadelphia, at its meeting on March 2, 1875: "The great mesozoic basin traverses York, Adams, Chester, and Montgomery Counties, in Pennsylvania, as well as New Jersey and New York, while detached portions are found in several of the New England States, in none of which are its characteristics more clearly defined than in Connecticut. During a recent visit to New Haven I had the privilege of examining the fine microscopic slides or thin sections which have been prepared by Mr. Dana from the traps of that region. It is of great interest to observe the striking resemblance of these rocks to our own from the same formation. To the eye, and even under the magnifying glass, they seem the same, whereas in fact they are of, at least, two different kinds. One kind, which has been described on several occasions before the Academy as that forming the Seminary Ridge near Gettysburg, is a greenish-grey compact dolerite (projected by me on the screen by means of the gas microscope, at a previous meeting), which, under higher magnifying power, shows white tablets of plagioclastic felspar and green crystals of pyroxene, with some chrysolite (olirine). Far different is the rock which has been previously referred to as syenite, and which has an apparently similar representative near New Haven. Under the microscope, however, the coarse rock from New Haven, resembling the others from that locality in everything but texture, differs materially from the specimen from Gettysburg. Since my return home I have examined two or three other slides of the Gettysburg rock, and find no essential difference between them. They contain hornblende and quartz, the others do not. The constituents of the coarse rock from both States were pyroxenite, plagioclase, magnetite, some chrysolite, some bictite, and rarely quartz."

*Cancer of the Bones of the Head.*—At a late meeting of the New York Pathological Society,* Dr. Janeway reported on Dr. Kipp's specimen of cancer of the bones of the head, presented at the last meeting of the Society. On examining it by the microscope it was found to consist of trabeculæ of connective tissue and lymphoid cells, varying from $\frac{1}{2000}$th to $\frac{1}{3000}$th of an inch in diameter. These cells were arranged in long tubular processes. The inference was, that the cancer had its origin in the antrum, and from that extended to the other bones of the head mentioned in Dr. Kipp's report.

*The Lymph of Small-pox.*—At the meeting of the Linnean Society on the 1st of April, Dr. E. Klein gave an account of his microscopical

* March 24, 1875.

observations on the lymph of sheep-pox. It has been shown that the virus resides in the solid particles of the lymph and not in its fluid portion. These solid particles were shown to be identical with the organisms (Schizomycetous Fungi), called by Cohn and Burdon Sanderson "Micrococci"; they are likewise produced by the pus-cells from the granules contained in their interior. Dr. Klein has produced the pocks on sheep by artificial inoculation of these germs. On examination of a pock so produced, the "micrococci" were found in the lymphatic spaces which are formed in the skin at an early stage. They occurred in masses or in myceloid threads. At a later stage signs of fructification were observed, and conidia of a *Penicillium*-like character were produced in the spaces. The same growth is found in the cavities of the pustules subsequently developed. Dr. Klein also produced the disease by the injection of lymph directly into the vein; the pustules formed were quite the same as those produced by inoculation, and the same *Penicillium* growth was found in their interior. These remarks were illustrated and supported by a series of drawings and by microscopical preparations.

*A Serious Error.*—Dr. Thacker, who is the editor of the 'Cincinnati Medical News,' and who is also a distinguished microscopist, has made a grave mistake in announcing that Messrs. Ross have simply followed Mr. R. B. Tolles, of Boston, in their manufacture of new glasses. 1st. Messrs. Ross were unquestionably the first to introduce the new form of objectives which they have now for some years adopted. 2nd. They certainly have not adopted any of Mr. Tolles' ideas. The following statement therefore, which is made by the editor of the 'Cincinnati Medical News,'* demands correction:

"It seems that since the superiority of R. B. Tolles', of Boston, new four-system lenses has been demonstrated, the distinguished English makers of objectives are abandoning their old formulas and instituting new ones. At a late meeting of the Royal Microscopical Society, Messrs. Powell and Lealand exhibited two glasses on a new formula; one, $\frac{1}{4}$th, showing the lines of *Amphipleura pellucida*, and the other, $\frac{1}{8}$th, showing *Pleurosigma angulatum*, $+$ 4000. This object was illuminated by direct light. The effect was to show the interspaces remarkably magnified, and the beads comparatively small; they stood out like minute spheres of pink coral on a white ground.

"The Messrs. Ross also have determined to abandon their old construction from the $\frac{1}{2}$-inch upwards, and adopt one devised by Mr. Wenham. In the new combination, it is stated, a great increase of brilliancy and definition is obtained by dispensing with six surfaces formerly used. The higher powers, from $\frac{1}{5}$th upwards, can be also used as immersion lenses by merely adjusting the collar to the mark 'wet,' thus avoiding the cost of extra fronts and loss of time in changing them.—ED."

*A Fungus in a Flamingo.*—Professor Leidy has read a paper before the Academy of Natural Sciences of Philadelphia,† which is in some measure similar to that which Dr. Murie published in this Journal

* April, 1875. † January 17, 1875.

some years ago. Professor Leidy remarked that a pair of flamingoes had recently died in the Garden of the Zoological Society at Fairmount Park. Dr. Chapman, who had dissected the birds, called his attention to the diseased condition of the lungs of one of them, the other not being affected in this respect. The posterior part of the lungs on both sides, contiguous to the abdominal air-sacs, was occupied by an indurated brown substance, in striking contrast with the usual bright roseate hue of the neighbouring pulmonary tissue. An incision made into the indurated substance exhibited a brown compact surface with greenish-black dots which corresponded with the bronchial tubes. On microscopical examination the substance was found to be pervaded with a fungous vegetation, and the greenish-black dots were due to the fruit heads profusely covered with coloured spores.

Professor Owen, upwards of forty years ago, mentioned the existence of a green mould he had observed in the lungs of a flamingo which died in the menagerie of the Zoological Society of London, but he gave no description of the plant by which we can recognize it. Since then many accounts have been given of the existence of fungous vegetation in the diseased lungs of various birds, but I think it has not been determined whether the diseased condition was due to the fungus, or whether this was a subsequent production.

The plant observed in our diseased flamingo belongs to the Moulds or Mucedines, and is evidently an Aspergillus. A number of species of this genus have been described, growing on various decaying substances. The common blue mould found in cheese and bread kept in a damp place is the *Aspergillus glaucus*. From this the mould of the flamingo is quite distinct in the structure of the fruiting receptacles, in which respect it more nearly resembles the *Aspergillus dubius*, growing on rabbit's dung. The Aspergillus of the flamingo I suspect to be the same as one described by M. Robin, under the name of *Aspergillus nigrescens*, discovered by him in the lungs of a pheasant (*Phasianus colchicus*) affected with phthisis.

In the flamingo mould the mycelium consisted of a dense flock of delicate ramifying filaments pervading the indurated pulmonary tissue, which consisted largely of nucleated cell elements and granules. The threads of the mycelium were branching, and occupied on the interior with clear globules appearing like rows of beads. The threads measured usually the $\frac{1}{500}$th of a millimeter or less in diameter.

The fruiting stems were straight, from $\frac{1}{4}$th to $\frac{2}{5}$ths of a millimeter long, not articulated, usually simple, and rarely divided approximating a right angle, near the head. They were about the $\frac{1}{250}$th mm. wide at the mycelial origin, and double the width approaching the head. The head continuous with the stem was pyriform; or the stem expanded into a globular receptacle, which was closely crowded with linear processes, or sporophores, supporting the spherical, translucent coloured spores. The latter profusely invested the heads, but were too ripe and readily detached to determine their exact arrangement in relation with the sporophores. These, on the contrary, remained firmly attached to the receptacle.

The receptacles measured from the $\frac{1}{60}$th mm. to the $\frac{1}{50}$th mm.

The stratum of sporophores was from $\frac{1}{166}$th mm. to the $\frac{1}{125}$th mm. thick. The spores were the $\frac{1}{333}$rd mm. in diameter.

By transmitted light the spores appeared so faintly coloured that the tint was undetermined; by reflected light, in mass they appeared of a greenish hue. The receptacles including the sporophores appeared fuscous by transmitted light, but white by reflected light.

In M. Robins' plate of *A. nigrescens* he represents most of the fruiting stems as articulated, but in our plant none of this character were detected.

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## NOTES AND MEMORANDA.

**An American View of the Advantage of High Angular Aperture.**—Mr. A. F. Dod, of Memphis, Tenn., U.S.A., has been inquiring into this subject, and has published the following very remarkable results in the 'American Naturalist,' * a journal which we had before very highly esteemed. Mr. Dod says:

"Dr. Carpenter lays down as a fixed law the statement that 'all who have made much use of the microscope are now agreed as to the superior value of objectives of moderate or even comparatively small angle of aperture for ordinary working purposes, the special utility of the very wide apertures being limited to particular classes of objects.' †

"It is now claimed that this no longer holds good, and our investigations were undertaken simply with a view to testing the correctness of this statement.

"The glass we selected as the representative of the wide angles was a 'four-system' immersion $\frac{1}{10}$th, of nearly 180°; the narrow angles with which it was compared were the best at our command, by leading makers of England, Germany, France, and America, and comprised both dry and wet systems. Bearing in mind the theory that the wide angles are only superior on diatoms and with oblique illumination, we discarded diatom tests, and used only central light.

"The first slide selected was a specimen of mosquito scales, dry. Under the $\frac{1}{10}$th of nearly 180° this object was beautifully defined, the structure of the intercostal spaces, longitudinal ribs, and terminal spines being all sharply and clearly shown. Even under so high eye-piecing as $\frac{1}{4}$th inch solid (equal to D), the object was splendidly illustrated. The narrow and moderate angles were then successively brought to bear on the same object, with the uniform result that, while not giving so good definition under low power eye-pieces, under the high eye-piece all utterly broke down. The next test selected was a slide of voluntary muscular fibre, in balsam. Here again the nearly 180° glass gave splendid results, the definition of the striæ being perfect, even under D eye-piece. The moderate angles were again brought on the field, with the same result as before.

"These facts seem to justify the claim that the law, as laid down, touching the general usefulness of the wide-angled glasses, is not now

* April, 1875. † Carpenter, 4th ed., p. 172.

correct, having obtained credence at a period when the difficulties attending their construction had not been thoroughly mastered; but that such is no longer the case. I feel sure that the advanced workers of this country already accept as true the conclusions arrived at by our committee; but I am also sure that by far the greater number of our microscopists still hold to the old faith."

It is hardly necessary to point out the absurdity of the statements made above, but their having been published in a journal of so good a repute as the 'American Naturalist' demands that they shall receive immediate contradiction. In the first place, the *fixed law* which Mr. Dod refers to is the very opposite. Dr. Carpenter refers to the subject in the most guarded language, and all that can be gathered from his statement is the fact that men who are engaged in real microscopic work prefer glasses with small angles to the very wide-angled objectives. And in this we can assure Mr. Dod that almost every *worker* with the microscope on this side of the Atlantic will most distinctly concur. Dr. Carpenter's position is this: that of two objectives, one of moderate angle (say a $\frac{1}{4}$th of 75°, or a $\frac{1}{5}$th of 90°) and another of wide angle (say 120° and 140°), *equally well corrected*, the one of moderate angle will be better for all ordinary work than that of the high angular aperture. And this universally admitted fact is shown very fully in the present number by the able paper of Mr. Slack. But it is necessary to point out where Mr. Dod has most probably erred. It is needless to indicate that his glass of 180° is manifestly an utter impossibility. But what we would say is this: that there is no statement made by Mr. Dod, as to whose glasses were tried, and we have no evidence as to whether they were perfectly corrected or not. It is quite possible that a glass whose corrections were imperfectly made would break down under a deep eye-piece. But then such a glass cannot be a good one. Besides, Mr. Dod does *not* state how much of his object he saw at once, a point of some importance. Altogether, we cannot congratulate the Memphis Microscopical Society on the wisdom of their latest move.

**A Grant for Inquiries on Staining Reagents in Microscopic Anatomy** has just been made by the Royal Irish Academy, to the extent of 25*l*., to Dr. Reuben Harvey. The sum is not very much, but the granting of it is a step in the right direction, and we are glad to see that the academicians have adopted it.

**The Locality of the Bermuda Tripoli.**—We have received the following note on this subject, which was originally communicated to the Boston Society of Natural History, at a recent meeting:

The Secretary read a note by Mr. Charles Stodder, on the locality of the Bermuda Tripoli, accompanied by a communication on the same subject by Professor Christopher Johnston, of Baltimore. In 'Science Gossip,' London, for May, 1874, is a note signed "F. K.," in reply to a correspondent who had inquired for the locality of the celebrated "Bermuda Tripoli," so rich in peculiar forms of Diatomaceæ, described by Ehrenberg and the late Professor J. W. Bailey. "F. K." says that "Mr. Geo. Norman, of Hull, England, found that it came from

Nottingham, Maryland." As the Nottingham earth came from our corresponding member, Professor Christopher Johnston, of Baltimore —and that it was possible that Nottingham was the original locality, was well known in this country independent of Mr. Norman—I applied to Professor Johnston for the authentic history of that deposit; to which he replied by the paper herewith appended. Mr. Norman's paper is in the 'Quarterly Journal of Microscopical Science,' January, 1861. In that paper he does not say that the Bermuda came from Nottingham, as "F. K." represents, but only suggests the possibility of the case, as American diatomists had before him. Since Dr. Johnston's paper was written, Dr. Josiah Curtis has visited that part of Maryland, and discovered numerous other localities of the diatomaceous earth, containing the same forms as the Bermuda and Nottingham deposits.

*About the Rediscovery of the "Bermuda Tripoli," near Nottingham, on the Patuxent, Prince George's County, Maryland. By Christopher Johnston, M.D.*

In 1854 I had the great pleasure of being a correspondent of Professor J. W. Bailey, of West Point, and during the year received from that distinguished gentleman valuable guidance, and also specimens of diatomaceous material, among others a very small portion of a buff-coloured dust, labelled "Bermuda Tripoli." From this I prepared a single slide, now in my possession, containing very beautiful forms, chiefly *Heliopelta*, *Coscinodiscus*, *Craspedodiscus*, *Aulacodiscus crux*, and *Eupodiscus Rodgersii*. At a later period I was in correspondence with my friend J. Sullivant, Esq., of Columbus, and while making some exchanges, I asked for "a good boiling of Bermuda Tripoli;" to which request Mr. S. replied, June, 1859, "I would send you a quantity if I had it. I have nothing but a slide, and I have been long struck with its resemblance to the Richmond earth. . . . . In a letter just received from Mr. Geo. Norman, he says, 'What a pity the locality of Bermuda Tripoli and its beautiful fossils has been lost;' and then adds 'that himself and Dr. Arnott had come to the separate and independent conclusion that they *never came from Bermuda at all*, but from Bermuda or James River in Virginia.' I have very little doubt of it, for there is a place called 'Bermuda Hundreds' on the James River. From the frequent intercourse between Baltimore and Richmond, you have an opportunity of following this up. I trust you will." Early in 1860 I sent my "Bermuda" slide to Columbus, where the beauty of the diatoms was much appreciated, and Bermuda Hundreds again the subject of remark, as appears by a letter from Mr. Sullivant, dated March 25, 1860. I had resolved to visit Bermuda Hundreds for the purpose of making an exploration, when, about the 1st of April, my valued friend, P. T. Tyson, Esq., State Geologist for Maryland, sent me a number of small parcels of "Tripoli," which he had procured in different parts of the state. One of these earths, marked *Nottingham*, attracted my particular attention, for I had the extreme pleasure to find in it the diatomaceous forms familiar on my Bermuda Tripoli slide, besides a host of others, and I at once was satisfied that the lost Bermuda Tripoli was before me, and its locality discovered. I at once communicated my discovery to Mr. Tyson, who was much gratified at being the means of leading to so interesting a development; and as he was about to visit Boston as member of the American Association for the Advancement of Science, which was to have its sitting in May, my friend offered to take a short note which I hastily prepared, together with some of the "new Bermuda earth," and lay both before the Academy. Mr. Tyson kept his promise. In the next month I received a note from that eminent physician, Dr. Silas Durkee, of Boston, of date June 9, 1860, making me acquainted with Charles Stodder, Esq., an associate of the Boston Natural History Society, and conveying a valuable and detailed catalogue of "the genera and species" of Diatomaceæ found by Mr. Stodder in the Nottingham earth. I had hardly convinced myself of the identity of the "Bermuda Tripoli" and the Nottingham earth, than I thought of my friend Mr. J. Sullivant, to whom I dispatched a parcel of the earth in question; and in his reply, dated June 4, 1860, he says, "I trust you have rediscovered the equivalent of the Bermuda Tripoli." Although I had identified the "Bermuda Tripoli" in the Nottingham earth, I could not abandon all hope of tracing the former to Bermuda Hundreds, on the James. Accordingly, in the summer of 1860, I made a pilgrimage to the latter place, situated upon the right bank of the river, above City Point, about one hundred miles nearly due south of Nottingham, and since made remarkable by an historic amphoric inclusion, but my visit was without other fruit than a surprise to the inhabitants, who failed to appreciate my zeal, but who nevertheless very kindly aided my search. About this time my friend Mr. Wm. S. Sullivant, of Columbus, sent a portion of the Nottingham earth with which I furnished him to Mr. G. Norman, of Hull, as I find in a letter, of date January 12, 1861, from Dr. J. M. Dempsey, of Charterhouse Square, with this reference: "In the last 'Quarterly Journal of Microscopical Science,' there is a short paper by Mr. Norman, of Hull, describing the fossil forms of Diatomaceæ, contained in a deposit forwarded to him by Messrs. Sullivant and Wormley, Columbus, Ohio, described or discovered by you at Nottingham, Maryland." The letter also contained a request for some of the earth, with which I complied at once, forwarding by the same conveyance a parcel to Mr. G. Norman, of Hull, and to my almost namesake, the venerable Christopher Johnson, Esq., of Lancaster, and included under the cover of each several other Maryland deposits. For these, Mr. Johnson wrote in acknowledgment a very kind letter, bearing date March 15, 1861, and Mr. Norman's reply soon followed, his letter being dated April 12, 1861. From this time until the present, Mr. Tyson and myself have supplied quantities of the Nottingham earth to very many correspondents; and upon looking over my own slide of the new Bermuda, nothing gives me so much satisfaction as the knowledge that I have, by the very probable discovery of the "Bermuda" locality, contributed so much to the pleasure of other microscopists.

The Use of a V-shaped Diaphragm.—At a meeting of the Memphis Microscopical Society, on the 21st of January, 1875, Mr. J. E. Smith said, that "some weeks since, when engaged in testing the extreme apertures of several objectives (including a $\frac{1}{6}$th and $\frac{1}{10}$th immersions, Tolles' new four-system formula), I used, for the purpose of cutting off the central ray, a simple piece of japanned iron plate—the ordinary ferrotype plate used by photographers. The usual diaphragm box and revolving plates were removed, leaving the under side of the stage entirely unobstructed; the piece of ferrotype plate was fastened by a screw near its edge farthest removed from the illuminating lamp, so as to entirely close the 'well-hole' of stage. Now, by *bending* the ferrotype plate it was easy to convert it into a ⊲ shaped diaphragm, with its open side adjacent to the mirror—and the included angle formed by under side of stage and the ferrotype plate depended entirely on the disposition (bending) of the plate. While using this rude and simple apparatus, with an objective of 170°, the ferrotype plate making an angle of 20° with under side of stage, the illumination being furnished by a German 'student's lamp,' I was surprised to find that I got strong views of transverse striæ on No. 18 of the Möller Probe-Platte, the same shell having utterly defeated the objective named on any previous occasion. Since I have made numberless experiments, using a variety of wide-angled objectives, and find the ⊲ diaphragm of singular advantage when resolving severe tests. As to the percentage gained by the use of the diaphragm, I hardly know—and this will probably vary with the objective used. I detail a single experiment, and leave others to form their own conclusions. With the Tolles' four-system ($\frac{1}{10}$th immersion) German student's lamp, and using aperture, say of 160°, *without* diaphragm, I usually see *A. pellucida* in Möller Probe-Platte (No. 20), exhibiting transverse striæ in *patches*, and strongest near the end of valve; turning the diaphragm into position and so as to include angle of 20° with under surface of stage, and little, if any, change made in the illumination, I now get easily a strong '*striæ*' of transverse lines, and evenly distributed from end to end of the shell. Similar results obtain when viewing Nos. 18 and 19 on same Platte. This much for work with oblique light. Another fact is perhaps of still greater importance. The diaphragm may be bent so as to include any angle, say from 5° to 50°, with lower surface of stage. We will assume, say an angle of 45°. Now, by a proper disposition of the radial arm carrying the mirror, arranging so that the diaphragm 'splits' the illuminating beam, i. e. one half entering the wedge-shaped aperture while the other half is excluded, a beautiful effect of black-shadow illumination will at once be recognized, and the intensity of the shadows is easily graduated at the will of the observer, in two ways; 1st, by a simple movement of mirror; and 2nd, by combined movements of mirror and diaphragm. The effects thus obtained are innumerable and within the reach of any intelligent observer. This black-shadow illumination gives promise of real value to those engaged in advanced investigations of structure, in fact it will be found to be a new power. This simple and rude appliance has been for weeks a *permanent fixture* to the stage of any instrument, and the ferrotype plate seems to bear any amount of bending. Its functions are obvious, the iron plate simply shutting out rays more or less diffused. The little bit of a 'fixing' constitutes, with the mirror, all the 'sub-stage apparatus' I have any use for."

**American Microscopical Society.**—At the annual meeting of the American Microscopical Society the following officers were appointed for the ensuing year: President, John B. Rich, M.D.; Vice-President, W. H. Atkinson, M.D.; Secretary, C. F. Cox; Treasurer, Prof. T. L. Orenieu; Curator, William Dean.

**The French Exploring Expedition.**—We learn that an exploring expedition will shortly leave Marseilles to make researches into the depths and animal organizations of the Mediterranean. Soundings and dredgings similar to those made by the 'Challenger' will be made by a steamer specially provided with microscopes, photographic apparatus, and means for preserving new or rare specimens of marine zoology.

**An American Postal Micro-cabinet Club.**—A club for the circulation and critical study of microscopic objects has been formed, its design and methods conforming mainly to those of the English club. The following rules have been prepared for the use of the organization, and Rev. A. B. Hervey, No. 10, North Second Street, Troy, N.Y., has consented to act as secretary until the first regular election of officers. Applications for membership may be made to him.

*Rules of the American Postal Micro-cabinet Club.*

1. This club shall be called the American Postal Micro-cabinet club.
2. Its object shall be the circulation, study, and discussion of microscopic objects.
3. Reliable persons accustomed to work with the microscope, and able to contribute to the usefulness of the club by sending good objects for examination, shall be eligible to membership.
4. Applications for membership may be made to the secretary, and should be accompanied by reference to some person, preferably a member of the club or a well-known microscopist, who is acquainted with the applicant.
5. Names of applicants known to be eligible shall be submitted to vote by the secretary, who shall send them around through the circuits in the letter packages. A four-fifths vote of all the members shall be necessary to election.
6. Members elect shall be notified of their election as soon as they can be placed in any circuit, either by the formation of new circuits or by filling vacancies in old ones. They shall then, and during the first week of every January thereafter during their continuance in the club, send to the secretary, as annual dues, the sum of fifty cents. If this subscription should prove insufficient to defray the expenses of the club, the secretary, with the approval of the President and managers, may give notice of an increase to any required sum not exceeding one dollar per year.

7. The officers shall be a President, Secretary, who shall also act as Treasurer, and two managers. They shall be elected by ballot by a plurality of votes cast, blanks for that purpose being sent around by the secretary in January of each year.

8. The secretary shall arrange the members in sections of twelve members each.

9. He shall send a box capable of holding one dozen slides to the first member of each section. Each person shall, within four days of the date of receiving it, put in a slide, preferably one which illustrates some new result of study or method of preparation, and mail the package, carefully directed and stamped, to the name and address next below his own on the list of members of the section. After completing each circuit the box shall be returned to the secretary, who shall start it on the next circuit. When it has completed the whole circle of all the circuits, it shall be returned to the first circuit again, when each member shall remove his own slide and replace it with another, mailing the box as before to the next member.

10. Slides placed in the box must contain no writing. Written labels should be soaked off or pasted over, and the slide designated by a number to correspond with the number of the owner in the list of members of the section.

The slides are to be very carefully packed in the box, to which is securely attached by a string, at a distance of two inches, a tag bearing a postage stamp and the address of the next member of the section. Nothing is to be placed around or upon the box which could invite a blow from the post-office stamp.

11. If any member should receive a box too much damaged to be safely used, or containing broken or damaged slides, or not containing the full number of slides indicated by the accompanying memoranda, he shall at once notify the secretary and the member who last mailed the box.

If the loss cannot be adjusted by exchange between the owner of the slide and the person who mailed it, the damaged slide shall be sent to the secretary, who shall compensate the owner, to an extent not exceeding one dollar for any one slide, out of any unappropriated funds belonging to the club. Cash on hand and in excess of the estimated expenses of the current year shall alone be considered subject to this claim. Differences of opinion in regard to damages shall be referred to the President, whose decision shall be final.

12. At the same time with the box, and to the same address, shall be invariably mailed a letter-package containing a list of members of the section and of objects in the box, and blank papers for memoranda, remarks, questions and answers, notices of exchanges sought or offered, &c.; also, at the proper times, voting lists for election of officers or the transaction of other business. Everything contained in the letter-package shall be considered the property of the club, shall only be removed therefrom by the secretary, and shall be by him filed or published as may seem most advisable.

13. The letter-package and the box of slides should accompany each other, and any member who does not receive either one within three days after the receipt of the other, shall promptly notify the secretary.

Notice shall always be sent by members to the secretary, one week previously, if practicable, of any change of post-office address, or of any absence from home which would cause more than ten days' delay in the forwarding of any package directed to them.

14. The secretary shall annually submit a detailed statement of receipts and expenditures to the managers, who shall audit the same on behalf of the club.—*American Naturalist*, April, 1875.

**A Micro-polariscope and Lantern.**—We learn from the 'Proceedings of the Academy of Natural Sciences' of Philadelphia, that at its meeting on the evening of February 2, 1875, Professor Persifor Frazer, jun., exhibited a combination of the polarizer, vertical lantern, and microscope, by means of which the manner in which different salts crystallized out of their solutions, together with the manner in which they affect polarized light, was explained and illustrated by solutions of potassium chlorate and urea in alcohol. The light from a lime lantern is passed through the elbow-tube polarizer, thence upward through the vertical lantern and the 2-inch lens microscope, when it is again reflected horizontally on the screen. After the formation of the crystals had been shown by plain polarized light, the analyzer was inserted and the characteristic colours of polarization produced. It was explained that while this method had the advantage of so magnifying the crystals produced from small quantities of solutions that their structure could be minutely observed, as well as the sudden molecular change which caused the polarizing effect, it was open to the objection of a very large loss of light—first, by the polarizer, and again by the microscope. It was suggested that a means of obviating at least a part of this difficulty would be the use of the parabolic reflector, in connection with the first condenser. Professor Frazer then proceeded to exhibit the microscopic structure of thin sections of some of the Palæozoic rocks found in York and Adams Counties, Pa. A map of the region whence the specimens were collected was first thrown on the screen and the geological formations described. After explaining the manner in which the thin sections were prepared, the following specimens were exhibited: A piece of diorite from the north-eastern corner of Saxony; a foliated chlorite slate; ferruginous gneiss; Nes'silicon steel ore; diorite; quartzite rock, with magnetic iron ore, from the north-eastern part of York County; hornblende slate; limestone, containing particles of a substance probably apatite; a syenite from Germany, with hornblende, quartz, and orthoclase; and a syenite from near Gettysburg.

# CORRESPONDENCE.

## Recent Works on the Diatomaceæ, and on Objectives.

*To the Editor of the 'Monthly Microscopical Journal.'*

Denstone, Ashbourne, *March* 29, 1875.

Sir,—I am desirous to bring under the notice of the readers of the 'Monthly Microscopical Journal' the two following works which have recently come into my hands: (1) Dr. J. Schumann's 'Diatomaceæ of the High Tatra,'* and (2) Otto Müller's 'Recent Microscopic Objectives Compared.'†

Dr. Schumann's pamphlet is, beyond all dispute, the most profound work the world has yet seen on the application of the microscope to Diatomaceæ,—and has no second. The most cursory glance will convince anyone that the writer is thoroughly competent to speak with authority on all the subjects he treats of.

Before Dr. Schumann's book reached me, and while I knew it only by reputation, it was on my mind to translate it, or, at least, to abbreviate it for this Journal; but an inspection of the work itself soon showed me the impracticability of such a scheme. The most valuable portions—those wherein he sets forth in exact mathematical formulæ the relation of the lines to each other, and the principles he deduces therefrom (pp. 18–26), and the rationale and estimate of errors of observation (pp. 30–35)—admit of no fair abbreviation. I will content myself, therefore, with extracting only two of his remarks, out of many, that diatom-life, so far at least as the High Tatra is concerned, ceases at an elevation of 7802 Vienna feet, though Ehrenberg speaks of some as found on Mount Rosa at an elevation of 14,284 feet; further, that the structure of the frustule and the closeness of the lines depend on the elevation above the sea level (pp. 1, 38, 39, 85–94), and that consequently the size of the frustule diminishes, and the number of the lines in a given space increases, in proportion to the increase of elevation at which specimens of the same diatom may be found, and that the width of the lines diminishes about $\frac{1}{156000}$ of a Paris line for every 600 feet of elevation (p. 93).

As the next best thing to a complete translation of Dr. Schumann's pamphlet I could wish much to see his four plates with their explanations (pp. 101, 102) reproduced, photographically, if possible, in the pages of this Journal: they are perfect marvels of skill and delicacy. In this connection I may refer those who are familiar with the appearance of *Amphipleura pellucida*, when *perfectly* resolved, to Dr. Schumann's drawing of that diatom (plate ii. fig. 19). I would also here publicly thank Dr. Emil von Marenzeller, Secretary to the 'K. K. zool. botan. Gesellschaft in Wien,' for his kindness in sending

* 'Die Diatomeen der hohen Tatra,' von Dr. J. Schumann. Wien, 1867.

† 'Vergleichende Untersuchungen neuerer Mikroskop-Objective,' von Otto Müller. Berlin, 1873.

me the only copy I possess, and for the courteous letter which accompanied it.

The other work I have mentioned, that by Otto Müller, is one of smaller pretensions, and restricts itself to a comparison of fifty-one not *very* modern objectives; amongst which are forty-seven German, three American (by Grunow), and one French lens (an old $\frac{1}{16}$th inch by Chevallier, 1847).

The German glasses are confined to those by Hartnack, Bénèche, Gundlach, Zeiss, and Möller, while many eminent names are conspicuous by their absence, e. gr. Plössel, Seibert, Schröder, Schieck, Wappenhans and Merz; to say nothing of Hasert, with whose lenses Dr. Schumann did all his admirable work. English objectives also seem to be utterly unknown to him,—at least he never mentions them. Some may remark that I have omitted Nobert from my supplementary list of names, who somehow enjoys a high reputation in England as a maker of objectives; but the plain truth is, no one in Germany knows him *in that capacity*. As a ruler of test-lines he is well known, but his countrymen have hitherto shut their eyes to his merits as a maker of lenses. I believe also he has never exhibited such at any of the competitive exhibitions which take place every year at some one or other of the large German towns. Germany indeed is full of small men who put "Optiker" after their names, on the strength of their selling spectacles which they do not make. I might also have omitted Merz, as he figures rather as a maker of telescopes than of microscopic objectives.

Yours faithfully,

W. J. Hickie.

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## Rough Measures of Angular Aperture.

*To the Editor of the 'Monthly Microscopical Journal.'*

Hartley Court, Reading, *May* 14, 1875.

Dear Sir,—On a former occasion I drew the attention of the Society to the possibility of measuring apertures by means of miniatures.

The evident principle on which this must be done is the detection of image-forming rays, and this should not be a blurred or confused but a really good image.

The method which I adopt is somewhat novel. Removing the condenser, an adapter is fitted to its upper part so as to receive an object-glass front downwards. The microscope is then used with a low power for looking down the interior of the objective fixed below. A 3-inch giving a low power, armed with a C eye-piece, is generally sufficient. Thus arranged the instrument is for greater convenience placed horizontally. A board is erected on a level with its axis, ruled with radiating lines terminating in a divided arc. On this arc are two carriers for two small benzine lamps, regulated to give small, steady flames. These are now gradually separated till their miniatures become indistinct.

For a Ross or Powell and Lealand stand, the arm may require a little lateral movement to bring the miniature flame into view. A better plan is to fix the objective to the stage by means of a clamping plate cut with the usual screw to receive the objective to be measured (front downwards).

The angle contained between the centre and the two lamps will then give the useful angular aperture.

Of course this method may be greatly refined by substituting illuminated patterns instead of the flames, or in front of them.

A method sufficiently near may also be used by means of two lamps such as described, with only one objective. If the eye-piece be taken out, the miniature of the lamps will be seen down the tube, within the objective to be tested.

Cover one lamp. Observe if the field on replacing the eye-piece is exactly one half illuminated. Next cover this lamp and uncover the other: move it if necessary till the other half of the field is similarly illuminated. Now view both lamps at once with the eye-piece inserted as before. When the lamps are properly arranged a curious figure will be seen in the centre of the field, caused by the intersection of two adjacent circles of light by the dark field. These circles should exactly touch each other in the centre. Very similar results will be obtained by both methods. This plan has been tried many years ago. Opticians get a very fair idea of aperture by simply looking down an objective placed close to the eye, with its nose gradually turned from a lamp. The angle described at the point of vanishing of the light gives a very good rough estimation of the angle.

I am yours faithfully,

G. W. Royston-Pigott.

P.S.—An aperture of nearly 180° can only be considered as an optical curiosity of no practical use. When, however, it is desirable to see markings developeable only by extremely oblique light, extremely high apertures doubtless show some appearances actually impossible of detection with a low aperture; as, for instance, Nobert's lines and other striations. In such cases the least aperture competent for the purpose is the most valuable, and a series of measurements of working angles for given objects is now very much wanted. I have found an Iris diaphragm (used now for about five years) placed between the objective and eye-piece at the nose of the microscope, and furnished with a lever movement, capable of giving interesting results for measurement of angle roughly.

# PROCEEDINGS OF SOCIETIES.

## Royal Microscopical Society.

King's College, *May* 5, 1875.

H. C. Sorby, Esq., F.R.S., President, in the chair.

The minutes of the preceding meeting were read and confirmed.

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

Mr. Slack said that, in addition to the list which had been read, they had received a donation from California from Mr. Hanks, who, it would be remembered, was some time ago elected a Corresponding Fellow of the Society. This gentleman was living at San Francisco, and had sent from there a valuable collection of objects, chiefly specimens of minerals, as a present to the Society. These would be carefully examined, and brought under the notice of the Fellows at an early opportunity. Mr. Hanks had also sent some diatomaceous earth in the solid state ; and this Dr. Matthews had undertaken to examine, and the results of his examination would be reported to the Society.

The thanks of the Society were voted to Mr. Hanks for his present.

The President said that at the last meeting a valuable and interesting paper by the Rev. W. H. Dallinger and Dr. Drysdale was not read on account of the shortness of time at their disposal ; but it was suggested that as it would be printed in the Journal before the present meeting, the Fellows of the Society would be enabled to read it, and that any discussion arising out of it might be taken on the present occasion. He had read the paper himself, and was very much pleased with the manner in which the experiments had been carried out. Not having studied monads, or other minute bodies of that kind, he hardly felt qualified to say more upon the subject ; but he should much like to hear the remarks of any gentlemen present who had studied them.

In the absence of any further observations on the subject,

The President said it had also been suggested that there might be some discussion upon the paper which he read at the preceding meeting ; of course, he should be very happy to answer any questions, or to afford any further information desired.

Dr. Pigott felt sure that the Fellows of the Society were very much obliged to the President for bringing the subject before them ; it was one which had been very much neglected, and which he was glad to see so ably taken up. He was himself but little acquainted with the subject ; but he should like to ask one question, and that was—what was the highest power which was found available in these researches ?

The President said that everything would depend upon the nature of the object to be examined ; as a rule, he always used the lowest power possible, especially with the binocular.

Dr. Pigott inquired what was the highest power used in the examination of blood-globules—could a $\frac{1}{4}$ inch be used ?

Mr. Slack said that much higher than that could be used; he had himself employed powers as high as $\frac{1}{20}$ inch to show the bands with a portion of a blood-corpuscle.

The President said it was simply a question of light; in making these observations it was merely necessary to make the object fill the entire field of view, so that the eye should not be distracted by the light coming in at the sides. He therefore used either a power sufficient to make the object fill the field, or an apparatus to stop out all light except that which came through the object. He should in all cases advise the use of the lowest possible power in order to obtain the largest possible amount of light, so that good definition might be obtained with the narrowest slit.

Mr. Frank Crisp inquired what advantage the new eye-piece had over the old one. With all deference to the President, it appeared to him to be more complicated instead of more simple, for where one had to push in this and pull out that, it seemed to be more troublesome than exchanging the eye-pieces.

The President said his chief object had been to obtain portability; and by reducing the number of pieces of apparatus by merely adding a prism and a slit to an ordinary eye-piece, he thought he had contrived a method by which the instrument was rendered as portable as possible. It should, however, be mentioned that the instrument with which he performed 90 per cent. of his work was the binocular form, and that he only resorted to the old form when the object was too small to be examined in any other manner.

Mr. Slack believed that none of the cloudy bands were resolvable by higher powers into a series of minute bands, and this might be taken as an indication of an exceedingly complex combination of rays.

The President said there were bands in a few substances which could be divided into two; but these were quite exceptions to the general rule, which was that of a dark band shading off towards its edges. The great interest was of course centered in the connection between these bands and physical questions.

Dr. Matthews inquired if the prism was in any way affected by the quality of the object-glass, or in the matter of over or under correction?

The President said it would not signify at all; and even supposing that the object-glass were quite chromatic, it would not affect the result. It was, however, important to have a correction at the eye-piece in order to get the whole of the spectrum in focus at the same time.

Dr. Pigott supposed it would be in some degree modified by the lenses which were placed between the eye-piece and the eye.

The President said the plan was simply to make the eye-piece a rough achromatic combination. He had never very closely examined the construction of it, as he trusted to Mr. Browning in the matter; but it was a double convex and a plano-concave in combination, and in the result it gave a beautifully flat field. The adjustment in the eye-piece was necessary, as otherwise if the focus were adjusted for red the blue end would be out of focus. By the plan which he had adopted lately, the same result was arrived at by a little plano-convex lens placed below the object.

Dr. Matthews inquired what would be the result if the eye-piece were entirely non-achromatic?

The President said he had never tried how that would be, but he was sure they would get the same ultimate results, and that the spectrum would be the same, though not all in focus at the same time.

Mr. Slack read a paper "On Angle of Aperture, and its Relations to Surface Markings and Accurate Vision."

The President, in proposing a vote of thanks to Mr. Slack for his paper, expressed his entire concurrence with the remarks it contained; he had occasion some time ago to examine specimens of rocks, and very soon came to the conclusion that a large angular aperture was far inferior to a small one, and that the latter was immensely superior in penetrating power. He fully agreed with Mr. Slack that they ought to have object-glasses made on these principles, and that they ought to encourage opticians to make them in order that persons might have what was really of the greatest use for research, instead of what was only of value for a few special objects.

Mr. Wenham said he agreed with Mr. Slack in the general purport of his observations, and he should himself always choose the glass which did the work required with the smallest angle. As a matter of fact the value of aperture was scarcely appreciable between 150° and 170°, and as regarded the extreme marginal rays, generally, he believed they did more harm than good on account of the quantity of false light which they admitted. He did not really believe that large apertures did the slightest good, and felt sure that with an angle of 90° they might do all that was required.

Mr. Stephenson said that through the kindness of Herr Zeiss he had been able to examine a series of his glasses, and could quite confirm all that Mr. Slack had said as to their remarkable working. Persons were much mistaken in supposing that a diatom was the proper object with which to test the quality of an objective, and he ventured to say that at their late soirée many people thought that *Surirella gemma* was exhibited by Mr. Slack for the purpose of showing the markings, whereas it was shown only to prove how well so extremely low an angle was capable of performing.

Mr. Frank Crisp thought it was of no use to abuse English opticians for making objectives of high angle; people would have them; and the question became one merely of demand and supply. It was really the fault of microscopists, and it was they who required to be educated on the matter.

Dr. Gray, in reply to a question from Mr. Slack, said he was chiefly struck with the great distance through which this glass would work. He thought, however, that there were a great many English opticians who made cheap educational glasses which would do the same thing as this one.

Mr. Slack thought that no one had yet produced one like it with an angle of 48°, nor did he know of any like the one with which he showed *Surirella gemma* at the soirée, of 68°.

Dr. Pigott said he had been much surprised at the price of some of these low-angled English glasses; they were, he believed, made at a distance, and he had heard that the original cost of some of them was as low as half a guinea. Those which he had seen were quite incapable of doing what was done by those of Zeiss, which were so true in centering that when tested by the light from a globule of mercury they showed the diffraction rings with great accuracy, but the cheap English glasses would not do this because they were in fault both as to centering and curving. Some years ago he got an $\frac{1}{8}$ inch, and on testing it he found that the rings showed an exact representation of Saturn and his rings seen perpendicularly to the plane of the rings, instead of which it ought to have shown the rings to be all nearly the same in fineness. He considered that the accuracy of the rings showed the value of Zeiss' glasses to be tenfold greater than that of the cheap English glasses. All opticians would agree that to make a really good low-angled glass would be easier than to make a high one; but then, as Mr. Crisp had said, "demand creates supply." He should like to mention that in 1862 he had the good luck to possess some fine glasses by Powell, and amongst them was an $\frac{1}{8}$ inch of 70°. It was at that time about forty years old, and he could do with that glass what he had never been able to do with others.

Mr. Wenham said that before they decided the matter they ought to know really what angular aperture was. He did not believe that in the published list of a single maker the angular aperture was actually correct as stated. He had found that by stopping them down their performance was improved, because otherwise a great many extraneous rays were admitted which caused a milkiness. He thought that a diaphragm stop could be adapted to these objectives with great advantage.

Mr. Slack said he had upon the table a microscope with one of Zeiss' glasses, $\frac{1}{4}$, 48° aperture, and he had just fitted to it one of Ross's E eye-pieces, and it worked well. He asked if any cheap English objective would stand any eye-piece so deep?

Mr. C. Stewart said he had recently the opportunity afforded him of comparing Zeiss' glasses with those of Hartnack, and really the great working room in the case of Zeiss' was remarkable. Besides this, there was great flatness of field, great penetration, and a large amount of light. This amount of light was of great value in looking through opaque tissues. In making a comparison with Hartnack's highest powers he should say that Zeiss' was almost, if not quite, equal in sharpness of definition. He tried them with a slide of Topping's—of teased muscular fibre of the pig—and he thought that Zeiss' seemed to show over Hartnack's a rather flatter field, more penetration, and there was more working room; but there might have been a trifle more sharpness of definition in favour of Hartnack's.

Mr. Slack said he had felt a good deal of difficulty in bringing this subject before the Society because of having to mention names. So far as he knew, no first-class work had been lately employed upon any low-angled glass in this country, unless in the case of one specially ordered. Dr. Gray appeared to be under the impression that the cheap low-angled English glasses were good; but he had seen many, and could only describe them as being moderately bad, and as far as standing any deep eye-piece was concerned they simply broke down altogether. A small angle, with the help of Mr. Wenham's reflex illuminator, had a great increase of resolving power.

The President thought the general opinion expressed was decidedly against making glasses as toys. Opticians were guided by demand; but then Mr. Richard Beck used to say that their best customers were the people who did nothing at all. He hoped that the discussion of the subject would tend to lead makers to produce really good objectives with a happy medium of angle. What they wanted was a glass really suited for the purposes of research.

Donations to the Library since April 7, 1875:

| | From |
|---|---|
| Nature. Weekly | *The Editor.* |
| Athenæum. Weekly | *Ditto.* |
| Society of Arts Journal. Weekly | *Society.* |
| Journal of the Linnean Society. No. 79 | *Ditto.* |
| The Canadian Journal | *Institution.* |
| La Teoria della Riproduzione delle Diatomee. By Conte Francesco Castracane | *Author.* |
| Le Diatomee nella eta del Carbone. Memoria del Sig. Conte Francesco Castracane | *Ditto.* |
| Le Diatomee in relazione a la Geologia. Memoria del Sig. Conte Francesco Castracane | *Ditto.* |
| A box of minerals, and some diatomaceous earth in the solid state | *Mr. Hanks.* |

H. W. Jones, Esq., was elected a Fellow of the Society.

WALTER W. REEVES,
*Assist.-Secretary.*

## *Scientific Evening, April* 21, 1875.

On this occasion the libraries of King's College were placed at the service of the Society by the kindness of the authorities, and the attendance was very good in spite of violent rains. Excellent lamps were lent by Messrs. Baker, How, and Parker, and the display of objects equal to any former occasion, as the subjoined list will show.

### *Exhibitors and Objects.*

The President, H. C. Sorby, Esq.: Some new apparatus for the study of the spectra, and showing its application.

Messrs. Beck: Podura scale under an immersion $\frac{1}{8}$th, with a peculiar illumination, showing corrugations and fine black lines from end to end of the scale. All sign of the note of exclamation markings had disappeared.

Mr. W. A. Bevington: The fructification of *Trichomanes radicans.*

Mr. Thomas Bolton: A new species of Ophrydium (*O. pediculum*).

Dr. Braithwaite: Section of leaf of *Rhopala Pohlii*, showing lignified trabecular cells between the upper and lower surface; also cuticle with immersed stomata.

Mr. John Browning: The specula of nitrate of didymium and thallium.

Mr. Frank Crisp: Ahren's binocular microscope, with prisms of Iceland spar.

Mr. F. Fitch: Tongue of Norway ant (under side).

Mr. Fitzgerald: Selenite flowers under a new form of polariscope.

Mr. Henry Hails: Internal casts of Foraminifera, Cliona borings, &c.

Messrs. How: Recent Foraminifera, the chambers becoming filled with glauconite from Australia.

Mr. Thomas Howse: Section of caryopsis of wheat; section of pistil of vegetable marrow, showing pollen tubes, &c.

Mr. J. F. Gibson: The Colorado potato-beetle, *Doryphora decem lineata.*

Mr. J. W. Goodinge: Acari of bat.

Mr. John E. Ingpen: *Clava squamata*, from the Mediterranean.

Mr. William T. Loy: A series of dissections, showing the entire anatomy of the large and small water-scorpion, *Ranatra linearis* and *Nepa cineria.*

Mr. S. J. McIntire: Poduræ alive, *Templetonia nitida*, and some of the smaller species.

Mr. W. W. Reeves: Young spongilla alive, sent from Chester by Mr. Thomas Shepheard.

Messrs. Ross: Scale of *Lepisma saccharina*, mounted between prisms of 35°, showing radiating lines by oblique vision with a $\frac{1}{10}$th object-glass.

Mr. Sigsworth: Stellate crystals from office gum.

Mr. J. W. Stephenson: Some new forms of Polycistina.

Mr. Charles Stewart: Section of venereal wart and dilated lymphatics of the skin.

Mr. H. J. Slack: *Surirella gemma* displayed in beads with $\frac{1}{6}$th objective by Zeiss, angle of aperture 68°, and Wenham's Reflex Illuminator, to show what resolving power could be obtained with a small angle.

Mr. Swift: Podura scale shown with his low-angle $\frac{1}{6}$th objective, and a new portable microscope lamp.

Mr. Topping: Injected transverse section of hoof of horse, and sections of skin of horse from various parts of the leg.

Mr. F. H. Ward: New British species of *Lepisma* (*Lep. inguilinus*).

Mr. John Young: Sections of fossil plants from the Lancashire coal measures.

Mr. Tuffen West: Some nice drawings of microscopic objects.

Mr. Parkes: His new patent reading and microscope lamp.

## Medical Microscopical Society.

Friday, March 19, 1875.—Dr. J. F. Payne, President, in the chair.

*Spinal Cord in Infantile Convulsions.*—Dr. Sydney Coupland exhibited specimens from the case of a child, aged six months, dying in convulsions, secondary to cancrum oris. He described great dilatation of the capillaries and small vessels of the cord, especially in the commissural part of the grey cornua, and enlarged perivascular spaces, the maximum of enlargement being in lower part of medulla, particularly near the central canal, but the canal itself and its epithelial lining seemed perfectly healthy. He considered the perivascular dilatations to be most probably secondary to the convulsions, and not their cause, but due to over-distension of the vessels during hyperæmia of the cord; and drew attention to the similarity of the appearances now seen to those described by Dr. Dickenson in cases of diabetes. Mr. Needham had observed the same changes in the brain in cases of hydrophobia, and in cases of heart disease where there had been much congestion. The President thought the changes described not uncommon; they were mentioned long ago by a French writer as owing to wasting of the brain in old age, and lately by Dr. Lockhart Clarke in paralysis of the insane. These appearances may not be pathognomic; and Dr. Dickenson could not find them in the parts of the brain which when injured cause a diabetic condition; they are found too in seemingly healthy brains. His first-described perivascular spaces are by some considered lymphatic; but may they not be the tunica adventitia of the artery separated, and under diseased conditions filled with blood or inflammatory material? In the case given it was not likely they were owing to simple mechanical distension of the vessel, for, if so, why should the vessel not remain distended? it would scarcely again collapse. They were more easily explained by wasting of the brain substance, and consequent filling of the perivascular spaces—whether tunica adventitia or not—with serum, since a vacuum cannot exist, and this wasting a severe illness or bad feeding might bring about.

Dr. Greenfield had always observed a catarrhal condition of the central canal of the cord in cases of convulsions, and what the President described as tunica adventitia he thought an additional structure to help support the vessel.

Dr. Coupland in reply stated he had in preparing the spinal cord prepared it first for twenty-four hours in spirit and water, and then in weak chromic acid. He did not know if the child were badly nourished, nor how long it had been ill.

*Urate of Soda in the Heart.*—Mr. Ward exhibited a specimen where in a gouty subject the coronary artery was found plugged, the plug containing acicular crystals of urate of soda. Death was sudden, from rupture of left ventricle; the heart substance was friable.

The President had seen sudden death in cases of plugging of the coronary artery. Dr. Greenfield stated that fatty change in the heart was said to follow plugging of the coronary arteries: he had not observed it uniformly. He quoted Dr. Quain's cases of plugged coronary artery; in all death was from ruptured heart.

Mr. Golding Bird asked if any chemical test had been applied to the crystals. He had once succeeded in obtaining under the microscope, by the addition of weak acid, crystals of uric acid from the acicular crystals of urate of soda contained in a section of "gouty" cartilage.

Mr. Ward, in reply, had not applied any chemical test; the heart substance was not fatty, and he did not know which coronary artery was plugged. The crystals did not polarize.

*Blood-crystals of Rat*, exhibited by Dr. Pritchard, were obtained after killing the animal with æther; a drop of the blood with water is placed on a glass slide and covered at once; on coagulation the crystals are seen in the spaces between the fibrin. If cemented thus they keep a long time, being "in vacuo."

*Myxoma.*—A specimen exhibited by the President, in which the connecting filaments were apparently tubular, and not merely, as described, flat processes folded to form a "gutter." The preparation was in glycerine, but the same appearance was seen when fresh.

## South London Microscopical and Natural History Club.

At a meeting of this club, held on Tuesday, February 16, at the Angell Town Institution, Brixton, a lecture was delivered by Dr. Huggins on "Spectrum Analysis and its Astronomical Results." A large attendance of members and their friends testified to the interest which the subject excited, and the lecture, which was illustrated by many interesting experiments, was listened to with the greatest attention by those present.

The annual meeting of the club was held at the above Hall on Tuesday, March 16. Dr. Robert Braithwaite occupied the chair. Two new members were elected, and thirteen proposed for election at the next meeting. The following officers, proposed for the ensuing year, were unanimously elected: As President, Charles Stewart, M.R.C.S., F.L.S.; as Vice-Presidents, Hector Helsham, M.D., F.R.C.S.; W. T. Suffolk, F.R.M.S.; and Robert Braithwaite, M.D., M.R.C.S.E., F.L.S., F.R.M.S.; as Treasurer, Henry Robinson; as Committee, B. D. Jackson, F.L.S., F.R.M.S.; B. Neighbour; T. Rogers, F.L.S., F.R.M.S.; N. Stowers, M.R.C.S., L.S.A.; J. W. Stephenson, F.R.A.S., F.R.M.S.; C. W. Stidstone; W. J. Parks; J. F. Wight, and E. Dadswell; as Honorary Secretary, Frederick Hovenden; and as Honorary Reporter, Thomas G. Ackland.

The Treasurer's report for the year just closed showed a balance in favour of the Society of 77*l.* 19*s.* 2*d.*

The report of the Committee was then read by Mr. B. D. Jackson. From this it appeared that 38 members had been elected into the club during the past year, the total number of members now being 245. The death of Mr. Henry Deane, the first President of the club, and one of its founders, was alluded to, and a tribute paid to his scientific knowledge, and the amiability of disposition which placed it at the service of the youngest student. It was reported that a collection of Lepidoptera had been acquired by the club; and the report concluded by an appeal to the members to help still more, by practical work and original investigation, towards the attainment of the objects of the club.

The President (Dr. Braithwaite) then gave his annual address, in which he passed in review the advance of microscopical science during the past year, noticing in some detail a paper read by Dr. Hooker on "Carnivorous Plants," and also the researches of Sir John Lubbock, Dr. Darwin, and others, into the question of the fertilization of plants. He concluded by some interesting remarks upon the deep-sea soundings carried on by the 'Challenger,' under the direction of Dr. Carpenter.

Votes of thanks having been accorded to the officers of the club for their services during the past year, the meeting separated.

On Tuesday evening, April 6, the annual soirée of the club was held in the tropical department of the Crystal Palace. About 1500 were present on this occasion; and the various courts of the Palace were occupied by a brilliant company, who divided their attention between the interesting objects exhibited under the microscopes, and the larger and livelier, but equally interesting, objects shown in the aquarium, while sweet music was discoursed to the visitors by a contingent from the Crystal Palace band. The exhibitors were 270 in number, and included representatives of the Royal Microscopical Society, Medical Microscopical Society, Old Change Microscopical Society, and the Tower Hill, Quekett, Croydon, Greenwich Amateur, Sydenham and Forest Hill, Blackheath, West Kent, and New Cross Microscopical Clubs, in addition to those objects exhibited by members of the South London Microscopical Club. The microscopes were judiciously arranged in different parts of the Palace, the representatives of each club being, as far as was possible, placed in the same group. The reading room was a centre of considerable attraction, the microscopes in this room being principally devoted to objects illustrative of pond-life, among which we noticed the familiar Rotifers, Melicerta, Conochilus, Stephanoceros, and the like; *Volvox globator* was admirably exhibited under several of the microscopes; and Hydras were also well shown. The circulation in plants was shown, the objects chosen being Nitella, Anacharis, and Vallisneria. The Colorado potato-beetle and *Trichina spiralis* excited interest from the public reputation which these creatures have obtained. Circulation was shown in the blood of a newt, tadpole, carp, gold fish, and frog. The arrangements of butterfly scales and diatoms in the form of flowers were much admired; and groups of natural flowers received much attention. Foraminiferous shells from China, Australia, and the Mediterranean, were exhibited under different microscopes, as also was Atlantic mud from a depth of more than 2000 fathoms. Of miscellaneous objects we can only mention the Lord's Prayer, written on the twenty-thousandth part of an inch, and an exhibition of the metal thallium under the electric spark. To pass from "food for the mind" to "food for the body," refreshments were provided by the Crystal Palace contractors; and the counters loaded with refreshments testified that this department was ably conducted. The soirée would appear to have been in all respects a perfect success, and the visitors did not separate until a late hour.

An ordinary meeting of the club was held on Tuesday, April 20, Mr. Charles Stewart, M.R.C.S., F.L.S., in the chair. Twelve members were balloted for and duly elected, and the certificates of six members, proposed for election, were read. The President then made an interesting address on Respiration, which subject was illustrated by the objects exhibited by the members. A paper having been announced for the next meeting on "*Nonvascular cryptogams*," the meeting separated.

## South Australian Microscope Club.

Although the number of microscopists in a limited population like that of South Australia is necessarily small, it is gratifying to know that a Microscope Club has been recently formed in Adelaide, under favourable auspices. The idea of forming it arose from a successful exhibition of microscopes and objects by members and friends of the Philosophical Society. It was found that some fine instruments and objectives were in the hands of private gentlemen resident in the colony, and it was suggested that it would be useful to form a club for the purpose of mutual improvement. About eighteen gentlemen united to form the club, and during the last year monthly meetings have been regularly held at the offices of one of the founders. The plan of proceeding has been simple. The first half hour of each meeting has been spent in examining new or interesting objects and any novel piece of apparatus received by any member since the last meeting. The remaining portion of the evening has been spent in the study of some special subject notified by the chairman appointed at the previous meeting. Mr. Smeaton has acted as secretary and convener of the meetings, and during the year the following subjects have been studied, viz. insect preparations, mounting in balsam and resinous media, polarization of light, spectrum analysis, dental tissues, starches, preparation of diatoms, and photography as applied to the microscope. Numerous objects have been exhibited, those collected in South Australia having attracted special attention. The stands brought under the notice of the club have been Beck's large binocular, Beck's educational, Collins's Harley binocular, a medium-sized Powell and Lealand, Hartnack's model, and one or two others of smaller construction. The objectives examined by the members have been a complete series of Beck's dry objectives, up to $\frac{1}{20}$th inch; Collins's objectives; a $\frac{1}{10}$th inch by Ross, working wet or dry without change of front; a $\frac{1}{16}$th inch Powell and Lealand, with immersion arrangement; and some very good objectives made in France.

It will be seen that the club has found ample material for work; and although it cannot hope to rival the great societies at home, it has been proposed to imitate them by giving a public exhibition during the year, whereby it is expected that a large accession of members will be attracted to the club.

## Memphis Microscopical Society.

The Society met at the usual hour and place, with a large attendance of members, attesting an increasing interest in its objects and proceedings.

Dr. W. A. Edmonds was unanimously elected to active membership, and Dr. J. V. Herriott, of Valparaiso, Indiana, as a corresponding member.

A donation was received from Dr. Chris. Johnston, of Baltimore, consisting of finely preserved specimens of deutzia leaf, cuticle of ladies' slipper, section of lignite, fossil diatoms, &c. Also from Henry

Mills, Esq., of Buffalo, three specimens of filterings from Niagara River water, accompanied by a brief but interesting account of what they contained. For all which a hearty vote of thanks was passed.

The President, Dr. Cutler, read an account of the microscopical examination of a pathological specimen sent the Society from a neighbouring city. The case seems in many respects a remarkable one, baffling the best medical skill. And the microscope, although demonstrating that the disease was *not* cancer, as was supposed, fails to discover its exact nature. A paper was also read from J. E. Smith, Esq., of Ashtabula, Ohio, giving the measurement of all the numbers on the Möller Probe-Platte, as made by Professor Morley, of Hudson, Ohio, and compared with his own observations. Letters of acknowledgment were also read from T. W. Starr, Esq., and Henry Mills, Esq.

The President then read his paper on the microscopic examination of water from a pond, "Happy Hollow," of yellow fever notoriety. In this water he found a number of curious and interesting species of infusorial life, and several new and hitherto undescribed forms. This water proved so rich in life that a description of all it contains could not be condensed within the limits of one paper, and a portion is reserved for a future meeting.

A vote of thanks was then tendered the President, after which the Society adjourned until the next regular meeting, February 4.

## San Francisco Microscopical Society.

The annual meeting of the San Francisco Microscopical Society has been held some time since, but the report has only now reached us, and it seems it was well attended by resident members, and the reports of its various officers show a thriving and energetic state of affairs.

Mr. Hanks, who has been President of the Society since its organization, read a very full and complete paper, giving a brief history of the Society, showing what had been done by the members during the past year, and which contained many valuable suggestions, extracts from which we give a place below.

The report of the Treasurer, Mr. Ewing, was very satisfactory indeed; and the assets of the organization, in the way of instruments, library, furniture, objects, &c., with the cash on hand, show what a few determined ones can do when they are in earnest.

After the reading of reports, the election of officers for the ensuing year took place, resulting as follows: President, Wm. Ashburner; Vice-President, H. C. Hyde; Recording-Secretary, C. Mason Kinnie; Corresponding-Secretary, Charles W. Banks; Treasurer, Charles G. Ewing.

Louis Rene Tulasne, of Paris, was elected a corresponding member of the Society.

Mr. J. P. Moore donated a pamphlet entitled 'New Mexico,' which was valued as containing a list of hot springs in that country, and from which he hoped to present, from time to time, samples of animal and vegetable life. The Secretary announced additions to the library in the way of six volumes of the 'Monthly Microscopical Journal and Transactions of the Royal Microscopical Society.'

Dr. Harkness donated a sample of the *Palmella cruenta* (gory dew), stating where it could be found in this city at the present time.

Mr. J. P. Moore donated a bottle of caoutchouc, stating he had found it useful for making cells and fixing covers; a sample of wild cotton, from Barbacoas; a sample of Liber used by the natives of the above locality for blankets, and two slides mounted by him with fibre of the same.

Mr. Scupham presented samples of rock composed of tertiary shells, from near Folsom, California; rock composed of fresh-water shells and protozoans, Cache Valley, Utah: silicified oakwood, Roseville Junction; Arenaceous slate, showing crystals of peroxide of manganese, Green River, Wyoming; and specimens of the *Tillansia usneoides* with seed pods, from Galveston, Texas.

*President Hanks' Report.*

San Francisco, Feb. 4, 1875.—To-night ends the third year since the organization of the San Francisco Microscopical Society. It is with pleasure that I announce to you that it has been a year of great prosperity. None of the members have died, none have been seriously sick, an increased interest has been manifested in microscopical science, not only by the growth of our Society and by the deep interest of our fortnightly meetings, but generally throughout the state. A desire has been shown to assist the earnest workers of this Society by sending objects for examination, and by calling attention to many strange and beautiful things that would otherwise have been lost.

Not only has the Society increased its apparatus, but many members have furnished themselves with first-class instruments, with which they pursue the fascinating science at their homes, bringing the result of their labours to the meetings, there to exchange ideas and to comment upon the result of their investigations.

It has been stated by persons of great experience that few cities in the Union were so well provided with good instruments as San Francisco. This is owing directly or indirectly to the influence of our Society.

Although I have said that we have greatly advanced in the study of microscopy, yet, in effect, we have only just begun. If we have much, very much to learn yet, we may feel that we have laid a good foundation upon which to erect the superstructure. We are particularly fortunate in one respect—we are in a new and undeveloped country. Unlike Europe, every inch of which (figuratively speaking) has been placed in the field of the microscope, we have vast unexplored regions within our grasp, and the scientific world is looking to us for results.

It has been the custom of our Society to give entertainments to our friends from time to time. During the year three of these have been given. One large reception was held on May 4, at Mercantile

Library Hall, and was well attended. Two of less magnitude were given at our rooms. These meetings not only give pleasure to our guests, but foster a taste for microscopical study, by giving our friends an opportunity to see what they could only otherwise guess at or remain in ignorance of. There are many people in San Francisco who have no idea of the power of the modern microscope; to them our meetings must be instructive as well as entertaining.

Our future receptions must be more instructive than those past, from the fact that our members are more generally provided with good instruments and objects, and have learned to display them in a manner more satisfactory. I believe we can in no way do so much for the cause of science, as by the continuance of these exhibitions.

Finding our rooms too small for our growing Society, the question arose whether we should remove to another location, or enlarge the rooms we already occupy. The latter course was decided upon, and the result has been our present cosy quarters.

In closing this report, I wish to call the attention of the Society to the importance of publishing our proceedings. By doing so we can by exchange obtain those of other societies, and thus learn what is being done elsewhere. If our first publication should not be all we could desire, we have another year before us, in which we may hope to improve.

The regular meeting of the San Francisco Microscopical Society was held on Thursday evening last, with a good attendance of members. President Ashburner in the chair. Messrs. A. W. Jackson, of the University of California; H. Scamman, of Downieville; H. Mollineux, Theodore H. Hittell, Charles Troyer and Dr. J. M. Willey of this city, present as members.

Dr. Gustav Eisen, Professor of Zoology, University of Upsale, Sweden, was elected a corresponding member.

The Secretary announced the receipt of six additional volumes of the 'Monthly Microscopical Journal,' completing the series, and the February number of the 'American Naturalist.'

Mr. H. G. Hanks donated a copy of the 'Cincinnati Medical News' containing notices of meetings of microscopical societies.—*Cincinnati Medical News*, April.

## Biological and Microscopical Section of the Academy of Natural Sciences of Philadelphia.

Dr. J. Gibbons Hunt made a very interesting verbal communication upon the subject of *amplifiers for the microscope*, in the course of which he remarked that from the time of the first observation by the aid of more than two convex lenses, an almost constant effort had been made by opticians to fit in the best intermediate glasses, and yet further improvement in this respect was confidently to be looked for. The amplifier which he had upon the table consisted of a concavo-convex lens, with its concave side turned towards the eye, and so placed within the body of the microscope as to stand at a considerable distance from the objective. This adjustment of position was best accomplished by having the amplifier screwed to the end of a tube arranged with rack-work in such a manner as to traverse six or eight inches, because we could thus compensate for a want of complete correction in the objectives employed.

The advantages obtained by using an amplifier were, in the first place, gain in magnifying power, as could be seen in his microscope upon the table, when, with an amplification of only 800 diameters, afforded by a four-tenth of an inch objective, he had on exhibition the *Navicula angulatum* resolved into dots all over the field, which was apparently more than sixteen inches across. By the aid of an amplifier we also gain a greater focal distance, and an increase of flatness of field.

Amplifiers have been employed in telescopes for the past fifty years, but ten or twelve years ago they were only adapted to microscopes, in this city at least, by one or two amateurs. Subsequently, Mr. Tolles, of Boston, saw them in use here, and on his return home made one, apparently with gratifying success, as he has since kept them in stock. Some few years since, Mr. Dickinson, of New York, wrote a paper upon amplifiers, claiming that by their aid he could obtain a power of 100,000 diameters; but objects thus magnified are visible only as dim shadows, similar to those shown by the solar microscope, quite unfit for data in scientific work. Such amplification, however, may be employed upon diatoms, the resolution of which does not require definition.

Dr. J. G. Richardson inquired of Dr. Hunt whether, in his opinion, the $\frac{4}{10}$th objective associated with his amplifier, as he had it upon the table, and eye-pieced so as to give a power of 800 diameters, was equal to his Powell and Lealand's $\frac{1}{16}$th immersion lens, combined with the A eye-piece.

Dr. Hunt replied that on histological work the results were not quite so good, but on *Pleurosigma angulatum* he considered them fully equal. The combination of amplifier and objective which he used was, however, a merely accidental one, so that a skilful optician would probably be able to arrange the lenses more efficiently, and thereby enable microscopists to obtain this greater amplification at a much lower cost, and yet with definition good enough for scientific work. Dr. Pigott's aplanatic searcher appeared to be a modification of the amplifier, but had proved so unsatisfactory in his hands that he had entirely laid it aside.

Dr. Hunt also exhibited a beautiful specimen of the *Protococcus nivalis*, or red snow, which he believed had been discovered for the first time within the United States, by Mr. Harkness, of California, who found it growing upon the Sierra Nevada mountains. For a long time it was a matter of dispute whether this organism belonged to the animal or the vegetable kingdom; but from observations made upon specimens brought from the polar regions by Captain Parry in 1815, and which grew in bottles of snow, its vegetable nature had been demonstrated. In the growing stage this plant is of a green colour, and it is only the resting spores which present the brilliant red hue from which it derives its name. Dr. Hunt stated that on examining portions of the *Protococcus nivalis* under the micro-spectroscope he had found that its colouring matter entirely blotted out the violet end of the spectrum, leaving the red, yellow, and orange untouched.

Dr. J. H. McQuillen showed a specimen of muscular fibre from the sheep, which, after the simple method of preparation of allowing it to remain between two of his own teeth for five hours, he had placed in glycerine and teased out with mounted needles, thus obtaining a magnificent view of the ultimate fibrillæ of the muscle.

Dr. J. G. Richardson exhibited a fine specimen of a vertical section from the mucous membrane of the tongue of a calf, mounted in balsam, which at his urgent request had been loaned to him from the Army Medical Museum. He desired to call the attention of members to the fact that each individual epithelial cell, throughout almost the whole thickness of the membrane, displayed its outline and nucleus with perfect distinctness, and that therefore the statement made when balsam preparations were last under discussion, that they showed hardly anything, was inaccurate.

Dr. J. G. Hunt exhibited a similar specimen of his own mounted in glycerine, and remarked that when thus prepared the epithelial cells were displayed, not shrunken, *but of their full size*, and that those important elements, the connective-tissue fibres, were clearly visible, instead of being lost to view as in the balsam preparation.

Dr. Richardson observed that even if the *fresh* glycerine preparations exhibited these delicate fibres more plainly, yet the specimen preserved in balsam displayed the muscular-fibre cells with far greater distinctness, and the absolute permanence of objects mounted by the balsam method constituted one of its most important recommendations.

Dr. H. C. Wood, jun., stated that the glycerine preparation appeared to be superior to that mounted in balsam, and moved that in order to settle this question, about which there had been so much dispute, these specimens should be referred to a committee composed of Drs. J. H. McQuillen and James Tyson, for examination and report.

Dr. J. G. Hunt exhibited an exquisite specimen of the liver of a common fly, showing with remarkable clearness the arrangement of the hepatic cells and ducts, and stated that he proposed mounting a series of preparations displaying the structure of the liver from its simplest form in the Articulata up to its most complex arrangement in the human organism.

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### Erratum.

In the description of the woodcut Fig. 5, at p. 207 of this volume, the words "Lobelia" and "Cineraria" have been transposed and placed opposite the wrong spectra.

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END OF VOLUME XIII.

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