ART. II.

1. *Principles of Scientific Botany; or Botany as an Inductive Science.* By Dr. J. M. SCHLEIDEN, Extraordinary Professor of Botany in the University of Jena. Translated by EDWIN LANKESTER, M.D., F.R.S., F.L.S., &c., Lecturer on Materia Medica and Botany at the St. George's School of Medicine.—London, 1849. 8vo, pp. 616. With five Plates and numerous Wood-engravings.
 2. *A Manual of Botany; being an Introduction to the Study of the Structure, Physiology, and Classification of Plants.* By JOHN HUTTON BALFOUR, M.D., F.L.S., F.R.S.E., Professor of Medicine and Botany in the University of Edinburgh.—London, 1849. Small 8vo, pp. 642. Illustrated by numerous Woodcuts.
 3. *Elements of Botany.* By M. ADRIEN DE JUSSIEU, Membre de l'Institut, Professeur au Muséum d'Histoire Naturelle, &c. Translated by JAMES HEWETSON WILSON, F.L.S., &c.—London, 1849. Fcap 8vo, pp. 750. Illustrated by numerous Woodcuts.
 4. *Rudiments of Botany: a Familiar Introduction to the Study of Plants.* By ARTHUR HENFREY, F.L.S., Lecturer on Botany at St. George's Hospital, &c.—London, 1849. 18mo, pp. 250. With illustrative Woodcuts.
 5. *Zur Entwickelungs-geschichte der Farrnkräuter, vom Grafen LESZCZYC-SUMINSKI.*
- On the History of the Development of Ferns.* By Count LESZCZYC-SUMINSKI.—Berlin, 1848. 4to, pp. 26. With Six coloured Lithographic Plates.

EVERY physiologist is acquainted with the name and reputation of Professor Schleiden, who first distinguished himself about twelve years ago by those researches on the origin and development of vegetable cells, which, if they did not originate the researches of Schwann on the development of the animal tissues, furnished him with important aid and guidance;* and who about the same time promulgated certain novel doctrines regarding the process of fecundation in flowering-plants, which have ever since remained the subjects of active controversy. His reputation as a botanist, however, by no means rests on these works alone; for he has published several other excellent monographs, especially one on the Cactæ, which have contributed to raise and extend it; and the treatise now placed before us in an English dress, is already in its third German edition, which sufficiently indicates the appreciation it has acquired in the country which gave it birth. It differs in its general character and arrangement from all preceding treatises on Botany; and presents the anatomical and physiological departments of the subject in a far more scientific aspect than they have ever before been made to exhibit. It is full of original observations of great value; and has this remarkable merit, that the author has taken very little upon trust, but has examined into almost every point for himself. We need not say, then, that its reproduction in our own language, under the able editorship of Dr. Lankester, (who has had the advantage of Mr. Henfrey's assistance,) is a great boon to the British botanist, who has too often laid himself open to the reproach of ignorance with regard to the progress of his science elsewhere, and espe-

* See Brit. and For. Med. Rev., Vol. IX, p. 497.

cially in Germany. Moreover, the appearance of this work, concurrently with two other botanical treatises of which Vegetable Anatomy and Physiology form a large part, affords a cheering indication that the younger botanists of this country are not any longer content with being mere collectors of species, accumulators of what Professor Schleiden sarcastically calls so much *hay*; but that they are setting themselves to acquire a knowledge of the structure and development of plants, and are likely to take their fair share in the labours whereby that knowledge may be extended and rendered precise. We cannot refrain from quoting the following passage, which amusingly and at the same time forcibly displays the errors of the mode in which too many botanical researches have been carried on. When speaking of the small amount of our knowledge regarding the development of the *Algæ*, Professor Schleiden remarks:

“In some degree, certainly, the cause lies in the artificial, senseless methods of research that infect the whole science of Botany, which has hitherto directed its attention far more to herbaria than to living plants; and hence, those who live far inland become Algologists, whilst botanists residing near the seaside busy themselves in describing some little dried *Jungermannia* brought from Java; and European investigators thus too often devote more attention to tropical plants than to those that are indigenous to their own country, and are in their own neighbourhood.” (p. 149.)

It is to be borne in mind, however, that the general term Botany includes two subjects, really as distinct from each other as are Zoology and the Anatomy and Physiology of animals; and although neither in the animal nor the vegetable kingdom can these two departments be advantageously studied in complete isolation from each other, yet it is absolutely necessary for their advancement that there should be a division of labour; and we would not therefore ridicule or undervalue the labours of any, who, having first made themselves acquainted with the general outlines of the whole, choose any single part of the domain as the portion which they will especially cultivate. But that which we do, with Professor Schleiden, deem worthy of condemnation, is the spirit of the mere collector of dried plants, who estimates his botanical acquirements by the number of species of which his cabinet can boast, and is all the while in a state of profound ignorance of everything, save the external characters of his specimens. To such we imagine that Professor Schleiden's exposition of what really constitutes botanical science, would be just as intelligible as a treatise on the differential calculus or the lunar theory. To those, however, who have already studied Botany in the same spirit, or who are prepared to do so by their previous study of Animal development (to which science Professor Schleiden makes continual reference as being far in advance of botany, both as to its method and as to its attained results), this treatise will supply ample materials for the improvement of their knowledge, both by the amount of facts it contains, the excellence of the plan on which these are combined and made to evolve principles, and the suggestions to which almost every part gives rise in the mind of the reader who is competent to appreciate the work. The following brief summary of its contents will serve to indicate the method pursued by the author.

The work is divided into four books; of which the *first* is devoted to an exposition of the chemistry of plants, under the heads of inorganic and organic elements. The *second* treats of the “plant cell” in two chapters; one describing its various modifications of form, the other its vital actions,

both as an individual, and in connexion with others. The third book is entitled "Morphology," and embraces a general view of the form and structure of each principal tribe of the vegetable kingdom, commencing with the simplest Algæ, and proceeding upwards to the highest Cryptogamia; this exposition being conducted on the basis of the *history of development*, which is what constitutes its peculiar feature. The fourth book, entitled "Organology," is what we should have rather termed physiology, as it is intended to embrace an account of the "general phenomena in the life of the entire plant," and of the "special phenomena in the life of the entire plant," as well as of the functions of special organs. In the appendix we find a section "on the use of the microscope in botanical investigations," which formed a part of the introduction of the original; another part of the introduction, which consisted of an abstract discussion on methology, founded upon the study of the powers of the percipient mind in their relation to the objective phenomena of nature, has been altogether omitted, not, we think, at all to the detriment of the work as a scientific treatise on botany.

The task of translation has been, on the whole, extremely well performed by Dr. Lankester, especially considering that Professor Schleiden's style is none of the most facile. An apology is offered by Dr. Lankester, in his preface, for the inelegance that may be charged against some passages; which has resulted, he tells us, from his "anxiety rather to give a correct notion of the author's meaning, than, by the use of other language, to diminish the force of the original." We have met with scarcely anything that can be termed obscure; and, on the whole, there is little in the diction to indicate the foreign origin of the book. We are disagreeably reminded of this, however, by continually meeting with measurements, &c., expressed according to foreign standards, which we should have been much better pleased to find "done into English." In fact, we cannot regard any translation as at all complete, in which this has not been accomplished; and if the editor or translator would only reflect that by the neglect of this part of his duty he inflicts the labour which he has shirked on *every-one* of his readers who may desire to make himself acquainted with the author's statements on such points, he would not, we think, shrink from this complete fulfilment of the duty which he has undertaken. We have to note, too, that the references between one paragraph and another are frequently incorrect, altogether failing to guide us to the intended information. The index, however, is an excellent one, and we have found it both copious and accurate.

Having said thus much in regard to the general plan of the work, and of that portion of its execution which devolves upon the translator, we might be expected to proceed with a detailed examination of the author's views. Such an examination, however, would be altogether foreign to the general plan and scope of our Journal; and we must content ourselves with taking up a few subjects which are likely to have the greatest interest for our readers. But before we bring to a close this general sketch of Professor Schleiden's treatise, we feel called upon to animadvert most seriously upon the tone and spirit which are displayed in several parts of it, as being totally unworthy of a man who occupies a high scientific position, and who professes to make truth his sole aim. It would seem as if the author could never express his dissent from the opinions of a brother botanist, without giving vent to a sarcasm or an expression of

ridicule, sometimes directed against him personally, sometimes against the method of research he has employed, sometimes against the school of philosophy to which he belongs. There is scarcely an eminent contemporary botanist to whom he does not thus apply the lash in one part of his work or other; in fact, we believe that the only exception is our own Robert Brown, whose name, so far as we have observed, is never quoted but with respect. Now there is an old proverb that "those who live in glass houses should not throw stones;" and Professor Schleiden would do well to profit by it. For his love of originality and his disposition to dissent from usually-received doctrines, often (it would appear) for the mere sake of dissenting, have led him into no small number of rash statements and assumptions, which fairly expose him,—more, perhaps, than any other botanist of the day,—to the attacks of rivals and critics. In fact it is not a little remarkable, that the inferences he drew from those researches on cell-development and on the process of fertilization, which first made him known to the scientific public, and on which his *general* reputation is still chiefly founded, should be now in great part disproved. And yet it cannot be questioned that those researches have been the indirect means of educating what we now believe to be truth, by stimulating the zeal and sharpening the acumen of other observers; and that in constantly dwelling, as he has done, upon the history of development, as our chief guide in the classification and interpretation of phenomena, Professor Schleiden has given a much truer *direction* to botanical inquiry than it had previously taken, and has thus laid the foundation of an edifice on which others may be proud to labour.

The second work on our list is far less ambitious in its design; the author's object being, not to build up any system of his own, but "to give a comprehensive and, at the same time, condensed view of all departments of the science." The following extract from the preface will sufficiently indicate the scope of the work:

"Attention is directed, first, to the elementary structure of plants, and the functions of the simplest tissues, and then to the compound organs, and the functions which they perform. In the consideration of these subjects, the works of Jussieu and Henfrey have served as a model. The application of physiology to agriculture, both as regards the cultivation of plants and their diseases, is brought under notice, the works of Liebig, Mulder, and Johnston having been consulted. In the important subject of classification, much aid has been derived from the standard work of Lindley. The system adopted is that of De Candolle, but in the arrangement and definition of the natural orders, Walker Arnott has been chiefly followed. Many important hints have been derived from Henslow's excellent Syllabus, as well as from the systematic work of Endlicher. In detailing the properties of plants, care has been taken to notice all those which are important in a medical and economical point of view; Christison, Royle, Burnett, and Lindley, supplying valuable data. In the chapter on the geographical distribution of plants, a very general view is given of the principal facts brought forward by Meyer, Schouw, Humboldt, Berghaus, Watson and Forbes; and in fossil botany, the labours of Brongniart, Ansted, and Hooker, have been made available. . . . By combining together in the Manual, information which the student has to acquire by the consultation of several volumes, it is hoped that the work may be useful as a text-book." (pp. ix, x.)

The illustrations are, for the most part, the same with those which were executed for M. Adr. de Jussieu's treatise; but others have been added from various sources. We have great pleasure in giving to this treatise our warm commendation, as being on the whole admirably adapted to its designed

purpose, presenting the different departments of Botany in their due relation and subordination to each other, and in a manner calculated to interest the student, and to lead him on from the simpler and more elementary, to the higher and more difficult subjects. In particular, considering how deficient was the botanical teaching of the Edinburgh school in our day, in everything like a true appreciation of the fundamental facts and principles of the science, it is refreshing to find the present occupant of the chair fully recognising the importance of physiological investigations, not merely for their own sake, but as affording materials essential to the determination of the true alliances and affinities of plants. We could certainly have wished that Professor Balfour had more completely emancipated himself from the trammels in which the too exclusive study of flowering plants is almost certain to involve the botanist, and had more pointedly directed the attention of his readers to the study of the morphology and physiology of the Cryptogamia; since every fresh discovery affords a new confirmation to the doctrine we have long and perseveringly preached, that the essential phenomena of vegetation can only be rightly comprehended, when they are studied under their simplest conditions. Thus we shall presently show that the history of the process of "cell-fission" has been more truly appreciated from observations on the *Confervæ*, than from those made on any other tribe of plants or animals; and that from the conjugation of the simplest *Palmellæ* we may derive our most comprehensive expression of the real nature of the "act of generation." If every botanist would apply himself to the examination of a few of the most common forms of vegetation within his reach, and would follow them through their successive stages of development and reproduction, we should soon be in possession of materials of the highest value in general physiology, as well as in the history of that particular tribe. We have a most remarkable illustration of the truth of our assertion, in the success which has attended the labours of a distinguished amateur, Count Suminski; whose researches on the development of Ferns will take rank, we venture to affirm, with the most remarkable botanical discoveries of the age. We shall give an account of his labours in their proper place; and shall at present only further remark in regard to his work, that the plates which accompany it (lithographed from his own drawings) present the most remarkable combination of artistic beauty with fidelity and distinctness of delineation, that it has ever been our good fortune to see in illustrations of vegetable anatomy.

The 'Elements of Botany' of M. Adrien de Jussieu forms part of the Elementary course of Natural History adopted by the 'Conseil Royal de l'Instruction publique,' for the use of the Colleges in France, being a companion-volume to that of Professor Milne-Edwards on 'Animal Physiology and Zoology,' and to that of M. Beudant on 'Mineralogy and Zoology.' Having been written for the same purpose as Professor Balfour's,—namely, as a text-book for students,—it would be unreasonable to expect in it any original contributions to our knowledge of the subject; and it is in many respects well adapted to its purpose, though it bears the marked impress of the older French school, which, though at one period the most advanced, has by no means kept pace with the progress of modern botanical science. The remarks which we have made as to what we consider the chief imperfection in Professor Balfour's treatise, apply still more strongly to this; the whole system of anatomy and physiology

being founded almost exclusively upon the higher plants, and the history of development but little brought into notice. Still it was in many respects an advance upon previous text-books; and the English translation, which is very creditably executed, would have been a useful and acceptable introduction for botanical students in this country, if it had not been superseded by Professor Balfour's treatise, which, though framed upon the same general plan, has the advantage of being much more on a level with the present state of the science.

Mr. Henfrey's little 'Rudiments' are designed simply to furnish the beginner with a simple and practical introduction to the study of plants; and they are admirably adapted for their purpose. They possess this great advantage over most other elementary books,—that being the composition of a botanist who has given proof of his acquaintance with the highest departments of the science, the student who follows its guidance may be assured that he will have nothing hereafter to unlearn; which is the case with too many of the introductions to science written by mere compilers having no practical knowledge of their subject.

Having thus endeavoured to place before our readers a correct idea of the character of each of the works upon our list, we shall proceed to lay before them the results of recent inquiries upon three subjects which may be considered to be of most special interest to the animal physiologist;—namely, the general doctrines of cell-development, the circulation of fluid, and the act of generation.

Formation and multiplication of cells. The wall of the vegetable cell, which has usually been regarded as a simple membrane, is now well known to be made up, in most if not in all instances, of two layers of very different composition and properties. The inner of these layers, which has received the name of "primordial utricle," appears to be the one first formed, and most essential to the existence of the cell; it is extremely thin and delicate, so that it escapes attention so long as it remains in contact with the external layer, and is only brought into view when circumstances occasion its separation from this; it seems to consist of an azotized compound, probably of an albuminous nature; and it appears to participate actively in the vital operations of the cell. The external layer, on the other hand, though commonly regarded as the proper "cell-wall," seems to be generated on the external surface of the primordial utricle, after the latter has completely inclosed the cavity and its contents, so that it cannot be regarded as essential to the cell; it is usually thick and strong in comparison with the other, but it may possess various degrees of consolidation, from mere mucus to a firm and tenacious substance; it is composed of cellulose, a substance nearly identical with starch; and it does not appear to take any active share in the vital operations of the cell, its principal object being to protect, locate, and isolate the matter it contains. This external layer may consist of many laminæ, the results of successive deposits from the surface of the primordial utricle; and the continuation of this process of deposition of concentric layers may occasion the gradual contraction of the cavity of the cell. After the outer layer has been completely formed, however, the additional matter is usually so disposed as not entirely to line it; sometimes taking the form of a spiral fibre, sometimes being deposited in somewhat irregular patches, and sometimes forming layers which are interrupted by perforations, so that the contracted

cavity still sends radiating prolongations through the secondary deposit as far as the external cell-wall. These various modes of deposit are concerned in the production of the numerous forms of tissue, of which the vegetable histologist has to take note, but of which it is not requisite here to give any detailed account.

The granular matter contained in the interior of the cell, being usually coloured, has received the name of "endochrome," which is a convenient designation of the aggregate contents of the vegetable cell; and all the vital phenomena which the cell exhibits have their seat in it. Amongst these phenomena, there is one which, although formerly recognised only in a small number of aquatic plants, had been surmised to be present in every cell at a certain stage of its growth,—a surmise which derives strong confirmation from the recent observations of Schleiden and others. The phenomenon to which we allude is the movement of rotation of a portion of the cell-contents,—chiefly, as it would appear, of a viscid layer of albuminous matter, carrying along with it granules of starch and chlorophyll, by whose progress the movement is recognised,—over the inner surface of the primordial utricle. In most plants of the families of Characeæ, Nymphaeaceæ, and Hydrocharaceæ, in which the movement has been long known, we may observe a single broad current, sweeping up one side of the cell and down the other, and returning continuously into itself; the ascending and descending currents do not come into absolute contact, being separated by a clear space in which no motion can be detected; but we have sometimes observed a large granule project so far from the border of one current, as to entangle and carry with it a granule projecting in like manner beyond the edge of the other stream. If the cell be carefully tied across, the current is in a short time reestablished in each segment, as if the cell had undergone a natural subdivision.

The vigour of the movement increases and diminishes in accordance with the general vital activity of the cell; as appears, not merely from observation of its ordinary growth, but also from the exciting or depressing effects of physical agents. In the cells which exhibit this form of circulation, there is no fixed nucleus; but one of the moving aggregations of granules is usually much larger than the rest, and seems to have a nuclear character.—A movement essentially similar, but differing in some of its conditions, was long since observed in the hairs of several flowering-plants; and it has been especially studied by Schleiden, who has now collected several hundred examples from the most various families of Cryptogamia and Phanerogamia, and has obtained these from different parts of the fabric. The current, in these cases, is never so broad as in the previous form, and usually presents itself as a thread-like stream passing through a stratum of motionless matter. This stream may be single, passing up one side of the cell, and down the other, as is usually the case in the long and tubiform cells of hairs; but several distinct currents may exist in the same cell; and these are observed to have a common point of departure and return,—namely, that collection of granular matter into one mass attached to the wall of the cell, which is known as the "nucleus" or "cytoblast." Here, too, it would appear that the currents are connected with the general activity of the life of the cell; and that their cessation indicates the cessation of its formative powers.—The young cells of the common "snowberry" are spoken of by Schleiden as exhibiting this movement very characteristically.

The "nucleus," in the latter form of this phenomenon, might seem to exhibit itself as the centre of the vital activity of the cell; but the still more active circulation first described, in cells which have no distinct nucleus, or in which the nucleus, being carried along in the current, cannot be regarded as having anything to do with the production of that current, shows that the force which creates it must be diffused through the entire cell. And this we believe to be the truth with respect to the vital powers of the cell in general. The nucleus is certainly not essential to its production, and frequently does not make its appearance until an advanced stage of development; and yet the cells in which it is absent exhibit no inferiority of endowment to those in which it is present from the first. Perhaps we should be correct in regarding it as concentrating in itself, where it exists, a portion of the forces which are elsewhere diffused through the "endochrome" and the "primordial utricle;" rather than as having any powers peculiar to itself; and this idea is confirmed by the facts next to be stated in regard to the multiplication and development of cells.

It may be stated with certainty as a general fact, that every cell owes its origin in some way or other to a pre-existing cell; but the mode in which new individuals are generated is by no means constant, and there are some forms of the process which are not yet clearly understood. That which is already known, however, leaves no room for doubt that in every instance a portion of the "endochrome" of the parent-cell is the starting-point of its successor, and that although the "primordial utricle" may be also concerned, the outer cell-wall is entirely passive in the matter. The most common method of multiplication in plants, is the subdivision of the original cell into two halves, such as is seen very characteristically in many of the simplest forms of cryptogamic vegetation, in which every cell is a distinct individual. The process has been most completely studied, however, in the *Confervæ*; and the observations of Mohl,* confirmed by Henfrey and Thwaites, and we may add by ourselves, give the following as the chief stages of the process. The endochrome first appears slightly constricted in the middle of the length of the cell, and the primordial utricle is inflected around it; and by the increase of this constriction, there is gradually formed a sort of hour-glass contraction across the cavity of the parent-cell, by which it is divided into two equal halves. The two surfaces of the infolded utricle produce a double layer of permanent cell-membrane between them; and the formation of this may be observed to commence, even before the complete separation of the cavities of the twin-cells. This deposition is not confined, however, to the contiguous surfaces of the young cells, but takes place over the whole exterior of the primordial utricle; so that the new septum is continuous with new layers which are formed throughout the interior of the original cell-wall, and which constitute the proper walls of its offspring. It does not appear that in this process the nucleus performs any essential part; for we find it taking place where no nucleus can be distinguished, in virtue, it may be surmised, of a sort of mutual repulsion between the two halves of the endochrome, leading to their spontaneous separation. Where a nucleus is present, this also undergoes subdivision, so that half of it is appropriated by each of the twin-cells; but the constriction of the endochrome may frequently be observed to commence, before there is any indication of that of the nucleus.

* Vermischte Schriften, p. 362.

The foregoing is the method according to which the extension of vegetable structures from cells already in existence appears most commonly to take place. Among the lower Algæ, for example, we find the single cell giving rise to an amorphous cluster composed of aggregations of distinct individuals imbedded in a gelatinous "matrix;" to a prolonged filament composed of a single file of cells; or to a flattened leaf-like expansion; according to the mode and direction in which the subdivision takes place, and to the degree in which the new cells continue to be held together by the cell-wall of the parent. It is in this manner, again, that the cell-multiplication takes place in that germinal mass in which not merely the higher plants, but the highest animals also take their origin; and the process continues to take place on the same plan, until certain parts of that mass begin to undergo transformation into heterogeneous structures, in which case we commonly find it superseded by some other method. But we still meet with it in the multiplication of the cells of certain tissues, especially cartilage; in which the phenomena just described appear, from Dr. Leidy's observations,* to be precisely repeated.

But we sometimes observe that new cells originate in little bud-like prominences on the surface of the parent-cell, which are developed in continuity with that from which they arise. This is well seen in the *Conferva glomerata*, a common species, which increases (as pointed out by Mr. Thwaites), not merely by the repeated subdivision of the cells at the extremity of its filaments, but by the origination of new cells from every part of their surface. A certain portion of the primordial utricle seems to undergo increased nutrition; for it is seen to project, carrying the outer cell-wall before it, so as to form a protuberance, which sometimes attains a considerable length before any separation of its cavity from that of the parent-cell begins to take place. This separation is gradually effected, however, by the infolding of the primordial utricle, just as in the preceding case; and thus the endochrome of the young cell is completely severed from that of its parent, and its separate individuality may be said to commence from that time. We may consider this process of "budding" as differing from that of "subdivision" only in this,—that whilst, in the latter case, the individuality of the parent-cell is lost by the *equal* subdivision of its cavity into two similar parts, it is retained in the former through the *unequal* division of the cell, of which only a small portion is pinched off (so to speak) to form the new cell, while the greater part remains unaltered.† Of the extent to which the multiplication of cells by this budding process takes place among plants, we have as yet no certain knowledge. It is obviously the regular mode of growth in the Characeæ; in which the long tubiform cells that form the axis give off, at their points of junction with each other, circular rows of buds, from each of which is developed a whorl of lateral branches. And the same mode of increase is seen in the "ferment-cells" of yeast, which, whilst rapidly multiplying under favorable circumstances, shoot forth little buds from one or even both extremities, from each of which a new cell is developed. There is no reason to believe that, in this process, the "nucleus" takes any direct

* See Amer. Journ. of Med. Sciences, April, 1849; and p. 277 of the present volume.

† We have here, in fact, in their very simplest forms, the two methods of *fissiparous* and *gemmiparous* reproduction, which are carried out, not merely as regards the individual components, but in the entire fabric, of many animals, as well as of plants;—Polygastric animalcules of every kind affording us an illustration of the one, whilst not only Zoophytes, but also the inferior Mollusca and Articulata, present examples of the other.

share: since the evolution of buds may take place from cells destitute of nuclei; and even where nuclei exist in budding cells, they do not seem to be in any way connected with the process, since the buds are not observed to originate in or near them.

In cases where a very rapid production of cells is requisite, it would seem to be affected by the primary separation of the endochrome into numerous parts, each of which acquires for itself a covering of cell-membrane; so that a whole brood of young cells may thus be at once generated in the cavity of the parent-cell, which subsequently bursts and sets them free. Of this plan we have the most characteristic examples in the bodies termed "zoospores," set free by many of the inferior Algæ; these were formerly regarded as the proper generative products, but are now considered simply as detached *gemmæ*. Thus, in *Achlya prolifera*, a plant composed of tubiform cells, which grows parasitically upon fish, the end of the filament dilates into a large cell, the cavity of which is cut off from the rest by the formation of a partition; and within this dilated cell, an irregular circulation of granular particles can for a time be distinguished. Very speedily, however, it appears that the endochrome is being broken up into a large number of distinct masses, which are at first in close contact with one another and with the walls of the parent-cell, but which gradually become more isolated, each seeming to acquire a proper cell-wall; they then begin to move about within the parent-cell, and, when quite mature, they are set free by the rupture of its wall, to go forth and form new attachments, and give origin to new filaments. The whole process is sometimes begun and completed in no more than an hour.—Although this method of endogenous cell-production is chiefly met with among the lower cellular plants, yet there appear to be some cases among the higher tribes, in which parent-cells give origin to a new brood in their interior, by a process somewhat similar, without the successive duplication which is certainly their more usual method of cell-multiplication. This seems the case, for example, in the "embryo-sac" of Phanerogamia; which at one time contains only a mixture of albumen and starchy matter, but which is afterwards filled up by a mass of cells that have incorporated these materials into their own substance. It is by no means impossible that the nucleus may here subdivide, and that each separate particle may constitute a centre around which a certain amount of endochrome may become aggregated. If this view be correct, the process would be strikingly analogous to one of the modes of endogenous cell-development in animals. The following, however, according to Schleiden, is the mode in which the new cells are produced within the embryo-sac: "The nitrogenous substance, or *protoplasma*, collects into a more or less perfectly spherical body, at length sharply defined, the nucleus of the cell (cytoblast); upon this is deposited a layer of protoplasm, which expands as a vesicle, and forms the subsequent lining of the cell; at a very early period, the whole becomes inclosed in a wall of cellulose, and the cell is completed." (p. 568.) Other observers, however, altogether dissent from the statement that a nucleus or "cytoblast" is first formed, and that the cell-membrane springs from it.

The first *visible* stages of the development of new cells do not always take place, however, in the interior of a pre-existing generation; for cells sometimes appear to originate *de novo* in that mixture of starchy and

albuminous fluids, which, being the appropriate pabulum for vegetable cells, has been denominated *protoplasma*. Thus it is affirmed by Schleiden that "a globule of nitrogenous substance originates; in this a cavity is formed, it grows, and the complete cell has a delicate coat of cellulose, without our being able to determine the epoch of its production." (p. 568.) This he considers to occur in fluids capable of fermentation, and to be the origin of the "ferment-cells." There is no sufficient proof, however, that this "globule of nitrogenous substance" is anything else than a cell-germ, set free by the parent-cell that elaborated the protoplasma, or by some other; since the minuteness of its size, previously to the commencement of its evolution into a cell, quite sufficiently accounts for its escaping observation, excepting when the protoplasm is carefully examined with very high magnifying powers. We have been informed by Mr. Thwaites that he has frequently seen parent-cells emit cell-germs, or incipient cells, of such minuteness, as to require a power of from 700 to 1000 diameters to render them distinctly visible; and as it is obvious, from Schleiden's Appendix, 'On the Use of the Microscope in Botanical Investigations,' that he sets no value on any power higher than 200 diameters, it is not difficult to account for his persistence in his original statements as to the formation of cells from spontaneously-aggregating cytoblasts. Several parts of this Appendix contain some very curious statements, which show the author's ignorance of the present state of microscopes and of microscopic knowledge in this country. Thus, after praising the instruments of Schiek and of Plössl as the best that are made at the present time, and ranking next to these the microscopes of Pistor, Oberhauser, and Chevalier, he coolly continues:

"The more recent English instruments seem to be so much inferior to those we have mentioned, that they bear no comparison. I likewise have not seen any of these, but there certainly is no lack of clever observers in England; and since, therefore, no microscopical botanical researches of any importance have been very recently furnished by England, excepting those of Robert Brown, and since what observations the English have made can frequently be easily refuted by a cursory glance through our glasses, this deficiency can only be attributed to the defectiveness of their instruments." (p. 577.)

His translator very properly corrects this statement, and pithily remarks that "we have plenty of microscopes, and those the best in the world; but we have had but few observers (in botanical inquiry). Our microscopes have been used rather as playthings, than as the instruments of profound physiological research." We trust that, by this time, Professor Schleiden has made himself acquainted with what has been recently done in this department of investigation by Thwaites, Henfrey, Ralfs, and others; whose researches furnish the best reply to his sarcasm, and are of merit sufficient to remove the stigma under which, with a few honorable exceptions, British botanists have long been content to lie.

Circulation of sap. We are called upon by Professor Schleiden to give up the opinions which botanists have been accustomed to entertain upon this subject, and which, he assures us, are utterly untenable and absurd. "There are," he assures us (p. 107), "absolutely no vessels for the distribution of the nutritive fluid in the body of plants; and no person would take the trouble of looking for them, or would imagine he saw them

anywhere, but one who goes to the investigation of plants labouring under the false and pernicious prejudice in favour of the supposed unhappy analogy between them and animals."—The doctrine current amongst vegetable physiologists we believe to be as follows. The fluid taken up by the roots contains little solid matter, and no large proportion of carbonic acid in solution. In its ascent through the roots and stem of Exogens, it passes through the newer woody tissue and the porous vessels, without affording to them in any great degree the materials of growth. Being conducted through the leaf-stalks to the leaves, it undergoes in these organs a most important change; a large proportion of its water is exhaled, so that its solid matter is concentrated; and a considerable quantity of carbon is introduced in addition from the air, so that many new compounds are formed, and the sap acquires the power of affording nourishment to growing tissues, and of imparting the materials of the secretions to the groups of cells in various parts of the fabric whose office it is to separate them and store them up. This elaborated sap is generally believed to return from the leaves into the bark, and to descend through its inner layers; affording the materials to the cambium-layer, for the production of new woody and cellular tissue; and also, by passing inwards through the medullary rays, conveying to the older layers of wood the materials requisite for their consolidation. With respect to the mode in which the elaborated sap thus descends, there has been a difference of opinion; some maintaining that it is conveyed by distinct anastomosing vessels, termed "vessels of the latex;" whilst others regard these vessels as of special occurrence in a few plants, and consider the latex to be something different from the elaborated sap, which they believe to find its way through the cellular tissue of the bark.—In the stem of Endogens, its channel has not been so distinctly assigned; nor, if it be true that no additions take place to its lower portion, would it be necessary to suppose that a descent of sap takes place for any other purpose than the extension of the roots. Still it has been generally held that the ascent of the sap takes place through the large open vessels and woody bundles of these stems; whilst in its descent, the elaborated sap has been supposed to pass more especially through the cellular substance in which these are imbedded.

According to Professor Schleiden, however, nearly every one of these positions is incorrect: the only fact admitted by him being the movement of fluid from the roots towards the leaves as the chief evaporating surfaces. He asserts in the first place, that, save as a comparatively rare and exceptional phenomenon, the porous vessels do not convey fluid, but contain air only; secondly, that the movement of the sap is from cell to cell, throughout the entire stem, each cell taking from it that which it needs for its own formative processes; and thirdly, that the idea of a return of elaborated sap or nutritive fluid from the leaves is altogether fallacious. He justifies the first of these statements by reference to the numerous tribes of Cryptogamia which have no trace of vessels, and to the absence of vessels in many important organs in which the formative processes are going on with great activity, as, for example, the stamens and ovules; and remarks that, as nothing is more certain than that the nutritive fluid which the single cells need must be taken up from other cells, it is superfluous to imagine another mode of transit of the sap for less frequent and less important

cases. Now we cannot but be struck with the very limited comprehension of physiological truths which these arguments display. Every one admits that fluids pass with considerable readiness from cell to cell; and that this is quite sufficient in cases where there is no occasion for a very rapid movement. It was long since demonstrated by experiment, that in the *Algæ* every part of the surface absorbs for itself alone, there being no transmission of fluid, even by passage from cell to cell, from one part of the plant to another; how absurd, then, to hold up the absence of vessels in such plants as a proof that they cannot be subservient to the conveyance of fluids in plants, in which there is a constant and rapid transmission of fluids from the extremities of the roots to those of the branches. In none of the *Cryptogamia*, save in the *Ferns*, do we perceive that the absorbing and exhaling surfaces are so far separated from each other, as to indicate the need of such a provision; and in the *Ferns* we find vessels abundantly present in the roots, stem, and leaf-stalks. Where the motion of fluid is simply for the supply of what is needed for nutrition, as in the growing parts alluded to by Professor Schleiden, we can well understand that vessels are not required; for we have abundant examples to the same effect in the non-vascular tissues of animals. But to deny that the vessels convey blood into muscle or skin, because there are no vessels in growing cartilage or in the germinal mass, in both of which active formative processes are being carried on, would be an argument of which our readers must see the absurdity.

Nor can we attach any great weight to the direct observations to which Professor Schleiden refers, as proving his position that the vessels of plants convey air only. We have ourselves frequently dissected the vascular bundles under water, in the manner adverted to by him; and have found that whilst the true *tracheæ*, or elongated spiral cells, contain air so long as they retain their original character, the continuous *ducts* or vessels were filled with fluid, if the plant was in a state of active vegetation. Moreover, it rests on good authority, although we cannot say that we have ourselves repeated the observation, that when a vine-stem through which the sap is ascending is cut across, the fluid may be seen with a magnifying-glass to exude from the mouths of the cut vessels. The presence of vessels in nearly all cases in which a determinate current exists in a particular direction, which we should have regarded as a pretty clear indication that the vessels serve for the conveyance of the fluid, is considered by Professor Schleiden as a *result* of the flow, which has forced its way through the partitions of the cells, and, having thus created channels, has then abandoned them. We cannot but observe a relation so constant between the amount of leafy surface, and the rapidity of exhalation, on the one hand, and the proportion which the area of the vessels bears to that of the stem, on the other, as to induce us to be very unwilling to give up the notions hitherto entertained on this point, without some stronger disproof than Professor Schleiden has afforded. The greater the extent and activity of leaf-surface in proportion to the diameter of the stem, the greater will be the necessity for a very facile channel for the transmission of fluid from the roots; and thus it is that in the vine, the clematis, or the cane, we find the ducts so large and so freely pervious. The vessels, too, are usually larger in proportion to the closeness and density of the woody structure; thus in the *Coniferæ*, to which we are surprised that

Professor Schleiden did not advert in support of his argument, there are no vessels; but the woody cells are of unusual size and softness; and neither the leaf-surface nor the activity of exhalation are considerable in proportion to the diameter of the stem. On the other hand, in the oak and mahogany, whose wood-cells are very narrow and firm, and whose leaf-surface and activity of exhalation (being deciduous trees) are much greater, we find large and very continuous ducts. We by no means deny that the sap ascends through the woody as well as through the vascular tissue; but the latter, where it exists, appears to be its special channel, at any rate during the period of most active vegetation,—as to which, indeed, even Professor Schleiden is obliged to make a partial admission in favour of the vine and a few other trees, which he regards as exceptional cases.

We do not regard this first question, however, as one of equal importance to the others; since Professor Schleiden fully recognises the fact that a steady flow does exist between the roots and the leaves, regulated in amount by the exhalation from the latter; confining himself to a denial of the speciality of its channel. The point we have now to discuss is, whether the nutritive materials are drawn by the various growing tissues from the ascending sap, or whether they are derived from that which is supposed to have been elaborated in the leaves, and to have been transmitted by them, in some mode or other, to the parts where the most active formative processes are taking place. It seems to us that this question may be better decided by a reference to the great general phenomena of vegetation, than by observations or experiments directly contrived for its determination, which are open to various sources of fallacy, and may be differently interpreted according to the preconceived views of the inquirer.—There is, we believe, no difference of opinion as to the fact, that the essential nutrient materials supplied by the soil to the roots of plants are (independently of mineral matters) water, carbonic acid, and ammonia. All experiments hitherto made concur to prove, that neither the roots nor any other dark surfaces of plants can decompose carbonic acid; and we must therefore regard it as absorbed by them in a state of solution in water. Now to form gum, starch, sugar, cellulose, or any such ternary compounds, it is requisite that the carbonic acid should be decomposed, its carbon united with the oxygen and hydrogen of the water, and its oxygen set free; and all our most exact knowledge of the processes of vegetation concurs to indicate, that this cannot be accomplished except under the direct influence of light. We cannot conceive, then, how any such changes can be effected by the cells buried in the interior of the stem, as can generate these products from the materials taken in by the roots; and their presence in the ascending sap, in larger and larger amount as it rises, seems to us to be sufficiently accounted for by the opportunity which the current will have, of dissolving the substances previously contained within the tissues through which it passes.—But further, all physiologists have until recently accorded in the opinion, that the leaves or leafy surfaces are the principal channels for the introduction of carbon into the plant; deriving this, under the influence of light, from the carbonic acid of the atmosphere, and combining it with the elements of water supplied by the ascending sap, to form a far larger quantity of solid matter, than could have been generated from the materials which

were taken in by the roots. And we cannot find that Professor Schleiden has furnished any adequate reasons for the abandonment of this opinion, or has given any sufficient reply to the inquiry,—what becomes of all the carbon which is separated by the leaves from the atmosphere, and which is unquestionably much more than they can appropriate to their own nutrition?

We must continue, therefore, to believe—until more positive evidence has been adduced to the contrary,—that the leaves do perform the functions which have been assigned to them; not merely getting rid of the superfluous water of the ascending sap, which would seem to be their chief office in Professor Schleiden's estimation; but also effecting such a change in that which remains, as shall render it more fit for the nutrition of the various components of the solid fabric. And it follows as a corollary from this proposition, that the materials applied by these several components to their own formative processes, must be derived chiefly, if not entirely, from the fluid which has been exposed to the sun and air in the leaves, and but little in comparison from that which is in course of ascent towards them. We may freely admit, however, that the use of the term "descending current," to express the movement of the elaborated sap, is inappropriate. It will frequently pass in a horizontal, or even in an ascending direction; and it is doubtful whether the flow can be said to take place in a stream within tubes or passages, or whether the dispersion of the liquid is not solely accomplished by transmission from cell to cell. The former opinion has been held by many physiologists, who have regarded the "latex" in the light of elaborated sap; and who have considered the system of anastomosing tubes found in the leaves and bark, and known as *cinenchyma* or "laticiferous tissue," in the light of a capillary system, destined to convey the nutritive fluid from the organs in which it is prepared to those in which it is to be appropriated; whilst the peculiar movement of fluid which may be seen in these vessels has been regarded in the light of a capillary circulation. By Professor Schleiden, however, and by several other physiologists, it has been affirmed that the presence of this latex, and of the laticiferous vessels is an exceptional fact;—that the latex (or milky juice) cannot be regarded as the same with the nutritious sap, because it contains matters which are poisonous to the plant;—that it must therefore be considered rather in the light of a special secretion, whereby these poisonous matters are withdrawn from the nutritive fluids;—and lastly, that the supposed circulation of fluid in the milk-vessels is an illusion, the movement being merely the result of the discharge of their contents by their own contraction, when the part which contains them is detached before examination. We must quote the passage with which Professor Schleiden introduces the discussion, as a choice specimen of his mode of treating the doctrines from which he feels called upon to dissent.

"On the subject of the milk-vessels, I am rather afraid of saying too much than too little, for, owing to the total neglect of a correct scientific method, and the puerile sporting with hypotheses, without any foundation or guiding principle, the question regarding them is loaded with such a heap of nonsense, that the best way in beginning upon it is, in the first place, to throw overboard all that has been hitherto done, and commence entirely *de novo*, instead of undertaking the thankless task of cleansing this true Augean stable." (p. 114.)

Now, in the first place, it is by no means proved that the laticiferous tissue is restricted to plants with milky juices, although it has been peculiarly examined in such; on the contrary, we have seen it in plants which do not "milk;" and there is no reason, therefore, to regard their existence as an exceptional fact. So again, if the presence of a fluid contained in them be general, instead of being limited to the plants in which that fluid is milky, we have the more reason to regard it in the light of nutritive or elaborated sap. The argument derived from its containing poisonous matters seems to us a very insufficient disproof of such a doctrine; for, though these matters may be poisonous to the plant when separated and concentrated, their want of any poisonous action on the tissues which surround the thin-walled cinenchyma seems to prove that, when blended with other substances in a diluted state, they can have no such injurious character. With regard to the movement of the fluid in these vessels, it is obvious that it can only be distinguishable in cases in which the current is rendered evident by the opaquer particles which it carries along; and hence, we believe, has arisen the notion, that it is confined to plants with milky juices. But if the existence of such a current be admitted in the latter, we can scarcely deny its existence in other plants which possess laticiferous vessels, although the transparency of the contents of these vessels may prevent us from distinguishing it; and even where the fluid may be, on the whole, transparent, a floating particle here and there may indicate the course of the stream. We have distinctly seen it, for example, in a detached portion of the leaf of *Vallisneria spiralis*, which shows the movement of fluid within the individual cells so beautifully; and, although we admit that the injury which the leaf has received renders such an observation of little value as *establishing* the fact of a circulation of latex, yet if the fact *be* established by observations on uninjured plants with milky juices, it serves to extend it to others. We quote the following statement from Professor Balfour's treatise, as a valuable confirmation to those assertions of other observers, which Professor Schleiden, in our opinion, very unfairly calls in question; and Professor Balfour's testimony is the more important, as being given with a full knowledge of the objections which have been raised on the ground of injury to the part.

"From observations made last summer, I am disposed to agree with Schultz's statements. It is true, as Mohl remarks, that any injury done to the part examined causes peculiar oscillatory movements, which speedily cease. Thus if the young unexpanded sepal of the Celandine is removed from the plant and put under the microscope, or if the inner lining of the young stipule of *Ficus elastica* be treated in a similar manner, very obvious motion is seen in the granular contents of the vessels, and this motion is affected by pricking the vessels or by pressure. In order to avoid fallacy, however, I applied the microscope to the stipules of *Ficus elastica*, while still attached to the plant and uninjured; and I remarked that, while pressure with any blunt object on the stipule caused a marked oscillation in the vessels showing their continuity, there could, nevertheless, be observed a regular movement from the apex towards the base, independent of external influences, when the stipule was simply allowed to lie on the field of the microscope, without any pressure or injury whatever. This movement continued for at least twenty minutes during one of the experiments, and I have no doubt might have been observed longer. It is of importance to distinguish between those molecular movements which are caused by injury and pressure, and those which depend on processes going on in the interior of the living plant. My experiments

are by no means complete, but they lead at present to the adoption of Schultz's opinion relative to the existence of cyclosis." (p. 128.)

We are far from regarding the question, whether or not this movement of the latex is equivalent to a circulation or diffusion of elaborated sap by distinct vessels, as satisfactorily determined; but we think that there is at any rate sufficient evidence in its favour, to induce us to look with some suspicion at the dogmatic denial of Professor Schleiden; still more, to prevent us from admitting the justice of his assertions regarding the folly and ignorance of those who hold the affirmative doctrine. We no more regard these vessels as *necessary* to the diffusion of the elaborated sap from the leaves, than we consider the straight porous tubes as *necessary* for the ascent of the crude fluid; but when they are present, we are inclined to regard them as the principal agents in this function, performing more rapidly and directly that which would otherwise be more gradually and indirectly effected by the transmission from one cell to another. That the latter, however, may be, and frequently is, the only mode of diffusion, we have little doubt; and what we chiefly contend for, is the fact of such diffusion of nutritive fluid from the leaves, taking place in Exogens through the bark and medullary rays, and in Endogens probably through the whole cellular matrix of the stem. And, in addition to the evidence which we have already adduced in favour of this generally-received doctrine, we may notice the following facts, which are all strong indications, in our apprehension, of its truth.—In the first place, the peculiar principles which the plant may generate are seldom found in the ascending sap, in any larger amount than would be accounted for on the idea that they have been dissolved during its upward flow; and, if these principles have not been deposited in the wood, we believe that they are never found in the fluid of the ascending current. On the other hand, the leaves and the bark almost always contain these principles or others nearly allied to them, although frequently in a state of less concentration than that in which they may be found elsewhere; as if the leaves were the laboratory in which they are formed, and the bark the channel through which they are conveyed to the parts where they are to be separated and stored up.—Secondly, the history of parasitic plants appears decidedly favorable to the same view. For the parasites possessing green leaves (belonging, for the most part, to the order Loranthacæ) connect themselves especially with the *woody* structure, with which their own becomes connected as by a natural graft; and seem to grow indifferently on a great variety of trees, whose special or peculiar products are very unlike; as if they drew their nutriment from the crude or ascending current, which is nearly the same in most trees, and elaborated it for themselves. On the other hand, the leafless parasites attach themselves, commonly by suckers, to the bark of the plants they infest, to which they are obviously very injurious; and they are much more restricted as to the species from which they derive their nutriment; as if they are dependent upon elaborated sap of a particular quality.—Thirdly (but to this we attach less importance than we do to the experiments ready prepared for us by nature), it has been found that, when plants have been made to absorb mineral matters, which can be easily detected by chemical tests, these first make their appearance in the wood, then in the leaves, and lastly in the bark.

On the whole, then, we see no adequate reason for calling in question the usually-received opinion, that the elaborated sap is diffused from the leaves, where it is prepared, to the several parts of the plant which are to appropriate it; and we think it probable that this diffusion takes place more particularly through the cellular tissue of the leaf-stalks, which is directly continuous with that of the bark; whilst the fibro-vascular bundles which occupy the centre, are the channels of the ascending sap, transmitted to them from the wood for elaboration.

True generative process. The tendency of late researches on this subject has been to demonstrate the universality of the principle for which we contended on a former occasion (Vol. II, p. 371), with regard to the concurrence of two sets of organs,—equivalent on the one hand to the conjugating cells of the inferior Algæ, and on the other to the pollen-grain and ovule of Phanerogamia,—in every true “act of generation.” For the remarkable discovery which has been very recently made by Count Suminski, in regard to the generation of Ferns, affords the key to the facts which had been previously in course of progressive accumulation, in regard to the existence, in the various tribes of Cryptogamia, of *antheridia*, containing bodies analogous to the *spermatozoa** of animals, which had been surmised by many botanists to be male organs, but without any proof. The very idea of any such function, indeed, being totally opposed to Schleiden’s doctrine of vegetable generation, he is unsparing of his ridicule of those who have entertained it; not hesitating, for example, to affirm that “it appears fully certain that neither in the history of their development, nor in their structure, nor in their physiological relation, do they show the most remote analogy with the anthers of the Phanerogamia; and, consequently, that the application of this designation, together with all the visionary matter (the pretended theories) based upon it, are altogether devoid of all foundation, and therefore do not belong to science.” (p. 185.) Now unless Professor Schleiden had positive proof that these curious organs perform some other function, and that they do not take any part whatever in the generative process, he most certainly had no right to speak in this tone of the surmises of botanists of equal ability with himself, supported as they were by many very remarkable analogies, although not entitled to take rank as demonstrated facts. And the consequence is, that having committed himself by this violent declaration, he may find himself compelled either to make a very humiliating retraction, or to uphold his own doctrine in spite of evidence to the contrary, sufficient, we feel assured, to make an impression on any unprejudiced observer. We do not feel much doubt as to which course he will adopt; and venture to prophesy that, in a few years’ time, he will find himself standing alone amongst scientific botanists, in his assertion of the non-sexuality of the Cryptogamia, of the utter meaninglessness of the “spiral filaments” (as he terms the *spermatozoa*), and of the entire absence of analogy between the reputed “sexual” organs of flowering plants, and the true sexual organs of animals.

* We use this term simply to express the exact similarity of these bodies to the corresponding bodies in the seminal fluid of animals, but without any idea of imputing to them, in either case, the attribute of distinct animality.

We shall now describe the results of Count Suminski's researches, and point out their bearing on the general doctrines of vegetable generation.—It has long been well known that the first product of the spore of the Fern is a flattened leafy expansion, spreading itself over the surface on which it grows, and imbibing its nourishment through radical filaments developed from its under surface. This "primary frond," as it was termed, seemed to perform the functions of a cotyledon; obtaining and preparing nutriment for the production of the true leaves, stem, and roots, which seemed to shoot forth from near its centre; and decaying away, when these more permanent parts were fully evolved. In 1846 the discovery was made by Nægeli that this "primary frond" contains *antheridia*, that is, collections of cells containing moving filaments, analogous to those which had been previously observed in Mosses, Hepaticæ, Characæ, and Fucacæ. Their existence, however, in such a situation could not be accounted for, and seemed to render the relation of these bodies to the generative process yet more improbable than before; and alluding to the search which had been previously made for antheridia in the adult Fern, Schleiden remarks in his usual style, when speaking of any who do not implicitly adopt his opinions—

"For my part, I am surprised that no one has as yet insisted on the presence of the organs of sense, as eyes and ears, in plants, since they are possessed by animals; such an assumption would not be a whit more absurd than the mania of insisting upon having anthers in the Cryptogamia, simply because they are found in the Phanerogamia." (p. 196.)

He has the grace, however, to acknowledge the correctness of Nægeli's observation, though without attempting to solve the problem of the purpose of these curious bodies.

The conformity between the history of the development of the animal spermatozoa, as described by Kölliker, and that of the spermatozoa in the primary frond of the Fern, as described by Nægeli and Suminski, is so extraordinary, that the representations given by the one would equally serve as delineations of the other. In the interior of a prominence that forms on certain of the large cells of which the primary frond of the Fern is composed, a new cell of peculiar aspect is seen, which is distinguished by its not containing chlorophyll, but a collection of granular particles, which after a time present somewhat of a definite arrangement; this cell enlarges, and its contents are gradually developed into a collection of secondary cells, which completely occupy its cavity, and are adpressed against each other. Within every one of these secondary cells is developed a single spermatozoon, having an ovoidal body, which possesses (according to Count Suminski) three cilia on each side, whereby its movements are effected, and a filiform tail, which dilates into a smaller ovoidal body at its extremity. The parent-cell ruptures and sets free the contained sperm-cells; and the spermatozoa, which had previously lain coiled up in their interior, at last make their escape, and begin to move actively over the surface of the primary frond, through the layer of mucilaginous fluid which has been ejected with them, and the moisture with which the inferior surface of the frond is always bedewed.

But besides the *antheridia*, or parent-cells of these spermatozoa, which are usually very numerous, the "primary frond" of the Fern contains a

small number of other bodies, which were remarked by Nægeli, who considered them as antheridia in a different stage of development; but whose true nature has been detected by the acuteness and industry of Count Suminski. Of these bodies, which may be termed *pistillidia*, every one contains a single "germ-cell," which appears to be analogous to the "embryo-sac" of the ovule of Phanerogamia, but which lies naked at the bottom of the cavity of the pistillidium, instead of being surrounded (as in them) by the cellular parenchyma of the "nucleus." It is thus exposed to immediate contact with the spermatozoon, which finds its way down through the chimney-like passage of the pistillidium, and applies the dilated extremity of its filiform tail against the germ-cell; at the same time itself undergoing a considerable increase of size, apparently by the absorption of the fluid through which it has moved. Two, three, or more spermatozoa have been usually seen by Count Suminski in each pistillidium; as if to ensure the fertilization of the germ-cell. Soon after this contact has taken place, a new cell is seen within the embryo-sac, which appears to originate in the union of the contents of the "sperm-cell" and of the "germ-cell," being thus the result of a true "act of generation" taking place in the "primary frond" of the Fern; and it is from this parent-cell, as Count Suminski has beautifully shown, that the young Fern originates, by the process of subdivision and multiplication, just as the embryo of the flowering plant originates from its "embryonic vesicle." But there is this remarkable difference between them; that whereas the latter is nourished by the store of food previously laid up in the seed, the former continues to imbibe its nutriment directly from the leaf-life expansion from the midst of which it was produced; so that, physiologically speaking, the primary frond still acts as a cotyledon to the growing embryo, although its homological resemblance would rather be to the receptacle of a head of flowers.

The young Fern, thus assisted in the early stages of its development, gradually puts forth its leaves and roots; and finally evolves what has been hitherto considered as its fructification. The *spores* of the Fern, however, must now be regarded, like the "zoospores" of the Algæ which are not the product of conjugation, in the light of detached gemmæ or buds; their peculiarity being that they are "flower-buds" rather than "leaf-buds," having for their office to develop the true apparatus of fructification, in which we have male and female organs so distinctly present, that all question of the analogy of the animal and vegetable sexes seems to be now at an end. We may state, in confirmation of Count Suminski's statements, that we have ourselves been enabled, through his kindness, to verify some of the most important parts of his observations, in company with two of the most eminent botanical observers of our time; and that he clearly demonstrated to us the existence of the pistillidia, and the origin of the embryo-fern from the bodies they contain. The actual contact of the spermatozoon with the embryo-sac is, of course, not likely to be seen, except after long and careful watching; but Professors Ehrenberg and Müller have satisfied themselves of the truth of the Count's statements on this point; and we must say for ourselves, that we found his delineations of what he *was* able to show us, to be so entirely borne out by the actual specimens, that we could scarcely refuse to credit the statements of so accurate an observer as to the rest. We may remark, however, that a magni-

fyng power of 300 or 400 diameters, and some experience in the interpretation of microscopic appearances, are requisite in the verification of these remarkable observations.—Since writing the preceding account of them, we have perused a Memoir by M. Albert Wigand (Ann. des Sci. Nat., Mars, 1849), who, while confirming the statements of Count Suminski in all essential particulars relative to the structure and contents of the antheridia and pistillidia, does not hesitate to deny the penetration of the spermatozoa into the pistillidium, simply because he has not witnessed it, and thinks it *à priori* improbable. It is evident to us from his drawings, that this author has not employed sufficiently high magnifying powers or an adequate illumination.

It can scarcely be doubted that wherever cells developing spermatozoa are present in cryptogamic plants, they are to be regarded as the *male* portion of the reproductive apparatus, to which an equivalent *female* portion must somewhere exist; for now that the true function of these curious bodies has been demonstrated, their *solitary* existence would be as meaningless, and we may say as impossible, as their presence in any tribe of animals without corresponding female organs. Hence as antheridia have been discovered in a large portion of the Cryptogamia, we can scarcely doubt that the real “act of generation” takes place in them all; since, as we formerly pointed out, the conjugation of cells in the simplest Algæ, and the fertilization of the ovule in the Phanerogamia, are phenomena so essentially analogous, that a process of the same character might be expected to take place in all the intermediate groups. That neither antheridia nor pistillidia have yet been found in the Fungi, nor with absolute certainty in Lichens, cannot be accepted as a valid objection to this view; since it is obvious from the history of the process in Ferns, that we must scrutinize the entire structure of these tribes, in every stage of their evolution, before we can be entitled to declare the absence of sexual organs,—which no one would have even dreamed of finding in the “primary frond” of Ferns.

The fructification of Mosses consists of thecæ or urns containing spores, which are known in their early state as pistillidia; the antheridia have long been recognised in this tribe, whose sexuality has hence been suspected by many bryologists. The careful examination of the structure and development of Mosses, however, instituted some years ago by Mr. Valentine, led him to the conclusion that the antheridia could have no share in the reproductive function, seeing that *their* period of maturity is long anterior to that of the theca and spores. But it was suggested by Mr. Thwaites not long since, that the process of fertilization might take place in the young pistillidium, and that the entire theca with its contained spores might be its product; and the discovery of Count Suminski renders this supposition highly probable. For if in the Ferns the fertilizing process is antedated (so to speak), so that not only the thecæ, but the plant which bears them, should be the product of an act of generation in the “primary frond,” it cannot be regarded as improbable, that in the Mosses it should be so far antedated that the thecæ alone should be thus produced. The probability appears still stronger when we descend to the Hepaticæ, in many of which the resemblance of the whole plant to the “primary frond” of the Fern is so strong, that we have heard the latter designated as the “Marchantia-like expansion.” In these plants, antheridia as well as

pistillidia have long been known; and it can scarcely be doubted that, as in the "primary frond" of the Fern, a true act of generation is accomplished between their products, the germ-cells of the latter being fertilized from the germ-cells of the former; the result being the development, not (as in the Ferns) of a plant with a woody stem, true roots, leaves, and thecæ or spore-cases, but simply of the thecæ themselves. Thus, the "spores" of the Mosses and Hepaticæ must be regarded, like those of the Ferns, in the light of *gemmae*, destined to evolve the organs by which the true generative process is effected. It may seem strange to affirm that the theca is not a part of the plant which bears it, but is really its offspring; but this is not more strange than that the Fern should be the true offspring of its primary frond, instead of being evolved from it, as hitherto supposed, by a process of continued development.

The early history of the development of the Fungi has not yet been studied sufficiently, to enable us to give even a probable guess as to the period at which the true generative process takes place. This much, however, may be remarked; that as mycologists have for some time been coming to regard the *mycelium*, or filamentous expansion from which the fructification is developed, in the light of the true plant, and to look upon the reproductive body which constitutes what is popularly considered the fungus as really a subordinate part of it, we may expect to find the "act of generation" performed in the mycelium, perhaps by a conjugation of the filaments, after the fashion of that of the inferior Algæ; and this supposition derives support from the fact, that the reproductive powers of the mycelium, which forms the "spawn" of mushrooms, are not inferior to those of the fructification. The sporules of Fungi, if this view be correct, must be considered as *gemmae*, like-those of the higher Cryptogamia.

With regard to Lichens, we shall at present confine ourselves to the remark, that their near alliance, on the one hand to Algæ, and on the other to the Hepaticæ, leaves little room to doubt that what is true of those two groups must be true also of them. It has been stated by Link that he has found antheridia in Lichens; but we do not know that he has discovered spermatozoa in these.

In the class of Algæ we find the generative process performed under two forms which are apparently very distinct, yet which are seen on a closer examination to be but variations of the same fundamental type. We have already alluded to the "conjugation" which has been observed in the simplest cellular tribes, and in some of the filamentous, as being an "act of generation" in its most elementary form. In the Fucaceæ, on the other hand, which rank among the highest of the Algæ, and in the Characeæ, which are now usually referred to the same general division, we find distinct "sperm-cells," which have long been known in the Characeæ, but the discovery of which in the Fucaceæ is comparatively recent. In the latter tribe, the "sperm-cells" are sometimes developed in the same conceptacles with the "germ-cells," sometimes in distinct conceptacles on the same plant; whilst sometimes (as in the common *Fucus vesiculosus*) the two sets of conceptacles are seated on different individuals. The number of "sperm-cells" is beyond all comparison greater than that of "germ-cells;" and this we find to be the general rule, in plants as well as in animals, as if to secure the fertilization of the latter, in spite of the

accidents that may prevent the spermatozoa from meeting with them. The true "spores" of the Algæ are really the "germ-cells," which seem to be commonly fertilized, like the ova of fishes, after they have been set free from their conceptacles. But almost all Algæ propagate also by detached gemmæ, which often constitute what is known as a second form of fructification, and which, when they are carried about by ciliary action, are known as "zoospores;" and this is the only method of propagation at present known to occur in the Ulvæ and some other tribes. There can be little doubt, however, from the analogy of the tribes to which these are most closely allied, that an act of conjugation or impregnation takes place at some period or other of their development.

We may sum up our present knowledge of the true "act of generation" in plants as follows:—Its essential character everywhere consists in the union or mixture of the contents of two cells, the "sperm-cells" and the "germ-cells;" but this may take place after three modes. 1. In the simplest cellular plants, where every cell appears to possess the same endowments, so that there is no kind of specialization of function, the generative act consists in the "conjugation" of two cells, between which no difference whatever can be traced. In what may be considered the lowest forms of this process, *both* cells discharge their contents, and the new body, or sporangium, is formed between them by the mixture of their endochromes; so that neither cell of the pair can be considered in the light of a "sperm-cell" or a "germ-cell." We have here the *exact converse* of the process of multiplication by fission, already described; for as, in the latter, one cell divides itself into two equal halves, between which no difference can be traced, so in the act of conjugation, two cells, apparently similar, reunite, and form but a single new cell between them. That union, however, seems to develop a new force, in virtue of which the process of subdivision may continue until that force be expended. In the higher forms of this process there is still the same mixture of endochromes by the actual rupture of the cell-walls, and the escape of their contents; but this mixture takes place, and the sporangium is formed (as in many of the Desmidiæ and Zygnemata), within one of the conjugating cells, which must thus be regarded as the "germ-cell." We have here an indication of the specialization that is apparent in those more complex fabrics, in which certain cells are set apart for the performance of the act of generation, and in which the "sperm-cells" and "germ-cells" are quite distinct in their endowments. 2. In the higher Algæ, and in most, if not all, other Cryptogamia, the process is effected by the agency of moving filaments, resembling the spermatozoa of animals, which bring the essential part of the contents of the "sperm-cells" into such a relation with the "germ-cells," that a transudation may take place from the former to the latter, without any rupture of the walls of the latter. Of this process, the true germ is the result. This germ may be cast at once upon its own resources, as in the Algæ; or it may be nourished by its parent structure, as in the Ferns. It may at once proceed to the development of the new plant, similar to that from which it sprang, as it seems to do in the Algæ; or it may develop itself into a body containing a mass of gemmæ, each of which may produce a new plant, as in the Hepaticæ and Mosses; or it may spring from the temporary portion of the plant, and evolve the whole permanent fabric, as in Ferns, the latter producing gemmæ

from which a new generative apparatus arises.* In none of these cases does the parent appear to furnish that accumulation of nutritive matter for the development of the product of the germ-cell, which, when included in a common envelope with it, would constitute a true "seed." 3. In the Phanerogamia, the "sperm-cell," or pollen-grain, does not evolve self-moving filaments, as in the Cryptogamia, but puts forth long tubes, which convey its fertilizing contents to the "germ-cell" after a different fashion, still, however, fulfilling the same essential purpose. We adverted, on a former occasion, to the concurrence of nearly all the most recent and trustworthy observers, in the denial of Schleiden's assertion that the embryo is formed within the extremity of the pollen-tube; the fact appearing to be rather, that the embryonic vesicle is formed within the embryo-sac without any actual penetration of the pollen-tube, and that the mixture of the contents of the "sperm-cell" and "germ-cell" is accomplished by transudation through the membrane of the pollen-tube and that of the embryonic vesicle. The germ-cell is here imbedded in a mass of nutritious matter, which, with the embryo, constitutes the "seed;" and it is upon this store that the young plant subsists during the early stages of its development. The first leafy parts which are put forth,—the cotyledons,—have a merely temporary function, being evidently destined to obtain and elaborate the materials required for the evolution of the more permanent organs; and we might regard the subsequently developed parts as originating from them by gemmation.†

We have thus endeavoured, so far as our limited space would allow, to place before our readers some of the most interesting results of those recent investigations in physiological botany, which bear most closely upon animal physiology; and we cannot but think that it must be as apparent to them as it is to ourselves, that these two departments of science are so closely related, that they will be most successfully pursued in concert, and with a mutual good understanding between their respective votaries. We feel ourselves called upon to protest most strongly against the denunciations which Professor Schleiden has thought it proper to utter against all those, who labour at drawing closer the bonds of union between these two great kingdoms of organized nature. We are no friends to the vague and superficial analogies, which men, who had no true insight into their relation, have amused themselves with drawing; but we do contend that real and fundamental analogies exist, which are clearly to be recognised by those who possess a sufficiently comprehensive knowledge of the phenomena, and a sufficient amount of philosophic sagacity, to be able to

* This last would be reckoned by the disciples of Steenstrup as a case of "alternation of generations;" the true Fern, originating from the fertilized germ, being one generation; and the primary frond, originating from a gemma, being in their apprehension a second generation. We shall return hereafter (Art. VIII) to the discussion whether the term "generation" can be rightly applied in this or any similar case of gemmiparous reproduction, which comes more properly, in our apprehension, under the term "development;" and shall at present only remark, that the case fully confirms our general doctrine (Vol. I, p. 205), that in every instance of such alternation, it exists really between the products of sexual and non-sexual reproduction, and that it never occurs between the products of the true generative process, which invariably repeat the same type.

† Professor E. Forbes carries his idea of the succession of generations in flowering-plants to such an extent, as to regard the cotyledons as forming one generation, the first true leaf a second generation, and so on. According to this doctrine, the germinal membrane of the animal embryo, which is the distinct analogue of the cotyledons, and the chick which is developed through its means, constitute two distinct generations, instead of being the successive phases of the evolution of one and the same individual.

distinguish the essential from the accidental, the real from the formal. The simple fact that the animal, like the plant, is evolved from a single cell, and that even in the most perfect condition of each, all its functions are but manifestations of different kinds of cell-life, should at once indicate that the most comprehensive principles of vital action must be common to both, and that the differences in their structure must have relation to the difference of the conditions under which they are to exist, and the difference of the results they are to evolve. The "idea" of a plant is that of a being possessed of the power of combining certain inorganic elements into organic compounds, and of building up, at the expense of these, an organized fabric, which shall afford materials for the nutrition of animals; but this being is not endowed with sentient attributes or locomotive powers; it is usually fixed to one spot from the time its germ first begins to develop itself, and its whole life consists in formation and reproduction. On the other hand, the "idea" of an animal is that of a being possessed of sentient existence, but that existence is (in our present state at least) dependent upon, or inseparably connected with, an organized structure: powers of formation there must be, as in plants; but these are exercised upon organic compounds, supplied directly or indirectly by the vegetable kingdom; and the animal must be endowed with locomotive faculties to go in search of these, or with some means of bringing them within its reach: and the whole apparatus of nutrition seems destined simply to form and to maintain that of animal life, by which the conscious existence is brought into relation with the world around, so as to receive impressions from it, and in its turn to act upon it; whilst that of reproduction secures its renewal. Considering that the plant and the animal have the same starting-point,—the single cell,—we might naturally expect that the "act of generation," of which that cell is the result, should be the one whose performance in the two kingdoms might display the greatest resemblance; and we think that no one who appreciates the details we have given can fail to perceive how remarkably this is the case. It is curious, however, to observe, that in *no single* tribe of plants is the reproductive process performed *on both sides* after the pattern of that of animals; for whilst in the Cryptogamia the "sperm-cells" bear so strong a resemblance to those of animals, that they might be mistaken for them, the "germ-cell" is naked, instead of being inclosed within a yolk and yolk-bag; whilst in the Phanerogamia we find ovules resembling the ova of animals in all essential particulars, but fertilized by a different development of the sperm-cell.

Notwithstanding that we have felt called upon, in our discussion of every one of the subjects upon which we have touched, to express our dissent from Professor Schleiden's views, and to condemn the spirit and temper in which he has put them forth, we should not be doing justice to his labours if we did not also repeat our statement, that by his elucidation of a vast number of other topics, which it does not fall within our province to discuss, he has contributed most successfully as well as diligently to the advancement of botanical science. We regard the plan of his work, and the method of investigation on which it is based, as far superior to that of any other systematic treatise on the subject; and no one can rightly comprehend the present aspect of Botanical science, without a careful study of it.