In females we have an additional element in the catamenial functions, which we can call to our aid in forming an opinion as to the probable issue of a case, and its importance in prognosis will be seen by the accompanying groups of cases. Of nine patients in whom the disease had existed for a period of twelve months or more, and in whom the menstrual functions continued in a normal state, only two got worse whilst under treatment, an average of only about 22· per cent. Of twenty-five patients in whom there had been evidence of pulmonary affection for half a year and onwards, with absence of the catamenial discharge for at least three or four months, twenty lost ground with more or less rapidity, an average of 80· per cent.; and the most unfavourable cases, or those least amenable to treatment, will be found amongst those in whom the catamenia disappears nearly at the same time, or very shortly after, the pulmonary affection is manifested. Its reappearance in any case will, I believe, be always found coincident with some general re-establishment of health; for though its absence must not be considered incompatible with improvement, recovery under these circumstances very seldom occurs in any marked degree.

From these facts, then, we may conclude that for any individual case free from the more dangerous complications, the average duration of two and a half years may be expected; and this period will be more or less prolonged, and a degree of present improvement may be anticipated, according as the case in question presents those symptoms which are indicated above as warranting a favourable prognosis.

Art. III.

Vegetable Morphology: its History and Present Condition. By Maxwell T. Masters, F.L.S., M.R.C.S., &c., Lecturer on Botany, St. George's Hospital.

The basis of botanical science, the most important of all its subdivisions, that one, indeed, upon which all the rest depend, is morphology, or the accurate investigation and observation of the plant and its varied parts, in order to obtain an insight into their mutual relations and significance, and by these means to discover the principles of their construction. The great value of some hypothesis as furnishing a centre round which the ever-increasing host of newly observed facts may be gathered, and their import explained, leads us to add a word of caution which may perhaps appear superfluous, but which the experience of every day proves is not so. We have no right confidently to assume that in laying down so-called "laws," philosophers have actually discovered the plan by which it has pleased the Creator to build the universe or fashion its inhabitants. It more befits our ignorance to acknowledge that such expressions as unity of type, special adaptation, and the like, are merely relative; that they serve our purpose of collating facts and rendering them intelligible, and that by their means we do really arrive at a clearer insight into the truth. Mr. Buckle, in his 'History of Civilization in England,' vol. i.,

p. 28, alludes to this in the following pertinent words:—"A law of nature being merely a generalization of relations, and having no existence except in the mind, is essentially intangible; and therefore, however small the law may be, it can never admit of exceptions, though its operation may admit of innumerable exceptions. Hence, as Dugald Stewart rightly says, we can only refer to the laws of Nature by a sort of figure or metaphor; this is constantly lost sight of, even by authors of repute, some of whom speak of laws as if they were causes, and therefore liable to interruption by larger causes, while other writers pronounce them to be delegated agencies from the Deity."

The principal points in the doctrine of vegetable morphology are so perfectly well-known that it is unnecessary in this place to dwell at any length upon them, but there are many circumstances connected with the history of vegetable morphology which seem to be imperfectly known even to professed naturalists. It is hoped, therefore, that a short account of the progress of the doctrine, embodying likewise some of the facts connected with its present condition, may not be entirely unacceptable, even though no pretensions be made to completeness in these respects.

The first indications of what we now call morphology are doubtless to be met with in the writings of Aristotle and Theophrastus; take for instance the following passage from Aristotle—"As a general rule, a plant possesses potentially both root and stem in every part;" or this from Theophrastus—"Some organs exist only according to analogy, and others, though the same, yet exist in a different manner." Perhaps the most remarkable passage of this sort is one derived from the writings of Nicholas of Damascus, who was probably indebted to Aristotle for the idea. Nicholas was a poet, historian, and statesman, and was sent in the latter capacity as ambassador to Augustus by Herod the Great, B.C. 5. He wrote two books on plants, compiled chiefly from older authors, especially Aristotle, to whom, indeed, until disproved by Meyer, the work had always been attributed, and as such had been commented on by Albertus Magnus. "The wise men among the antients," says Nicholas, "regarded all leaves as fruits, but they thought the amount of moisture was too great to allow of their attaining maturity and solidity by the influence of external heat and evaporation produced by the sun. The undigested moisture therefore takes the form of leaves, and the objects for which the leaves are produced are merely that the sun may draw off the moisture through them, and that they may protect the fruit from his heat. Thus the leaves are also, properly speaking, fruits, and as said before, it is only the moisture ascending through them which converts them into leaves. This is the explanation of the often fruitless olive-trees, for as soon as digestion is effected, the undigested moisture separates first of all from the more delicate portion and forms leaves, while the digested portion becomes flowers, and when in the autumn this part is matured, the fruit is produced and makes its appearance at the end of the stem at the place appointed for it by Nature." This, which Meyer calls an anticipation of the metamorphosis, is of interest in a purely physio-

logical point of view. Not only is there here inferred an intrinsic identity between the leaf and the fruit, but that identity is asserted for the physiological reason that the sap, instead of being retarded and reserved in the tree for the purpose of aiding in the formation of the fruit, is hurried into the leaves, and there evaporated by the action of the sun. The general principles here involved are strikingly in accordance with those received and promulgated by modern physiologists. If evaporation and transpiration be allowed to go on to too great an extent, the leaves indeed may become larger and more numerous, but at the expense of the fruit. Check the undue exercise of the functions of the leaf, retard the flow of the sap by removing a ring of bark, and the sap is thereby accumulated and concentrated, and hence serves to increase either the quantity or the quality of the fruit, or it may be both. Thus, in this noteworthy passage the morphological assertion is corroborated by physiological observation and reasoning, a method of procedure of the highest possible value, and as such laid down as a canon to be followed whenever it is possible to do so, by De Candolle, Jussieu, and all eminent botanists. From this time up to the time of Albertus Magnus there was little or no progress, but on the contrary, much retrogression in botany, as in other branches of learning. Albert, a Dominican friar, who died in 1280, was, for the age in which he lived, an intellectual giant, and possessed enlarged ideas on botanical as on other subjects. Speaking of life as manifested in plants, he says—"It is only evidenced in the functions of growth, of imbibing nourishment, and of reproduction, and with these circumscribed limits correspond the nearly homologous nature both of the external and internal parts of plants, and the powers which they possess of reproducing their kind from any part whatsoever, as well as by seed."*

It is not necessary here to do more than mention the numerous systematic writers of the sixteenth and seventeenth centuries, as the way in which they treated their subject has little bearing on the theory of morphology. It will suffice merely to say, that they in general speak of what we now call petals as "*folia.*"

Joachim Jung, professor at Hamburgh, who died in 1657, has shown in his works that he possessed opinions on the subject of morphology which would meet with acceptance in the present day, in evidence of which may be cited the following extracts from his 'Doxoscopia." He thus defines the stem—"*Quicquid florem fert aut fructum est caulis.*" In alluding to what we should now call compound flowers with tubular florets, he says, "*Sunt et flores quidam falso staminei dicti, qui rectius ex flosculis cavis sive fistulosis arcte farcti, sive tubulosi, dicerentur, staminei dici possunt qui solis staminibus constant.*" In the chapter on the flower, after stating that leaves are not only flat, and have definite breadth, but are occasionally hollow and cylindrical, he continues—"*Ita quoque inter folia florea recenseri possunt et recensentur etiam quæ interminatam sive in se recurrentem habent latitudinem.*" So also in

* The preceding quotations have been derived for the most part from Meyer's History of Botany—a most valuable contribution to the History of Botany.

the 'Isagoge Phytoscopica," published after the author's decease by J. Vagetus, the distinction between root and stem, the difference between leaves and foliaceous branches, the transition from the ordinary leaves to the "*folia floris*," and the true nature of the involucre, are all clearly explained. As for the flower, it consists, says he—1mis. *Vel ex meris planis foliis, figurâ ac situ similibus uti flos Tulipæ.* 2dis. *Ex foliis meris planis, sed figurâ ac situ diversis ut flos Iridum Gladioli.* 3tiis. *Ex foliis quasi planis et corniculis ut in Aquilegiâ.*"

After this time, and until 1759, when Wolf published his 'Theoria Generationis,' there seems to be little worthy of record in the history of morphology. The very remarkable works of Wolf have been so strangely neglected, that their great merits are even now hardly appreciated. The most noteworthy facts relating to the treatise just mentioned, are the researches into the development of the flower; herein he opened up a new line of investigation the full importance of which has not been sufficiently recognised till comparatively recently. He describes the internal structure of buds as consisting of a cellular substance to which the rudiments of the leaves are to be considered as appendicular organs. He attributes the formation of the flower to an arrest of growth arising from diminished vegetative action. The order of development in the successive whorls of the flower is explained by Wolf in a manner not quite in accordance with modern researches on this subject; nor is his hypothesis, that the stamens are to be considered as buds axillary to the petals, at all consonant with their true position with reference to the petals. This notion, however, somewhat modified, has of late years been supported by Agardh and Endlicher.

In reference to the metamorphosis of plants, neither Linnæus nor Goethe have expressed themselves so clearly as does Wolf in an essay on the Development of the Intestinal Canal in the Chick, published in the Commentaries of the St. Petersburg Academy of Sciences, 1766. After speaking of the homologous nature of the leaves, the sepals and petals, an homology consequent on their similarity of structure and identity of origin, he goes on to state that the "pericarp is manifestly composed of several leaves as in the calyx, with this difference only, that the leaves which are merely placed in close contact in the calyx, are here united together;" a view which he corroborates by referring to the manner in which many capsules open and separate "into their leaves." The seeds, too, he looks upon as consisting of leaves in close combination. His reasons for considering the petals and stamens as homologous with leaves, are based upon the same facts as those which led Linnæus, and, many years afterwards, Goethe, to the same conclusion. "In a word," says Wolf, "we see nothing in the whole plant, whose parts at first sight differ so remarkably from each other, but leaves and stem, to which latter the root is referrible." "If," he continues, "the organs of a plant, with the exception of the stalk, are thus referrible to the leaf, and are mere modifications of it, a theory, showing the manner in which plants are generated is obviously not a very difficult one to form, and at the same time the course is indicated

which we must follow in propounding it. It must first be ascertained by observation in what way the ordinary leaves are formed, or in other words, how ordinary vegetation takes place; on what basis it rests, and by means of what powers it is brought into existence. Having gained this knowledge, we must investigate the causes which so modify the general mode of growth as to produce, in the place of leaves, the parts of the flower." The basis of all these modifications he attributes to a gradual diminution in the powers of vegetation. It may here be remarked that during the interval between the publication of the 'Theoria Generationis' and that of the essay in the St. Petersburg 'Transactions,' to which reference has just been made, Wolf seems to have abandoned the notion that the stamens were buds peculiar to the corolline leaves, for in the latter essay he refers the stamens to leaves also; and it is worthy of notice that while the 'Theoria Generationis' was published one year before the 'Prolepsis Plantarum' of Linnæus appeared, Wolf's essay in the St. Petersburg 'Transactions' was not printed till six years after the publication of the 'Prolepsis.' These facts render it rather difficult to assign the priority either to Wolf or to Linnæus; but when we consider that Wolf's first essay was published before the 'Prolepsis,' that his second essay was an expansion of the first, that there is no proof that he was under any obligation to Linnæus, or had even perused the 'Prolepsis,' and when we further consider (as all physiologists will admit) the far higher scientific merits of Wolf's essays on this subject than of that of Linnæus, we need not hesitate to give Wolf the merit of having been at once the pioneer and the exponent of the metamorphosis.

Of Linnæus' essay in the 'Prolepsis,' it is not necessary to say much, as it has been more read and is more generally known than the writings of Wolf. The 'Prolepsis Plantarum' was published at Upsal in 1760 among the 'Amænitates Academicæ.' In this essay, published in the name of his pupil Ullmark, Linnæus refers all the parts of the flower to leaves, and this view is established by the consideration of numerous instances in natural as well as in monstrous flowers, where the parts of the flower are either like ordinary leaves, or are replaced by them, which could not be the case were the two organs not homologous, for, says he, "the liver cannot become the heart, nor the heart the stomach." The greater stress is laid on these particulars, because it has been said that Linnæus made no investigations in this subject; and his essay in consequence has been unjustly depreciated. The truth is, the essay is based upon original researches and incontrovertible facts, and by their aid a similar result was arrived at as Wolf had attained to, from the study of progressive development, over and above the observations that he made of a similar nature to those of Linnæus. But Linnæus associated with his facts, hypotheses which could not be satisfactorily borne out, hypotheses which Wolf, with his knowledge of the internal structure of plants, and his enlarged ideas on physiology, would never have originated. The petals, for instance, were considered to be buds axillary to the sepals, the stamens, again, as buds axillary to the petals, and so on. But the members of each successive

whorl of the flower are not axillary one to the other—to say nothing of the improbability of one single leaf being the representative of a bud. The asserted relationship, too, between the whorls of the flower and the cortical and woody layers of the stem is equally untenable. The whole subject is, moreover, complicated and obscured by the fanciful theory of anticipation, by which he supposed a flower to be a shoot, modified as to its leaves and hurried on in its growth, so that the growth of five years was, in the case of the modified shoot or flower, compressed into one year. From these impeding circumstances is it, no doubt, that so little attention was paid to this essay, and the foliar nature of the floral whorls, although pointed out by Wolf and again by Linnæus, was not received as an admitted fact till after the publication of Goethe's essay, to be presently mentioned. Before quitting the subject of the 'Prolepsis,' it may be mentioned that Linnæus, speaking of the buds, compared them to so many distinct generations, as in the similar instance of the *volvox globator*, thus expressing an opinion quite in accordance with the modern doctrine of metagenesis.

Goethe's famous essay on the metamorphosis of plants was first published in 1790, thirty years after the publication of the essays of Wolf and Linnæus. Mr. Buckle has, however, drawn attention to a passage in the 'Italianische Reise,' which shows that Goethe had glimpses of the discovery in or before 1786. Much misapprehension has arisen as to what degree of merit is really due to Goethe, for while some unhesitatingly ascribe to him the merit of being the foremost labourer in this field, others as unjustly deny him all praise, and say that he was forestalled by Linnæus. It is indeed true that in many points he was anticipated by previous writers, especially by Wolf, but no one can peruse Goethe's essay without acknowledging that with him the idea was an original one. From Linnæus he directly derived but little, from Wolf nothing. In justice to Linnæus, however, it must be stated that Goethe acknowledged that Linnæus had had a greater influence on his mind than any one save Shakspeare and Spinosa, and that he not only prefixed to his essay a quotation from the 'Prolepsis,' but also devoted a chapter to its consideration. A perusal of this chapter will bear out what is above alleged regarding the originality of the idea with Goethe, as may be further seen in the historical sketch of his botanical studies, which he published many years after the original publication of the essay. Of course we cannot overlook the indirect influence which the writings of Linnæus and other botanists during the thirty years' interval which has been mentioned, must have exerted on Goethe's mind, still it is evident, from his lack of early scientific training, as well as from an impartial consideration of the essay itself, that Goethe framed his theory from original independent research and thought, and it may indeed have been as Turpin remarked, that the freedom from the dust of schools may have contributed to the development of the idea of the organic unity in plants, because its originator was unshackled by the details of a multitude of ever-varying forms, and by a terminology often superfluous, because expressing the same thing under different names. We may

safely admit that had it not been for Goethe's clear enunciation of what Schleiden terms the only really scientific principle which botany can be said at present to possess, neither the essays of Linnæus nor those of Wolf would have sufficed to establish the theory on so firm a basis as that on which it now stands.

It is to the elder De Candolle that science is peculiarly indebted for demonstrating the value of Goethe's essay. No reader of this treatise will fail to recognise how much that has been written on the subject in our own time, by authors of far greater botanical knowledge than Goethe, has been anticipated by the great poet. Compare, for instance, the alternate expansion and contraction of the lateral organs of the flower, on which Goethe lays so much stress, with A. Braun's theory of the rejuvenescence of plants, and with the series of vibrations in the metamorphosis of which he speaks. Many similar instances might be adduced. Goethe, too, distinctly recognised the true nature of the so-called nectaries, when he stated them to be intermediate stages in the passage of petals into stamens, and explained in a similar way the "corona" of passion-flowers, of *Narcissus*, and other organs not distinctly referrible to the corolline or to the staminal whorls.

This notion is opposed to that of Schleiden, who believes these formations to be secondary productions from the petals, and not independent foliar organs.* And this is no doubt true in some cases, as shown by organogenic researches, but it does not invalidate the truth of Goethe's opinion in other cases, as witness the following instances which seem to favour Goethe's views. The flowers of *Narcissus montanus* growing in the Botanic Garden at Oxford constantly produce anthers on the margin of the *corona*, and in some instances the *corona* is divided into distinct filaments, each surmounted with an anther, a deviation from the ordinary arrangement which seems to show clearly that the *corona* is composed of a number of confluent petaloïd filaments whose anthers are generally suppressed; an opinion first enunciated by Dr. Lindley, and which is surely confirmed by the close affinity between *Narcissus* and *Pancratium*, in the flowers of which latter plant the stamens are connected together at their bases by a petaloïd expansion. The petaloïd scales of *Brodiæa*, of *Vellozia*, and the petaloïd filaments of *Allium*, may all be cited in support of this notion. Some of the rays of the crown of the passion-flower have been likewise observed to be replaced by anthers, while in *Passiflora murucuja* the rays are actually combined into a cup like that of *Narcissus* or of *Melia*. In the case of *Saponaria*, the writer has shown that the scales on the petals of these flowers are in reality referrible to the adhesion of two antherless filaments.†

Goethe says, in support of his opinion—"If the formation of the petals is the result of expansion, that of the *corona* is due to contraction, as is the case with the stamens." Goethe also clearly showed the nature of the so-called nectaries of *Aconite* and *Nigella*, when he

* A similar opinion is held by M. J. Gay, the most recent writer on this subject.—Ann. des Sc. Nat., 1859.

† Journal of Proceedings, Linnæan Society, vol. i. p. 159.

referred them to the petals, but in this he was, as has been already stated, forestalled by Jung.

Goethe's explanation of the formation of the fruit is so well known that little need be said concerning it, especially as from De Candolle downward the writers on the fruit have compiled their systems of classification on the basis laid down by Goethe. It is here necessary to remark, that Wolf's explanation of the structure of the compound fruit is quite as explicit as that of Goethe, and based upon exactly similar facts, but the writings of Wolf were not known to Goethe till long after the first publication of his essay; and although Linnæus had asserted the foliar nature of the *Pistillum*, from having seen the style of *Carduus* replaced by two green serrated leaflets, there is nothing in the writings of Linnæus so explicit as to the constuction of compound fruits as may be found in Goethe's memoir. The nature of buds, their homologies with seeds, the phenomena of vegetative reproduction and growth as evinced in the formation of buds, and the successive production of node after node, are all clearly explained by Goethe, who on this account also may be considered as the pioneer of that theory of rejuvenescence which Braun has brought to such perfection. There are certain objections which have from time to time been raised against Goethe's theory, as well as to those of other writers before and after him; but it will be more convenient to mention these in conjunction with the present condition of vegetable morphology than separately.

A very simple method of grouping the various modifications met with in the conformation of plants in general, though one not usually adopted, is to classify the several organs or parts of plants into groups, according as they belong to the alimentary, the tegumentary, the reproductive, the fibro-vascular, and the appendicular systems. A very slight acquaintance with plants will suffice to show that many of their organs might as well be included in one as in another of these groups, and in truth might without impropriety be placed in both; nevertheless the arrangement will be found a convenient one, and probably as little liable to objection as any arrangement of the kind can be in the present state of our knowledge, or rather ignorance of the relations between structure and functions in plants.

The alimentary system contains the organs devoted to the nutrition and growth of plants, including multiplication by "*gemmæ*" or buds. The tegumentary or cortical system is sufficiently explained by its title, it corresponds to the exo-skeleton of animals. The reproductive system needs no explanation, the fibro-vascular system corresponds to the endo-skeleton of animals and might be merged with the first group; the last group, the appendicular, is one inserted here more in deference to generally received opinion than from a conviction of the real necessity of establishing such a system as distinct. We shall endeavour to show as we proceed, how little ground there is for retaining such a distinction, and how the organs so classed might more justly be referred to some of the other groups just mentioned.

The groups are arranged according to their relative frequency and importance; thus, all plants, however simple, have an alimentary

system; in fact, the simplest conception we can frame of a plant is, as a gelatinous mass of matter endowed with a mysterious principle, vital or physical, it matters not which for our present purpose, manifesting itself in the nutritive functions performed by the humble organism. The zoospores of some of the cryptogamic plants are at first mere masses of protoplasm—e.g., *Vaucheria*, which have a separate existence for a time, an existence too marked by the function of active locomotion, a function denied to plants higher in the scale. These zoospores speedily become invested by a membrane which completes the "cell" by forming the cell wall. Indeed, it is doubtful whether a cell wall of membrane, differing in chemical constitution, and notably in physical and vital endowments from the primary nitrogenous mass, be not essential to all plants in their adult condition.* The zoospore does not fulfil its ultimate purpose, the formation of a new plant, till it has become invested by the cell wall. Hence it may be assumed that the alimentary system and the integumentary system are represented in all plants in their adult condition, although perchance in some of the lowest plants, for a time, the organism may live without an integumentary cell-wall.

The integumentary system which is universally present in plants, presents itself, as we have seen, in the lowest plants, simply as the cell-wall. Even in this state it undergoes morphological changes; in some cases it becomes soft and gelatinous, and in those cases where several cells are aggregated together, the walls of the cells become inseparably fused, so as to form an integumentary layer common to the whole plant. The peculiar markings on the *Desmidieæ*, according to Mr. Tuffen West, are due to the bulging of the cell-wall at regular intervals in the same manner as the simpler kinds of hairs, and the velvet-like surface of the petals of flowering plants are produced. In flowering plants in general, we have the integumentary system represented by the epidermis and by the cortical layers, structures of much greater complexity than the integumentary system of Thallogens; but even in *Algæ* we have a remarkable foreshadowing of the cortical layers of higher plants, while it can hardly be doubted that a relation of strict homology exists between the gelatinous envelope of some *Algæ*, such as *Palmella* before mentioned, and the cuticle, which overlies the epidermis of flowering plants, and which brings to mind the basement membrane on which the epithelial cells rest, save that the position of the latter with reference to the membrane is reversed, so that the comparison between the anterior elastic lamina of the cornea and the cuticle of plants, would be more apt, though it must be distinctly understood that nothing more than an apparent relation is intended in the comparison. The variations and increased complexity of the integumentary system are perhaps due to, or at least they are co-existent with, the altered circumstances under which these plants have to exist.

For anything we yet know to the contrary, the physiological process of reproduction is essentially the same in the highest as in the

* Cf. Beale's Archives of Medicine, 1861.

lowest plants. In the latter, fructification takes place in any or all the cells, apparently indifferently, but as we proceed higher in the scale we find certain cells specially assigned for this process, and even special organs placed in more or less determinate positions, adapted for the due performance of this function. This distinction in function and local position is one of the first links in the chain reaching from the lowest to the most highly endowed plants. But although in the last mentioned plants there is greater complexity of structural arrangement, and the newly-formed plant partakes also of a higher degree of complexity, yet the formation of a spore and its fecundation by the spermatozoïds, does not appear to differ essentially from the formation of a germinal vesicle and its fecundation by the agency of the pollen cells. In speaking of the reproductive system, it will of course be understood that reproduction by sexual agency is alone intended, as the numerous methods of multiplication by means of buds and the like, appertain more closely to the processes of nutrition and growth than to those of reproduction properly so called.

The fibro-vascular framework is merely a modification of the cellular system, and like it no doubt takes part in the nutritive processes, at least in its young condition, before it becomes blocked up by secondary deposits. It is, however, in its morphological aspect that we here consider it, and as forming a sort of endo-skeleton. Co-existent with it, leaving out of consideration a very small number of exceptions, is the presence of an *axis*. When the plant continued in its primitive cellular state we had a *thallus*, now when fibro-vascular tissue is added we have an "axis," a distinction of as great practical importance as that between vertebrate and invertebrate animals. This fibro-vascular system is foreshadowed among certain *Algæ*, having elongated cells in their centre, surrounded by horizontal cortical cells, while in *Vaucheria*, *Bryopsis*, the stalk supporting the frustule of some *Diatoms*, and many other instances we have no vague representation of the *axis*.

Hardly is the fibro-vascular system developed, before indeed it is perfectly so, than the appendicular system becomes represented by leaves. In *Riella helicophylla* the leaves are even placed spirally. *Jungermanniæ* have two parallel rows of leaves. Mosses have their leaves arranged spirally, and so we pass through Ferns and Lycopods, where stem and leaf are well marked. All these plants produce spores. And now we reach the higher groups of plants characterized by the formation of flowers and producing seeds, in which an embryo is formed in contrast to the spore of so-called flowerless plants. The flowers being merely modifications of the axis and of the leaves derived from it at that point, while the seed may be regarded as homologous with the leaf bud, so very generally found at the junction between the leaf and the axis or stem.

To recapitulate then, it may be affirmed that all plants have alimentary, tegumentary, and reproductive systems, and the most lowly organized plants, such as *Thallogens*, have no others. All plants but Thallogens have, in addition, a fibro-vascular system forming an axis

or stem, divided for the most part into two portions, an ascending one or stem, a descending one or root, co-existent with which is the presence of an appendicular system, if such really be a distinct system, represented by leaves or leaf-like organs. Lastly, in true flowering plants we have in addition to the other systems, the appendicular system more highly developed, and forming what are called the parts of the flower, calyx, corolla, stamens, pistils, &c., and particularly we have a formation of seeds containing an embryo plant. In Thallogens, the humblest of plants, the nutritive function is inseparably conjoined with the circulatory, the respiratory, and the vegetative functions, and this is the case also, though to a less extent, in more highly organized plants, but as there seems, so far as we can yet see, no special organism even in the higher plants set apart for the fulfilment of any one of these processes to the exclusion of the others, and as they may all be included under the general head of nutrition, so here the term alimentary system is intended to comprise all those processes which in the animal kingdom are carried on by distinct organs or sets of organs specially adapted for the purpose. The functions of nutrition therefore which in the lowest plants are carried on in the cells, the only organs such plants possess are in the higher plants carried on in the roots, the stem, the leaves, &c., but still it is by means of cells that the functions are carried on, by cells essentially differing but little from those constituting the entire plant in less highly developed organisms, so that these organs of the higher plants are hardly comparable to the organs of animals in which difference of structure is accompanied by a corresponding diversity of function. If then, physiologically speaking, all the processes of life may be performed by one or more cells, why not, morphologically speaking, may we not have but one organ as we have, indeed, in purely cellular plants—the single cell or the aggregate of many such in the thallus? Thus much every one will admit; but in the higher plants, as they are termed, where conjoined with an increased complexity and diversity of minute anatomical structure, though still purely cellular in its nature, we have an increased number of parts, such as roots, leaves, and flowers, it is assumed that more than one organ is present, as indicated by the terms axis and appendages, the latter being too often looked upon as distinct parts appended to the pre-existent axis; but there is strong evidence to show that, morphologically speaking, there is only one primary organ, call it thallus, axis, frond, or what you will, this one organ purely cellular, being even in its simplest condition quite capable of fulfilling all the essential vital processes. One can imagine such a plant fulfilling all the conditions necessary for its own existence, a very selfish existence it is true, and an existence which Nature as we see is by no means contented with. Plants, like all other works of the Creator, do not live merely for themselves but for others; they carry out the maxim, "*Nemo sibi vivat*," on principles that are not always to the individual equitable principles, and to this end it becomes necessary that they should be endowed with properties which would not be essential, were the life of a plant only of use to itself. Not only are new properties

conferred, but the power of existing under very varied conditions, the power of struggling against adversaries. How is all this effected? not by any real change in internal structure, but by slight modifications of it, not by any addition of new organs, but by the adaptation of the existing fabric to suit the altered circumstances. We know too little at present to dogmatize on these matters however, and it behoves us to be cautious in setting down as a consequence what may merely be a coincidence, although the relation of cause and effect is much more perceptible among plants than among animals. Hence, then, on physiological, no less than on morphological grounds, it is quite consistent with our present knowledge to affirm the existence of but one morphological element in plants, represented by the cell, the thallus, the stem or the leaf in the various groups of plants, and to consider that in the higher groups at least we have this primordial element subjected to various and complex modifications; thus, on this view in the higher plants, the axis and the leaf are considered as parts of one and the same organ co-existing in the majority of cases, and both subjected to those modifications included under the term metamorphosis, while in other cases the one part predominates over the other to its partial or complete suppression. According to this theory of vegetable construction, those otherwise anomalous transitional forms between leaf and stem may be explained.

It would hardly be proper in this place to enter into many details in support of the opinion just expressed; a few instances may suffice to show that there is in reality no such defined limit between axis and appendages as has been attempted to be laid down. Thus, the commonly expressed opinion that the axis increases by the addition of new cells to the extremities and to parts already formed, while nothing is added to the upper part of the appendages; in other words, that they grow from the base, although undoubtedly true in many cases, is by no means invariably so, as the researches of that excellent phytotomist Trécul fully show.*

In plants that consist of mere congeries of cells, as in Thallogens, we frequently have those cells multiplying in such a manner as to produce a leaf-like expansion, as in *Ulva*, for instance. A similar tendency is manifested in higher Cryptogamous plants, as in *Marchantia*, or in the pro embryos that result from the germination of Ferns, &c., where the leaf-form evidently precedes that of the axis. In flowering plants the predominance of the primary leaf-formation is shown in such cases as *Lemna*, and even in the embryo of dicotyledonous plants; the cotyledons are but little in arrear of the axis as regards their development, and may often be considered at the period of germination in advance of the axis, as well as in many monocotyledonous plants—for instance, *Tulipa*† *Allium*.

Moreover, the opinion that in some instances the stem and young shoots of plants are formed from the decurrence and fusion of the leaves is by no means unsupported by facts. Some of the foregoing

* Ann. Sc. Nat., troisième Ser. Bot., tome xx. p. 211, &c.
† Germain de St. Pierre: Bulletin de la Soc. Bot. de France, 1855, pp. 96, 159.

instances may perhaps be cited as merely leaf-like modifications of the axis, and may thus be considered like the leaf-like branches of *Opuntia, Xylophylla, Ruscus, Pterisanthes, Podostomaceæ*, &c., as instances of analogy rather than of homology; but before the truth of this objection can be admitted, the difference between the axis and its appendages must be more distinctly defined than it seems to us can be done at present.

It has been denied by many who contend for the distinctness of the axis and its appendages, and of the necessary pre-existence of the former, and consequent absolute dependence of the leaf-like organs on the axis, that no intermediate stages between stem and leaf exist. If the previously cited cases be not sufficient to impugn this statement, what shall we say to cases such as those afforded by the leaves of *Guarea* and *Trichilia*,* where the leaves after a time assume the condition of branches and develop young leaflets from their free extremities, a process less perfectly seen in some of the pinnate-leaved kinds of *Berberis* or *Mahonia* to be found in almost every shrubbery?†

In the animal kingdom it is stated that there is no real homology between the organs of one great class and those of another, but simply a relation of analogy; and the same distinction is considered to exist in the sister kingdom, but, as it appears with far less reason. The organs of nutrition for instance of a Fungus, are to all intents and purposes the same as in a Rose, although the structure is more complicated and modified in the one than in the other. There is no proof that the cells of the lower classes of plants are different organs from those of the higher; wherein does the difference consist? In origin they are alike, true they are modified in the higher classes of plants, but never to such an extent as to conceal their true nature, and those cells whose nutritive functions are most active are absolutely similar to those in the lower plants, and remain so as long as their functions are active; in origin then, in structure, in function, the nutritive cells are identical; why, then, say that there is no relation of homology, but only the more remote one of analogy?

The difference between the reproductive organs in the lower and higher groups of plants, and the different manner in which their functions are fulfilled, do indeed present much greater obstacles to the notion of their essentially homologous nature, obstacles arising from our imperfect knowledge; that such obstacles are far from fatal is shown, however, by the identity in origin and primary structure of the reproductive organs in the various groups of plants, and we may confidently look to future investigations into this intricate and difficult subject to overcome the difficulties now in the way of the hypothesis, to establish bonds of connexion between the various groups where none now can be shown to exist, and to demonstrate among the Cryptogamic groups especially what has been done so successfully

* Dr. Alexander: Proceedings of the Linnæan Society, May 6th, 1852. Also Dr. Grisebach: Flora of West Indies, *Guarea*.

† Still more conclusive are the fronds of some of the *Podostemaceæ*—e.g., *Lophogyne*, etc.—*See* Tulasne: Monogr. Podostem. Paris, 1852.

among the Phanerogamic—the truth of the principle, Unity in Variety.

Many have been led by the acknowledged inadequacy of the doctrine of final causes to account satisfactorily for the existence of this principle, and by the confessedly entirely hypothetical existence of "ideal types" to make deeper researches into the circumstances that bring about this general harmony of structure. In the number of this Journal for October, 1858, Mr. Hinton concludes an able paper on this subject by an assertion "that organic form is the result of motion in the direction of least resistance;" and similar conclusions are arrived at by Mr. Spencer, who in a paper on the "Law of Organic Form," published in this Journal in January, 1859, says the forms of all organisms are dependent on their relation to incident forces, including under this title those forces to which they are passively subject, and those which they experience as the result of their own action. There cannot be the slightest doubt of the general truth of these statements, impossible though it be at present to reconcile all the known facts relating to the growth of plants with them. Such, for instance, as the division of the cells in one direction or another, the direction of roots and stems, the different ways in which the same object is effected under apparently similar conditions, the influence exerted by the quantity, quality, and locality of the nutriment required by the growing and living organism. These are a few among other instances where the form is affected more or less by the agencies just referred to, but probably to a greater degree by causes of which we know at present little or nothing. Mr. Spencer, indeed, does not rely wholly on such agency, for he says, "conjoined with the law of hereditary transmission this may be the principle underlying all morphology"—a view harmonizing in some degree with that of Mr. Darwin, who says, "we have merely to conceive an ancient progenitor, plant, or animal constructed on the existing general pattern, but whose descendants have become subjected to successive slight modifications, each modification being profitable in some way to the modified form, but often affecting by co-relation of growth other parts of the organism." "The general pattern of an organ might," he continues, "become so much obscured as to be finally lost by the atrophy, and ultimately by the complete abortion of certain parts, by the soldering together of other parts, and by the doubling or multiplication of others, variations which are within the limits of possibility."

Upon morphology of necessity depend the various systems of classification in plants, whether they be professedly artificial, or, when a higher end is aimed at, as in the so-called natural system. In the one, the object is to detect with facility the species of any given plant; in the other, to learn as much as can be learned of its nature and its relationship to other plants; thus, the increased difficulty of the natural system is amply compensated for by the amount and value of the information gained in its prosecution. In both plans the object is the same—to throw together individual plants into groups called species, these again into higher groups, such as genera, orders, &c.

The true nature and limits of species, nay, even the fact of their very existence or the reverse have furnished a constant bone of contention. With one writer, the twist of a petal, the notching of a leaf, if tolerably constant, constitutes a species, while another overlooks slight variations, and masses into one readily definable group a host of plants presenting considerable diversity in form and appearance. Another says species are merely arbitrary creations of the botanist and have no real existence in nature. In questions of this kind the opinions and evidence of those naturalists, who devote their whole attention to this subject, who have the opportunity of examining and comparing large suites of specimens from every possible locality, and grown too under every possible variety of external conditions, whose minds are trained to the careful philosophical investigation of what features are of importance, and what not, of facts which in one group are of the highest value for classificatory purposes, in another of no value at all, is of greater weight than that of other naturalists, whose views are less philosophic, and whose studies range over fewer plants, confined to more limited areas and subjected to fewer variations in external conditions. If the opinions of the latter be nearest to the truth, we shall be obliged almost to assume that there is little or no variation in species, these are the observers, to use Hudibrastic phrase,

> "Who can distinguish and divide
> A hair, 'twixt south and south-west side."

and thus, what their opponents would consider as trifling variations, are considered by the hair-splitters to form distinct species. But if the views and the practice of the former class of observers be most consistent with the facts of the case, then we must assume, as most of us have done, until the advent of Mr. Darwin's wonderful book, that each species is liable to vary within certain limits as yet not defined, and probably very different in degree in different species. But whether or not Mr. Darwin's conclusions be just, there cannot be the slightest doubt, that he has done good service in lessening the breach between the two classes of observers before referred to, and by showing how truly valuable and mutually important are their observations, conducted though they may be on somewhat opposite principles. It is unnecessary here to enter at any length into Mr. Darwin's views, as they have been already commented on in this journal, and his notions as to the origin of, and relationship of existing species are generally known. For a clear exposition, however, of the rules and methods employed by systematists in framing their modes of classification, the thirteenth chapter of Mr. Darwin's book may be with great profit consulted. In his eyes, "the natural system is genealogical in its arrangement, with the grades of difference between the descendants from a common parent expressed by the terms genera, families, orders, &c." An illustration that he gives of this view of classification is so apt and embraces so many of the views held and maintained by him with equal learning and candour, that it may with much propriety be here inserted.

"If we possessed a perfect pedigree of mankind, a genealogical arrangement

of the races of men would afford the best classification of the various languages now spoken throughout the world; and if all extinct languages, and all intermediate and slowly-changing dialects had to be included, such an arrangement would, I think, be the only possible one. Yet it might be, that some very ancient language had altered little, and had given rise to few new languages, whilst others (owing to the spreading and subsequent isolation and states of civilization of the several races descended from a common race) had altered much, and had given rise to many new languages and dialects. The various degrees of difference in the languages from the same stock, would have to be expressed by groups subordinate to groups; but the proper or even only possible arrangement would still be genealogical; and this would be strictly natural, as it would connect together all languages, extinct and modern, by the closest affinities, and would give the filiation and origin of each tongue."*

Before quitting this subject we may be allowed to add a few words on the value of botany, especially of morphology, and classification in the preliminary education of the medical student. It seems to us that the value of botany in this point of view has been hitherto almost overlooked, and students have been, and still are required by some of the examining bodies, to attend a short course of lectures on this subject in their first summer session, when their time should be occupied with subjects of a more practical nature. The result is, in the great majority of cases, that a listless attendance is given by the students, who are utterly unprepared for such instruction, and who find it impossible without previous elementary acquaintance with the subject to get more than a superficial smattering of botanical science, which is speedily forgotten, and conduces to no good result. Of late, however, there have been symptoms of an improvement in this respect, as manifested by the regulations of the University of London, which, in place of demanding an examination in this subject at the first M.B. examination, now very properly requires it to be taken up at the preliminary scientific one. The College of Surgeons allows students at their option to be examined in botany and zoology at their preliminary examination, in subjects of general education. The two great requisites for the successful physician, says Dr. Watson, are "skill in observing and skill in acting;" without the former, the latter is but shameless empiricism; without the latter, the former is but unprofitable pedantry; this being so, it becomes a question how best to foster that faculty of observation possessed by all in very varying degrees. Herein lies the pre-eminent value of a training in some one branch at least of natural history, as preparatory to the effective study of disease at the bedside. It may be asked why a training in mathematics or logic, or why an intelligent study of the principles of language, should not be equally as advantageous to the student of medicine as a knowledge of the principal facts in some one or more branches of natural history. This is not the place to enter at any length into this question; all that need be said is, that both the methods of observation and the methods of reasoning in use among naturalists, are far more nearly akin to those used by physicians in the study and treatment of disease, than they are to those employed by the mathematician or the philologist,

* Darwin: Origin of Species, p. 422.

and this must of necessity be so, seeing that medicine is but a branch of natural, or as it may be called, of vital science, and which cannot be subjected to the same rules and tests as the mathematical or the physical sciences. In support of our opinions, we would especially call attention to the mode of describing plants now employed by botanists. The object is to convey in as terse a manner as possible, a correct description of a plant, omitting nothing essential, inserting nothing superfluous; to accomplish this satisfactorily, much practical knowledge is indispensable, great precision in the use of language, great discrimination in order not to confound things which appear alike, but are in reality different, and equal care not to set down as diverse, things which are essentially the same. In few branches of knowledge is the adage, "*Nimium ne crede colori*," more constantly brought to mind; in few, is there greater necessity for the student not to be led away by the immense variety of form and appearance from the great principle of unity in variety. We would recommend those who have not considered this subject to compare the mode of correctly describing plants as given in a little pamphlet of Dr. Lindley, entitled 'Descriptive Botany,'* with the ordinary mode of reporting cases in hospitals or in the public prints. If such a comparison be made, we feel assured that there will not be many dissentients from the opinion we have ventured to express as to the value of a training in botanical science as preliminary to the more strictly professional branches of education.

* Bradbury and Evans. London, 1860.