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Rev. Patrick Keith F.L.S.

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XXXII. *Phytological Errors and Admonitions.* By the Rev.
PATRICK KEITH, F.L.S.*

THE profound research, the acute discrimination, and the eloquent expression of facts, of arguments, or of opinions displayed in the writings of M. De Candolle, have obtained for their author a high and well-merited reputation, whether as a botanist or phytologist; so much so, that we shall not err if we apply to him the well-known maxim of Horace, which says,

..... Cui lecta potenter erit res,
Nec facundia deseret hunc, nec lucidus ordo.

De Art. Poet. 40.

I had been repeatedly led to make this application of the maxim in perusing different portions of his *Physiologie Végétale*; but especially in a late perusal of the *Considérations Préliminaires* which he affixes to that work, and in which he presents to the notice of the reader a succinct and correct view of the true principles of phytological investigation, marking specifically the path to be pursued and the errors to be avoided. The structure of the several organs of vegetables, as in a state of rest, is the first branch of the study of the phytologist—that is, the anatomy of the plant; their agency in the œconomy of vegetation is the second—that is, the study of the forces with which the organs act, or by which they are acted upon. In the former case we are to explore with the most rigid scrupulosity their hidden and internal structure; in the latter, we are not to ascribe to the vital energies of the plant effects that are merely tissual on the one hand, or chemical on the other; nor to its tissual or chemical properties, effects that are merely vital; but we are to be careful to assign to every effect its true and proper cause†.

Here we have, doubtless, the golden rule of all phytological investigation. Yet the investigator can make use of it only according to the best of his ability; and in the difficulty of accounting satisfactorily for the various phænomena of vegetation, it is not surprising that phytologists should have been occasionally seduced into error, and led to assign effects to wrong causes. The grand stone of stumbling is to be met with in accounting for those effects which are evidently vital, and in which the life of the vegetable seems to approximate closely to that of the animal. This is the respect that has led phytologists to overstep the limits which bound the legitimate scope of vegetable investigation, and to ascribe to vegetables faculties, the existence of which is at best but extremely dubious. Tissual susceptibilities did not seem to them sufficient

* Communicated by the Author.

† *Phys. Végét.*, tom. i. p. 6.

to account for certain phenomena of vegetable life, and hence they assumed the aid of susceptibilities of a higher order,—susceptibilities which in the animal subject are termed cerebral. Yet the leading advocates of this opinion have not been novices; they have not been men of mean reputation, whether in literature or in science; they did not take up the doctrine at random; they took it up deliberately and advisedly, and hence their opinions are entitled to our respectful consideration. Among them we find the names of Bonnet, Watson, Percival, Darwin, Smith. We do not say that M. De Candolle's notice of them is not respectful; but he regards the doctrine as supported more by arguments drawn from imagination and feeling, than from reason and experience, that is, from the heart rather than from the head*; and whether this implies anything of a compliment, we leave the reader to determine.

I find that my name is associated, in a foot note, with that of Bonnet, Percival, and Smith, not for having defended the doctrine of vegetable sensibility, I presume, for that I evidently controverted, but for having mooted the question in my *System of Physiological Botany*, and for maintaining, or seeming to maintain, a doctrine that may be said to imply sensation, namely, that of a vegetable instinct. At a loss to account for the singular, and hitherto inexplicable fact of the irresistible descent of the radicle, in the process of germination, through the intervention of any cause, whether chemical or mechanical merely, I thought it was not absurd to suppose in the vegetable subject, the existence and agency of a cause analogous to that of instinct in the animal subject, but not identical with it†. But although a zoologist is allowed the aid of instinct to account for the *unaccountables* of the animal œconomy, yet let me admonish the phytologist who has any respect for the opinion of the critics, to beware how he lays claim to the aid of an analogous principle to account for the *unaccountables* of the vegetable œconomy. He will be set down immediately as a fool or a blockhead. His case is without remedy, and his sentence without mercy. He will be told that the doctrine “is palpably inadmissible;” that “it is absurd;” and that “it is merely a betraying of his ignorance of the cause.” Did Newton know the cause which he points out to our notice by the term gravitation?

* *Phys. Végét.*, tom. i. p. 29.

† [Agreeing entirely with Mr. Keith in this view of the subject, we may add, further, (adopting Macleay's distinction of relations of analogy from those of affinity, and employing a new word much wanted,) that this vegetable principle, though strictly *analogous* to instinct, is not merely not *identical* but not even *affinal* with it.—E. W. B.]

He knew merely the laws by which a certain principle acts, and he gave it a name. Beyond that he knew nothing. The same is the case with the physiologist who talks of a vital principle, and with the phytologist who ascribes the descent of the radicle to a cause analogous to that of animal instinct. He knows very well that he is ignorant of the cause any further than as producing effects that can result only from the exertion of the vital energies of the plant; and to the energies thus exerted he gives the name of instinct. The best and most acute of our phytologists have felt the strong leaning that exists in the human mind to the admission of some such principle. What says Dutrochet in his *Observations sur la Motilité des Végétaux*? “En voyant cette diversité de moyens employés pour parvenir à un même fin, on serait presque tenté de croire qu’il existe la une intelligence secrète qui choisit les moyens les plus convenables pour accomplir une action déterminée.” (p. 132.)

For myself, I have abandoned the use of the term instinct entirely; not because the admission of vegetable instinct is a confession of our ignorance of the cause,—for it is better to confess our ignorance of a cause than to adduce a false one,—but because it is a term calculated to mislead the phytological student, as being so long usurped by the zoologist, and a term, the legitimate use of which seems to me to be that of the denoting of the act of an individual being regarded as sentient, rather than that of the denoting of the growth of any organ or fabric, whether animal or vegetable. Hence, as I disclaim the doctrine of vegetable sensation*, I disclaim, of necessity, that of vegetable instinct also.

Still I cannot adopt the opinion of those who ascribe the descent of the radicle merely to the force of gravitation. To me the phænomenon seems to be essentially a vital process, which the phytologist is not to confound with any process that is essentially either chemical or mechanical. This is an error, against the commission of which M. De Candolle is at great pains to guard his reader in his *Considérations Préliminaires*†, and yet, after all, he no sooner finishes his admonitions than he seems to me to fall into it himself; for he announces his adoption of Mr. Knight’s theory of the descent of the radicle,—that is, the accounting for a vital phænomenon upon a principle merely mechanical‡.

It is not my intention to enter into the merits of Mr. Knight’s hypothesis at present: a more convenient season may arrive. What I have to remark now is, that M. De Candolle, on what

* Phil. Mag. and Annals, N.S., April 1832.

† *Phys. Végét.*, tom. i. p. 7.

‡ *Ibid.*, tom. i. p. 30; ii. p. 821.

ground I know not, unless as involved in the theory of Mr. Knight, adopts an additional opinion, which I cannot help regarding as erroneous also. It is the opinion by which vegetating roots are supposed to receive their increments of length solely by the extreme points. "Les racines ne s'allongent que par leur extrémité*." That this was the result of some experiments of Duhamel and of Mr. Knight, we do not mean to deny; but it is good only as far as it goes. You are not to deduce from it a rule for all plants whatever. If you do so, you embrace a manifest error which I did my best to expose, so long ago as 1819, in a set of experiments published in No. 76 of Dr. Thomson's *Annals of Philosophy*. Yet I do not find that any one ever condescended to notice them, or to embrace the truth which they establish on account of them. Truth, lovely though she be, if she hopes to be well received, must come with a recommendation in her right hand, that is, a recommendation from some great and acknowledged authority; for there are many sciolists who would rather be in the wrong with a writer of high reputation than in the right with one of minor celebrity. Yet I find that Mr. Lindley has at last taken up the subject, and exposed the erroneousness of the old opinion†, so that from henceforward it is to be believed that botanists will no longer be found to cling to this palpable error. "On the 5th of August," says Mr. Lindley, "I tied threads tightly round the root of a *Vanilla*, so that it was divided into three spaces, of which one was 7 inches long, another 4 inches, and a third, which was the free growing extremity, 1 inch and $\frac{3}{8}$ ths. On the 19th of September the first space measured $7\frac{1}{8}$ inches, the second $4\frac{3}{8}$ inches, and the third, or growing, $2\frac{1}{8}$ inches." A root of *Aerides cornutum* gave a similar result, and established the fact of the general elongation of the root in the plants specified. Yet Mr. Lindley gives all the weight that is due, or perhaps even more than is due, to the experiments of Duhamel and Mr. Knight; for he thinks it is possible that the peculiarity of elongation by the extreme points may be universal in exogenous plants. But the experiments which I instituted on the radicle of the bean show that it is not.

"On the first of October 1818, I sowed some tick beans in a small earthen pan filled with garden mould.

"On the fourth the radicle of the most forward had protruded about $\frac{1}{3}$ th of an inch beyond the integuments, when I marked it with ink at the point, in the middle, and at the base, as clearing the integuments; so that the marks were about $\frac{1}{6}$ th of an inch from each other.

* *Phys. Végét.*, tom. i. p. 41; ii. p. 822.

† *Intro.*, p. 228.

“On the fifth the radicle was $\frac{1}{4}$ th of an inch in length, and the marks nearly as before with regard to their relative distances, but removed evidently from the integuments, so as to admit a fourth or additional mark adjoining the integuments. The radicle, which was originally upright, was now bending down.

“On the sixth the radicle was $\frac{1}{2}$ an inch in length, the first mark being within two or three lines of the point; the second at about $\frac{1}{6}$ th of an inch from the first; the third at about $\frac{1}{6}$ th of an inch from the second; and the fourth at about $\frac{1}{3}$ th of an inch from the third, as well as perceptibly removed from the integuments.

“On the seventh the radicle was $\frac{3}{4}$ ths of an inch in length. The first mark was still within two or three lines of the point; the second was at the distance of $\frac{1}{4}$ th of an inch from the first; the third was at the distance of $\frac{1}{4}$ th of an inch from the second; and the fourth was at about $\frac{1}{8}$ th of an inch from the third, being but little more than its original distance, but removed to the distance of $\frac{1}{3}$ th of an inch from the integuments.

“On the eighth the radicle was one inch in length, the first mark being still near the apex; the second at the distance of about $\frac{1}{3}$ rd of an inch from the first; the third at the distance of $\frac{1}{3}$ rd of an inch from the second; and the fourth nearly as before.

“On the ninth the radicle was $1\frac{1}{4}$ inch in length, the three marks next the base being nearly as before, and the mark next the apex being the only one that was carried down.

“On the tenth, and as long as any further observations were made, it was still the lower extremity of the radicle that was carried down. But enough had been previously observed to show that the assumed peculiarity of the elongation of the radicle is founded in a mistake; and that the root in its incipient state, like the stem in *its* incipient state, is augmented by the intromission and deposition of additional particles throughout its whole mass, or ‘by a general extension of parts already formed,’ though it may afterwards, like the stem, become so firm and compact as no longer to admit of augmentation in that way*.”

As connected with the presumed mode of the root's augmentation, M. De Candolle takes occasion to speak of the spongioles in which the small fibrils of the root terminate. He represents them as being composed essentially of a cellular tissue. “Les spongioles des racines sont essentiellement composées de tissu cellulaire†.” Yet this account of their internal structure does not seem to me to accord well with the

* Annals of Phil., No. 76.

† *Phys. Végét.*, tom. i. pp. 71, 41.
Third Series. Vol. 5. No. 27. Sept. 1834. 2 E

facts of the case, at least as they have presented themselves to my inspection. To me the spongioles seemed to present the palpable rudiments of a vascular tissue also.

It had been asserted by Duhamel and others, that the extreme fibrils of the roots of plants die annually, in the winter, and are renovated in the spring, in the manner, it may be presumed, of leaves. Mr. Knight admitted the position with regard to bulbous roots, but denied it with regard to the roots of trees. To satisfy myself on this point, I took up portions of the roots of several different plants, trees chiefly, which brought the structure of the spongioles unavoidably under my notice.

On the first of September 1828, I took up a portion of the root of a plant of the horse-chestnut, which had come up from seed in the preceding spring. The fibrils, which were for the most part in a vigorous state, presented the appearance of a cylindrical mass of pulp, of a whitish colour, inclosing a firm and central filament, and terminating ultimately in an enlarged and flattened point,—the spongiole of De Candolle,—which, when cut open, was found, as well as the rest of the fibril, to inclose a fine and central filament, or rather a bundle of such filaments united into one cord, like the filaments that compose the nerves of animals. A few had begun to decay, and their decay was indicated by the shrinking and shrivelling up of the external and pulpy mass. But if some were thus in decay, others were beginning to shoot out fresh and vigorous from the larger divisions of the *chevelure*, so that there was no appearance of the commencement of any regular denudation of the root, whether by the decay of fibril or of spongiole.

On the 17th I took up the whole plant, and the appearances were as before.

On the 15th of November I took up a plant of dwarf box, but did not find a single fibril in decay. The newly formed ones were covered with a white, dense, and fine down throughout the greater part of their length, but at the origin and point they were smooth. They were also more tapering than those of the horse-chestnut, and had less of the club-shaped appearance.

On the 28th I took up part of the root of an apricot, but found no fibril wholly in decay. The outer coat of some of them was indeed in decay, but the central filament was still full of moisture, and a new coat forming.

On the first of December I had some portions of the root of an elm-tree taken up, at about ten or twelve feet from the caudex. They were well furnished with fibrils, many of which were in decay, but their loss was more than compen-

sated by a new and vigorous progeny, protruding, like little buds, from the surface of the larger divisions. Like those of the horse-chestnut, they terminated ultimately in a soft bibulous and club-shaped appendage, or spongiole, furnished with a central filament, that seemed to inclose others still smaller. The external and pulpy coat of the spongiole was often found to be in decay, while the inclosed and central filament was still quite sound.

From the above observations respecting roots, the two following inferences are obviously deducible.

I. The root is never wholly denuded of its fibrils, or spongioles, as the branches are denuded of their leaves. A partial decay, with a partial renovation of these organs, seems to be occurring at all seasons; but a total denudation of the root occurs at no season. If, with Mr. Knight, we admit of a total denudation of bulbous roots, I think it will not go beyond such as are taken up out of the soil for the winter; for if the bulb is allowed to remain in the earth, it is to be believed that new fibrils will have begun to be protruded before the old ones have finally decayed.

II. Spongioles cannot be said to be very well described by representing them as composed merely of a cellular tissue, for surely the central filament is to be regarded as exhibiting the rudiments of a vascular tissue also; and if in some cases it is so very fine and minute as to be scarcely perceptible, still it is abundantly perceptible in the cases now specified. I regret, however, that I have not had access to a copy of M. De Candolle's *Organographie*, for there, I doubt not, the structure of spongioles is more minutely detailed, and in such a way, perhaps, as might have precluded the necessity for any particular remark. What I have said is founded on such representations of the structure of the spongiole as are to be met with in his *Physiologie Végétale*.

Charing, Kent, Aug. 1st, 1834.

P. KEITH.

XXXIII. *Proceedings of Learned Societies.*

GEOLOGICAL SOCIETY.*

1834. **A** PAPER "On the Quantity of Solid Matter suspended in the Water of the Rhine," by Leonard Horner, Esq., F.G.S., F.R.S., was first read.

The experiments referred to in this paper, were made by the author at Bonn, in the months of August and November. The apparatus which he used was a stone bottle capable of containing about a gallon, and furnished with a cork covered with greased

* Continued from p. 70.