

XX. *On the Origin and Office of the Alburnum of Trees. In a Letter from T. A. Knight, Esq. F. R. S. to Sir Joseph Banks, Bart. K. B. P. R. S.*

Read June 30, 1808.

MY DEAR SIR,

IN my last communication I endeavoured to prove that the bark of trees is not subsequently transmuted into alburnum ; and if the statements that I have there given be correct, they are, I conceive, decisive on the point for which I contended : and if the bark be not converted into alburnum, the experiments of DUHAMEL, and subsequent naturalists, and those of which I have given an account in former memoirs, afford sufficient evidence that the bark deposits the alburnous matter. If the succulent shoot of a horse chesnut, or other tree, be examined, at successive periods in the spring, it will be seen that the alburnum is deposited, and its tubes arranged, in ridges beneath the cortical vessels ; and the number of these ridges, at the base of each leaf, will be found to correspond accurately with the number of apertures through which the vessels pass from the leaf-stalks into the interior bark, the alburnous matter being apparently deposited (as I have endeavoured to prove in former memoirs) by a fluid which descends from the leaves, and subsequently secretes through the bark.\* I shall therefore venture to conclude that it is:

\* Phil. Trans. 1801, p. 336.

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thus deposited, and shall proceed to enquire into the origin and office of the alburnous tubes.

The position and direction of these tubes have induced almost all naturalists to consider them as the passages through which the sap ascends ; and at their first formation, when the substance which surrounds them is still soft and succulent, they are always filled with the fluid, which has apparently secreted from the bark. They appear to be formed in the soft cellular mass, which becomes the future alburnum, as receptacles of this fluid, to which they may either afford a passage upwards, or simply retain it as reservoirs, till absorbed, and carried off, by the surrounding cellular substance. The former supposition is, at first view, the most probable ; but the latter is much more consistent with the circumstances that I shall proceed to state.

Many different hypothesis have been offered by naturalists to account for the force with which the sap ascends in the spring ; of these hypotheses two only appear in any degree adequate to the effects produced. SAUSSURE, jun. supposes that the tubes contract as soon as they have received the sap in the root, and that this contraction, commencing in the root, proceeds upwards, impelling the sap before it : and I have suggested that the expansion and contraction of the compressed cellular, or laminated substance (the tissue cellulaire of DUHAMEL and MIRBEL) which expands and contracts with change of temperature \* after the tree has ceased to live, might produce similar effects by occasioning nearly a similar motion and compression of the tubes, the coats of which are, I believe, universally admitted not to be membranous. But both these

\* Phil. Trans. 1801, p. 345.

hypotheses are inconsistent with the facts that I have now the pleasure to communicate to you.

Selecting parts of the stems of young trees, from which annual branches had sprung in the preceding year, I ascertained by injecting coloured infusions into the stems, through the annual shoots, that the tubes which descended from the latter, were, at their bases, confined to that side of the stem from which they sprang, and to the external annual layer of wood. Deep incisions were then made into the stems of other trees immediately beneath the bases of similar annual shoots, by which I am quite confident that all communication through the alburnous tubes, with the stem, was wholly cut off: yet the sap passed into the annual shoots in the succeeding spring, all of which lived, and some grew with considerable vigour. I, at the same time, selected many lateral branches, about three lines in diameter, in a nursery of apple trees, which I could easily secure to the stems of the adjoining trees to prevent their being broken. I then made an incision, more than two lines deep in each, on one side, and at the distance of six or seven lines another incision, equally deep, on the opposite side; and as I am quite certain, from the texture of these branches that the alburnous tubes passed straight through them, I am equally certain that every alburnous tube was at least once intersected. Yet the sap passed into these branches, and their buds unfolded in the succeeding spring, the incisions having been made in the winter. But I have repeated the same experiment after the leaves have been full grown in the summer, and still the branches have continued to live.

All naturalists have agreed in stating that trees perspire

most in the summer, when their leaves have attained their full growth, and of course that much sap must ascend at this period; yet at this period the tubes of the alburnum appear dry, and to contain air only; which induced GREW to suppose that the sap rose in the state of vapour; a supposition by no means admissible. Yet it is, I conceive, evident that the sap can not rise, as a liquid, through dry tubes, nor in any state through intersected tubes; and therefore it appears probable that it does not rise at all through the tubes of the alburnum, and that those tubes are intended to execute a different office.

If the sap do not rise through the tubes of the alburnum, it must rise through the cellular substance; yet the passage of any fluid through this has been denied by almost every naturalist, probably because coloured infusions have not been observed to penetrate it, and because many naturalists have considered it as mere compressed medulla. MIRBEL, however, contends that the fluid which generates the new bark exudes from it; and although a fluid, capable of producing the same effects, exudes from the bark, when detached from the alburnum, I am much disposed to coincide with him in opinion, having observed a new bark to be generated on the surface of the cellular substance of pollard oaks, in detached spaces.\* And if the sap in sufficient quantity to generate a new bark can pass through the cellular substance of an oak, it appears possible at least that the whole of the sap may ascend through it. Coloured infusions do not, I think, in any degree, pass through the bark of trees, yet it is evident that the sap passes readily through it; and therefore, should it be

\* Phil. Trans. 1807, p. 7.

proved that such infusions do not penetrate the cellular substance of the alburnum, the evidence which this circumstance would afford would be very defective.

Amongst other experiments that I made to ascertain whether the cellular substance of the alburnum would imbibe coloured infusions, I took off branches of two years old with the annual shoots and leaves attached to them, in the summer, from trees of different species; and I effectually closed the alburnous tubes with a composition formed of calcined oyster shells and cheese,\* and this was covered with a mixture of bees wax and turpentine, so as to effectually exclude all moisture. A part of the bark was taken off each branch, in a circle round it, a few lines distant from its lower end, where the tubes had been closed; and each branch was then placed in a decoction of logwood, in a vessel deep enough to cover the decorticated spaces. At the end of twenty hours, or somewhat longer periods, these branches were examined, and the coloured infusion was found to have insinuated itself between the alburnous tubes, in many instances apparently through the cellular substance. This was most obvious in the walnut tree, the young wood of which is very white. The principal object I had in view in making this experiment, was to detect the passages through which I conceived the sap to pass from the bark into the alburnum.†

From the preceding circumstances, I am disposed to infer that the sap secretes through the cellular substance of the alburnum; and through this I conceive that it must ascend

\* I have found this composition, and this only, to be capable of instantaneously stopping the effusion of sap from the vine, or other tree, in the bleeding season.

† Phil. Trans. 1807, p. 7.

when the tubes were intersected in the preceding experiments, and in those seasons of the year when the alburnous tubes are empty, though the sap must be rising with great rapidity : and I shall endeavour to shew that the presence of the sap in the alburnous tubes, during that part of the year in which trees, when wounded, bleed abundantly, does not afford any decisive evidence of the ascent of the sap through those tubes.

In the last spring, when the buds of the sycamore first began to prepare for unfolding, I found that the sap abounded in the points at the annual branches ; and at the same time it flowed abundantly from incisions made into the alburnum near the root. But when similar incisions were made at the distance of eight or ten feet from the ground, not the least moisture flowed ; and the tubes of the alburnum appeared to contain air only. I also observed that the sap flowed as abundantly from the upper as from the under side of the lower incisions, if not more abundantly, and so it continued to flow to the end of the bleeding season.

The sap must therefore have been, by some means, thrown into the tubes above the incisions, for the quantity discharged from them exceeded more than a hundred times that which the tubes could have contained at the time the incisions were made, even had every tube been filled to the extremity of the most distant branch. And, as it has been shewn that the sap can pass up when all the alburnous tubes are intersected, there appears, I think, sufficient evidence that it must in this case have been raised by some other agent than those tubes.

Through the cellular substance I therefore venture to con-

clude that the sap ascends, and it is not, I think, difficult to conceive that this substance may give the impulse with which the sap is known to ascend in the spring. I have shewn that the bark more readily transmits the descending sap towards the roots than towards the points of the branches ;\* and if the cellular substance of the alburnum expand and contract, and be so organised as to permit the sap to escape more easily upwards from one cell to another, than in any other direction, it will be readily impelled to the extremities of the branches : and I have shewn that the statement, so often repeated in the writings of naturalists, of a power in the alburnum to transmit the sap with equal facility in opposite directions, and as well through inverted cuttings as others, is totally erroneous.†

If the sap be raised in the manner I have suggested, much of it will probably accumulate in the alburnum in the spring ; because the powers of vegetable life are, at that period, more active than at any other season ; and the leaves are not then prepared to throw off any part of it by transpiration. And the cellular substance, being then filled, may discharge a part of its contents into the alburnous tubes, which again become reservoirs, and are filled to a greater or less height, in proportion to the vigour of the tree, and the state of the soil and season : and if the tubes which are thus filled be divided, the sap will flow out of them, and the tree will be said to bleed. But as soon as the leaves are unfolded, and begin to execute their office, the sap will be drawn from its reservoirs, and the tree will cease to bleed, if wounded.

The alburnous tubes appear to answer another purpose in

\* Phil. Trans. 1804, p. 5.

† Ibid.

trees, and to be analogous, in some degree, in their effects, to the cavities in the bones of animals ; by which any degree of strength, that is necessary, is given with less expenditure of materials, or the incumbrance of unnecessary weight ; and the wood of many different species of trees is thus made, at the same time, very light, and very strong, the rigid vegetable fibres being placed at greater distances from each other by the intervention of alburnous tubes, and consequently acting with greater mechanical advantage, than they would if placed immediately in contact with each other.

I have shewn in a former communication, that the specific gravity of the sap increases during its ascent in the spring, and that saccharine matter is generated, which did not previously exist in the alburnum, nor in the sap, as it rose from the root : and I conceive it not to be improbable, that the air contained in the alburnous tubes may be instrumental in the generation of this saccharine matter. For I discovered in the last autumn, that much air is absorbed, or at least disappears, during the process of grinding apples for the purpose of making cider, and that during this absorption of air, the juice of acid apples becomes very sweet, and acquires many degrees of increased specific gravity ; and a similar absorption of air, with corresponding effects, is well known to take place in the process of making.

I shall conclude with observing, that in retracting the opinion I formerly entertained respecting the ascent of the sap in the alburnous tubes, I do not mean to retract any opinion that I have given in former communications respecting the subsequent motion of the sap through the central vessels, the leaves, and bark ; or the subsequent junction of the descend-



ing with the ascending current in the alburnum: every experiment that I have made has, on the contrary, tended to confirm my former conclusions.

I am,

MY DEAR SIR,

Your much obliged, obedient Servant,

THOMAS ANDREW KNIGHT.

*Elton, June 15, 1808.*