

XV. *New Observations upon Vegetation.*  
*By Mr. Mustel of the Acad. of Sciences*  
*at Rouen; translated from the French.*

Read Jan. 14,  
 1773. **M**ANY celebrated writers, induced by the analogy, which they observed betwixt the vegetable and animal kingdoms, have admitted the circulation of the sap in the one, in a similar manner to the circulation of the blood in the other.

This important point of vegetable œconomy produced a diversity of opinions, and has not yet been sufficiently cleared up.

Dr. Hales, in his *Vegetable Staticks*, does not seem to embrace the system of the circulation of the sap; but he does not prove the contrary\*.

\* *Il ne prouve pas contre.* This certainly is a mistake. Dr. Hales, in the IVth Chapter of his *Physical Staticks*, not only declares openly against the doctrine of the circulation of the sap, and overturns the arguments alledged in favour of this opinion; but he produces several new experiments, which prove directly the impossibility of such a circulation. (See p. 144, &c.) His reasons have been thought so convincing, that the system of the circulation in plants has been ever since exploded in England; and that they have had a similar effect abroad, appears from the following quotation from a book of the ingenious Mr. Bonnet, F. R. S. of Geneva, intitled *Recherches sur l'usage des feuilles*, printed in 1754, p. 269. "Pour moi, persuadé de la fausseté de cette opinion (que la sève circuloit comme le sang) par les expériences de M. Hales (Ch. IV.) &c." M. M.

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Mr. Du-Hamel, in his *Physiology of Trees*, contents himself with relating what has been said for or against this opinion; but, though he sufficiently hints that he does not believe it true, he determines nothing about it. The friends of the circulation in plants have never been able to find in them any thing analogous to that powerful organ, which is the promoter of it in animals; for want of such an organ, they were forced to imagine valves and paps in the lymphatick vessels of plants, by means of which the liquors once introduced into the sap vessels were supposed to be hindered from going back; but, unfortunately, no body has ever been able to discover these valves and paps, so different from the simple contrivances, by which nature is used to arrive at her ends.

An experiment, which I made, and of which I propose giving an account in this paper, throws a great light upon this question, as well as upon several others; and the conclusions deducible from it appear to me decisive.

On the 12th of January, I placed several shrubs in pots against the windows of my hot-house, some within the house, and others without it. Through holes made for this purpose in the panes of glass, I passed a branch of each of the shrubs, so that those on the inside had a branch without, and those on the outside one within; after this, I took care that the holes should be exactly closed and luted. This inverse experiment, I thought, if followed closely, could not fail affording sufficient points of comparison, to trace out the differences, by the observation of the effects.

The 20th of January, a week after this disposition, all the branches that were in the hot-house began to disclose their buds. In the beginning of February, there appeared leaves, and towards the end of it, shoots of a considerable length, which presented the young flowers. A dwarf apple-tree and several rose-trees, being submitted to the same experiment, shewed the same appearance then as they commonly put on in May ; in short, all the branches which were within the hot-house, and consequently kept in the warm air, were green at the end of February, and had their shoots in great forwardness. Very different were those parts of the same tree, which were without and exposed to the cold. None of these gave the least sign of vegetation ; and the frost, which was intense at that time, broke a rose pot placed on the outside, and killed some of the branches of that very tree, which, on the inside, was every day putting forth more and more shoots, leaves, and buds, so that it was in full vegetation on one side, whilst frozen on the other.

The continuance of the frost occasioned no change in any of the internal branches. They all continued in a very brisk and verdant state, as if they did not belong to the tree, which, on the outside, appeared in the state of the greatest suffering. On the 15th of March, notwithstanding the severity of the season, all was in full bloom. The apple-tree had its root, its stem and part of its branches, in the hot-house. These branches were covered with leaves and flowers ; but the branches of the same tree, which were carried to the outside, and exposed to the cold air, did not in the least partake

partake of the activity of the rest, but were absolutely in the same state, which all trees are in during winter. A rose tree, in the same position, shewed long shoots with leaves and buds; it had even shot a vigorous branch upon its stalk, whilst a branch which passed through, to the outside, had not begun to produce any thing, but was in the same state with other rose-trees left in the ground. This branch is four lines in diameter, and eighteen inches high.

The rose-tree on the outside was in the same state; but one of its branches drawn through to the inside of the hot-house, was covered with leaves and rose-buds. It was not without astonishment that I saw this branch shoot as briskly as the rose-tree which was in the hot-house, whose roots and stalk, exposed as they were to the warm air, ought, it should seem, to have made it get forwarder than a branch belonging to a tree, whose roots, trunk, and all its other branches were at the very time frost-nipt. Notwithstanding this, the branch did not seem affected by the state of its trunk; but the action of the heat upon it produced the same effect as if the whole tree had been in the hot-house.

It would be useless to give an account of the diary I kept throughout the course of this interesting experiment. It may be sufficient to observe, that the walk of nature was uniformly the same. The interior branches continued their productions in a regular manner, and the external ones began theirs at the same time, and in the same manner, as they would have done, had they been left in the ground. The fruits of the in-

terior branches of the apple-tree were, in the beginning of May, of the size of nutmegs; whilst the blossoms but just began to shew themselves on the branches without. I shewed Mr. Du-Tillet, of the Academy of Sciences at Paris, on his passage through this town, the effects of my experiments, and likewise communicated to him another observation, which chance occasioned, and ought not to be omitted.

I observed that three of the flower buds of the apple-tree had been gnawed off by a snail in such a manner, that all the petals and stamens had disappeared, being eat up close to the calyx. This not having been entered by the snail, the basis of the pistillum and the embryo were preserved.

I took it for granted that these flowers would bear nothing; but I was soon convinced of my mistake. Almost all of them bore fruit; the apples were perfectly formed, and six or seven pretty large ones too were seen upon each bunch. On the other hand, the snail had spared some other bunches, (doubtless because more difficult to be got at;) but out of ten or twelve flowers in each bunch, not above one or two shewed any signs of fruit. This suggested to me the idea, that, when the flowers of trees are full blown, the prevention of the natural fall of the petals and stamens gives a greater assurance of the fructification; and on several times repeating the following experiment, I convinced myself that it did so. In imitation of the snail, I cut with my scissars the petals of apple, pear, plumb, and cherry blossoms, close to the calyx. Almost every one of those,  
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which were thus cut, succeeded, whilst several of the neighbouring flowers mis-carried.

Thus did a snail teach me how to render a tree fruitful ; nor is it the first time that animals have been the instructors of mankind. I confess, however, that this process is not very practicable in a large orchard : but it might be adopted in an espalier ; in which one would chuse to procure a great deal of fruit from trees of the best sort. It may indeed be questioned, whether the suppression of the stamens would not render the fruit barren ; and in fact I found, that, though the flowers of the dwarf apple-tree, whose petals and stamens were eat up by the snail, gave me apples equally large and beautiful, and that, when I came to open them, I found the capsules formed as usual at the center of them ; yet they were entirely empty, without the least appearance of a pip. Absolute fructification consequently did not take place ; since botanists, with reason, call nothing fruit but the seed, which contains the germen, which is to perpetuate the species. All the other parts, being only intended to co-operate in the formation and preservation of the seeds, perish of course, when once the seeds are come to maturity and perfection, and the work of nature fulfilled.

Another remarkable thing in these apples is, that in the upper part there was found a much deeper cavity than usual. It was eight or nine lines deep. The orifice of this cavity was bordered by five tubercles, indented and somewhat elevated ; but there was no vestige of the calyx, which, it is well known, remains always to the

upper part of apples and pears, and is commonly called the eye.

I now return to my first experiment; the consequences of which, as I have described them, seem to prove,

I. First that the circulation of the sap does not take place in plants, as the circulation of the blood in animals. This may be deduced from the following observations.

The tree in the hot-house went through all its changes during the winter, and the branch exposed to the open air underwent none; consequently the sap, which was in action in the root, stock, and head, of the tree, did not circulate through the branch without; which had no share in the vegetation of the roots and trunk. It might, indeed, be argued that the cold air, to which this branch was exposed, stopped the circulation, and therefore that the first experiment would not be decisive; but the inverse of it seems fully so.

The tree placed on the outside of the hot-house continued, during the whole winter, in the state of numbness, natural to all trees, which are exposed at that season; but one of its branches, which was in the hot-house, put forth successively its buds, leaves, blossoms, and fruits. Whilst therefore the root of the tree, to which this branch belonged, was in the ground so frozen, that the pot itself, in which it stood, was broken by it, whilst the stock and top of the tree were so covered over with ice, that many of the branches were killed; this branch alone did not in the least partake of the common state of numbness and suffering, and was on the contrary in full vegetation.

tion. The sap in it must have been extremely rarefied, and in very quick motion, whilst that of the tree was greatly condensed, and in total inaction. How is it possible to conceive a circulation of the sap from such a frozen root and stock, to a branch full of vigour, and loaded with leaves and flowers? Surely this experiment must appear conclusive against the system of circulation; since in this case it could at best only be admitted to have taken place in the vegetating branch; and that would very improperly be termed circulation, which should be confined to one limb.

II. This experiment proves, that each part of a tree is furnished with a sufficient quantity of sap to effect the first production of buds, flowers, and fruits. There is little probability that the branch drawn into the hot-house should have derived its sap from the roots of the tree: as they, at that time, lay in a very small quantity of earth, rendered extremely hard and dry by the frost, they could have but little liquor to spare; and even this, considering the congealed state of the lymphatick vessels of the stock, could have found no passage to the branch. This branch must of course have been enabled to continue its vegetation by the quantity of sap with which it was provided, the consumption of which must have been supplied at the first breaking of the frost. This truth, now demonstrable by experience, had been pointed out before by a multiplicity of other facts. Every body may have observed that a tree, which has been blown down in autumn, though separated from its trunk, begins the same vegetation, that it would have done if it had remained standing.



ing. Its buds open, it bears leaves, and even shoots, which sometimes are very long, and must be the effects of the sap it contained. It is true, indeed, that this appearance does not continue long, because the provision of sap once exhausted, without being renewed, every thing must of necessity perish.

An effect of the like kind often deceives us in trees, that have been newly planted, and in scions, which produce flowers and even fruits, without ever having taken root. But in this case the symptoms, which would seem to promise life, are on the contrary the forerunners of death; because the leaves, being from their nature the most powerful organs of transpiration and dissipation, the graft is the more readily exhausted, when there is no root to furnish it with a fresh supply of nutritive juices.

III. This experiment proves that it is heat, which unfolds the leaves, and produces the other parts of fructification, in the branch exposed to its action.

Autumn is the time, in which Nature employs itself as it were clandestinely, under the cover of the leaves, in forming the buds, which contain the rudiments of the leaves, blossoms, and fruits, that are to be produced in the course of the succeeding summer. These buds prepare and work themselves out, during the winter, under the rough coats, that are destined to preserve them from the injuries of the weather. As soon as the warm weather in the spring begins to be felt, the buds open, and their coats, which then become useless, drop off, and give place to the productions, which  
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they contained and preserved. Immediately after this, the blossoms, flowers and fruits make their appearance. This is the usual operation; but in the case before us, nature was, as it were, surprised by art: what she should not have done till spring, she did in the winter, because the heat of the hot-house produced that expansion, which, according to the natural course, ought to have been effected by the rays of the sun darting less obliquely than before upon the horizon. There is no doubt but it is to heat, either natural or artificial, that this expansion is owing; and the experiment proves that it is only in that part of the tree, which is exposed to the effect of heat, that the sap, which in every other part remains torpid and inactive, is put into motion, and produces vegetation. From this, it appears that the vegetable oeconomy is different from the animal, and that those, who endeavoured to establish the circulation in both, carried their analogy too far.

This fact, now established, furnishes a good reason why in the tapping of the maple and sugar-birch-trees, so much liquor runs out on one side, and none at all on the other. It is well known that, if during the time of a frost, or a summer's day, towards noon, you bore a hole on the side of the maple-tree exposed to the south, you will get a great quantity of liquor from it; and that if you bore the north-side at the same time, you will not get a drop. The cause of this evidently appears from what has been said. One likewise sees why trees exposed to the south lose a great many of their branches, and sometimes die altogether, in the course of a severe winter; whilst

trees of the same sort, but placed to the north, or in some other exposition, will stand the hardest frosts. This is particularly remarkable in the ever-greens, whose resinous and oily sap being liquefied by the heat of the sun, the tree cannot escape suffering a great deal, whenever it is surprized in that state by the night frosts. Those observers, who attend to this, and know how well pines, firs, and bays succeed, when planted on the back of mountains exposed to the north, will take care not to place such kind of trees in a southern aspect, in hopes of their succeeding better by it.

Many other consequences might be drawn from these experiments; but the bounds, I have assigned to this paper, do not allow it. I propose examining them more at large in a treatise upon vegetation, which, I hope, the observations and experiments I have made, may render interesting and useful.