XVIII. *On the Direction assumed by Plants. By Professor* Macaire *of Geneva. Communicated by* P. M. Roget, *M.D., Sec. R.S.*

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§ 1. *On the Curling-up of Tendrils.*

THE plants with tendrils are very numerous. According to Mr. Palm there are about five hundred, divided into seventeen families. Of these, one hundred and sixty have a ligneous stem, eighty-three are perennial herbs, and one hundred and seventeen are annuals.

My experiments on the mode of curling-up of these organs were made on the tendrils of the *Tamus communis**, a plant of the family of the Asparageæ. The tendrils of this plant seem to be a thread-like degeneration of the footstalk of a leaf, whose place they occupy on the stem of the plant. They are at first straight, and are implanted perpendicularly on the stem, so as to form almost a right angle with it; the extreme end of the tendril only has a slight tendency to bend towards the stem. When the tendril of the Tamus is touched by any solid body whatever on a point of its surface not too far from the extremity, it contracts itself from the outside inwards, forming at first a hook and then a curl, so as to embrace the body closely if that body be circular; if angular, the knot is only tight on the angles, and bulges out on the surfaces. When a first knot is tied, the end of the tendril continues to roll itself up in a coil, though not in contact with the body in that part, and the coil slides over the external object, coming nearer and nearer to it so as to embrace it several times: in the mean while, the other end of the tendril continues also to contract itself. In this way as many as seven or eight knots are formed. I have frequently seen three tied before my eyes within the space of a quarter of an hour on a metallic wire, small branches of wood, a pencil, my finger, &c. The contact of any solid body whatever is sufficient to produce this effect; so much so, that although the tendril is evidently destined by nature to support the creeper to which it belongs, by means of the surrounding plants, yet if it chances to meet a part of the very same plant of Tamus of which it is itself a portion, the contact causes it immediately to roll itself up around that portion.

* Since this paper was read the author has been informed that *Tamus communis* is a plant without tendrils. The plant on which he made the experiments here referred to is a common weed in the gardens in Switzerland. Being without the means in this country of identifying it, he must supply the information on a future occasion, only adding that *Smilax aspera*, another of the Asparageæ, has been suggested to him as being probably the plant.

The tendril of the Tamus is very smooth, and its surface contains neither resinous nor glutinous matter, nor hair of any kind. When slightly rubbed between the fingers, it does not contract itself. To obtain its curling up, it is necessary that the contact should be only on one side. If the solid body round which the tendril had begun to coil be removed, it continues to contract itself in the air, but without fastening the knot on itself, the empty ring remaining constantly open. If, after a little time, an object of suitable size and form be introduced in this empty ring, it contracts anew and the knots tie themselves firmly on this new body.

If the tendril be left without internal support after its contraction, it does not turn again, nor resume its primitive direction in a straight line; on the contrary, the contractions are soon extended over the whole of the tendril so as to give it the appearance of a corkscrew.

The same thing happens to those tendrils the extremities of which are attached to a supporting body. This arrangement has the effect of preventing the tearing of the tendril when the plant is shaken by the wind, by giving it the shape and elastic properties of an helicoidal spring (*ressort à boudin*). I placed a small portion of a branch in contact with a tendril of Tamus; when it had begun to contract itself and the first knot had been tied, I let the branch go and it remained suspended. Not only did the tendril support the weight of the branch, but it continued to roll itself up around it, raising it more and more by each knot. Ten rings were thus formed around the slip, regularly arranged in a spiral by the side of each other. The branch was entirely covered over by them, and as there was no room for more, the tendril continued to contract itself in the air towards its base, and to form empty rings in the form of a corkscrew, having nearly the same dimensions as those on the branch.

When a body, such as an iron rod, too heavy to be supported, is placed in this way, the knot formed becomes loose and the rod drops. If the tendril rolls itself round a body that is soft and not elastic, such as a piece of packthread, it presses it tightly enough to render its diameter visibly less in the part where the knots are tied. This pressure may even be rendered sensible to the touch if the knot be suffered to form round the finger, and it goes on increasing to a certain extent.

When the tendril lays hold of an elastic body having a conical shape, such as the flat part of a leaf rolled up in a funnel, the knots slide over the leaf as they are formed and suffer it to escape. When a tendril of Tamus has begun to curl near its extremity and to fasten itself round any object, if the upper portion of the same tendril chance to meet with another exciting body, another part of the same branch for instance, it may curl over again in a spiral at this point and tie its knots there. The same thing may happen a third time; and in this way may be seen in the same tendril two or three portions closely wound round an object, while the remaining part of the tendril is loose and detached. The contraction of the tendrils of the Tamus always takes place in the same direction, and the curling is turned inwards, whether there be or be not an object round which it may occur: and more than this, when a round body is put in contact with the external surface of a tendril in its straight form, it does not curl round the body which excites the movement, but on the contrary from the outside inwards, although there is nothing there for it to embrace.

This tendency to curl only on one side has induced me to examine the tendrils, to see whether I could discover by means of the microscope a special anatomical structure that might explain this peculiarity.

I have made observations with the microscope of AMICI and a magnifying power of 800 times on very thin slices of the tendril, taken some from the external, and some from the internal side. When the specimens were cut out of tendrils already contracted in a spiral form, the only visible difference was that the cells on the internal side appeared very little elongated and almost square; while, on the contrary, they were much narrower and longer on the opposite side. But this appearance was evidently mechanical and produced by the contraction itself; for when I examined slices cut in all directions from tendrils still in their straight form and not contracted, I found the anatomical structure to be precisely the same in all, and indeed similar to that of those leafstalks of the same plant which are not susceptible of being curled up.

I caused a straight tendril of Tamus to curl by the excitement occasioned by the touch of a pencil. Two knots were closely tied within the space of ten minutes. I then cut out the tendril near its base, so as to interrupt its communication with the plant; the tendril, though kept in the light and in a warm and damp atmosphere, did not continue to curl itself.

When a straight tendril of Tamus is placed in a tumbler full of water, so as not to touch its edges, it does not experience any contraction, but is just as ready as before to curl up by the first contact of any solid body.

I have immersed a tendril many times over in a solution of gum-arabic, leaving it to dry between each successive immersion, so as to form round it a thin coat; the tendril so covered had not in any degree lost its power of contraction by the contact of a solid body, and the coat of gum did not seem to prevent the close adherence of the knots.

Liquid ammonia in a diluted state has no influence on tendrils. Alcohol and eau de Cologne seem to possess a slight exciting influence on the contraction of tendrils, which are thereby bent in the form of a bow, but do not continue to curl, and they appear not to experience any bad effects. When immersed, with the same precautions, in sulphuric acid diluted with water, the tendril contracts itself immediately and energetically. It curls up in a spiral form, though there be no object present round which it might roll itself. The knots, at first very loose, continue to tighten up to a certain point, although there always remains an empty space in the middle of the rings. After some time, the part touched by the acid becomes disorganized and dries up.

Diluted nitric acid has the same effect as sulphuric acid, but the contraction is less rapid. The fumes that escape from the acid, without any actual contact with the liquid, cause the tendrils to contract and curl in the air, though in a less prompt and energetic manner than when immersed in it. The parts of the tendril that have been steeped in nitric acid, are afterwards found withered and dead. The tendrils, on the contrary, that have curled up by simple exposure to the nitrous vapours do not wither.

A solution of corrosive sublimate seems slightly to excite the contraction of the tendrils of the Tamus; but after a few hours the tendril so treated withers and dies.

I immersed in prussic acid, prepared by Scheele's process, a tendril of Tamus in its straight and vigorous state, carefully preserving it from contact with the vessel. I kept it there for two minutes and then took it out. The tendril did not seem to have experienced any alteration; but when I placed in contact with it a foreign body to excite it to curl up, no contraction could be produced; and although the tendril was to all appearance fresh and healthy, it could not be made to curl round. When a tendril that has been previously excited and has begun to curl itself in a spiral form is plunged into prussic acid, it loses the power of continuing its contraction, and the knots that were commenced do not become tied. Two days after such an experiment a tendril was found precisely in the same state in which it had been left, although the curling had begun on a branch of proper size, and the tendril appeared endowed with complete vegetative power. I tried if I could destroy with ammonia this narcotic influence of prussic acid, but without any decided results. A fresh and healthy tendril of Tamus was excited till it began to curl, and then immersed for two minutes in prussic acid. The knot in its incipient state was placed on a branch of precisely the same size as that by which it had been excited, and I observed every hour what took place. During five days that the experiment was followed up, no change was produced: the knot begun did not become tighter, nor was any other knot formed. The tendril remained to all appearance perfectly healthy; but the prussic acid seemed to have destroyed in it all power of contracting farther. These experiments require to be continued and multiplied; but they warrant the conclusion, that whatever be the cause of the irritability of the tendrils, their curling-up cannot be explained, as Knight thought, by the unequal action of the light on both sides of the tendril; nor as Decandolle admitted, by the obstacle afforded to vegetation by the contact of the leafstalk with the body on the side where it touches it. The rapidity of contraction in the tendrils of the Tamus cannot be accounted for by so slow a process. This irritability is a vital property inherent in the organ itself, but which ceases when it is separated from the parent plant, and which, as I have already shown to be the case with those plants called sensitive, is excited, modified, and even suspended or destroyed by the influence of poisons, either of the vegetable or mineral kingdom.

§ 2. *On the Inclination of Stems towards the Light.*

Every one knows that the branches and green parts of plants have an invariable tendency to direct themselves towards the most luminous quarter of the place in which they grow. The experiments of M. Teissier, who caused plants to vegetate in a cellar where they received air by an opening that did not give light, and light by a window which did not admit air, have shown that it was really light and not air that the plants were in search of. Decandolle imagined that he could account for this phenomenon by the inequality of the development of a plant that receives light only on one side. The lighted side, he says, decomposes more carbonic acid, solidifies a larger portion of carbon in its tissue, and becomes more solid; at the same time it exhales more water, and consequently contains more of the inorganic matter left behind, which contributes to harden it still further. The other side remains softer, the fibres are more elongated, and the consequence is the bending of the branch towards the side which is least elongated, that is to say, towards the light. This illustrious botanist has extended the same reasoning to the curling of tendrils; but I have already attempted to show that it could not be applied to this rapid physiological action in those of the Tamus.

It was necessary to investigate if there exists a special attraction exercised by light on the green parts of a plant, and in particular if a plant so placed as to be able to move, could be brought to change its position under the influence of light. My experiments on that head have been made under two circumstances of a different nature.

The subjects of the first were some special plants of the family of the Nayadeæ, the *Lemna minor* and *Lemna polyrhiza* (duck-weed), which naturally float on the water. These plants form a kind of anomaly in the vegetable kingdom, in as far as they consist of a single floating leaf, having submerged roots and producing at their margin new leaflets that often detach themselves from the mother plant, like the polypus, to form a separate individual; they are in consequence viviparous. I have nevertheless ascertained by experiment that they receive from the action of light the same influence as other plants; thus, if left on the water in complete darkness, the new leaves that shoot appear blanched and yellowish. If immersed entirely in spring-water and exposed to the rays of the sun, they remain green and vigorous, and emit a great quantity of bubbles of oxygen gas that gathers by degrees in the upper part of the bell.

I arranged an oval and elongated vessel so that, being half-full of water, one-half of it was kept in darkness by means of a diaphragm which was placed in the middle, and kept one line above the surface of the water. The part of the vessel that was intended to be kept dark, was carefully covered over with many sheets of black paper: the other half of the vessel was freely exposed to the influence of light. The apparatus was arranged in a locality perfectly free from disturbance and from the agitation of the wind. A great number of floating plants of Lemna were successively placed in the dark part of the vessel. Though they could freely move on the water by the smallest impulse, I never saw them come near the lighted part or move into it, even when the experiment was continued long enough to allow the symptoms of blanching, already mentioned, to manifest themselves in the Lemnæ. No motion towards the light was ever perceived. It is to be understood that, for such a result, it is necessary that there should not be any possibility of shaking or vibration in the liquid, and a first experiment, undertaken without sufficient precautions in that respect, gave results by which I was at first deceived. My doubts on this result were excited by the following experiment. In a tumbler completely covered with thick black paper, I put some water on which I placed a few floating plants of Lemna. A narrow slip was cut out in the paper on the side opposite to the light, so as to bring a single ray across the vessel. The apparatus, covered over with a thick book, was left in a place not exposed to vibration. The Lemnas did not change the position which they had been placed in, and, far from arranging themselves, as I expected, in a line along the luminous ray, they did not move at all towards the illuminated space. Varied in different ways, the experiments with the Lemnas have always given negative results to the supposition that they have a real attraction exercised on them by light.

I have endeavoured to repeat the experiments on other plants. I have ascertained that seeds of peas, French beans, mustard, &c. put on floating pieces of cork in a vessel filled with water, not only did germinate, but that the plant produced could develope itself without any other care than replacing the evaporated or absorbed water. It could thus furnish a complete vegetative course, expand its leaves, make its flowers blow, and even bring its fruit to maturity; only the stems are very slender and the leaves smaller than usual.

The germination of seeds took place pretty nearly in the same way, whether they were exposed to light or kept in complete obscurity. As it had been said that light exercises an influence on the direction of roots, I made an experiment with four peas, placed on floating pieces of cork. The first (No. 1) was left to germinate and grew in a common glass tumbler; the second (No. 2) in a tumbler covered with black paper, but leaving a thin stream of light to enter by a slit in the paper; the third (No. 3) in a glass coloured blue, and admitting only a thin stream of blue light; and the last (No. 4) was kept in complete obscurity. The seed in the free light developed itself well, and in twenty-four days exhibited a strong stem of a fine green colour, five inches long and well-stocked with leaves. Nothing remarkable occurred in the roots.

No. 2, receiving only a ray of common light, germinated well and reached its full growth. After eighteen days its stem was four and a half inches in length. The root was very long, six inches at least, and was covered with small branching roots, which had all been formed on the side of the opening that admitted light, and had directed themselves towards it. I turned the plant over so as to place the side of the large root deprived of small radicular threads opposite to the luminous slit. Five days afterwards, radicular fibrils had sprouted on that side; and those which had developed themselves before, had indeed not perished, but had ceased to grow. The experiment was completed after thirty-four days. The root was then a foot long, was covered on all sides with radicular threads, and had turned itself into a spiral form. The stem was slender, very little coloured, and had a length of ten inches and a half.

No. 3, receiving the blue rays, had, after eighteen days, a stem only three inches long, and the root measured four inches. The radicular threads were all on the side opposite to the slit giving admission to the blue light. I changed the position of the plant, and placed the radicular threads on the luminous side. Thirty-four days from the beginning of the experiment, radicular threads had sprouted on the side of the root where there were none before, and the root had also taken a spiral shape. It was seven inches long; the stem, nine inches long, was very slender and almost white.

No. 4, in the dark, germinated after five days, but a few radicular threads alone developed themselves without a stem.

These trials were repeated on peas and mustard-seeds, and gave results of the same nature, as far as concerns the curling of the roots in a spiral shape under such circumstances, and the tendency of the white light to favour, and of the blue light to hinder, the growth of radicular fibrils.

The experiments relating to the direction of the stems were made pretty nearly in the same way as with the Lemnæ; only the vessel in which the floats were placed was much larger, and the diaphragm was not so near the surface of the water. The consequence was that the darkness of one-half of the vessel went on increasing from the diaphragm towards the extremity of the covered portion, and though very intense at that last part, was nevertheless not complete. As soon as the seeds had germinated, they were placed on the cork floats, in which I had bored a hole through which the root emerged into the water. The float was placed in the dark part of the vessel, far enough from its side to avoid the capillary attraction, and it could be moved by the gentlest impulse. It is hardly necessary to say that the apparatus was kept perfectly free from shaking or from the action of the wind.

On the 14th of May, a pea in germination was placed on the float in the dark part of the vessel: it grew very slowly, and on the 22nd began to exhibit a very short and almost white stem. This stem by degrees became longer and longer, spreading itself along the surface of the water, and on the 8th of June it had reached the diaphragm. It formed then a long stem, etiolated, slender and white, running along the surface of the water; its length was two feet. The cork float had not moved forwards a single line towards the luminous aperture; only the weight of the plant throwing itself, as it were, all on one side, had by degrees bent down the cork float, but on the same spot it before occupied. When once the stem of the pea had passed under the diaphragm and had reached the illuminated part of the vessel, it grew in an erect position, became green, and began to develope leaves.

This experiment, very often repeated with peas, French beans, &c., always gave similar results. Never did any attraction of the whole plant towards the light manifest itself; and however long a stem it might be necessary to produce in order to reach the diaphragm, that was always the means to which the plant had recourse.

In order to vary the mode of experiment, I placed on a cork float in the obscure part, a plant of French beans already developed, green and strong, and the roots of which were immersed in the water. The green stem took little or no ulterior development; but from the neck of the root there grew out another stem, white and etiolated, that spread itself along the water to reach the diaphragm and the light portion of the vessel; there it grew erect and gave forth its leaves. The float had not changed its place, and, as before, had only been bent down by the weight. The same experiments repeated with green and vigorous peas, gave precisely the same results.

Germinated seeds of mustard placed on very light floats in a tumbler half-full of water and surrounded with black paper, so as to admit only a luminous ray, were put in the dark portion, but very near the aperture. One of the plants grew and sprouted a stem that went all round the tumbler to come and spread its leaves in that part of the vessel where the luminous aperture was, and once there, the plant did not pass beyond that point. It grew erect in that part, though the light was too weak to render it entirely green. No motion was perceptible in the float; and it is very remarkable that, being close to the luminous aperture, the plant in search of the light had taken this long circuit rather than communicate to the float the slight motion that would have placed it in possession of it. I venture to conclude from these experiments, that the direction of plants towards the light is not the result of an attraction, properly so called, which, similar to the physical attractions, could carry over the attracted body towards the agent.

The ingenious supposition of DECANDOLLE of a mechanical bending, due to a greater solidification of the fibres on the light side than on the other, appears to me not to be more applicable to the direction of stems towards the light, than to the curling of tendrils; for, by the arrangement of the apparatus, the lightest side was not always the same, and the stems advanced straight towards the light without incurvation or bending.

There remains to examine the hypothesis of DUTROCHET, who admits that there exists in the stems and branches of plants a system of cells progressively increasing in size from the centre to the circumference in the central parts of the stem, and from the circumference inwards in the cortical portion. He supposes that these cells are gorged with sap by the influence of endosmose, and that this endosmose tends to give a bending in contrary directions to the two systems, so that the stem is inflected in the direction that predominates. He admits too the existence of a fibrous system placed in the central cellular organization, and supposes that its fibres are also decreasing in size and are inflected by being filled with gas or oxygen endosmose. The incurvation of this tissue always takes place inwards, and that of the cellular system outwards. According to DUTROCHET, the influence of light has a tendency to diminish the filling up of the cells and impair their power of incurvation, and, on the contrary, to increase the production of oxygen gas that creates a bending in the opposite direction in the fibrous tissue. He has grounded this theory on some curious experiments. For instance, by cutting in a longitudinal direction a stem of *Medicago sativa* bent towards the light, he saw that the part of the stem which had been in the light increased its curvature, while the other, on the contrary, became first straight and then bent in an opposite direction. There was then, according to him, an antagonism between the two slips, and this fact was sufficient to overthrow DECANDOLLE's theory, in which, as the exterior slip is supposed to be the only active one, its incurvation ought to have been preserved or even increased in the primitive direction. I have repeated the same experiment with the same result on many stems or footstalks, among others on the footstalk of the leaf of *Calla œthiopica*. But in trying to repeat the sections in all directions, I observed that the bending of the slips was always towards the bark, or outwards, and that the position of the stem with regard to the light had nothing at all to do with it. This phenomenon is due to the contraction or the resistance to any elongation of the exterior cuticle.

Thus, when a segment of stem or footstalk is immersed in water with its cuticle entire, the liquid, penetrating into the cells, fills the cellular tissue, renders it turgid, and there results an elongation of the central parts; but as the fibres of the cuticle cannot follow this elongation and therefore resist it, the segment of stem or stalk is inflected towards the resisting side, or the cuticle, and the incurvation becomes manifest outwards. If, leaving the central cellular system entire, I only removed the cuticle, the immersion of the slip in water, though it brought on the turgid state, did not create incurvation, which yet ought to have appeared if it were, as DUTROCHET supposed, a necessary result of the cells decreasing in size. The same consequence followed when the segment was immersed in water after the cuticle had been slit in two or three places, taking care that the incision did not go beyond it; the imbibition took place, the cellular system was swollen and elongated, but the stem remained straight because the cuticle no longer presented any resistance.

The cuticle alone put into water curls by the shortening of the fibres pretty nearly like a wet rope, so that, far from lengthening it, the liquid has a tendency to shorten it.

Besides, DUTROCHET's hypothesis that light has a tendency to diminish the endosmose by the vacuum produced by the exhalation in the cells, appears not to agree with the supposition of the same author, by which he attributes the ascension of the sap to endosmose. He admits that it is through the cellular membrane of plants and by endosmose from cell to cell that it produces the ascensional march of sap, a liquid which he supposes to be inferior in density to that inclosed in the cells. In the endosmose experiments it is usual to take liquids rather different in their specific gravities; and it seems that if we admit that the sap contained in the cells of the upper part of the plant, where it has been already elaborated, may have a specific gravity sufficiently greater than the surrounding water to let it come in by endosmose, it is not so easy to understand this difference of density from cell to cell as being sufficient not only to allow endosmose to take place, and still less to produce the enormous ascensional power which is admitted to exist in the sap. It occurred to me that the effects of endosmose in such a case might be ascertained by means of experiments.

I took three glass tubes of unequal diameters, such as could be put one inside the other. One of the apertures of each tube was closed with a piece of bladder, and after a solution of sugar of 1·13 specific gravity had been introduced into them, they were placed the one within the other: the last or largest tube was dipped in pure water. The height of the liquid in each tube was carefully ascertained, and the endosmose then observed. The liquid in the largest tube did, as usual, increase in bulk by the replenishing endosmose of the water outside of it, and the effect went on gradually increasing for several days. But during this time, the liquid of the two other tubes did not experience any change of level, the difference between the respective densities of the solutions they contained and the liquid of the larger tube not being sufficiently great to produce endosmose. This experiment, many times repeated, both with alcohol and with solutions of gum or sugar, always gave the same result, and seems to show that a succession of cells would not act otherwise. The transmission of a liquid from one to the other by way of endosmose either would not take place, or at all events would be too slow to account in any manner for the rapid ascent of the sap.

In repeating Dutrochet's experiments on the turgid state that the cellular tissue of plants takes in water, and employing for that effect coloured liquids, it occurred to me, that the absorbed liquor was not inside the cells themselves, but in the intercellular spaces, which accords with the rapidity of the result, which it would be impossible to understand if the liquid were to proceed from cell to cell and through their walls.

It occurred to me also, that if endosmose was the only or principal agent of the ascension of the sap in plants, that is to say of vegetation, this phenomenon ought to be influenced by the two indispensable agents of vegetation, heat and light. As there is something rather mysterious in the phenomenon of endosmose, it was not impossible that such might be the fact. Dutrochet, from an experiment on the cæcum of a fowl, concluded that a rise in the temperature increased both the rapidity and the quantity of endosmose. In his experiments on acids he even announces having discovered that the temperature changes the direction of endosmose, which goes from the water towards the acid, or from the acid towards the water, according as the thermometer is low or high. At all events he does not generalize this singular result, which, according to him, belongs only to the acids; but as a great many saps have an acid reaction, the result ought to be, that, according to the state of the temperature in those cases, the sap should enter the tree or flow out of it: this seems to be in opposition to all known facts.

In all the experiments I have tried, and they have been numerous, I have never been able to ascertain that heat had any apparent influence in promoting endosmose. It is obviously necessary to deduct the effect produced by the dilatation of the liquid by the caloric, or wait until it has returned to its previous temperature, before measuring its bulk. With this precaution, the quantity of water introduced into tubes perfectly similar and containing the same solution of sugar, was found to be exactly equal in the same time, though one of the tubes was maintained at a temperature of 65° C. (149° Fahr.) and the other at 10° C. (50° Fahr.). The ascent of the liquids by endosmose proceeded at exactly the same rate when the tube was kept at a constant low temperature as when it was much more heated, and then brought back again to the usual temperature of the atmosphere. Heat does not appear to me to have any influence in increasing either the quantity or the rapidity of endosmose.

Similar experiments were made, preserving for the endosmose apparatus the same temperature, but exposing one to the influence of light and keeping the other in complete darkness. The tubes and the liquids were absolutely similar, and there was no difference whatever found in the rapidity or quantity of the ascent of the liquid in the tube. When the same endosmose apparatus was exposed alternately to light and to darkness, the same decreasing rate of the endosmose was observed that would be seen in an apparatus constantly kept in darkness or in the light, and no special influence of light could be discovered.. Thus, to give an instance of one of the numerous experiments performed, the mean rising of the liquid in an endosmose tube has been in lines,—

		Lines.
During one hour	in the light	3
	in darkness	1·9
	in light	1·8
	in dark	1·8
	in light	1·7
	in dark	1·7
	in light	1·2
	in dark	1
	in light	0·7, &c.

Many times repeated, and with various liquids, these experiments have always given the same results; and I think I am entitled to conclude from them, that heat and light have no apparent influence on the endosmose phenomenon.

There appears to me, from all these facts, very little probability that endosmose actually performs in vegetation, and in particular in the direction of stems, the important part that has been attributed to it.

§ 3. *On the Direction of Leaves.*

It is known to every one that the leaves of plants have a tendency to assume the same constant position with respect to their two surfaces. One of these surfaces is generally of a deeper green, smooth and glistening*, the nerves of the leaf being very little if at all prominent; this is the surface naturally turned to the sky, and it is on that account called the upper, or superior surface. The other is of a paler green colour, full of little asperities or covered with short hair; it has little or no varnish, and, as it is naturally turned towards the earth, it is called the under, or inferior surface. As far as regards the anatomical structure, M. Adolphe Brongniart has found out that the under surface of leaves contains a greater quantity of pneumatic cavities communicating with the external air by the apertures of stomata than the upper surface. It is to this accumulation of air that they owe, it seems, their whitish colour; and some leaves, like those of French beans, when placed in water under a vacuum, allow this air to disengage itself through the stomata, and, as it is replaced by water, the inferior surface of the leaf becomes of the same green colour as the upper surface. A long immersion in water without having recourse to a vacuum causes the air gradually to replace that liquid, and gives an uniform colour to the two surfaces of the leaf. In some leaves, in those of the Grasses for instance, there is hardly any difference between the two surfaces. In aquatic leaves, as in those of the *Nymphæa*, the air that exists in the pneumatic cavities appears, according to Dutrochet, to come in and out of them by the vessels of the footstalk. It seems that this air contains a little less oxygen gas than the atmosphere. Dutrochet makes out that the oxygen contained in the air of plants goes on diminishing from the leaves, where it is in the ratio of 16 per cent., to the roots, where it is only 8 per cent. His experiments have been made on *Nymphæa lutea*.

The tendency of leaves to present their varnished surface to the sky and their pale surface to the earth, though very general, is not altogether without exceptions. Thus, the misletoe presents its leaves in all directions. The *Ruscus aculeatus* and some grasses turn their unvarnished surfaces upwards. It is the most deep-coloured surface that is directed towards the sky; and if it were to the presence of air that they are indebted for their paleness, it is the surface most provided with pneumatic cavities that would be directed towards the earth.

Among naturalists, Bonnet is the first who has examined the phenomenon of the direction of leaves. He has shown that if the position of a leaf be changed, so as to present to the sky its under surface, the leaf turns itself over spontaneously more or less quickly. This turning over is more rapid in young leaves than in old ones,

* I have ascertained that this varnish is produced by a thin coat of wax, or rather myricine soluble in sulphuric ether.

but towards the end of autumn it ceases to manifest itself. As BONNET believed that the leaves turn themselves over in the dark as well as in the light, he imagined that this phenomenon is the consequence of the under surface of leaves containing fibres liable to contract themselves by humidity, and their upper surface fibres that contract themselves by the influence of heat. He went so far as to construct, in support of his hypothesis, artificial leaves, of which the upper surface was of parchment and the lower of linen, and he states that the heat and the damp produced in them the same motions as in natural leaves. According to him, the dampness of the earth determines the under surface of the leaves to turn themselves on that side, and the heat of the sun acts in the same way on the upper surface. This opinion was the more singular in BONNET, from his having observed that the leaves turn themselves over in water as well as in the air, and from his believing that this change of position in the two surfaces of leaves is the result of the contortion or the inflection of the footstalk.

DUTROCHET indeed attributes to the influence of light the turning over of leaves, but he considers the flat part of the leaf as passive, and the footstalk as the only agent in its motion. He quotes besides no other experiment in proof of the influence of light but those of BONNET, and especially the one in which some leaves of a cherry-tree, placed under the shadow of a table so as to receive light laterally, put themselves in a vertical position with the end downwards.

He gives for the chief ground of his opinion respecting the action of light in this phenomenon, the fact that having placed a *Convolvulus* in the circumference of a wheel rapidly revolving, the leaves after eighteen hours presented their upper surface to the centre of rotation and their under one to the circumference. He concludes from this experiment that leaves present naturally their upper surface to the light; but, as DECANDOLLE justly observes, it would have been more natural to conclude from it that they present it to gravitation. He recapitulates his opinion in these words:—"I have attempted without success to discover how light acts in determining the torsion of the footstalk when the leaf is turned over. It seems evident that in this circumstance the influence of light is exercised on the flat part of the reversed leaf, and that this influence is transmitted to the footstalk whose contortion it produces; but I do not see here what is the connection between cause and effect. The turning over of a leaf depends on two different causes:—first, the disposition of the footstalk to raise itself towards the sky when it has been accidentally bent towards the earth; secondly, the disposition of the footstalk to bend towards the light, but only when it presents its upper surface to it; the flat part of the leaf is entirely passive."

I know no more recent paper on this subject, which appeared to me to be far from being sufficiently elucidated, and I have therefore endeavoured to add something to what was already known.

I thought it was necessary first to ascertain clearly whether light was really the agent that caused the turning over of leaves, and afterwards to study the mode of its action on the two surfaces of those organs.

After I had verified most of the experiments of Bonnet on the turning over of the leaves of a great number of plants, as far as relates to the rapidity of the phenomenon, the frequency of its reproduction on the same leaves, &c., in leaving the plants exposed to the light, I placed many plants in such a manner that their inverted leaves remained in complete darkness. Contrary to Bonnet's observations, I found that the leaves in that case did not regularly turn over. It often happened, for instance in a branch of lilac, in a plant of *Polemonium cœruleum*, that the footstalks and the flat part of the leaves so kept in darkness, turn about first in one direction, then in another; but this happened equally to the leaves left in their natural position. This appears to me to be the consequence of the state of uneasiness in which the privation of light puts the leaves; they seem to move about as if in search of this agent, and this cause, as well as the elastic reaction of the footstalk, when drawn from its natural position by torsion, which process I have always avoided, may have led the observer into error.

With the conviction that light is the only agent whose influence causes the turning over of leaves, I made the following experiment. In cold weather (45° Fahr.) I placed a vase of geranium in such a manner that one of its leaves, young, though fully developed, without altering in the least its position or that of the vase, should be entirely covered from the light on its upper surface, by a double screen of black paper that did not touch it, while, on the contrary, the under surface was strongly lighted by a mirror placed below at the proper inclination. Three days after, the flat part of the leaf had begun to turn over; and after six days, the leaf was entirely inverted, so as to present its upper or varnished surface towards the mirror. The apparatus was kept in the same state during a fortnight, and the leaf continued in the same position, the upper surface downwards towards the mirror, and the under upwards towards the screen. The mirror and the screen were then simultaneously removed, and the leaf was left presenting to the diffused light its under surface that was uppermost. After a few days the leaf turned itself over again, and the torsion took place by a motion of the flat part on the point where it is inserted in the footstalk. This experiment has been repeated many times on various plants with the same results, and seems to me conclusive.

When the two surfaces of leaves are exposed to light at the same time, they do not change their position, but seem to suffer; if the under surface be turned towards the mirror, while at the same time the upper surface being shadowed by other leaves, does not receive as large an amount of direct light as the other surface receives of reflected light, the flat part of the leaf contracts itself so as to render it almost globular. The edges of the upper surface are thus presented to the mirror and cover over the under surface.

Some geranium leaves were covered with a screen of black paper attached to their upper surface by a little gum or a pin. In that case, all the marginal parts of the flat part of the leaf separate themselves from the screen, bend downwards, and the leaf takes a globular form round the point tied to the screen. It succeeds in that manner to expose a part at least of its upper surface to the diffused light below the shade of the screen. When the same experiment is repeated with a mirror placed so as to cast light upon the under surface of the screened leaf, the bending downwards of the marginal portions of the flat part is still more marked and rapid; the mirror giving a stronger light to those parts of the upper surface thus brought to receive it.

When the screen is large enough to allow of no light reaching the upper surface of the leaf, even after the bending of its flat part in a globular form, and no mirror is used to supply the deficiency, the leaf turns yellow and dies.

Light, then, is the direct and indispensable agent in the turning over of inverted leaves, and causing them to assume their natural direction. Its influence is the more rapid, all other circumstances being alike, the greater the difference between the two surfaces of the leaf experimented upon. Thus the leaves of French beans, or of raspberry-bushes, where the two surfaces are of very different colours, turn over completely when inverted, and frequently in less than two hours. In the lilac, where the two surfaces of the leaves are very similar to one another, the turning over is slow and does not seem complete; the flat part of the inverted leaf assuming often a spiral form.

Bonnet and Dutrochet agree in admitting that the turning over of leaves always takes place by a flexion or torsion of the footstalk. The last-named author has even said that a footstalk alone could raise itself erect by the action of light as well as if it were part of a whole leaf. My own observations have convinced me that the flat part of the leaf, or even a separate portion of it, can turn itself over.

In some leaves, for instance in those of the lilac, the *Polemonium cœruleum,* it is always in the flat part alone of the leaf that the motion takes place, and the leaf turns itself into a spiral form to come back by degrees to a regular position. In others, on the contrary, the motion takes place in the footstalk. This is the case in the geranium, the French bean, the raspberry-bush, the horse-chestnut, the plane-tree, the judas-tree, &c. In this latter I have seen the turning over of the leaf take place without any change of position in the footstalk, but by a sort of rotation of the flat part on that portion of the footstalk where it is inserted, and where there is a natural swelling, which swelling was much increased by the change. The footstalk then formed an obtuse angle with the flat part of the leaf, instead of a very acute angle as usual.

In this experiment, as in all the others, the branches had been inflected and tied in a convenient position for the light, but without touching the footstalks, in order to avoid the reaction of torsion.

A vase of geranium, the leaves of which had all directed their upper surfaces towards a window, had been reversed so as to present to the window the under surface of its leaves. All the young leaves turned themselves over by moving on the footstalk, either bending it downwards or turning it over at its base. Out of the thirteen leaves which had turned themselves over, nine repeated the same process, chiefly by a torsion on the base of the footstalk: the others dried up. On reversing the plant a third time, the leaves did not turn themselves over any more, but became dry. The experiments were made during the autumn, the least favourable season for them; for in the spring the same leaf turns itself over as many as fifteen times. When, therefore, the turning over of most leaves can be freely effected, it takes place in most cases by the motion of the footstalks; but even in plants where it is effected by this means, it can take place in the flat part of the leaf itself. Thus, I placed a geranium leaf, covered with a black paper screen fastened on its upper surface, so as to have it illuminated underneath by means of a mirror. It is evident that if the footstalk had been bent or inflected, the position of the leaf with respect to light could not have been more favourable than it was. The screen would have been placed between the mirror and the upper surface, just as it was between it and the sky; and the light of day would have fallen on the under surface then turned upwards, as the light of the mirror fell. In this dilemma, the flat part of the leaf bent itself downwards, turning from the screen as far as the pin which held it would allow; and the edges, continuing their motion so as to unite below the screen, gave a globular shape to the leaf, and thus exposed a considerable portion of the upper surface to the reflected light, and shaded from it the under surface.

In the same experiment, a young leaf which was accidentally shaded on its upper surface by the screen, though at some distance, bent its footstalk to avoid it. Thus on the same plant, and at the same moment, each leaf took a different, but the most appropriate mode to place itself on the best possible position with regard to the light. The same experiments tried on the *Polemonium cœruleum* gave very similar results.

Many other experiments exhibited the faculty possessed by the flat part of leaves to turn itself over independently of the footstalk. Thus, after having ascertained that, as had been already seen by Bonnet, leaves turn themselves over when immersed in water, just as they do in the air, I placed in a glass full of water some leaves of geranium, French beans, &c., of which the footstalks were passed through a hole bored in a stick, and in such a way as to leave outside the flat part alone of the leaf. When its under surface was presented to the light, the flat part of the leaf turned itself over in curling up. The same thing happened with leaves whose footstalks had been entirely removed, and replaced by a small wooden pin fastened in the hole of the stick to make the flat part of the leaf steady. A leaf deprived of its footstalk and freely suspended in water so as to expose to light its under surface only, had curled itself up and had taken a form somewhat globular, so as to cause a great portion of its upper surface to receive the light.

These experiments seem to show that the flat part of the leaves is far from being passive in their turning over, as Dutrochet had supposed. The turning over takes place either by the action of the footstalk, or by the motion of the flat part, according as each of these modes will most conduce to the final result. Thus, when I immersed in water an entire branch of geranium so as to expose the under surface of its leaves to the light, all the young leaves turned themselves over in three days by moving on the point of insertion of the flat part of the leaf into the footstalk, and without the curling-up of the leaf. It has been the same when the footstalk has been kept on a leaf fixed in the bored stick, so as to allow the footstalk perfect freedom of motion in placing it in the hole.

I have endeavoured to ascertain whether this action of light on the surface of leaves, which so evidently exists, would be so powerful as to excite in them a real physical attraction or repulsion. With this view I placed on a moveable cork-float some leaves of the raspberry-shrub (*Rubus idœus*), which are among those whose turning over is most energetic. I placed the float on a glass full of water, and arranged the leaves so that their under surface alone was exposed to light, all the other sides of the apparatus being carefully darkened by means of black paper screens. The whole was kept free from all vibration, and an index showed the position of the float by means of a graduated circle. The leaves turned themselves over by a torsion of the footstalk at the point where the flat part is inserted into it, but there was not the least motion in the float.

The same experiment was repeated many times with leaves of French beans, maple (*Acer pseudoplatanus*), clover (*Trifolium pratense*), &c., and gave the same results.

In a leaf of geranium placed under the same circumstances, the turning over took place by a bending of the footstalk, which thus brought down the flat part of the leaf so as to present the upper surface to the light. In reversing the apparatus, and replacing the leaf in an inverted position, the footstalk raised itself up and regained its primitive situation. There was no motion whatever in the float. The same result was obtained with a leaf of *Camellia japonica.*

When the flat part of the leaf was alone the subject of experiment, it curled up without the float changing its position.

When the plant itself was left floating freely in the water, properly counterpoised, the leaves placed in an inverted position with respect to the light turned themselves over by a motion of the footstalks; but the body of the plant itself remained stationary.

It would appear, therefore, that there is neither attraction nor repulsion, in the material and physical meaning of the word, between light and the surface of leaves.

In compound leaves, the turning over sometimes takes place on the common footstalk, as in the raspberry-shrub, and sometimes on the particular footstalk of each leaflet, which turns itself either on the point situated near the insertion of the footstalk to the flat part of the leaf, as in clover, or else on the point of insertion to the common footstalk, as in the horse-chestnut. The removal of one or more leaflets does not prevent the remaining portion turning itself over when placed towards the light in an inverted position.

I wished to ascertain which of the rays of light had the most influential action in producing the turning over of leaves. In order to make absolutely conclusive experiments on this subject, it would have been necessary to operate by means of the rays of light analysed by the prism; but I had not at my disposal the apparatus of a heliostat, &c., which would have been necessary, and had I possessed it, it would probably have been difficult to arrange the experiment properly; I was consequently obliged to confine myself to the use of coloured glasses. But with the view of ascertaining what degree of confidence they deserved, I analysed with a flint-glass prism a ray of light transmitted through each of the coloured glasses I made use of. I could thus easily find out their degree of purity. The following are the results of this trial.

Glass coloured red by protoxide of copper—spectrum perfectly pure.

Glass coloured blue by cobalt—spectrum contains a little red.

Glass coloured green by chrome—spectrum pure contains very little yellow.

Glass coloured yellow by silver—spectrum contains many orange rays.

Glass coloured violet by manganese—a very fine spectrum, but it contains red rays and a few others, chiefly blue.

For the purpose of observing the effect of coloured glasses on the turning over of leaves, I made use of only three colours,—blue, red and violet.

Three leaves of the same size and age, taken from the same geranium, were placed in an inverted position towards the light; the first behind a blue glass, the second behind a red, and the third behind a violet one. In all these experiments, and they were repeated a great many times, the leaves turned themselves over in the violet and blue rays, and remained motionless in the red. The most remarkable effect was in the blue.

It now remained to investigate the influence of light acting on the two surfaces of leaves with relation to the physiological functions of these organs, so important in vegetative life. These functions are chiefly the exhalation and the decomposition of carbonic acid. I propose now to examine the influence of light under these two heads.

§ 4. *Action of Light on Exhalation.*

Every one knows that plants exhale in the atmosphere a large quantity of water, and that this function is chiefly ascribed to the leaves. To these organs, indeed, the exhalation of water, properly so called, seems to belong; the other parts of the plant losing only an infinitely smaller portion of water, apparently the result of the common evaporation which every humid body experiences in the air. DECANDOLLE calls this last-mentioned loss of water *deperdition*, to distinguish it from *exhalation* properly so called.

SENEBIER has alleged that the vegetative exhalation is nothing, or nearly so, in the dark. This may be true when the temperature is very low and the atmosphere very damp; but I have ascertained, in accordance with DUTROCHET's results, that in summer and in ordinary circumstances, exhalation continues during the night, though in much less quantity than during the day.

I have attempted to determine what difference could be perceived in the exhalation of leaves when the one or the other of their surfaces was exposed to light. I began by ascertaining that, when the leaves of a great many different plants are thus exposed in the air or in the sun's rays at the same temperature, the loss of weight they experienced in the two cases is proportional during the same time; at first, however, rather greater when their under surface is exposed to light than when it is the upper or varnished surface. Thus, in two hours, horse-chestnut leaves lost water in the ratio of 13·6 per cent., when the under surface was exposed to light, to 11·2 per cent. when it was the upper one; with pear-tree leaves, the ratio was 8·45 to 7·75 per cent. But the more the leaf dries up, the quicker the difference diminishes; and when no more real exhalation exists, but there is only deperdition of water, it becomes nothing, and the leaves lose, during an equal time and at the same temperature, a proportional weight of water, whatever be the surface exposed to light.

To ascertain the variations in the exhalation properly so called, I weighed the leaves recently gathered, either with their footstalks only, or with the branch on which they grew; I immersed them afterwards by the footstalk or branch in a bottle filled with water of which the weight had been accurately taken. One of the surfaces of the leaf was then exposed to diffused light and the other covered with a screen. After a given time, I weighed accurately the leaf and the bottle, and the loss gave the amount of exhalation. The same leaf was then inverted and placed in a contrary direction, so that its other surface was exposed to light during a space of time precisely equal to the first. The loss in weight disclosed to me the quantity of the exhaled water. The temperature was carefully observed, though I had ascertained by direct experiment, so far agreeing with SENEBIER's, that heat had very little influence on the exhalation itself. I could besides reverse many times during the day the experiment on the same leaf, and thus obtain alternately for its two surfaces the same circumstances of heat and light. For leaves that have a great tendency to turn over, those of the raspberry-bush, for instance, it was somewhat difficult to maintain them without contorsion of the flat part of the leaf with their under surface exposed to light. With leaves of this description the experiment on that surface could not be continued above two hours.

I have made numerous experiments on a great many species of leaves, in all temperatures and in all weathers, but it would be tedious to give the particulars of them. I shall only report the general results.

1st. A leaf immersed in water by its footstalk increases at first in weight a little more during the same time, when its under surface is exposed to light than when it is the upper one that is so. This result is the consequence of the absorption being a little greater in the first case; but at the end of the experiments the leaves have pretty nearly the same weight they had at the beginning.

2nd. As soon as sufficient time has elapsed to allow water to penetrate into the leaf, the loss by exhalation is, in all temperatures and in every atmospherical state, much greater when the under surface of leaves is exposed to light than when it is the varnished side. The proportion has varied a little in the experiments, and according to the nature of leaves, but in general the loss has been three times more considerable; for instance in the maple, the horse-chestnut, the pear-tree, the plane, &c. In some cases, the difference has been still greater; but sometimes it has been only double. It is obvious that to this enormous increase of the loss of water, which takes place in the inverted leaves, a loss already so considerable in the ordinary state, is to be mainly attributed the state of uneasiness, followed by withering and death, which is the consequence of this position when it is forced upon them; and we thus see one of the ends of nature in giving them the means of delivering themselves from this evil.

By means of the coloured glasses, I endeavoured to estimate the influence of each of the rays of the spectrum in the production of this phenomenon. Out of a great many series of experiments I shall select only one.

In two hours, a leaf of raspberry-bush, weighing twenty-three grains, placed in 300 grains of water, exhaled, with its under surface exposed to the light, in a temperature of 20° C. (68° F.),—

	grs.
In diffused light	4·3 water.
In the blue rays	6·3 water.
In the yellow rays	2·0 water.
In the green rays	2·0 water.
In the red rays	1·0 water.
In the dark	0·4 water.

This leaf, the next day, during an equal space of time, and in the same temperature, exhaled, with its upper surface exposed to the light,—

	grs.
In diffused light	2·2 water.
Under blue glass	2·8 water.
Under yellow glass	0·5 water.
Under red glass	0·4 water.
In the dark	0·0 water.

In all the experiments the exhalation was greater in the blue rays than in the others, whether it was the upper or the under surface of the leaf that was exposed to the influence of light. The blue rays excite a greater exhalation than the diffused light; but this light has more influence than the other rays. The red is that in which the exhalation is the smallest.

§ 5. *Action of Light on the Decomposition of Carbonic Acid.*

It is known that this phenomenon, which in plants effects the assimilation of carbon, and perhaps also of oxygen gas, takes place only under the influence of the solar light on the green parts of vegetables. SENEBIER has advanced that it is independent of the (*cuticle*) green organs, and takes place in the parenchymatous matter. This is true as long as the plant is not disorganized. Thus, for instance, a slice cut in the parenchyma of a leaf of *Rochæa falcata*, weighing ten grains, gave out in the sun, when immersed in water containing carbonic acid in solution, a certain number of bubbles of oxygenated air. The bubbles appeared to form themselves in the exterior cells, and seemed as if they could not disengage themselves without trouble; they remained attached to the fragment, and it was necessary to shake the vessel in order to make them come up. An equal weight of the parenchyma of the *Rochæa* having been pounded, so as to destroy all the cells, was exposed to the sun in an equal quantity of the carbonic acid solution and during the same time. No oxygenated air was produced. The same negative result was obtained when the expressed juice of this leaf was put in the sun with water, although it contained a great deal of the green matter that fell to the bottom of the tube. When I put into the water a slice of the leaf of Rochæa of the same dimensions as the first, but in which the cuticle had remained with a small layer of green matter, although this fragment weighed only five grains, or the half of the preceding one, I saw a regular series of bubbles disengage themselves from the stomata. I could measure in an equal length of time with the two tubes plunged in the same solution of carbonic acid, three times as much oxygenated air from the slice covered by its cuticle than from the one that was not so covered.

The same experiments repeated with a great many other plants gave similar results, and show,—first, that the green chromule alone is not endowed with the property of decomposing carbonic acid, and that this faculty is the consequence of a physiological action of the cells; secondly, that this decomposition is increased by the agency of the vessels and pores that exist on the cuticles of leaves.

In order to appreciate the influence of light on the production of oxygen gas when it acted on one or the other of the surfaces of leaves, I took two leaves of the same plant as equal as possible in weight and surface, and placed them in two similar bell-glasses, inverted over the same weak solution of carbonic acid or over spring-water. One of the leaves was placed in such a manner as to present to the sun its under surface, and the other its upper one. The other side of the bells was covered with black paper. The gases produced were carefully measured after the experiment, and the temperature was noted.

I have made a great many experiments on a great variety of leaves; and without giving the details in this place, I will only add, that when the leaves are in their natural position, that is to say, with their varnished surface exposed to the light, the bubbles of oxygen gas are produced much quicker and in far greater quantity than when the under surface is exposed to light. During the same length of time, two or three times as much gas is formed in the first case as in the second; and the difference is the more marked the longer the experiment is continued, for the production of oxygen by the leaf whose under surface is exposed to the sun goes on constantly diminishing.

I devised another rather striking mode of making the same experiment. In one of my trials, I had been surprised to observe that the leaves of *Camellia japonica* did not, when exposed to the sun in spring-water, disengage oxygen gas by their stomata, but that bubbles of this gas went off through the footstalks. I ascertained afterwards that this fact had already been mentioned by DUTROCHET. It afforded me the means of showing the difference of the action of light on the two surfaces of the same leaf.

To make this experiment, I place in two large tubes filled with spring-water, two Camellia leaves having an equal surface, and their footstalk directed upwards. One of the leaves has its upper, the other its lower surface exposed to light, and the opposite side is shaded. I leave the apparatus for at least five or six hours in the dark, after which I expose it to the diffused light at the temperature of 20° C. (68° F.); the direct rays of the sun not being necessary to the production of the phenomenon. After twenty minutes of exposure, there appear on the leaf, the upper surface of which is exposed to the light, numerous bubbles of a gas containing 85 to 90 per cent. of oxygen, disengaging themselves from the aërial vessels placed in the centre of the footstalk, and whose apertures are clearly visible. The bubbles are very small, and so numerous and so rapidly emitted, that it is absolutely impossible to count them; they chase one another, and are all gathered up at the top of the tube. Begun at half-past nine in the morning, for instance, the bubbling lasts with the same activity for an hour, after which it ceases. Towards the end, the current of bubbles is a little less rapid; but the smallest number I was able to count at twenty minutes past ten o'clock, was 120 bubbles per minute. In the leaf whose lower surface is exposed to light, the gas begins to disengage itself only after thirty minutes of exposure to light; the current of bubbles is much less rapid, and during the whole time of duration I could always easily count them. The greatest number produced was 145 in a minute; and while the other leaf still gave 120 of them per minute, this one gave only thirty. The bubbles ceased at the same moment in both leaves, and consequently lasted only three quarters of an hour in the leaf whose under surface was illuminated. I was not surprised, therefore, in measuring the quantities of disengaged gases, to find that the leaf of Camellia exposed to light in its natural position had given three times as much oxygen gas as the other whose position was inverted, all other circumstances being similar, and both being plunged in the same liquid. When the apparatus is again placed in darkness, a new accumulation of carbonic acid takes place in the cells of the leaves; and if they remain in it a sufficient time, the disengagement of bubbles of oxygen gas by exposure to light begins again, with the same differences as to time and proportion, according as the leaf is illuminated on its upper or lower surface. I have seen the same leaves producing the same phenomenon for seven or eight days in succession, by keeping them alternately in the dark and in the light. The rays of the sun are not indispensable; and the experiment, which is easy to perform and pleasing to witness, succeeds very well in diffused light.

I have ascertained that some other leaves, among others those of the *Laurus thymus*, Portugal laurel, &c., give similar results.

In conclusion, I have endeavoured to show:—

1st. That light is the only agent in the turning over of leaves.

2nd. That it does not act by a physical attraction properly so called.

3rd. That the turning over of leaves takes place sometimes by a torsion of the footstalk, sometimes by a curling of the flat part of leaves.

4th. That the blue rays appear to be the most, and the red the least active in effecting the turning over of leaves.

5th. That the exhalation of leaves is much increased when their under surface is exposed to light.

6th. That the decomposition of carbonic acid and the disengagement of oxygen gas are, under the same circumstances, considerably diminished.