THE

TREE-LIFTER

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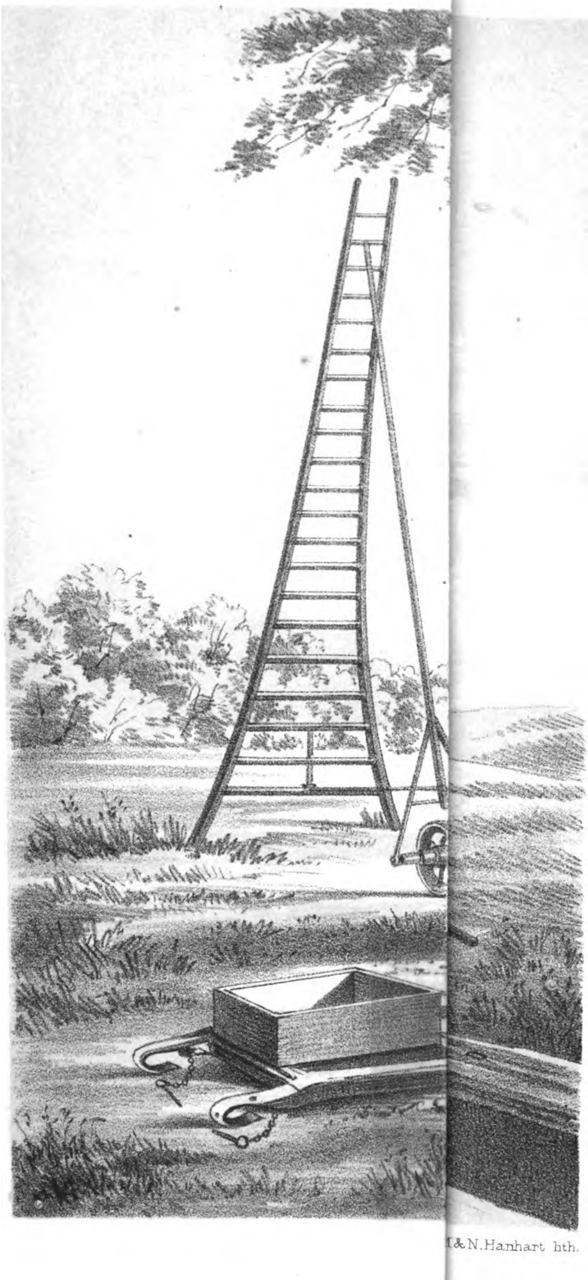

M & N. Hanhart lith.

THE

TREE-LIFTER

OR

A NEW METHOD OF TRANSPLANTING FOREST TREES

BY

COL. GEORGE GREENWOOD

He who has planted a tree has set the elements to work for him

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THE TREE-LIFTER.

PART I.

PRACTICAL PART OF TRANSPLANTING.

ADVANTAGES OF THE SYSTEM.

AMONG the advantages of transplanting with 'the tree-lifter' may be reckoned its cheapness. Its simplicity is such that the whole may be performed, and even single-handed, by a common day labourer. One man may plant one tree per day, of from twenty-five to thirty feet in height. To transplant trees without the ball of earth requires great skill, care, labour, and expense in tracing out the small fibres of the roots, whose extreme points, with their supposed spongioles, could by no delicacy of operation be retained, and which after all are nearly valueless. All transplanted trees are the better for being watered; but with the ball of earth this is by no means necessary. To transplant without the ball of earth, and not to water, for at least two summers, is hopeless. This is a great expense, besides staking and tying, which plants with the ball of earth do not need. The growth of trees transplanted with 'the tree-lifter' is not checked; but without a ball of earth, trees transplanted, with whatever care, or at whatever expense, are checked in their growth for eight or ten years, and if they do not die, they become living scarecrows.

In fact, trees transplanted by 'the tree-lifter' are very much in the same situation as those *prepared* for transplanting, as it is called, in the old-fashioned way, by cutting a trench round them. This method was originated in the time of Charles II., by Lord Fitz-Harding, as Evelyn tells us. But the trees transplanted by 'the tree-lifter' gain the great advantage of making their new roots in the ground where they are to remain for ever, and escape the injuries of a subsequent removal.

The best months for transplanting the generality of English trees, with the ball of earth, are July, August, and September.

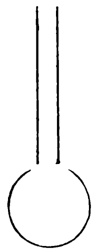
Over the nursery plant, as a single tree, the transplanted tree has the advantage of a start of from twenty-five to thirty years, besides saving the expense of the material, and carpenter's work, for at least two fences for each tree.

The system recommended would have peculiar advantages for planting or for keeping up avenues.

DESCRIPTION OF THE TREE-LIFTER.

A pair of wheels, 8 feet in diameter, standing 4 feet 6 inches apart, or the same width apart as the common carts and waggons of the country. Twenty spokes. Width of the tire, $2\frac{1}{8}$ inches. The wheels quite straight, and *undished.* An iron axle of 3 inches diameter throughout, and perfectly straight. An iron wheel, fixed with a linch-pin, on each end of the axle, outside the box of each wooden wheel. The iron wheels to have six spokes, ending in wooden handles projecting one foot beyond the rims. One wooden handle fixed on the rims between each spoke. The ends of these twelve handles to be just within the rims of the wooden wheels. The entire machine thus forming simply a windlass on wheels. A strong iron ring playing loose on the axle, and a strong iron hook playing loose on this ring, to hold the weight when raised.

Shafts 10 feet 4 inches from the axle; to take on and off the axle by means of a hook and screw. Five chains, 12 feet 6 inches in length, with a hook at each end; one chain, 14 feet 6 inches in length, with a hook at each end, and with six round links at each end, to distinguish it from the other chains. A box with six compartments to hold the chains. Two strong planks, 8 feet in length, with a hole bored in each, to fit on to a pin on the shafts, along which, and across the axle, the planks are to rest when carried on the machine. A box or thill on the shafts to carry four blocks for the wheels, spades, pickaxes, &c. A



South American surcingle, and three or four rope traces with a hook at each end. A strong rope to be attached by one end to the axle. At the other end a strong iron ring to receive the chains which encircle the ball of earth, and to be attached to the hook and ring on the axle, when the weight is raised.

Possibly the dimensions here given might be increased with advantage.

'The draught axle' or timber wheels should, like this machine, have straight arms to the axle. If the arms are bent, the lower parts of the wheels stand nearer together than the upper parts. But when the shafts are raised to take up the load, the lower parts of the wheels are wrenched from one another; and when the shafts are hauled down to raise the load, the lower parts of the wheels are again wrenched together. This, with heavy loads and rough deep ground, requires force which no machinery will stand long.

The only argument which I know in favour of bent arms to axles is one which I never heard mentioned or saw stated: it prevents *wabbling.* It indeed creates friction between the boxes and the ends of the axles, since the wheel inclines to run up the arm which is bent down. But this is not so bad as the friction and wear and tear caused by wabbling. With straight arms, wabbling can only be prevented by having the arms and boxes very long. This increases the friction; perhaps as much as results from forcing a conical wheel to run straight instead of circling outward from the carriage.

DESCRIPTION OF THE PRUNING SAW, AND PRUNING LADDER.

The best instrument to prune small trees with is a carpenter's *turning saw*, with coarse teeth, set wide for the purpose; having a large handle, with a hook to attach it to boughs or the rounds of a ladder, and admitting of the blade being taken in and out by screws, and replaced when broken. The saw is held by the round part of the handle while sawing a branch from below upward; and all branches should, if possible, be begun from below, to avoid tearing the bark and last layers of wood as the branch falls. A chopping instrument, such as a bill-hook, besides bruising the bark and splitting the wood, is apt to cut too close, or not close enough, or both; that is, to begin by cutting too close, and to finish by not cutting close enough. Or if it finishes close to the stem, great risk is run of injury to the bark or branches above that amputated. In the case of cutting too close, the parts from which the new healing growth is to proceed are injured; in the case of not cutting close enough, a dead stump is left to be enclosed by the annually increasing stem, which is probably rotten before it is enclosed. Besides, a chopping instrument is not adapted to getting between branches to thin them out. These saws will pass between branches which are too close better even than the knife. One of these blades fixed on a light rod is the best instrument to clear leaders which cannot be reached with a knife or a hand-saw. In this case, work the saw in a line with the stem of the tree; not across it. These saws may be bought at Coleman's, cutler, Haymarket.

The pruning ladder should be triangular; that is, to the Kentish fruit-ladder, wide at the base and narrow at the top, should be added a single prop. The lower end of the prop should diverge into two branches, to receive a wheel like that of a wheel-barrow. When lowered, the ladder is placed and wheeled on this prop. When reared, the prop being attached to the ladder by a rope, it is pulled towards you by raising the ladder by one of its lower rounds. If well made this ladder is perfectly wieldable, and safe at the height of upwards of twenty feet.

The ladder is easily detached from the prop, and used singly, if required. It is the only safe ladder against a tree. A common ladder must have four bearings; and if either of these should give way, the ladder will turn over. A wide-based ladder needs only three bearings; that is, it is as safe from turning over with one bearing above as with two.

DESCRIPTION OF WATER-CASK.

I have found the following sort of water-cask very useful; either for common watering, or for the conveyance of liquid manures:—

A pair of old gig-wheels, 4 feet in diameter. A 36-gallon cask, 32 inches high. Swing this cask between the wheels by two iron arms, 15 inches of the cask above the arms, 17 inches below them. Or the arms which pass through the boxes of the wheels may be attached to an iron circle large enough to receive the cask, which will rest on the circle by three supports.

An iron handle to pass over the top of the cask, and sufficiently free from the cask to allow of its being tilted. The handle to take on and off the axle with a hook and screw.

A pair of old gig-shafts to take on and off the handle.

A leathern hose, with wire inside, at the lower part of the cask, just long enough, when turned over the cask, to reach the bottom of it on the other side. The hose to take on and off with a screw. A lid, opening with folding flaps, fastened by a bent hasp, which will pass over and secure the hose while in movement. A couple of leathern buckets. A hook before and behind the cask, to carry the buckets.

A wrought-iron bowl, to lade with.

This cask may be used either by hand, or with a horse, or donkey. It may be locked going down hill, as recommended for 'the tree-lifter.'

According to circumstances, or the power of approaching the tree, the hose or the buckets may be used, or the cask may be tilted.

DIRECTIONS FOR PRACTICE.

Choose a tree of from 20 to 30 feet in height, with several leaders, or with some unduly large branches. Prune the tree previously to digging round it, so as to take from the head at least as much as will, in all probability, be taken from the root, not by *cutting in* the head and branches of the tree, but by giving it a clear leader, and cutting out all branches large enough to compete with the stem, low enough to be reached by cattle, or the growth of which is upright or parallel to the stem. Cut all close to the stem. Dig a trench round the tree, at the distance of about 20 inches from it. Undermine the ball of earth, at the depth of about 3 feet, so that every root may be cut previous to lifting it. Tie some old carpeting or matting round the stem of the tree. Place the machine, without the shafts, on the planks, and block the wheels; the centre of the axle over the centre of the ball of earth, and the ring of the windlass rope at the foot of the tree. Place the chain with the round links horizontally, and loosely, round the lower part of the ball of earth, so that it lies on the ground in the pit. Cross the end held in the left hand twice over the end held in the right hand, and lay both ends down. Pass one of the other chains, as far as its middle, through the ring of the windlass rope. Pass the ends of this chain beneath the horizontal chain, and hook the ends back on the chain itself. Do the same with the four remaining chains. So that when the windlass is worked, these five vertical chains, being at equal distances from each other, shall bear an equal distress from the weight of the ball. Fasten the horizontal chain, by hooking it to any convenient part of the vertical chains. Raise the ball of earth with the windlass, and place the ring of the windlass rope on the hook attached to the axle. The ball of earth carried thus is pressed together by the chains acting towards one another, and the greater the weight the greater the inward pressure. A ball of earth resting on a sledge or truck soon shakes to pieces on the least movement, besides the difficulty and mischief done in lifting it on and off the truck.

The tree is lowered into the pit prepared for it with the same facility as it is raised. If it does not stand upright it is easily raised by the windlass, while earth is cast under where it is wanted. The unchaining is only reversing the enchaining. If during the carriage, owing to wet or the friability of the earth, the chains cut into the ball, and the load sinks, it is easy, by letting the load down altogether, to take up and tighten any one or every one of the chains.

Place the upper surface of the ball of earth at precisely its former level, and do not cover it; but raise a very slight ridge of earth round the outside of its circumference, so as to form a *pan or irrigation cup.* Tie some bushes round the stem to prevent cattle from rubbing it. The organisation of the bark is easily mechanically injured and destroyed by any bruise, or by cattle rubbing against it. It is the common error to believe that the bark is chemically *poisoned* by the oil from the skin of cattle.

It may be found necessary to stay the sway of the tree, while in movement, by ropes from the stem to the shafts.

In going down steep hills the machine may be held back by a horse ridden behind, and hooked on by the South American surcingle and single trace; or the machine may be very conveniently blocked by lashing a bar of wood across the shafts, close in front of the wheels, and taking out the screw which fastens the shafts to the axle. The horse will then bear back against the wheels instead of against the axle, and the friction against the wheels will increase or decrease directly as the downward impetus.

This application of a common mode of blocking wheels has the advantage of being entirely *self-acting.* It needs neither the attention nor personal service of the driver; nor is it left to his judgment at what time, or how much, or how little, to put on or to take off. Without any stoppage or trouble, friction is *acquired* at the exact time and in the exact quantity that is *required*, and ceases of itself as the necessity for it ceases.

The principle of locking two-wheeled carriages by creating friction on the wheels induces a slight downward pressure on the horse's back. But the long shafts act as a lever in the horse's favour, and the extra pressure on his back is not to be spoken of in comparison with the labour of bearing back a loaded cart.

That this is a fact any one may convince himself practically, by taking the water-cart which I have described down a hill which is so steep that he could not hold the cart back without this mode of blocking the wheels. Might not this mode of blocking be applied to two-wheeled carts?

PART II.

THEORY OF TRANSPLANTING, OR PHYSIOLOGY OF TREES IN REFERENCE TO TRANSPLANTING.

CHAPTER I.

THE FOOD OF TREES IS IMBIBED BY THE SURFACE OF THE ROOTS.

Proportion the head of a transplanted tree to the root.

THE supply must be equal to the demand; if not, scarcity will ensue. He who expects that a diminished root will support an undiminished head will be disappointed. This is the fundamental principle of transplanting. And in transplanting, the head must be curtailed exactly in proportion as you have to curtail the root.

Prefatory remark to entering on physiology of trees.

Before entering on physiology, I would say one word to defend myself from the charge of egotism and plagiarism. When I mention Sir Humphry Davy, I may say that immortal names are among those who have written on the physiology of trees. Yet so much doubt and difference prevail among the authors on the subject, that one cannot adopt a single opinion without opposing many, held by minds, perhaps, as clear and comprehensive as Sir Humphry's. It is, then, to save the reader's time if I lay down as certain what better men have doubted or controverted; or if I use the words, 'I think this,' or 'I think that,' in stating other people's opinions.

The course of the Treatise will go, with the sap, from the root, through the wood to the leaf, and back by the bark to the root.

The sap is absorbed by the roots.* Its course upward is through the whole of the wood, heart-wood as well as sap-wood, of the root, the stem, and the branches. It thence passes through the insides of the leaves and buds, and returns through their outsides into the bark, and descends again to the roots. From the ascending sap the growth in elongation of the stem and branches takes place; and from the descending sap the growth in girthing of them, and also the growth in elongation of the roots.

But I imagine that, besides this vertical or longitudinal flow of the sap between the roots and the branches, there is also a horizontal or transverse circulation between the heart-wood and bark; and that the joint elaboration of these two saps is essential to the formation of the new annual growth in girthing. This consists of a new sheath of wood, deposited outside the last year's growth of wood, over the entire surface of the stem, branches, and roots; and also of a new sheath

* Even this first principle is quite contrary to the opinions of the mass of physiologists, ancient and modern. Priestley, Sénebier, De Saussure, Liebig, &c., think the more head, the more root it will feed. I think the more root, the more head it will feed.

of bark inside the last year's bark. These opinions, in this order, will be discussed in the following pages.

With one exception, trees imbibe from all parts exposed to moisture, and transpire from all parts exposed to drought.

With the exception stated below, trees imbibe from every part of them which is exposed to moisture, and give off (transpire) from every part of them which is exposed to drought. The root is the part which is constantly exposed to moisture, and which furnishes the constant supply of sap to the tree. That part of the root which is universally believed to imbibe no moisture, the woody part, is the only part which does imbibe moisture; and that part of the root which is universally believed to be the only part which does imbibe moisture, the unripe ends or fibres, is the only part which does not imbibe moisture.

The food of trees imbibed by the surface of roots.

Food of agastric animals imbibed by the surface.

In fact, the mode in which food is first received into plants is the same as that in which it is received by what are called the agastric animals, or animals without stomachs, as some Medusas; that is, by absorption from the surface.

Let us first take the positive assertion that roots imbibe from any parts exposed to moisture; and afterwards consider the negative, that they do *not* imbibe by the immature ends of the roots.

Seeds imbibe by the surface.

Let us begin with the seed. The existence, and also the growth, of all seeds depend on the principle of absorption from the surface; that is, all seeds both *breathe* and *drink* by absorption from the surface. If seeds are to exist, they must be supplied with atmospheric air. If they are to grow, they must be supplied with moisture. And they inhale the air for their existence, and imbibe the moisture for their growth, by absorption from the surface.

We may regard the growth of 'mummy-wheat' as an idle tale. Lyell informs us that, in an experiment at Kew Gardens, 'out of 100 seeds of wheat, barley, and lentils, from Mr. Sam's collection in the British Museum, not one of them would germinate;' and that a friend of his found occasional grains of *maize* (first imported from America) in several parcels of corn sold by the Arabs as coming from the catacombs. Lyell does not, however, controvert the possibility of the growth of seeds after an entombment of 3,000 years. And supposing the presence of atmospheric air, and the absence of moisture, sufficient to cause generation, we know no reason why they should not grow. But we do know that if seeds are deprived of atmospheric air, by being hermetically sealed, even for a few months, they die.

Cuttings imbibe by the surface.

Again, the existence and growth of cuttings which have no root, old or young, prove absorption from the surface.

In the hot climate and on the arid hill-sides of Spain the olive is propagated by cuttings. These cuttings are old branches 7 feet in length. One end of such a cutting is buried about 18 inches in a pit, and concrete earth or clay is raised, like a pillar, round it, so that, at the upper end, only about 8 or 10 inches of the cutting is exposed to the atmosphere. Thus excretion of moisture is prevented, and secretion of moisture takes place, throughout about 6 feet of the cutting. In the course of two or three years the pillar of earth is gradually taken away; when a head has grown on a trunk 5 or 6 feet from the ground.

From Pliny's description of the planting the elm in vineyards, and Seneca the Younger's description of planting the olive, I imagine the Spanish to be a lineal descendant of the Roman method. Virgil also alludes to this mode of cultivating the olive in the Second Book of the Georgics:—

> Quin et caudicibus sectis (mirabile dictu !)
> Truditur e sicco radix *oleagina* ligno.

I have known cuttings of pinuses kept out of doors without heat, to live for two years, and even to make small shoots, without forming a symptom of a root. That these were fed for two years by the absorption of their wood from the earth, and not, as Priestley and Liebig would have it, by the absorption of their foliage from the air, is clear,—because, if cuttings are left without being placed in the earth, they die at once.*

* All the vineyards of the South have, from time immemorial, been planted and replanted from cuttings, as our gooseberries and currants are. Without the intervention of *new plants* generated from dormant seeds, the great majority of vines might doubtless thus run their pedigrees through sap which has never ceased to circulate up to the time and the plants of Bacchus. And thus each plant may call itself the contemporary creation of the jolly god, as each olive may of Minerva. This is very much at variance with the absurd received notion that cuttings and scions partake of the age and decrepitude of the plants from which they are taken.

If a long vine-branch is coiled round the inside of a flower-pot and covered with earth, as it is ten times as long below the earth as a common cutting, it will shoot with ten times as much vigour; and if heat is given, it will bear fruit the first year. It will also appear, from experiments which I shall detail, that if every root is cut or rubbed off a radish, it will grow, either in water or in earth. Nay, if the bulb is cut in two, and the upper half planted, this *rootless half*-bulb will grow.

The coiled branch imbibes from the surface.

Radishes imbibe by the surface.

In the first growth of the seed, in the growth of cuttings and of coiled branches, and in the growth of the half-radishes, there must be absorption from the surface without the aid of the small fibres or ends of roots, since no roots of any sort exist in any of these cases.

Let us now consider the negative assertion, that plants do *not* imbibe by the unripe ends of their roots.

The unripe ends of roots do *not* imbibe.

Take the end of a freely-growing root; break or nip the *silver* end across. An internal tube will be found, distinct from the external tube. The difference in the organisation of the internal tube from that of the outer one will be plainly seen by the naked eye, and very frequently a circle of cells or tubes round the outside of the inner main tube. These two main tubes are what an Englishman would like to call the outer and inner bark: but he must not think of doing so; he must call the outer tube the cuticle or epidermis, and the inner tube the parenchymatous under-layer or

Formation of ends of roots.

herbaceous envelope. Or, at least, they are equivalents to what would bear these euphonious titles above ground. Continue to break off small pieces of the root. A third tube will be found,—a ligneous thread, ending in the finest point, and growing gradually thicker. The thread being comparatively tough, the outer coatings may be pulled off it for many inches. The internal thread is the wood, and is the conduit for the ascending sap which approaches nearest to the extremity of the root. The external coatings are the bark, and the inner bark is the conduit for the descending sap, and the means of the growth of the root, as well in elongation as in diameter.

Ends of roots only bark, which is the descending, not the ascending, conduit.

The silver *ends* of roots are, in fact, a mere prolongation of the bark, without wood. Down the bark all physiologists allow a descending current, but none allow an ascending current. I believe, indeed, that there is a power of absorption and of lateral transmission of moisture *across* the bark into the wood, and that the layers of wood are the upward conduits for the sap. But if we suppose the possibility of the absorption of moisture by the immature and silver ends, we cannot suppose the possibility of its transmission upwards, where there is no wood, but only unripe bark in process of formation and deposition from above, which bark, even when ripe, is the descending, not the ascending, conduit; and though on extraordinary emergencies the sap may for a short distance flow up the downward conduits of the bark, it is rather too much to suppose that, in the ordinary course of nature, the sap should perpetually flow *both ways at once in the same channel.*

Experiments in proof.

As this question is of *vital* importance in *transplanting*, I will state the results of several experiments in support of these opinions. To begin again with the seedling. In March, 1836, I made some horse-chestnut seeds grow in water, in my barrack-room, and found that when the root was cut off when it was several inches long, the plant would still grow, and would continue to throw out fresh roots as fast as they were cut off. The existence of the plant was evidently independent of the root. The reason is, that unripe, that is unwoody, roots are incapable either of absorbing or of transmitting sap; but receive their own nourishment from above,—in this case, from the seed. Consequently, whenever I divided a plant from its seed, it died, though the root was perhaps twice the length of the plant, and though leaves were developed on the plant. Yet, when I allowed the seeds to remain attached to the plants till the roots had become woody, the plants grew in water for years. Moisture is taken up by absorption from the surface of the seed; the elaboration of the sap is entirely in the seed, and passes through the bands which unite it to the seedling, for the growth of the plant upward and of the root downward; and the seedling is entirely dependent on the seed for its life and for its growth till the roots have become woody. Still farther to prove this, I have

Seedlings not nourished by roots till they become woody, but by the seed.

placed horse-chestnut seedlings raised in water having roots about three inches long, with the tips and lower parts of the roots in a shallow saucer of water, and with the seeds and plants outside the saucer. All died and dried instantly. These, then, while alive, were supplied by absorption from the surface of the seed, and were not indebted to the immature roots, with their spongioles and capillary stomata. And where are the spongioles and capillary stomata which supply the surface of the seed?

Symmetrical growth on root of horse-chestnut.

One of these experimental plants lived for nearly ten years in water, and was then only killed accidentally. The first side-fibres or shoots which are developed on the roots of horse-chestnuts grown in water are regularly arranged in six vertical lines along the root. This is the only approach to *symmetrical* growth that I have ever remarked on the root of a tree. The side-fibres of the root come from the woody part of the main root, as branches do from the woody part of the stem; and I imagine that, when side-fibres are first developed on the main root, this main root is first becoming sufficiently woody to absorb, and to feed itself and the plant, independently of the seed.

Twin oaks.

The two bands which unite the seedling to the seed pass one to each division (cotyledon) of the seed. I have known four divisions or cotyledons in an acorn. In this case twin plants arose.

I know not how to question the roots of mature trees in reference to the absorption of their food by the ends; though I think that the free growth of trees transplanted with the ball of earth answers it in the negative. So does the growth of potted greenhouse plants, all the ends of whose roots are cut off. But certainly the answers elicited from the roots of seedlings are most clear and most decisive, and directly in contradiction to the only *fact* I have ever heard stated in favour of the absorption by the ends of the roots. And this fact, I have no hesitation in asserting, is *not* a fact. I give, in Richard's words, what I believe Sénebier first asserted:—

That a radish is fed only by its end a fallacy.

'Roots also extract from the earth the substances which are intended to serve for the growth of the plant. But all parts of the root do not perform this office, it being only by the extremity of their smallest fibres that this absorption takes place. Some say that they are terminated by little ampullæ, or spongy bodies, which are more or less tumid; and others, by a kind of absorbing mouths. Whatever be their structure, it is certain that the office of absorption is performed by these extremities alone.

'No experiment is more easily made than that by means of which the truth of this fact is undeniably established. If we take a radish or a turnip, and immerse in water the extremity of the radicle by which it is terminated, it will vegetate and shoot forth leaves. On the contrary, if it be so placed in the water that its lower extremity is not immersed, it gives no sign of development.'

First, I think I may fairly ask *why* 'some' say that each root ends in a sponge? and why other some say, in a capillary stoma? In either case have they *seen* the fact? Have some seen one of these bodies at the end of a root? and the other some, the other of these bodies there? Or are these two bodies so precisely alike that one cannot be distinguished from the other? To my eye, there is not much resemblance between a sponge and a capillary stoma; and any one who agrees with me here will, I think, also agree that in no science except botany would this extraordinarily loose mode of stating *facts* be tolerated. These, however, are not the facts which I have at present to deal with; but with the *great* fact asserted by all physiologists, which I have given in Richard's words,—that a radish with its end only in water will live and grow; with all but its end in water, will die.

In physiology, as in many other things, we are apt to find—

> That witnesses like watches go,
> Just as they 're set, too fast or slow;

and certainly my testimony directly contradicts this double assertion. I assert, that if the end only of the main root of a radish newly taken up from the ground is placed in water, in a room, but out of the sun, even to the length of an inch, the radish and its leaves will instantly die; precisely the same, that is just as quickly, as if no part of the plant were in water. On the other hand, if all the radish is placed in water except

Experiments in proof.

its leaves and the end of its main root, it will remain alive and continue to grow, even after the last inch of its main root is dead and dry. But the leaves will sometimes droop at first, unless they also have access to the water by their foot-stalks. Radishes have many side-roots (a new fact, perhaps, to most physiological *writers*), and I have found these results to be the same, whether the side-roots are taken off or left on, and whether the end of the main root is left in the water or out of it; or whether the end of the main root is cut off or left on the plant, or if the lower half of the radish is cut off, or if it is cut off within half an inch of the neck. When radishes have once been well saturated with water, they will remain alive for a long time under any circumstances.

If a radish is placed in a narrow-necked bottle, such as an eau de Cologne bottle, the absorption of water is very apparent by the decrease of water in the neck of the bottle. If the bottle is kept filled, so as to give all the radish the power of absorbing laterally, the radish will live. If the bottle is not kept filled, the radish will fade as the level of the water sinks to the immature parts of the radish.

On the 13th of April, 1850, I instituted three experiments similar to these; but in earth instead of water. The results were equally favourable to the position that the absorption of nutriment by roots is through the medium of their mature parts, and not through the medium of their immature ends.

No. 1. From a ridge-bed, covered with matting at night, in which early potatoes and radishes were growing, I took half a dozen radishes, rubbed off all the side-roots, and laid them sideways in the earth, in a flower-pot, with their heads out of the earth, and their immature white end-roots in the air, over the side of the flower-pot. All grew.

No. 2. Of another half-dozen, I cut the red bulbs across the middle, rubbed the side-rootlets off the upper halves, and planted them in a flower-pot. All grew.

No. 3. I tied half a dozen others to sticks, so that their heads and red bulbs were in the air, and their immature white roots buried about two inches in the earth. All died.

It is true that each experiment, for the first week, flagged with every warm gleam

> Purpureus veluti cum flos succisus aratro
> Languescit moriens;

and this flagging took place in celerity and intensity *inversely* as I have numbered the experiments. But they revived with every shower, and this in celerity and perfection *directly* as their numbers. Some plants even of No. 3 recovered their drooping partially after showers for the first few days. But this, I think, only goes to prove the position with which I started in the first edition of this treatise, that vegetables imbibe from all mature parts which are exposed to moisture, and excrete from all parts which are exposed to drought.

The radishes had been grown in very light mould, so that, by passing the fingers into it in taking the radishes up, I believe not a rootlet, main or lateral, was broken. The whole of the roots were left on No. 3, and much earth adhering to the side-roots of some. The weather was very favourable for the first week, the wind south or south-west, the thermometer about temperate, with constant heavy showers. For this reason, none of the experiments were watered for the first fortnight. On the 21st the wind got to the north, and the weather was sunny and dry. But, ere this, nearly all No. 3 were defunct. On the 29th, after a week's drought, some of the leaves of Nos. 1 and 2 looking flagging, the experiments were all watered. Care was taken that the water should only fall on the earth, without touching a leaf, in order to make certain that the revival (which took place) was caused solely by the absorption of moisture by the rootless bulbs—nay, in the case of No. 2, by the rootless HALF-bulbs.

On the 28th of May, when the experiments were between six and seven weeks old, I turned No. 1 and No. 2 into the open ground. On taking off the pots I found the roots in both cases matted round the balls of earth, and they had grown through the holes of the flower-pots. Can it be supposed that these long roots are useful to their plants only by their ends? Can it be supposed that the grand systems of roots of forest-trees are useful as absorbents only at their ends? If so, would these magnificent conduits of the upward sap, or could they, enlarge directly as their distance from their petty supply? I think the reverse of all this is the case: that the root absorbs laterally from the whole of its *mature* length.

No. 4. On the 7th of June, 1850, I took half a dozen radishes grown in an open bed, which the gardeners were pulling up because too large and too old for use; I cut off the lower halves of the bulbs, rubbed off the side-rootlets, and transplanted the upper half-bulbs to an open bed under a south wall, taking the precaution to diminish their heads by cutting off nearly all their large leaves. The stalk of one had grown up about three inches. For the three succeeding days the thermometer on the wall rose above 90°, on the fourth day above 100°. The plants were watered each day, but not sheltered from the sun. All grew. By midsummer, and after that, the thermometer frequently stood at 84° in the shade, and 111° in the sun, on the wall under which Nos. 1, 2, and 4 were growing. Not a leaf of either flagged. There were by this time flowers on the tops of the stalks in all three experiments: and in the autumn all bore seed most profusely, much of which ripened.

That branches are the same length as roots a fallacy, and that the

First comes the unsound datum, then follows the unsound theory. Roget remarks, in reference to roots, that 'as a constant relation is preserved between their lateral extension and the horizontal spreading of the branches, the greater part of the rain which falls upon the tree is made to drop from the leaves at the exact distance from the trunk where, after it has soaked through the earth, it will be received by the extremities of the roots, and readily sucked in by the spongioles.' This is the notion of indoor bookish theorists. I forget who first made the observation, but, as it is repeated by almost all writers on vegetable physiology, it deserves notice. Both the *facts* here *supposed* may be considered as vulgar errors. That is, *in general*, the horizontal extension of the roots will be found far to exceed that of the branches; and so far from its being true that *less* water falls under the head of a tree than outside it, there is every reason to believe that *more* falls there.

ends of branches drip on to the ends of roots a fallacy.

Branches shorter than roots, and the drip through them, not outside them.

In reference to the comparative length of the roots and branches, there is doubtless great difference in different trees. But generally it is probable that the aggregate bulk of timber in the root is equal to that of the trunk and head; and as the roots creep along the surface of the earth, they form a flat circle like a wheel round a single tree, instead of a spherical circle like a ball as the head does. Consequently, the roots make up by their horizontal length for their want of opportunity to shoot perpendicularly either upward or downward. Those who will not trouble themselves to dig for this truth, may see it in trees which are apt to throw up suckers, such as the elm or aspen. Suckers may be seen from these at four, or perhaps I might say

Form of the root a flat circle like a wheel: form of the head a globular circle like a ball.

at eight or ten, times the distance of the length of the branches from the trunk; and were we to imagine that part of the head of a tree which is above the lowest tier of branches taken off, and added to the ends of the lowest tier, so as to form a circle in one plane like a wheel, instead of a spherical circle like a ball, we should not have a bad idea of the general shape of the root, both in form and extent of circumference. Consequently, the extremities of the roots, with their supposed spongioles, are very far removed from the supposed drip from the outside of the head of a tree.

But in reference to this drip round the outside of the heads of trees, the phenomenon may be seen from umbrellas and roofs. But trees are neither umbrellas nor roofs, as those who take shelter under them will find. As long as their leaves and branches can absorb or hold on their surface the mass which falls, they will afford shelter. But after that the rain will not trickle outside the circle, but perpendicularly through every part of the head. Thus much for what takes place when the trees are in leaf. For six or seven months, however, they are without leaves. And in the spring they are without them, at the particular time when the great upward supply is required for the very formation of the leafy canopy which is *supposed* to supply the *supposed* circle of spongioles. This leafy canopy, far from supplying the circle around it and below it with *rain*, robs it of as much as is absorbed by the leaves, or as is evaporated from their surface. But this robbery is

The head robs the root of a little rain, but more than repays this by condensation.

much more than compensated by the *condensation* which takes place whenever the atmosphere is moist and warmer than the trees. For this *fact* I shall quote the admirable outdoor observer, White of Selborne: 'In heavy fogs, on elevated situations especially, trees are perfect alembics; and no one who has not attended to such matters can imagine how much water one tree will distil in a night's time, by condensing the vapour which trickles down the twigs and boughs, so as to make the ground below quite in a float. In Newton Lane, in October, 1775, on a misty day, a particular oak, *in leaf*, dropped so fast that the cart-way stood in puddles, and the ruts ran with water, though the ground in general was dusty.' And that part of a road which is overshadowed by a tree may, in summer, play the part of Gideon's fleece. In rain, it may be dry and dusty, when all beyond it is moist; and, in condensing weather, it may be drenched, when all beyond it is dry and dusty. This condensation much more than makes up for the small quantity of rain-water which is arrested by the leaves.

Roget remarks: 'We have here a striking instance of that beautiful correspondence which has been established between processes belonging to different departments of nature, and which are made to concur in the production of remote effects, that could never have been accomplished without these preconcerted and harmonious adjustments.' Fine words, certainly! but *præterea nihil.*

It is indeed possible, nay probable, that Almighty wisdom has designed an additional supply of water to the roots of the noblest of his vegetable works. But, granting this to be so, we are not to look for the hand of the Creator in an *imaginary* water-shed, from the outside of the head of the tree, on to an *imaginary* circle of spongioles, which, if they existed there, would probably absorb no water; but in the provision of an apparatus for condensing the watery vapour of the air, and for shedding it, through the whole head of the tree, on to the woody and really absorbing part of the root.

That this water-shed takes place, Roget may convince himself *oculis fidelibus*. For rain and condensation only fall from leaves or boughs after they have accumulated into much larger drops than those which fall as rain. And where trees overhang roads, these large drops, by the force of impact, eject the fine sand, and leave a surface of rough stones beneath the whole circle of the head of the tree, not merely round the outside of it. This fact may be *seen*.

Where branches form elbows, pointing downward, the dropping will sometimes almost amount to a stream, and will form a conspicuous mark on the road. Where boughs are horizontal, each drop falls perpendicularly, as soon as gravity overcomes cohesion; that is, as soon as the force of its weight is greater than its power of sticking to the bough. But not a drop of rain falls under trees in its natural form and size. In many cases accumulation goes on till the weight of water can bend the leaf to discharge its contents.

Rain and condensation often shed inwards or towards the stem.

Leaves are not fitted on to each other like tiles or slates on a roof; and it is impossible that the *water-shed* of trees should be outward or from the stem, because the continuity of the outward channel is interrupted at the outward end of every individual leaf. But it is by no means impossible that the water-shed should be inward or towards the stem. And this, the very reverse of Roget's fact, sometimes is the fact. Where leaves incline upward from the twigs, the twigs upward from the branches, and the branches upward from the stem, in rainy or condensing weather almost every drop of water is shed towards and down the stem; and the stem of a tree stands the model of a river, rich in the supply of water directly as the number and size of its branches.

This streaming of the stem may be observed in all trees, but differing in quantity as infinitely as the direction of the growth of their branches and twigs may differ. It is most remarkable and most profuse in Lombardy poplars. To this day they may be seen deluged in tears for the fate of their rash and hapless brother:

Quam platanus vino tam gaudet populus undâ.*

* In reference to Ovid's physiology of the plane, Macrobius writes: 'Is Hortensius platanos suas vino irrigare consuevit: adeò ut in actione quâdam, quam habuit cum Cicerone susceptam,

But how, by Roget's and the received system, shall these slim sisters of Phaëton drink their own tears? Their *capillary stomata* (since the jargon of science will mingle 'verbis Græca Latinis') are at the distance of the vertical height of the trees from their stems, instead of the horizontal width of the branches. This may be seen from suckers. And how is the poor oak to enjoy the drip from his own wide-spread branches, when its single capillary stoma is buried, perhaps, a hundred feet directly under the centre of its stem?

Owing to condensation, the ground underneath the head of a tree is much more watered than that outside it; and, besides this, it escapes the great evaporation of the summer drought and heat. For when the rays 'nimium propinqui solis' are the hottest, its canopy of leaves is the densest.

Argument from the 'Gardener's Chronicle' considered.

The editor of the 'Gardener's Chronicle' thinks that roots absorb only by their ends, because gardeners dig manure in only round the outside of the semicircle of the roots of their fruit-trees, without perceiving that the fact cuts against his own argument. If they were to dig it in nearer the stem their spade would destroy the really absorbent part of the root, the woody part; and, as 'omne majus continet in se minus,' they would also destroy the much-valued spongioles. By digging round the outside, they destroy little more than the

procario à Tullio postulâsset, ut locum dicendi permutaret secum: abire enim in villam necessariò se velle, ut vinum platano, quam in Tusculano posuerat, ipse suffunderet.'

then useless silver ends. These are instantly replaced, shoot freely through the loosened earth and manure, soon become woody, and absorb the chemically nutritious juices and gases evolved by the manure. Trees which require forcing *should* be top-dressed and irrigated either with water or liquid manure over *all* their roots, but manure should be only *dug* in outside their extremities.

The spade the destroyer of wall-fruit trees.

But the spade actually is the great unknown destroyer of our wall-fruit trees. On a practical *post-mortem* examination and inquest on them, the gardener finds that the only remains of roots are in the bad lower soil. And his verdict, instead of tree-slaughter against himself, is *felo de se*; that is, died because the roots *would* strike too low. But he omits to observe the reason of this, which is, that the spade has destroyed every upper rootlet as it was made. 'Agrum pessimum mulctari cujus dominus audit non ostendit villicum;' and I wish that the editor of the 'Gardener's Chronicle,' instead of quoting these misdeeds as authority, would correct them as errors,—would say to the man of practice, 'Don't dig so deep,'—and inform him that the roots of his trees would be happy to revel in the upper soil, if his spade would let them.

Some trees have a tendency to form buds and to shoot wherever stem, branch, or root is wounded. This tendency in the horse-chestnut is so strong, that I think it might be *inoculated* for branches wherever they were wished for. The wounds made by the spade are a frequent source of suckers from the roots of fruit-trees, and the careless or ignorant gardener will show a fine bed of suckers with a dead trained fruit-tree.

Unphilosophical remedies.

The only remedies to this suicidal diving propensity of the roots of fruit-trees, which science has as yet suggested, are to plant them on mounds, or on layers of tiles!! This is as bad as to order the canal to be dug deeper where it ran over the side! Roots, by nature, have so strong a propensity to keep the surface, that they may be observed, after diving from the wall to a grass-walk, to rise so completely *above* it as to be injured by the scythe, or by the hob-nailed hoof of the clod who has condemned them as divers, and who, when they emerge from the protection of the grass-walk, again forces them to take a header out of reach of his spade. But if the practice of the gardener is to be law, what does he do with the ends of the roots when he pots his plants? He ruthlessly cuts them all off smack and smooth! And, in this case, if he diminished their heads, his plants would not droop as they *do*, and as my plants do *not.*

Yet however false in reason and theory, practically the planting fruit-trees in the garden *on mounds* is of inappreciable advantage. For the fool who has raised the mound will not be fool enough to dig it down again, and so that part of the root which is of most value to the tree, the woody part, is fenced from the spade.

Planting forest-trees on mounds has, probably, arisen from the malpractice of the garden. The system is bad. It makes the roots of the young plant more accessible to drought, and less accessible to watering; while the roots of the older plant, when unfenced, are entirely denuded by cattle treading away the earth, and suffer both from gnawing and treading. Trees should be planted at the exact level which their roots are afterwards to pursue, and a pan made round them by a slightly raised *rim.* This saves time and water in irrigating them, while it prevents overflows from without, to which the *cup* system is liable.

The roots of a wall-fruit tree form a *horizontal* semicircular fan, as the head forms a *vertical* semicircular fan; the roots being kept up by the bad soil, and kept down by the action of the spade. And on account of the *lateral upward* growth of roots, alluded to in another place, the older the fruit-tree the less deeply the spade should go over its roots.

That roots absorb only by sponges or capillary stomata at their ends a *scientific vulgar* error.

This is a long story; perhaps I ought to say digression. But I cannot pass over the belief in the *spongioles* and *capillary stomata* at the ends of roots, and that the food of plants is solely absorbed by them, as one of those speculative theories and pretty notions which our marvel-mongering nature is so prone to adopt, and so loth to part with. The notion pretends to the authority of practice, and to stand on experiment and facts. It is not a *vulgar vulgar* error, but a *scientific* vulgar error; and should the man of *common sense* reproach me that I have wasted much time to prove what we need no ghost to tell us, my excuse is, that men of *uncommon sense* (I think, Sénebier first) have repeated these supposed facts one after the other, till they have become acknowledged data, which vitiate our physiological theories at their earliest source, in regard to the first absorption of food by plants.

Whether this is true or not of vital importance to transplanting.

Thus much in regard to vegetable physiology in general. In regard to transplanting in particular, the truth or falsehood of this *fact* is of every importance; since, if the life or death of a radish depended on the extremity of its root, an argument might be drawn that the life of other plants might depend on the extremities of theirs. But when we find this assertion to be totally devoid of foundation,—when we find the radish, deprived of all the immature parts of its root, absorbing nourishment for itself till it has replaced all its mutilations of head and heel,—we shall have the less horror of depriving our transplanted trees of their rootlets by *simple excision*; and we shall be the less apt to waste labour in taking up long *contused* roots (from this cause, almost sure to die) in, after all, the impossible attempt to get at the terminating ideal *spongioles* or *capillary stomata*, which are names as handsome as mingled Greek and Latin can make them, but *vox et præterea nihil.*

Ends of roots to a tree what children are to a commonwealth.

As long as the root is unripe,—in other words, unwoody,—it is wholly useless; that is, it has no upward conduit for the sap. The small fibres of the root bear the same relation to a tree as children to a commonwealth. So far from being a present source of strength they are an actual expense and outgoing; though, by their growth and maturity,—that is, when the unripe fibres become woody roots, and the children become men,—they are the very springs of vital energy.

CHAPTER II.

COURSE OF THE SAP FROM THE ROOT TO THE LEAF, AND BACK TO THE ROOT.

Course of the upward sap through the whole of the wood.

BUT, however much we may dispute on *how* the sap gets into the tree, we shall all agree that it *does* get in somehow; and, but for Dr. Lindley, I believe we should all agree on the *course* which it then takes.

The upward course of the sap is through the whole woody part of the roots, stem, and branches of the tree. This woody part has been hitherto divided by physiologists into two parts, the heart-wood or dead wood; and the alburnum or *sap-wood*; that is, the unripe and outside rings, or latest deposits of wood around the stem. It was considered that the heart-wood really was dead wood, and that the upward course of the sap was through the alburnum, or sap-wood, or latest rings of wood only. In March, 1832, I remarked the stems of some birch-trees, which I had cut down in the previous November, bleeding from the heart-wood. I was not then aware that Coulon had about this time observed the same thing in cutting down some poplar-trees in France. I afterwards satisfied myself, in various ways, that the whole of the wood is the conduit for the upward course of the sap.

The upward sap goes through the heart-wood; proof by experiment.

In April, 1832, I found a birch-tree in Dyrrham Park, of from 60 to 70 years' growth, which had a large scar from injuries from cart-wheels. The scar might have existed from 15 to 30 years, being perfectly indurated, and in parts turned to touchwood. I tapped the scar in the centre with a very large gimlet. I had not pierced an inch before it ran freely while boring. I bored about 3 inches in depth. The tree was about 9 inches through, in the direction of the bore. The dead surface-wood was perhaps a little more than a quarter of an inch in depth. The dropping was so frequent as almost to amount to a stream. This could only have flowed from the heart-wood, since no alburnum, or new wood, had been deposited on the scar for about twenty years.

It is stated that Coulon accidentally observed the sap flowing in the heart-wood, in felling some poplar-trees, as I believe, about 1830. His farther proof of boring with an auger, in my opinion, goes for nothing. As the auger must have pierced the sap-wood as well as the heart-wood, who shall say whether the stream which followed came from the sap-wood, or from the heart-wood, or from both, or how much from each? If, according to MM. Coulon, Desfontaines, and Thouin, it came from the heart-wood *alone*, the experiment would prove too much. It would prove that the sap-wood is not a conduit for the sap. This is not only nonsense, but *undoubted* nonsense.

The upward sap flows through the sap-wood; proof by example

Or if any one *does* doubt that sap-wood conducts the sap, let him look at the case of plashed hedgerows; where the entire heart-wood and pith are cut through, and a strip of sap-wood left no thicker than a lath : yet this thin conduit supplies sap to long thick branches sufficient to enable them to live and to grow permanently. I do not allude to *layers* laid sideways in the earth, but to plashers laid sideways in the air, as in hedges.

People have indeed always doubted, and some still do doubt, whether the heart-wood is a conduit to the sap; among others Dr. Lindley, in 1849 (nineteen years after Coulon's discovery), sticks to this old error. He says : 'When the tissue of the concentric layers is filled with secretions, it ceases to perform any vital functions. The *dead* and fully formed central layers are called heart-wood.'

The Doctor is not the first of physiologists, but he is the first of them who has told us the *reason* of the death of the heart-wood. It chokes itself with its own secretions. Yet, though the heart-wood has no longer room for the upward sap, it is not so choke-full but what, by-and-by, we shall find the Doctor forcing it to swallow the downward sap.

In fact, both the heart-wood and the sap-wood are conduits for the upward sap. To convince us that this is so, we want nothing further than these two proofs adduced by myself: that is, the boring through an old scar into the heart-wood of a birch proves, by the stream which follows, that the heart-wood is a conduit for the sap; and the existence of plashers proves that the sap-wood is a conduit for the sap. How I wish that Dr. Lindley would allow us this one bit of undoubtful ground in vegetable physiology to catch our breath on!

By what mechanical power is the upward sap raised?

Certainly, in respect to first getting the sap *into* the tree, we have had doubt and difficulty enough. Now for the next doubt and difficulty. Now for getting the sap *up* the tree. I was going to say, that no one has an idea of the cause, and laws, and mechanism by which the sap is forced up. But there I should be wrong. Every one has his own idea, and every one's idea differs from that of his neighbour. To show on what extraordinarily loose ground these ideas stand, I will quote two countenanced by perhaps the keenest intellect that ever wrote on the subject. Sir Humphry Davy thought that one cause of the ascent of the sap was the motion caused in trees by wind; that another cause was the contraction and expansion of the wood from alternation of heat and cold. Look into the hot-house and the hot-bed. In these neither of these causes exists. Not a breath of wind enters; nor is any alternation of heat and cold allowed. Yet in these the ascent of the sap is freest. And if we look out of doors, I should say that the sap would be a slow traveller if its ascent depended on wind and cold. Here, then, I cannot back the favourite, and have a sort of blind leaning for *Turgescence*, or Swelling. A dark horse, certainly! and I am all in the dark about him myself.

Perhaps the largest blocks of stone ever quarried by man (I do not except Pompey's Pillar for length) are from the granite quarries in Finland. One mode of rending these from their beds was to drill holes along their sides, and to fill these holes with water in a frost. Here 'weak water' splits the hardest substance by turgescence, or swelling, without a star on its frozen column. This plan was changed to plugging the holes with dry wood, and wetting these plugs simultaneously. Here, again, the mysterious and marvellous power of turgescence performs its Herculean task with *apparently* very weak implements. A spoonful of water and a small bolt of wood form the blood and frame of a giant who, give him fingerhold, makes a joke of Milo. He changes his name sometimes, though. When he acts on ice, I should have called him dilatation. When he acts by heat, his name is expansion. And to him is confided the growth of *geological vegetation*. His endogens are the Alps, the Himalaya, and the Andes; his exogens Vesuvius, Etna, and Madeira. Who shall guess from what depths within the earth these last receive their red-hot sap? But even to raise it after leaving the earth, through the pressure of the Atlantic, perhaps some 30,000 feet, is not a bad squirt. We have then, in the roots and stem, wood and moisture, the implements of turgescence,—a force unlimited in power. How to describe the steps by which it is to raise the sap a hundred feet, I confess I do not see. But I *guess* that some day it will be seen that turgescence, acting on *living* organisation, is the main *mechanical* power in the ascent of the sap. *Cut* a tree, indeed, or grub it if you please, and place it on its roots in a pond, and turgescence will by no means send the water up the *dead* wood. On the contrary, all but the immersed end of the tree, and its immediate neighbourhood, will dry as if it were in the timber-yard. This is as far as my horse Turgescence will carry me, which is not very far. And I end as I began—we are all in the dark about it!

According to an experiment of Hales, which has been since verified by others, the sap rises with a force sufficient to support the pressure of a column of water of double the height which would burst an ordinary hogshead cask. And we see that it ascends to prodigious heights, yet we are ignorant how or by what agency.

Where is the sap elaborated? Whence the new growth in girthing deposited?

We see also the miraculous and universal system of transubstantiation with which we are surrounded in the vegetable world, yet we are not only ignorant of the *chemistry* which produces this transubstantiation—which, from absorbed moisture and gases, forms the immense variety and the immense quantity of all 'the proper juices' and 'peculiar acids' of all the various woods, bark, leaves, flowers, scents, fruits, seeds, &c., of that most exquisite kingdom,—but we are even ignorant in what part of the plant the elaboration takes place, or from what part of the plant the new growth is deposited.

Sap supposed to be elaborated in the leaf.

After the ascent of the sap, it is *supposed* to be elaborated in the leaf. It passes from the wood into the woody fibrous, or stalky, parts of the leaf; thence into their spongy, porous (parenchymatous) parts; and thence backwards into the green inner bark of the shoot, which is called 'the herbaceous envelope.' So that the leaves and buds may be considered as the conduits, connecting the upward with the downward current of the sap. In descending through the bark, the elaborated sap is supposed to deposit the annual ring of the new wood (alburnum), and the annual ring of new bark (the new cortical layer) (both at once? or which first?), round the branches, stem, and roots, and also to furnish the new growth of the roots in length.

Growth in girthing supposed to be deposited from this elaborated sap in returning down the bark.

First office of the leaf, transpiration and excretion.

Whether any chemical change or elaboration of the sap really does take place in the leaf, greater than in any other part of the tree, I shall not question; but I think that possibly *one* chief duty of the leaf is to give off the supernumerary watery parts necessary to take up from the soil and conduct to their destination the carbonic acid, ammonia, and inorganic matters requisite for vegetable life, and that it exhales the unnecessary gases, &c., after the decomposition of the constituents absorbed from the soil. And is not the great accumulation of ash, or incombustible or inorganic matter, found in the leaf, as compared with that found in the wood, an argument that the leaf is the organ of excretion? The quantity of ashes or inorganic matter left by the leaves when burnt is perhaps from twelve hundred to two thousand per cent. greater than is left by the wood; that is, it is from twelve to twenty times as much. And it is possibly on this account that these organs of excretion are themselves excreted. The division of trees into deciduous and non-deciduous is not strictly correct. All are deciduous; that is, all defoliate or lose their leaves—those which we call deciduous generally in about six months, those which we call evergreens generally in about twelve months. Evergreen trees, however, differ in time, but each has its fixed period for defoliation.

Certainly the transpiration or giving off of water from the leaf, when exposed to drought, is very rapid, and the communication from the root to the leaf very rapid and constant; since on felling trees of thirty or forty feet high, while they were shooting in the summer, I have observed the shoots lose their turgescence; that is, droop, in the course of a few minutes from the time that the stem is divided from the root. The accurate and admirable Hales found that a sunflower, in dry, hot weather, gave off two pounds and a half, that is, two pints and a half, of water in twelve hours. At night, and in moist weather, the quantity was much less. Sénebier supposes that plants give off two-thirds of the water which they absorb.

Many physiologists imagine that the great use of the Leaves supposed by some to absorb the food of plants.

leaf is to *absorb* moisture. M. Bonnet tells us that the leaf is formed to absorb, chiefly from the lower part, because dew ascends. But as dew is a condensation of moisture which is suspended in the atmosphere, it cannot be said to ascend more than to descend; and the physiologist forgets that rain descends. But I do not think that M. Bonnet's facts are more to be depended on than his reasoning. He states that if leaves are floated on water on their upper surfaces, they will die as soon as if they were not put in water, but if they are floated on their lower surfaces they will remain alive as long as if their stalks were immersed in water. I tried this experiment in 1836, and found that the leaves floated on their upper surfaces remained alive as long, or rather longer, than those on their lower surfaces; one remained in part alive for six weeks. But the fact that detached leaves or branches placed on water, but with the ends of their stalks out, will remain alive much longer than if suspended in the air, proves lateral absorption. And on this assumption the wetted hay-band is placed round the stem of trees packed for long journeys.

Errors of Liebig.

I have only had opportunity of seeing these theories of M. Bonnet quoted. According to this class of physiologists, of which the great chemist Liebig is the modern oracle, when trees are in full leaf they receive their entire nutriment through their leaves from the atmosphere, and 'the complete dryness of the soil' would not then injure them. If this were so, if a branch were cut in full leaf, and suspended from those among which it grew, it should remain alive till the fall of the leaf; or when trees were cut down at midsummer, till the fall of the leaf the heads would remain alive, and the roots would immediately die. The reverse of this is the case; the roots remain alive, and shoot out without any assistance from the atmosphere, and the heads immediately die.

If leaves are picked, they will die sooner than if the bough is cut on which they stand; if the bough is cut, they will fade sooner than if the stem is cut on which the bough stands; and if the stem is cut, they will fade sooner than if the tree is grubbed. All this shows that the leaf is fed from the stem, and contradicts Liebig's notion, that leaves feed themselves and their parent trees from the atmosphere.

Cut a branch in a hot sun in June or July. In a few minutes its leaves will fade, dry, and shrivel. What keeps their living brethren fresh and succulent through the fifteen hours' drought and heat of our midsummer sun? A *constant* supply of sap from the roots. If, indeed, a drought in summer or autumn is continued so long as to deprive the roots of moisture, the leaves will begin to fade; but the leaves will recover immediately, if the roots *only* are watered. Doubtless, if the leaves also are watered, and wetted hay-bands applied to the stems, it will expedite matters. For doubtless plants absorb from all parts which are exposed to moisture, and excrete, that is, transpire, or give off water, from all parts which are exposed to drought.

Liebig tells us that 'leaves, twigs, and branches, when completely matured, as they do not become larger, do not need food for their support.' Why, then, do they droop when the supply is cut off, and revive when the roots only are watered? Let any physiologist of this school apply this *reasoning* to his own 'completely matured' body, and cease to supply it with food because it has ceased to become larger. But if leaves 'do not become larger,' the plants which bear them do. The formation of the bud, the downward growth, the growth in girthing, and the growth in elongation of the roots, are in full tide when the leaves have ceased 'to become larger;' and the leaves are doubtless essential to this increase. Suppose a gardener were to take Liebig's word, and to refuse to water his pot greenhouse plants when turned out for the summer. Not one would survive. And why do they require watering when plants in the open ground do not? Because the earth in the pots is exposed *all round* to drought from sun and air, and unless the earth were watered the roots would have no moisture to absorb. While in reference to plants plunged in the earth, the upper surface only of the ground is exposed, and the roots underneath the surface have always moisture to absorb.

To feel the force of the ἄριστον μὲν ὕδωρ we must go to the sallow south; but if, on Liebig's principle pot-plants are not watered, even in our moist English summer, their roots would have nothing to imbibe from, and the plants would die. What faith is the practical man to place in the theorist who puts him up to such secrets as these? Possibly Liebig may have taken his vegetable physiology on trust from others; but certainly this is one of a dozen monstrous theories with which this (so said) profound chemist would annihilate the very foundations of vegetable physiology, giving him the best. 'Mutato nomine,' his own words to Burdach apply to Liebig: 'All inquiry is arrested by such opinions, when propagated by a teacher who enjoys a merited reputation obtained by knowledge and hard labour.'

Liebig writes, in reference to ferns, &c.: 'They resemble in this respect the plants which we raise from bulbs and tubers, and which live, while young, upon the substances contained in their seed, and require no food from the soil when their exterior organs of nutrition are formed. This class of plants is even at present ranked amongst those which do not exhaust the soil.' According to this, we ought to be able to grow our potatoes without any soil at all: but, in fact, there is no crop which exhausts the soil *for itself* more than potatoes; there is no crop which is more *grateful* for a change of soil; and there is no crop whose growth differs more, according to the different soil in which it is grown. Any one of these three facts proves that the potato is nourished from the *soil*, not from the air: and under this idea the soil is twice hoed after the potatoes are above it; that is, it is 'flat hoed' and 'hilled up.' And Liebig himself says: 'The increase of crops obtained by the use of guano is very remarkable. According to the same authority (Garcilaso), the crop of *potatoes* is increased forty-five times by means of it,' and 'I applied to a field of *potatoes* manure consisting of night-soil and sulphate of magnesia (Epsom salts), and obtained a remarkably large crop.' Again: 'In the first year all the different parts of the field produced potatoes, but they succeeded best in those divisions which had been manured with peat, ashes, lime, and marl.' All these cases prove the growth of the potato to be from the soil, not from the air.

If plants draw their nourishment from the atmosphere, why do we find plants peculiar to peculiar soils? Let any one farm on the idea that the growth is to accrue from the atmosphere, not from the soil. If Liebig's views are correct, a landlord should put his land up to let, not by the quality of the land, but by the acreage of atmosphere which overhangs the land. If, however, Liebig would but condescend to follow the example of Dioclesian, and plant chemical kale instead of imperial cabbages, he would find that he must not leave the large leaves on his transplanted greens with a view of supporting the plants, but that he must cut the large leaves off because the plants cannot support them.

Liebig writes: 'The verdant plants of warm climates are very often such as obtain from the soil *only a point of attachment*, and are not dependent on it for their growth. How extremely small are the roots of the Cactus, Sedum, and Sempervirum, in proportion to their mass, and to the surface of their leaves!' In the next paragraph, he with much *naïveté* refers to experiments of Lukas at Munich; who, by mixing charcoal with the *soil*, gives a wonderful growth to '*young tropical plants*,' of all descriptions. Among other plants, 'a *cactus*, planted in a mixture of equal parts of charcoal and earth, throve progressively, and attained double its former size in the space of a few weeks. The use of the charcoal was very advantageous with several of the Bromeliaceæ and Liliaceæ, with the Citrus and Begonia also, and even with the Palmæ.' Here the roots must have served as more than 'only a point of attachment,' and the increased growth must have been from the soil, not from the air.

But, according to Liebig, there's nought like chemistry. He would do as much with his atmospheric chemistry as the currier with his leather. He generates and feeds his leaves by the carbonic acid of the atmosphere; and when he has done with them, he destroys them with the oxygen of the atmosphere. And he forms his trees by mechanical patchwork, and by juxtaposition, as he would a stalactite, or as he would the trees in the garden of a doll's house.

'When the food of a plant is in greater quantity than its organs require for their own perfect development, the superfluous nutriment is not returned to the soil, but is employed in the formation of new organs. At the side of a cell, already formed, another cell arises. At the side of a twig and leaf, a new twig and a new leaf are developed.'

Again :—

'The power of absorbing nutriment from the atmosphere, with which the leaves of plants are endowed, being proportionate to the extent of their surface, every increase in the size and number of these parts is necessarily attended with an increase of nutritive power, and a consequent further development of new leaves and branches.'

Again :—

'The organs of assimilation, at this period of their life, receive more nourishment from the atmosphere than they employ in their own sustenance ; and when the formation of the woody substance has advanced to a certain extent, the expenditure of the nutriment, the supply of which still remains the same, takes a new direction, and blossoms are produced. The functions of the leaves of most plants cease upon the ripening of their fruit, because the products of their action are no longer needed. They now yield to the chemical influence of the oxygen of the air, generally suffer a change in colour, and fall off.'

The Author of nature, and the author of the 'Chemistry of Physiology,' form their trees on widely different principles. No bud contains one leaf only, and perhaps, 'a *shoot*-bud' would be a more proper name than 'a *leaf*-bud.' But whether a bud contains the germs of leaves and a shoot, or of a flower and fruit, or of all these, these buds are all formed at the same time, and *in the year previous to their bursting*. The contents of each bud are *pre*ordained and *pre*arranged (in exquisite embryo) in the *pre*vious year. They are the offspring of no second chemical cause, but of the first cause. They are fairly conceived by the Creator, and borne in the womb of the bud for perhaps nine months. Any particular season of the year, or any atmospheric chemistry consequent to any particular season of the year, could no more change the contents of these buds as shoots to fruits than it could change lions to lambs. The generation of a new leaf is about as much an affair of chemistry as the generation of an animal is. And it is in consequence of each bud giving rise to a *family* of leaves that all nature's growth is *symmetrical*, and not made up of Liebig's patchwork.

In the case of a second or midsummer shoot,* or of

* I imagine that a second or midsummer shoot of the plane would be impossible without the previous defoliation of the tree, since the winter-buds are ensheathed in the footstalks of the leaves. The plane, therefore, is essentially and necessarily deciduous. And I have observed, at Madeira, that the plane is for two or three months without leaves, though our oak may in those climes almost be called an evergreen. The idea in Madeira is, that the old leaves of the oak are only displaced by the bursting of the spring buds. But the buds of every tree I know, except the plane, *may* burst without displacing the leaves, as they actually do in the second or midsummer shoot.

an accidental shoot from the bare stem of a tree, a regular bud is first formed; and, consequently, even these accidental shoots have symmetrical growth. But a leaf is never formed by itself; so that, in the case of an accidental shoot from a bare stem, it may be said that a bud is formed without a previous leaf, but a leaf is never formed without a previous bud, as Liebig would have it.

When the heads of coppice-stools or of pollards have been cut, their first year's growth is always late, because they have first to form buds before they can shoot.

The tulip-tree is the most exquisite exemplification of *the parturition* of a bud. If, between May and August, the transparent case is held up to the light, the enclosed leaf will be seen doubled on itself, and crane-necked. The case is broken by the protrusion, not of the leaf only, but of the whole shoot or contents of the bud; and the case, which remains for some time at the foot of each leaf-stalk, is not that which contained that leaf, but that which contained the embryoes of all its younger brethren. These younger brethren, which are beautifully packed nearer to the stem than the head of the leaf which is to be developed, are successively protruded farther from the stem than that leaf.

There is nothing in which trees differ more than in the folding of the leaf in the bud, though it is always the same in the same species. Some plants, as the vine, have each leaf beautifully folded over its batch of younger brethren. It has then no case; and it is a very frequent rule, though with many exceptions, that those leaves which have cases are either *doubled* or *rolled* on themselves, and those leaves which have no cases are *folded* over the remaining bud.

Neither has the relative time at which the fruit-bud or the shoot-bud *bursts* any reference to any general chemical cause, but to the particular constitution of the tree. For instance, the white-thorn, or May, develops its leaves and shoots before its flowers; the black-thorn, or sloe, develops its flowers before its leaves and shoots. And this last is perhaps the most general rule among fruit-trees. Many, however, develop flowers and shoots simultaneously.

With regard to the death of the leaf at the time of the ripening of the fruit, perhaps the only way in which leaves have any reference to the growth or to the ripening of the fruit is, that if there are too many leaves, their increase abstracts from the growth of the fruit, and their shade prevents the ripening of it. But do summer apples and pears, or do the plants which ripen their fruits in June and July, 'yield to the chemical influence of the oxygen of the air,' and defoliate then? If so, cherry-trees would be curious objects about midsummer; so would gooseberries, raspberries, currants, &c. Strawberries make their great growth after ripening their fruit.

Deciduous trees defoliate at the end of autumn, though this is very much an affair of temperature; that is, the same tree, in different latitudes, will keep its leaves *later* directly as warmth, but it will ripen its fruit *earlier* directly as the warmth of the climate. Evergreens shed one year's leaves at the end of winter. But neither of these defoliations has the slightest reference to the ripening of the fruit; and the time of ripening of the fruit has no reference to any general chemical causes, but to the particular constitution given to the plant by an Almighty Creator.

Liebig also makes plants play fast and loose in reference to their carbonic acid and oxygen. In the light, they absorb carbonic acid, and give off oxygen; *vice versâ*, in the dark. All plants throughout the globe are, in point of time, for six months in the year in the light, and for six months in the dark. Therefore, all evergreen plants and pastures absorb and give off each gas for equal periods of time; not in equal volumes, however, according to Liebig.

'The proper, constant, and inexhaustible sources of oxygen gas are the tropics and warm climates, where a sky seldom clouded permits the glowing rays of the sun to shine upon an immeasurably luxuriant vegetation. The temperate and cold zones, where artificial warmth must replace deficient heat of the sun, produce, on the contrary, carbonic acid in superabundance, which is expended in the nutrition of the tropical plants. The same stream of air which moves by the revolution of the earth from the equator to the poles brings to us, in its passage from the equator, the oxygen generated there, and carries away the carbonic acid formed during our winter.'

Now, in reference to 'the temperate and cold zones,' it appears to me a contradiction to say that the carbon of all our plants is formed from the carbonic acid in the air, and that the superabundance of carbonic acid in the air is formed by the plants. And in reference to 'the tropics and warm climates,' suppose this soldier's * wind to have conveyed the two gases to their opposite destinations, tropical heat generates aridity and sterility, unless where *the soil* is irrigated by nature or art. What farther proof can we want that the 'immeasurably luxuriant vegetation' is drawn by the roots from the soil, not by the leaves from the exotic carbonic acid imported on so grand a scale by Liebig from the north and south?

Then, the oxygen evolved by plants is essential to the breathing of man and animals. And 'thus, cultivation heightens the healthy state of a country,' and a previously healthy 'country would be rendered quite uninhabitable by the cessation of all cultivation.' It appears here that we poor beasts grow our air from our plants, as well as our plants from our air. But has Nature no plants without cultivation? And in countries where she has no plants, as on sandy deserts, or in regions of eternal snow, is the air impure and unwholesome? Or does more malaria hang over the wide wide sea than over tropical swamps, which are

* But perhaps Liebig will lay a down-line to the tropics above his up-line to the poles. And will he dispatch his luggage-trains of heavy carbonic acid gas by that? *Gravity* forbid!!

'the proper, constant, and inexhaustible sources of oxygen gas'?

Second office of leaves, formation of the winter-bud.

In deciduous trees, and in the greater part of English evergreens, if each leaf does not *form* a bud, at least each leaf is *accompanied* by a bud. And I imagine that one essential office of the leaf is the formation and *summer* nutriment of the *winter*-bud. Each bud forms a shoot, or a flower, or both, the next year. This is the general rule. But vast quantities of leaves fail to mature buds, vast quantities of buds fail to produce shoots, and vast quantities of shoots fail to grow. Wind, or an insect, or drought, or any thing which injures or destroys a leaf, will injure or destroy its bud, and consequently the next year's growth of that bud. This is one chief cause of the baneful effect of wind on trees. Besides this, the circulation of the sap is dependent on the leaf throughout the summer, as it is dependent on the bud in the winter. The leaf is the turn-table which shifts the sap from the up to the down-line in summer, as the bud is this turn-table in the winter. Summer and winter this traffic is constant; but as it is less in winter, so the plant and staff required to work it is less. Keep your turn-tables in order, or lose your traffic.

Third office of leaves, the changing of the sap from the upward conduit, the wood, to the downward conduit, the bark.

The growth in girthing is from the downward sap, or dependent on it; proof by experiment:

That the growth in the girthing, or diameter of trees, is a downward growth, that is, from the descending sap, or at least that the descending sap is necessary for the growth in girth, seems clear from this: If a ring of bark is taken off round a branch of a tree, so as permanently to lay bare the wood, and to intercept the return of the sap through the bark, as long as the branch lives it will continue to increase in girthing above the ring, but not below it; and when such a branch is sawed in two, lengthwise, each additional annual layer may be counted above the ring, but none below it. But if the growth in girthing were deposited from the upward sap, the parts of a branch below the ring would be more favourably situated for it than the parts above the ring; also, if notches are made up a stem, the new growth comes first on the highest, and descends in succession. From these facts it is believed that, after the sap has been elaborated in the leaf, in its descent through the bark it deposits the new growth in girthing. If, however, the sap is elaborated *solely* in the leaf, and if the growth in girthing is deposited *solely* from the descending sap in the bark, the growth in girthing of the plum-stock of a grafted peach-tree should be peach; but the stock remains still plum, its roots plum, its shoots plum, and its suckers plum. On the other hand, if the elaboration were wholly in the root or stem, and the new growth in girthing from the upward sap, the wood and leaves of the peach would become plum. But purple beech * and variegated sycamore grow for ever unchanged, though engrafted on common stocks, as a single branch of a plant accidentally variegated will for ever retain its character.

and supposed to be deposited from the bark.

When peach-scions are grafted or budded on

* Purple beeches may be raised from seed. I have one which I planted out in 1837, and have them of all ages since that.

plum-stocks from 4 to 5 feet high, the plum-stocks taper in the usual way, from below upwards; but in the course of years the growth of the peach *appears* to overpower the stock, and it will be seen to taper from above downwards. This over-growth says distinctly that it comes from above; but that this over-growth is plum, not peach, says as distinctly that it is not *solely* from above. I think it, then, probable that the upward sap may communicate laterally throughout from the wood to the bark; and that, for the growth in girthing, it may be necessary to bring together, on the common ground on which the new external layer of wood and the new internal layer of bark are deposited, a sap which has been subjected to a triple elaboration, namely, juices of the upward sap—the product of chemical decomposition, assimilation, and elaboration in the stem, and those of the downward sap—which have been subjected to respiration, transpiration, and elaboration in the leaf, and to all these processes in their descent through the bark; finally, that a fourth elaboration of those saps may take place, on their junction between the wood and the bark, for the deposit of the new growth in girthing there. I confess that this is terrible guess-work; but I choose and state the theory which appears to me to have the *least* guess-work. As I have said throughout, all is doubt and difficulty. We may at least acknowledge our ignorance in the affair. To be ignorant is bad enough; but to be ignorant of one's own ignorance is worse. If a man knows that he has lost his way, he will at least go carefully; he will be ón the look-out, and be the more likely to find it.

It is certain that great chemical changes, or elaboration of the sap, must take place in the root or stem *before it reaches the leaf*, since sap of very different qualities is drawn from the stems of different trees. Witness the sugar from the maple and birch, the resin from the fir, &c. &c., which are found in the heart-wood; also, the alteration of the heart-wood in density, and the change of sap-wood into heart-wood, argue elaboration in the stem, and deposit from the upward sap.

But elaboration certainly takes place *before* the sap reaches the leaf,

It is also certain that great chemical changes, or elaboration of the sap, must take place in the bark, or elsewhere, *after it has left the leaf;* since wood of very different qualities is deposited on the stem of the same grafted tree.

and also *after* it quits the leaf.

Of the fact which I have supposed probable, namely, the lateral communication of the sap throughout the wood to the bark, I will give proof immediately, while considering the office of the pith (or medulla, 'marrow'), and its rays or silver grain (medullary rays), which many have supposed to be the means of this lateral communication between the pith and the bark. But I will first state those theories, in reference to the growth in girthing, which have been most generally accepted.

Received theories of growth in girthing.

Grew said that the new layer of wood is formed from a viscid substance, to which he gave the name of *cambium*. But where is the cambium itself formed? How and where elaborated? Duhamel thought that the last year's layer of bark was converted into this year's layer of wood. We can *see* that this is not the case. Du Petit-Thouars thinks that the new layer of wood is formed by the buds (though I believe the origin of this theory is due to Darwin); that the fibres in the new layer of wood are the roots of the buds, which, at the bursting of the buds, run between the last year's bark and wood to the ends of the roots.

Dr. Lindley thus confidently and complacently concludes his statement of the theory of Darwin and Du Petit-Thouars:—'The elongation of the leaf-bud upwards gives rise to new axes with their appendages; their elongation downwards increases the diameter of that part of the axis which pre-existed, and produces roots.'

The argument from the grafted stocks is, I think, stronger against this theory than against the growth from the descending sap; for though the Doctor may adopt the idea of the lateral flow of sap, and make it necessary to irrigate the roots of the buds, it would scarcely have the power to metamorphose an actual growing fibre of peach-wood into a fibre of plum-wood.

The pith or medulla, and medullary rays or silver grain.

Dutrochet and Link bring us back to Hales's doctrine of the all-importance of the pith. Indeed, Dutrochet would establish the *omnipresence* of the pith. He tells us that each division between the annual layers of wood is a pith for the layer outside it (let us call these *concentrical* piths, to distinguish them from the *central* pith); and that, in addition to the original medullary rays, or silver grain, which run from the *central* pith or medulla to the bark, and which are annually prolonged through each successive concentrical pith and layer of wood—in addition to these, intermediate medullary rays are developed from each new concentral pith, which run from that new concentrical pith to the bark, and are annually prolonged. Indeed, as very few medullary rays *could* be developed in a seedling of perhaps half an inch in girthing, it seems only *natural* that the number of rays should increase with the growth of the tree. Otherwise, when the girthing of the tree had increased from half an inch to thirty feet, the medullary rays would stand very far apart at their outward ends; and in the bark of thirty feet circumference there would be only the same number of rays as in the bark of half an inch circumference. The new rays, however, have no right strictly to the name of *medullary*, since they do not originate in the central pith or medulla. The medullary rays, which appear like the spokes of a wheel when the stem is cut across, are, in fact, thin plates running the whole length of the stem, roots, and branches. In width they increase every year by the width of the new layer of wood across which they extend to the bark, and in length they increase every

Medullary rays longitudinal plates, only bounded in length by the height of trees and length of branches and roots.

year by the length of the new shoot of the branches and roots.

The shake, and cup-shake. The medullary rays and the concentrical piths are so far distinct from the wood, that what are absurdly called, and commonly believed to be, *shakes* arise in them. All the shakes which I have observed show on each side of the tree, from which I imagine they must be the result of disease in the *original* medullary rays of the seedling: for one can scarcely suppose sympathy between two opposite *new* medullary rays which have no junction with each other; nor would one have anticipated this in two opposite original rays. That the shakes pass through the bark shows, I think, that the rays of the wood communicate with those of the bark. These shakes often rise to a great height, and are never cured. A diseased concentric pith is called a *cup-shake.*

The concentric pith or cup-shake, may be called finite; the medullary ray shake, infinite, by comparison: that is, a cup-shake is conical, and cannot extend above the cone of the year's growth in which it is generated. All the timber, therefore, which is above that cone, or outside it, is sound. The medullary ray shake may be continually prolonged upward, downward, and outward and inward, with the growth of the tree.

The new medullary rays proceeding from the new rings of pith may be easily seen in oaks: and I think that the medullary rays may be seen to prolong themselves into the latest layers of bark, if the stem of a living oak-tree is cut across; at least, there is a white line across these layers of bark, opposite the end of each medullary ray.

Lateral as well as longitudinal flow of sap through the whole wood.

Some physiologists have supposed a lateral communication of the *sap* between each and all the annual layers of wood by means of these medullary rays, or silver grain. But to show what guess-work vegetable physiology consists of, others suppose that these rays are merely conductors of *atmospheric air* between the bark and the pith. That there is a lateral transmission of *sap* throughout the wood by some means or other, I think may be argued from the existence of ringed branches. Indeed, were it not for this lateral communication, whether branches were ringed or not, their buds, leaves, fruits, and shoots could only be supplied with upward sap from the last year's growth of wood on which they are placed; and the upward sap of every annual growth of wood, except the last, would be confined within the limits of the cone formed by each annual growth.

I have a bough of a pear-tree which I ringed for the space of an inch in June 1832, and which I cut off the tree in December 1843. It bore fruit for the last ten of these eleven years, though the rest of the tree had never borne fruit up to the last-named year. The branch was alive when I cut it off. The woody part above the ring is, owing to its annual growth in girthing, double the size of the ringed part.

It is clear that every part of the interior of this branch—that is, of the woody part of it—which existed when it was ringed in 1832 was in 1843 divided from the exterior bark, and consequently from every bud and leaf, by eleven annual sheaths or growths; and the upward sap, which nourished the bud, the leaf, the shoot, and the fruit in 1843, must have been supplied to them from the old ringed interior wood by *lateral transmission* through the eleven newer annual growths of wood.

The longitudinal pith-channels extend throughout the tree, from the pith of the original seedling to the finest ramifications of the roots and branches of the largest tree which is *entirely* alive.

It must, however, be observed that the cone formed at the top of each annual growth of wood is not a *closed* cone, but an *open* cone. The top of each cone is, in fact, a *crater*. The pith passes through this crater, and the top bud is seated on this pith. The pith of each side-bud also joins the pith of the twig to which it is attached, as the pith of each branch which emanates from the stem joins the pith of the stem. This may be seen to be so in Plate I.; and also, if you divide an end-bud of a horse-chestnut branch, or if you divide the branch at any of the joints, where one year's growth ends, and the next begins. The channel of the pith may be *seen* to be continuous through the head of each annual cone; of the same size as the upper part of the older growth, and considerably smaller than the lower part of the newer growth. The pith, in fact, tapers upwards precisely as the shoot does; and the pith of the new year's shoot, notwithstanding its communication with the taper top of the pith of the last year's shoot, stands with as broad a basis as that of the seedling. I think that this junction of the taper top of the pith of one year's shoot, with the broad base of the pith of next year's shoot, is the origin of the ideas that the pith of each shoot becomes annually smaller than that of the shoot above it, by the new annual pressure from without of the wood, and that the pith eventually disappears. Both these ideas are vulgar errors. Dr. Lindley believes (or did, in 1849) of the pith, that 'its office of nourishing the young parts being accomplished, it is of no farther importance, and dies.' This *may* be so, certainly, but I wonder what the Doctor's reasons are for thinking so; the same, perhaps, as for thinking that all heart-wood is dead-wood. What other parts of the tree is the Doctor prepared to dispense with? That a tree will live when the original central pith, and nearly all the heart-wood, are dead and *gone*, we know; and so will a man when one of his lungs is gone; and the man and the tree are equally benefited by the loss.

Diminution in the size of the pith and its disappearance vulgar errors.

The difference in size, between the top of the pith of one year's shoot and the base of the next, may also be seen in Plate I. And in regard to the disappearance of the pith, even the layers of wood, which may be counted on this board, give thirty years of age to the lower pith, and twenty-nine to the upper one; but they are possibly much older, and perhaps half a century of pressure from without has neither exterminated them nor even reduced the lower end of the upper shoot to the same size as the upper end of the lower shoot. And I think it probable that each is of the exact shape and size that nature formed it the first year it grew.

Again, you have only to examine a newly cut tree of any sort, and, if sound, you will see the pith, though around it you may count from 50 to 150 years' growth. In drying, after the trees have been some time cut, small cracks in the direction of the silver grain meet at the pith, and prevent its being seen. This accidental disappearance of the pith, immediately on the *death* of the tree, is another corroboration of the vulgar belief of the death and disappearance of the pith during the *life* of the tree. Every innumerable small side-twig of every innumerable small branch and root of the most gigantic oak, gives origin to a new series of annual cones of growth; and until internal death and decay supervene, the first annual pith of the original seedling communicates with these countless ramifications of branch and root, and by its direct and lateral elongations passes, through the tops of these innumerable myriads of cones, to every side or leading bud, and to near the termination of every the finest ramification of the roots.

Whether the pith is the conduit of the upward sap or not.

Many of the older, and some existing physiologists, maintain that the upward sap is solely transmitted by the pith; and the fact I have stated seems to favour the opinion that the pith may play a prominent part in supplying the bud, leaf, and new shoot with upward sap: but how could the eleven layers of new wood, which sheathed the sides of the ringed branch, have been kept moist with sap, except by lateral transmission from one layer of wood to the other, independent of the longitudinal channels of the pith? And that the upward sap is supplied even to the bud by the wood, and not by the pith, I think we may argue from the success of budding. In this beautiful process, the pith of the bud is totally disconnected from any other *central* pith. It is placed on the side-wood, and can only receive the upward sap by lateral transmission from that side-wood. The same may be said of the scion in crown-grafting; its pith is quite separated from any *central* pith.

Do the *central* piths of budded buds, grafts, and of the shoots of coppice-wood, communicate with Dutrochet's *concentrical* piths?

Dutrochet has, however, started the idea, that the outside of each annual growth of wood is a pith, which we have called *concentrical* piths, to distinguish them from the central pith; and it may be argued that it is possible that the central pith of a budded bud, or of the scion in crown-grafting, may communicate with a concentrical pith, and that the central pith of a shoot of a pollard, or of a coppice-wood stool, may originate in a concentrical pith. If the yearling shoots of pollards or coppice-stools are knocked off so as to have a part of the old wood on which they grew still attached to them, and if the lower ends of these shoots are split down the piths into the old wood, the piths will be seen each to originate in a point surrounded by the old wood, and in conjunction with no other *central* pith. But I should rather say that the *central* pith of each shoot will be seen to originate in an extra deposit of *cheesey* pith on the *last* so-called *concentrical* pith of the stem-wood. In the ash, the *cheesey* point of the central pith of the shoot is bright green, and the extra mass of cheesey pith of the stem-wood is a yellowish white; and I think their junction is very visible. This fact, if it is a fact, seems to confirm Dutrochet's bold suggestion, that the outside of each concentrical layer of wood really is a pith. I have always doubted this idea, because the substance of these concentrical piths appears so different from that of the central pith; their substance appears to be not only wood, but the hardest and most durable part of the wood. If the ends of fir-trees are left resting on the moist ground, these so-called concentrical piths will remain after the layers of wood have rotted away from between them.

In favour of Dutrochet's idea, I have observed that, if young Scotch firs are decapitated with a saw, the resinous sap may be seen to stand in drops on their concentric piths. The stems, also, of Scotch firs, when cut down, appear to bleed from them alone; and it is, perhaps, possible that the longitudinal upward flow of sap may be through these piths only, and the lateral flow through the silver grain, or medullary rays.

But whether the central pith of the shoot of a pollard or coppice-wood stool originate in the wood of the stem, or of the last crop of branches, or in the supposed outside concentrical pith of one of these, should either or neither of these observations be correct, I think the case deserves investigation. I also think that such a question as this being hitherto *unanswered*—(might I not say, hitherto *unasked?*)—proves that we have yet much to learn in the science, and that there are many things under heaven and earth little dreamt of in our '*physiology.*' Unless these concentrical piths *are* piths, the growth of the bud, when disconnected from the central pith below it, disproves De Candolle's ingenious suggestion, that the pith is the *cotyledon of the bud:* for the life of the seedling is dependent on its cotyledon,—that is, on its seed; and it perishes when disconnected from it, even after leaves are developed on the plant, and the roots are several inches long. But besides this, the cotyledon, or seed, is absorbed and vanishes in feeding the seedling, but the pith endures for ever. De Candolle states that, in the chestnut, the ash, and the vine, 'the pith is interrupted at each node, or annual shoot, by a kind of *woody* partition.' Here, I think, this very great physiologist makes a very great mistake. It is true, that in the ash and horse-chestnut, at the junction of one year's shoot with the next, the texture of the pith may be described as *cheesey*, instead of *spongy*. And in the vine, this cheesey pith separates the spongy pith, not only at the junction of each annual shoot with the next, but at each knot or side-bud. But there is nothing in the least resembling *wood* in this cheesey pith; and when it is scooped out, the pith-channel of the vine is of the same size through the knots as at any other part, and the pith-channel of the horse-chestnut and ash is of the same size between any two shoots as the pith-channel of the top of the lower shoot. In these two trees, the pith of every shoot and twig is surmounted with cheesey pith; so that every bud is placed on cheesey pith, and is as much divided from a spongy pith by a 'woody partition,' as the pith is interrupted at each node or annual shoot by a 'woody partition.'

If De Candolle's fact is true, that in the chestnut, the ash, and the vine, 'the pith is interrupted at each node or annual shoot by a kind of woody partition,' if this *fact* of De Candolle's is true, what becomes of his *theory*, that the pith is the cotyledon of the bud?

I do not think it would be an improper description of the piths of these trees to say that their spongy piths begin with, or are seated on, cheesey pith, and end in cheesey pith, on which the bud is seated.

Office of the pith not known. The only opinion which I should venture to express, in reference to the pith, is the negative one, that no one has as yet discovered its offices. If this is so, it is not saying much for our knowledge of vegetable physiology.

As the sap-channels are general, not peculiar, pruning increases the supply to the leader, &c. Were the upward sap supplied to each branch by longitudinal channels from the root, peculiar to that branch, the pruning or cutting out of branches would not benefit the leader and the remaining branches. The growth of these is, however, increased by judicious and gradual pruning, because the channels for the upward sap from the root are not peculiar, but general, through the whole wood of the stem, to every or any part or side of it, where nature, or chance, or *art*, allows it an outlet for growth and elaboration.

The reason of the extraordinary strength of the shoots of pollards and coppice-stools is, that they, the minor body, receive the sap transmitted by *all* the old wood, from *all* the roots, which supply was adapted and sufficient for the growth of the larger body,—the head lately cut off.

But it is only by lateral transmission that the new crop of shoots can avail themselves of the whole sap supplied to the stem or stool by the roots. Were the channels of the sap through the wood only longitudinal, the new shoots would only get the supply of those vessels on which they were seated, and they would have no freer growth than ordinary shoots. The sap, in fact, flows freely in any and every direction through the whole wood; and in forest pruning, that sap which Nature may be said to have intended for an amputated branch, she immediately converts to the extra growth of the leader and the remaining branches. Thus, where tall *clean* timber is required, the gradual, I had almost said *annual*, removal of ill-placed or unduly large branches does good in two ways; for, while undesirable growth is destroyed, an annual *fillip* may be given to the growth which is desired to an indefinite period.

A new layer of bark is

A new innermost layer of bark (the new cortical layer) is also formed each year, from the descending formed every year.

sap, corresponding with the increased girthing of the tree. The old or outward layers are stretched outwards, crack, and form the rough bark seen on old trunks. The yearling shoot has but one layer of bark, besides the outer cuticle, the two-year-old shoot two, and so on; and each shoot may be said to have as many layers of bark, as well as as many layers of wood, as it is years old. But with regard to the layers of bark, besides the sloughing off, the circumference of the earlier layers would be very disproportioned to that of the later ones. If the circumference of the bark of the seedling oak were half an inch, it would make a poor show when rent and divided over the outer circumference of a full-grown tree, supposing it to have existed. This growth of the bark may also be considered as partaking of the principle of the growth by juxta-position, since the annual new layer is a distinct coating or deposit of new growth on the inside of the bark, and not a growth or increase of parts already formed. It is from the downward sap, since in branches which are ringed it ceases to be deposited below the rings, but is continued annually above the rings.

De Candolle makes a distinction between the outer skin or covering of the leaf and annual shoot and that of all other parts of the tree. He calls the outer covering of the leaf and annual shoot the *cuticle*, and that of the rest of the tree the *epidermis*. There is certainly a difference between living skin and bark and dead skin and bark, and it might be as well if they had different names; but if we give the name of cuticle to the outer covering of the living bark, it will be found, with its green under layer of *parenchyma*,—the green, porous, spongy layer, which is called the 'herbaceous envelope,'—to extend over a much larger space of our forest-trees than De Candolle assigns to it. There is nothing in which even the same sort of trees differ more than in this respect. According to growth, soil, exposure, &c., the cuticle exists to a very indefinite period; and it would be hard to say where cuticle ceased and epidermis began. Living external bark, with a green under layer, may be found on oak, ash, beech, Spanish and horse-chestnut, sycamore, poplar, &c., &c., on parts varying from twenty to fifty years in age. And in the plane-tree, whose bark scales off as it dies, and thus admits light and air to the under layer, or herbaceous envelope, this may always be found *green* on any part of the stem or branches. On roots also from twenty to fifty years in age may be found a fine silvery cuticle which tears off like paper; though in roots, under ordinary circumstances, the under layer, or herbaceous envelope, is white, not green.

De Candolle states it as a distinctive characteristic of roots, as compared with the stem, that 'they do not become green even when they are exposed to the air and light.' And this opinion is universally held by physiologists; but it is an error. De Candolle, in proof of the opinion, states that the roots of the hyacinths grown in transparent glasses do not turn green. This is true of them, and also of the silver ends of woody roots; but it must be recollected that to neither of these can the air be admitted when they grow in water, or light when they grow in earth. It is however, I believe, true of these unripe ends of roots, under any circumstances. But when part of a woody root is accidentally exposed by the wearing away of a bank, &c., the layer below the outer circle will be found green, precisely the same as on a branch; though where the root goes under the ground, both nearer and farther from the stem, the under layer will be white. The layer under the outer cuticle may also be observed green at the commencement of the root of a young tree, when it is accidentally exposed near the neck of the plant. This is not a matter of opinion, but a mattter of fact, and we have only to use our eyes to see it.

That roots have no piths an error.

On the other hand, since writing this, I have observed that, in those parts of the stems of seedlings which pass through earth, the piths and the herbaceous envelopes are as white as those of roots. I say *those* of roots, for I have also observed that the tap-roots of seedlings have piths of precisely the same size as the stems. And I doubt not it will be found that the roots, as well as the stems, consist solely of alternate layers of pith and wood, with one outer skin or cuticle. If roots have no piths, what are the rays or silver grain in the roots of oaks? *Non-medullary rays?* In the experiments detailed farther on, with a view to induce the stems to grow downward to the light, and the roots upwards in the earth, some of the gemmules or stems grew upward, and came out of the hole at the then upper part of the inverted flower-pot. These plants passed through eight inches and a half of earth. On dividing them lengthwise I found that, in those parts of the stem which grew in the air, the piths and herbaceous envelopes were green, and in those parts which grew in the earth they were white. The piths ran the entire length of the roots as well as the stems; and where the necks of the plants divided the stems from the roots, the two piths were continuous, and of precisely equal size. It is the universal error in physiology, to believe that roots have no piths. Let any one divide a seedling horse-chestnut, and he will convince himself.

Each layer of bark is supposed to have its proper pith or cellular ring outside it. The green cellular or 'herbaceous envelope' under the outer cuticle is supposed to be the pith of the outer layer of bark, and to be to the layers of bark what the central pith is to the layers of wood; and throughout its whole extent there is probably a direct vascular communication between this green external pith of the bark and the internal central pith of the wood, by means of the medullary rays. This green *parenchymatous* pith of the bark is in communication with, and is in fact a continuation of, the *parenchymatous* parts of the leaves (the spongy porous parts, as distinguished from the woody fibrous parts), as the outer skin or cuticle of the stem is of the cuticle of the leaves and buds.

There is no *true* circulation of the sap like that of the blood of animals.

All physiologists talk of the *circulation* of the sap; and the expression must be used, though it is a very incorrect one; that is, no one, I believe, has asserted, nor can we suppose any one to imagine, that there is a *true* circulation of the sap of plants, like that of the blood of animals. By the circulation of the sap is meant merely its *ascent* through the wood into the leaves and buds, thence into the green outer pith of the bark, on which the leaves and buds are situated, and its *descent* to the roots, through the living parts of the bark. How the descent dies off and stops, it is difficult to imagine; but it is still more difficult to suppose that any part of the sap should *re-ascend*. The whole affair, however, is a matter of the merest conjecture.

The '*proper juices*' of plants are found in this green 'herbaceous envelope;' for example, resin in the fir: and the woods of different trees do not differ more in their proper constituents than the barks of different trees; and, *possibly*, as the first herbaceous envelope is burst and destroyed, the next ring of pith assumes its functions.

Let the practical man guard these external piths from external injury. Besides the gnawing of horses, cattle like to find soft-barked trees, such as Scotch firs, &c., of a size that they can take between their horns to rub their foreheads against, and do infinite mischief in this way. Trees that are too large for this are comparatively safe, as the side rubbing of cattle does not injure them so much; besides that, the dead epidermis of old trees is a great defence to them. It is the *mechanical* injury which is to be guarded against: the idea of *chemical* poisoning from animal oil is a fancy.

CHAPTER III.

UPWARD GROWTH OF THE HEAD, AND DOWNWARD GROWTH OF THE ROOTS.

Upward growth of the head and downward growth of the roots considered together.

I HAVE postponed the consideration of the upward growth of the head in elongation, in order to take it in conjunction with the downward growth of the root in elongation; because I think that each may be better understood by contrast with the other. This is a deviation from the order laid down in an early paragraph, but I leave that paragraph unaltered, because I think it may give the beginner a clearer idea of which growth is supposed to result from the upward sap, and which from the downward sap.

The upward growth of a tree, or lengthening of its shoot, is by enlargement of all parts of that shoot; and all parts of these parts progress bodily upwards.

The upward growth of a tree, as compared with its downward growth, may be said to resemble the growth of animals by *intus-susception*; that is, the growth of the shoots of the current year of the leader and branches, is a growth or extension of parts already formed by the upward and outward increase of all those parts from within. Besides the growth at their ends, all the parts of the shoot of the current year, to a certain degree, grow bodily upwards, or by what (by comparison with the downward growth) may be called intus-susception. I have observed this in plants which I have introduced through the window-sill, and trained against the shutter, by marking the position and upward progress of the stalk of each leaf where it joins the stem. Duhamel first pointed out this fact. With the exception of the parts of the shoot of the current year, no other part of a tree makes any upward progress. The downward growth of a tree, that is, the elongation of the roots, and the growth in girthing of every part of the tree, may by comparison be said to resemble the growth of *minerals* by *juxta-position*; that is, roots are lengthened only by the deposit of new growth at their ends, and they do not progress bodily through the earth. I have never remarked accurately how this is with plants grown in water: but I believe it to be the same as when they grow in the earth, and that this mode of growth is the result of the organic structure of the root, and not of the mechanical difficulty of forcing itself through the earth; though, no doubt, this peculiar organisation is a *contrivance*, and a very beautiful one, to overcome the mechanical difficulty.

The downward growth of the tree, or lengthening of the root, is simply by growth at the end of the root.

The root has as strong a first tendency downward as the stem has upward, though it puzzles our philosophy to account for either. The first downward tendency of the root is, however, soon counteracted by circumstances, such as the necessity of atmospheric aëration, the goodness of upper compared with lower soils, the intervention of rocks, chalk, &c.; and the

By what agency is the head directed upward and the root downward?

root is found to grow horizontally, or sloping upward or downward, according to the inclination of the ground. I consider the idea of the tap-root of the oak (except as a seedling) to be a vulgar error. I have never seen any trace of a root at any great distance from the surface of the ground; nor do the stumps of oaks, when grubbed, show any symptom of a tap-root more than other trees. But the question of the existence of tap-roots in large oaks or other timber-trees should not be argued as a matter of opinion, but proved as a matter of fact. The negative, indeed, cannot be proved. I could produce any required number of oaks without the tap-root, but this would not prove that others were without it. Let those who assert the affirmative produce *one* instance.

The celebrated Duhamel, wishing to protect his field from robbery from the roots of a row of elms, cut a deep ditch between the elms and his field. The roots, however were not to be done. They of course followed the surface of the ground, whether down, horizontal, or up hill, and took the ditch 'in and out clever' into the philosopher's field. Duhamel thinks these were very cunning roots, and that they had an instinctive notion of the treat they were to have on the other side of the ditch, and grew *at* it. Had the philosopher built a wall in the ditch, and then filled it in, he would have beaten the roots, on account of their inability to leave the surface and get under the wall.

Turgescence, and

Were we to add one step to the beautiful theories of Knight and Dutrochet respecting the growth of *adhesive* plants, and the tendrils of climbing plants, *from* the light, we might attribute the upright growth of the stem and the aberration of its branches from the perpendicular to the mere swelling (turgescence) of the cellular tissue of the new green shoot, and to the action of light on the swelling.

The action of light on turgescence, probably direct the growth of the head of the plant.

Cellular formation of the bark of green shoots.

It is asserted, on microscopic observation, that the bark of all new green shoots is entirely composed of two layers of cellular tissue. The cells of the outside layer of tissue decrease in size from within outwards; the cells of the inside layer decrease in size from without inwards. Thus the largest cells of each layer are next one another, or in the middle of the bark, and the smallest cells are on the two sides, that is, the outside and the inside of the bark. Owing to this formation, swelling would stretch a slip of bark perfectly straight, since each layer of tissue would tend to curve itself outward. But in plants which grow towards the light the inside layer of tissue is the thickest, and therefore the most powerful in its action; consequently swelling would bend a slip of such bark inwards or towards its shoot, and such a shoot would be held up by the inward pressure of its bark all round it, as a wall may be propped from both sides. But in the bark of plants, or parts of plants, which grow from the light, such as ivy, the tendrils of climbing plants, &c., the outside layer of tissue is the strongest; and the tops of the shoots of such plants tend to stand upright by the pull-

How turgescence acts on the cellular formation.

ing outward of their bark from all sides, like the mast of a ship. But light, by increasing the giving off of water (transpiration), decreases swelling; and when it falls unequally on plants, the forces on the illumined side will be weakened, and the shoot, according to the formation of its cellular tissue, will be turned either towards or from the light. Thus ivy, or the tendrils of climbing plants, are turned from the light towards any opaque body, while the generality of plants are turned from the opaque body towards the light. The whole of a plant kept in a room will grow sideways towards the window. The same plant placed where the light comes from above will immediately turn straight up. But though this principle is not so striking out of doors, it is perpetual and universal.

How light acts on this turgescence.

As the light falls equally on the leader of a tree, it goes up straight. In proportion as the leader intercepts the light from above, the branches grow towards the light at the sides; and if one outstrips its neighbours, the light from above turns its end up. If the lower tier of branches outgrows the tier above it, in turning up, it will take its neighbour with it. That tier will turn the tier above; and so in succession all the branches will grow upward. This growth is often seen in beech-trees.

It is this principle which prevents the boughs of a tree from growing against one another. In proportion to their opacity they grow one from the other.

Notwithstanding the original downward vertical determination of the root, and upward vertical determination of the stem, the annual vertical growth in elongation of a tree, either upward or downward, is nothing in comparison with the growth of it, which forms angles with a vertical line: that is, out of all the numerous points of elongation of the head of a tree, there is but one which can go vertically upward in continuation of the line of the stem; and out of all the numerous points of elongation of the root, there is but one which can go vertically downward in continuation of the line of the stem; and I believe that one ceases to do so very soon.

If the leader of a tree is killed, the light falling equally from above on many buds, a multiplicity of leaders may be developed; though if one grows more vigorously than the others, by overshadowing them from above with its own side-growth, it will force them to grow sideways to the light, and the tree will again become single-leadered. Without pruning, a tree may become round-headed from the merest accident; for instance, an insect, or a bird, or the wind destroying the top bud. A tree on the side of a steep hill may be seen to grow from the opaque side above it, and, after it has reached a certain height, to curve back again.

Dutrochet, Knight, Davy, &c., think that gravity directs the growth both of the

So far light would appear to be the principal agent in directing the growth of the heads of plants. Yet the majority of physiologists attribute the direction of the growth both of the head and of the roots of plants head and the roots of trees. Experiments in proof.

to gravity. Among the number are Dutrochet, Knight, and Sir Humphry Davy, men for capacity and clearness of intellect matchless among physiologists. I will therefore go at length into two experiments of Dutrochet and Knight, the conclusions drawn from which in favour of gravity have been enforced by Sir Humphry Davy. Dutrochet found that if beans, in a state of germination, were planted in holes through the bottom of a box filled with earth, the stems grew upward *from the light* into the earth, and the roots downward *towards the light* into the air; and the plants perished when they ceased to derive nutriment from their seeds.

Early in March 1844, I made experiments similar to those of Dutrochet, with results which, if at first they resembled, finally differed very widely from, those elicited by this eminent and most acute physiologist. And in considering these results, I think I shall be able to explain why the beans in Dutrochet's experiment died. I placed various seeds on the surface of large flower-pots full of earth, turned them over on wire-work, and hung the inverted pots from the woodwork at the upper part of my window, so as to have the lower sides of the seeds exposed to light and air from below, and their upper sides in contact with the moist earth above them. The immediate results showed a most remarkable determination of the first or tap-roots downward, and of the gemmules or stems upward. In all the experiments the first or tap-roots of all the seeds, without a single exception, came straight down into the air, and ceased to grow when the ends in the air were from a quarter of an inch to an inch long. None ever turned up again. The plants, however, threw out branch-roots from their necks, fixed them upward in the earth, and continued to grow. Those parts of the roots which remained alive exposed to air and light for six weeks and upwards turned green, as did the cotyledons themselves; that is, the two divisions of the seed.

At the same time, I took some horse-chestnut seeds, whose first or tap-roots had already begun to grow, and placed them so that these tap-roots pointed upward into the earth in the inverted flower-pot, and the seeds touched the wire below. In all the cases the roots immediately turned straight down and came through the wire into the air. The gemmules or heads of five also came down, and, on cutting away the wire, all five grew horizontally to the light at the window, and then grew diagonally upward and to the light. The gemmules or heads of the rest grew upward through about 8½ inches of earth, were drawn by the light with unerring precision through the hole of the flower-pot, and one through an accidental hole of about a quarter of an inch in diameter. They then turned short towards the light at the window.

In November, on taking the flower-pot off, I found that branch-roots had passed over the top of the ball of earth, 8½ inches high. I replaced the flower-pot with a chimney-pot 2 feet 7 inches high, and filled it with earth, leaving only the five plants whose heads had come out below. I placed the experiment out of doors, supported from below. The five plants grew in 1845. In the spring of 1846 I cut off all except one plant, and placed a second chimney-pot above the first one.

In March 1850 I placed a third chimney-pot on the second, making the column of earth 7 feet 5 inches in height. The roots had already reached the top of the second chimney-pot, about 5 feet in vertical height; and in June 1850 the plant itself was 7 feet 8 inches high. In the autumn of 1851 the roots were near the top of the third chimney-pot, having grown vertically upward about 7 feet. The plant itself was within an inch of 9 feet. In the spring of 1852 I raised the column of earth to 8 feet 1 inch, and in the summer the plant was 9 feet 8 inches high.

Upward growth of first gemmule when deprived of light.

In other similar experiments the soft gemmules, or stems, of garden beans and scarlet-runners forced themselves upward through about 10½ inches of earth, came through the holes of the flower-pot, and grew towards the light till the runners required training.

Hence it would appear that, while in the earth, the first gemmules have a straight upward tendency independent of light. If we attribute the straightness to turgescence, I do not see why the growth should be in a straight line *up* more than *down*, or in any other direction. But the gemmule seems beautifully endowed with an internal structure, differing from that of every other part of the plant, by which, when buried too deeply, it takes the most direct line to the atmosphere in which it is formed to flourish: though it would puzzle our philosophy to say the agent by which this growth is directed, as much as to name the agent which directs the growth of the root, tap or branch.

When the gemmules reach the air the agent appears to be light. At least, when the gemmules reach the air this straight upward tendency is immediately overcome by light. These stems, after having forced themselves straight upwards through eight or ten inches of earth, were drawn away from the spot where each emerged from the earth by light through the holes at the centre of the flower-pot. The plants were still *crane-necked*; that is, their heads were doubled on their stems as they came out of the seeds: and thus, while the stem of a plant whose seed has two divisions or cotyledons is forcing its way upward, its head is pointed downward, and the leaves are drawn through the earth *with the grain*. But for the *contrivance* of the crank or crane-neck, the leaflets, owing to their branching and extending one from the other, must be broken and torn to pieces by the great force necessary to thrust them and the stems bodily through the earth. This is a very beautiful *provision*. The roots, on the other hand, appear to grow in length through the earth, with very little, if any, pressure at all. This may possibly be in part owing to

Crane-neck growth of first gemmule, to shield it while *forced* through the earth.

Roots grow through the earth with very little force.

their elongating only at their ends, and to their not being thrust bodily through the earth. I have laid horse-chestnuts on the surface of a box of earth, and, by arching them over with layers of damp flannel which did not touch them, they grew, and the tap-roots struck downward into the earth and fixed themselves; though they had no foreign fulcrum to press from, except the weight of the seed, which was not perpendicularly above them. Can there be anything glutinous about the silver ends of roots, which enables them to adhere to the earth while their new growth is protruded through it? And I may ask here, if roots have spongioles or small sponges at their ends, are we to believe that these sponges are locomotive or stationary? Are we to believe that these delicate organs are thrust forward through the hard ground? Or are we to suppose that the perpetual new growth of root is perpetual new sponge, as its preceding sponge is converted into root?

As the stems of the plants grew, I heightened the moist canopy, and let in light only by one opening. All the stems grew towards that opening; and as often as the opening was changed from one end of the canopy to the other, the direction of the stems was changed; that is, besides the new growth towards the light, the parts of the stems which were already formed were bent towards the light. Three or four hours were sufficient to effect this change in these tender, drawn stems.

Observing in the various experiments that several grass-seeds grew downward with their heads towards the light, and as plants with single-divisioned seeds grow with their heads single and not with the crane-neck, I imagined that they had not perhaps the same power of forcing themselves through the earth which plants whose seeds have two divisions have. I therefore tried wheat, barley, and oats, which, as well as the grasses, are monocotyledonous; that is, have a single or undivided seed. A great quantity of root was first shown downward, and ceased to grow. The heads then came down and grew towards the light. The roots fixed themselves upward and the plants grew. When the heads of any engaged themselves against the earth, the stems bowed downward, and sometimes bent short before the heads were disengaged.

In the course of three weeks three plants of wheat forced their heads through about 7½ inches of earth, and showed themselves at the hole of the flower-pot. They, however, ceased to grow, being possibly beaten down by the watering.

Provision to enable the gemmule of a single-seeded plant to thrust itsel through the earth.

Probably one reason which enables these delicate organisations to dispense with the crane-neck in thrusting themselves through the hard earth is, that they are entirely single-leadered, instead of having leaflets branching sideways like the gemmules of double-seeded plantules. But, beside this, the single tender blade is *rolled* round on itself, and enveloped in a coarse, thick, white outer coating, pointed at the top. The instant the point of this tough sheath clears the earth, it opens and emits the green blade unscathed into the air. Here is again a beautiful *contrivance.* The barley and oats growing from below, ripened their seed, which grew when sowed.

I have not known an instance of a garden bean getting the head of its first gemmule below the wire; though, when the head was engaged above, the stalk would bow down, and in this state the branch-roots fixed themselves, and the plant grew, and threw out new shoots from the knots and from the neck, which grew towards the light: and I think that the organisation of the first gemmule of the garden bean, and perhaps the first gemmule of all plants, is different from the organisation of all other parts of plants. I think that the reason of the death of the beans in Dutrochet's beautiful experiment was, that their necks and tap-roots were too far detached from the earth to allow them to throw out branch-roots; and that, had they thrown out branch-roots, they would also have thrown out *branch-stems.*

Probable distinct organisation of first gemmule.

If any of the scarlet-runners engaged their heads so as to be unable to descend, they broke out, like the beans, from the necks. But the heads of the gemmules of many scarlet-runners came down, grew eight or ten inches horizontally across the wires to the light, and then up the side of the pot diagonally to the light, till they required support as usual. Some grew across the wires without touching them. One pressed constantly against the wires, and seemed only compelled to its course by the *mechanical* resistance from above. I placed a string for each plant to climb, and as they ascended I let the flower-pot down, so that at last it touched the floor of the room, and the plants the ceiling. They circle round their support against the sun, as it is called; that is, the course of their growth and of their sap in the half circle on the south side of their supports is from west to east, and in the half circle on the north side of their supports from east to west: in other words, they make the half circle which is farthest from the observer from right to left, and the half circle which is nearest to the observer from left to right.

Many plants do the same, and many others (as the hop) exactly the reverse. Nothing can alter these *determinations* of growth in these plants, which are, possibly in all cases, owing to the action of the same external agent, light, on different internal cellular organisation.

Mechanically, I have forced the heads of plants to grow downward, by placing the seeds and roots of beans in sponges, and confining their heads in glass tubes. I have mechanically forced the first or tap-roots of plants to grow upward by placing horse-chestnuts in earth, or half covered with water, and confining their tap-roots in glass tubes. Both the stems and the roots will, however, make every attempt to double back; and in doing so I have known the root grow spirally up the narrow tube like a cork-screw, and having at last (from becoming *thin*) turned, grow straight down through the screw. The tube being then quite filled up, the side-fibres of the upper end of the root grew straight up in a bunch. In plants which have overgrown their pots, the roots may be seen to grow straight up the side of the ball of earth, and in all directions around it, owing to the mechanical confinement in the pot. But I do not think that the roots ever return through the ball of earth towards the plant. The want of light may probably be considered a sufficient reason for branches never returning towards the stem. I think it more difficult to find a reason for this perpetually *centrifugal* (or, if *nova rerum nomina* be allowed, *ipsifugal*) determination of the root.

Strong downward determination of tap root, and probable distinct organisation of it.

In all the cases in all these experiments the plants grew permanently, and independently of their seeds. In all the cases where the gemmules or stems came down into the air they grew across the wires to the light at the window. None came from under the flower-pot on the sides towards the room, even though they emerged from the earth close to those sides. And in all cases, whether the gemmule grew upward through the earth, or downward into the air, the first or tap-root showed itself from above below, and never turned upward. But the branch-roots fixed themselves, and grew from below upward; and I therefore imagine that there must be something perfectly distinct in the nature or cellular organisation of the original first or tap-root of seedlings, as compared with that of their branch-roots: though I cannot guess by what agent the growth of the differing structure of the tap and branch-root is to be directed in their differing course, any more than I can guess the agent which determines the upward growth of the first gemmule of a seedling while it is in the earth, and before it reaches the light.

Still farther to test this idea, which these experiments gave me,—that first or tap roots alone would appear below, and that no branch-roots would do so,—in March, 1846, I placed cuttings of gooseberry and currant in a pot of earth inverted on wire, and suspended in the air. The cuttings struck, and no roots appeared below; the cause being, as I conceive, that, as there were no seedling-roots, so there were no tap-roots. In July I destroyed all the cuttings except one currant, placed the experiment on a support from below and removed the flower-pot. There were a great many roots growing in all directions round and over the outside of the ball of earth. I replaced the flower-pot with a large chimney-pot, which I filled with earth. In 1847 the main shoot of the currant slip was 4 feet 1 inch in height. In March, 1848, I placed a second chimney-pot on the first; and the plant grows and bears fruit now, 1853.

From the results of these experiments, I think it probable that the organisation of the first gemmule of a plant is peculiar; and I have not a doubt that the first radicle or tap-root of the seedling has a different cellular structure from that of the branch or side-roots. If these suppositions are facts, they are very interesting, as showing most clearly and beautifully the hand and design of the Creator. The same physical causes,—that is, moisture and turgescence, drought and exhaustion, heat, cold, light, atmospheric aëration, &c.,—acting on different cellular organisations, unerringly trace out to each part of the plant the course which it is ordained to pursue. A seed is deeply buried in the autumnal hoard of some animal; its first gemmule is endowed with an organisation which sends it directly upward. It no sooner reaches the atmosphere than its growth turns wherever it can find light, which is, in fact, generally wherever it can find room. The seed falls on the surface of the earth, a first root is struck out, whose vertical determination downward nothing can pervert; though lighter and softer than the earth, it pierces through the earth from above, even without the aid of a foreign fulcrum to press against. When this perpendicular *descensus* (as the *whole* root has been, perhaps improperly, called) has, by boring, buried itself, branch-roots strike out, which grow horizontally or vertically upward or downward, or at any intermediate angle, according to the level of the ground, at the exact proper distance from the atmosphere which the particular constitution of each plant requires.

Thus, at every turn do we find how minutely perfect in detail is the work of that Almighty hand which, in the gross, swings the countless orbs of the firmamental universe through infinite space!

Tap-root only proper to seedlings, and a *contrivance* for fixing them.

I believe myself, then, that the tap-root is merely a provision of the Creator for this first fixing of the seed; that it is only proper to seedlings, and that it ceases to be continued after the first year's growth. Will no clever experimenter invent a mode of putting this question to nature?

I do not mean but what a *tap*-root might, under peculiar circumstances, be continued *ad libitum* by a *main* root. But the circumstances must be *very* peculiar. For instance, were a monster manufactory chimney filled with good soil, and any tree planted at the top, you possibly might *necessitate* the growth of a main root of a hundred feet long vertically downward; and probably, were a tree planted below, a main root of the same length might be grown vertically upward: though, after the first year's growth, not one single inch might have the organisation proper to tap-roots. On the other hand, I think that, were a tree planted on a draw-well filled with soil, the vertical root would soon cease on account of the want of atmospheric influence. Were some *violent tap-rootist* to try this experiment, his descendants might supply the dockyard with a *pot*-oak, which, if drawn by the hydraulic press, and its radical or tartarean growth added to its vertical or æthereal growth, would double its proper measurement. Indeed, I marvel why tap-rootists do not pluck all their oaks like radishes or carrots, instead of, according to their doctrine, cutting them exactly in twain and leaving their lower halves to rot in the earth. But let any one in any soil dig a trench six feet deep close round an oak; he will soon give up the idea of a tap-root.

Magnis componere parva, in the beginning of April, 1846, I planted a horse-chestnut on a column of earth about 7 ft. high, formed by placing three chimney-pots one on the other. In the autumns of 1850, 1851, 1852, 1853, and 1854, I knocked off each year about a foot of the upper part of the chimney-pots, and denuded the roots to that extent: they are, however, branching. The ends of the branches, on reaching the sides, had apparently died, and new shoots from them had struck downward. There was not the least circular growth round the sides, as with plants in flower-pots; possibly, because the chimney-pots were encrusted with soot. Two of the upper roots were entirely cleared from the earth in 1850. The woody parts of them are still alive (1853), though it would puzzle us to say whence their wood is supplied with sap: I suppose, from the stem; if so, the upward sap, in this case, is a downward sap.

The 'herbaceous envelope' of these roots (that is, the surface of the bark immediately below the outer bark) is perfectly green where they are exposed to light and air, and perfectly white from the very spot where the earth protects them from the light and air. When denuded, the herbaceous envelope begins to turn green in about a fortnight.*

But an admirable experiment of Knight's furnishes the main fact from which it has been asserted that both the ascent of the stem and the descent of the root should be referred to gravity. When he subjected beans to a strong centrifugal force by making their seeds grow on the rims of wheels whirled rapidly by water, their roots grew from the centres of the wheels, and their stems towards the centres of the wheels. When the wheel was vertical, the growth of the plants was precisely as stated. When the wheel was horizontal, the growth of the plants was nearly horizontal; but the stems inclined upward and the roots downward in an inverse ratio as the degree of centrifugal force. Richard tells us that this experiment was repeated by Dutrochet, and the only difference was, that, in the case of the horizontal wheel, 'the inclination was much greater, and the radicles and gemmules had become almost horizontal.' This last witness appears to me to prove too much: for, granting the effects of gravity on the growth of plants to cease directly as the centrifugal force applied, the centrifugal force applied to each part of the plants in this experiment would diminish directly

* I have omitted to observe whether the under bark of the old roots of plants grown in water is green or white; if it is green, the colour must be owing to the action of light, *independently of atmospheric air.*

as the nearness of each part to the centre of the wheel. Therefore, gravity would again resume its powers over each part directly as the centrifugal force diminished, and thus should elevate the heads of the plants on the horizontal wheel. This observation will hold good, whatever may have been the degree of centrifugal force applied. I have not, indeed, any idea of the degree which actually was applied; and it is needless to mention the number of revolutions of the wheels in a given time, since their diameters are not given in either of the two accounts which I have seen of this most beautiful experiment; therefore, no correct notion can be formed of the degree of centrifugal force to which each part of the plants was subjected. But, as I have said, in any case the degree of that force must have varied in every part of the plants; that is, it must have decreased on the stems, and increased on the roots, directly as their growth; and when the heads of the stems had worked *up* the stream of centrifugal force to the centre of the wheel, 'where they soon met,' all action of the centrifugal force must have ceased on them; and had they not turned upward at right angles to the spokes of the horizontal wheel, they would have gone headlong *down* the contrary current of centrifugal force.

Sir Humphry Davy remarks on this experiment:— 'These facts afford a rational solution of this curious problem, respecting which different philosophers have given such different opinions; some referring it to the nature of the sap, as De la Hire; others, as Darwin, to the living powers of the plant, and the stimulus of air upon the leaves, and of moisture upon the roots. The effect is now shown to be connected with mechanical causes; and there seems no other power in nature to which it can with propriety be referred but gravity, which acts universally, and which must tend to dispose the parts to take a uniform direction.'

That the growth of the head and root is directed by gravity disputed.

I honour and envy the mind which, like Mr. Knight's, could foresee the probability of the marvellous result of this beautiful experiment; and if it is allowable at all *jurare in verba magistri*, where is the master whose word we would take sooner than Sir Humphry's? But surely our great philosopher is here too easily satisfied, at least if he means (as I understand him) that the direction of the growth of plants *in general* is *caused* by gravity. I cannot think, myself, that the direction of *any* of their growth is caused by it. But, on the contrary, as vegetable growth is in opposition to gravity, so I think it is caused by one of the great antagonist powers to the attraction of gravity, and cohesion,—namely, turgescence, or expansion.

This beautiful experiment, however, relates *only* to the *vertical* growth of plants upward and downward; and has no reference whatever to the growth of either head or root horizontally, or at any angle with the horizontal line, either upward or downward. Indeed, if the experiment proves anything, it proves that all vegetable growth must be vertical, either upward or downward. The experiment, too, is made on the tap-root and first gemmule of the seedling, the cellular structure of which I believe in each case to differ from that of all other parts of plants.

But looking on the experiment simply as regarding the vertical growth of the tap-root and gemmule of the seedling, or of any vertical growth of a plant, if we are to believe that this vertical growth is *caused* by gravity, it would be a case of *credo quia impossibile.* For to say that the sap or the new shoot—that either of these, the heavier, should be caused to ascend through the air, the lighter, by its weight, is as flat a contradiction in terms as to say that light is caused by darkness; what Gellius said of the *resurrection* of the Palm-tree may be said of the ascent or re-ascent of any tree, *adversus pondera resurgit.*

It is, indeed, the part of a thorough philosopher not to wonder at anything. Those who have no pretence to that character must wonder at everything; and, among others, at the attractive force of gravity. Why a stone when dropped from the hand in the air should fall towards the centre of the earth is, of itself, a most unaccountably marvellous *fact.* But this is in unison with our universal, everyday experience; and the philosophic may not, and the unphilosophic do not, wonder at it. But how infinitely more unaccountably marvellous would it be if, owing to the same force—gravity—one half of the stone were to fall towards the centre of the earth, and the other half were to fly off in the exactly opposite direction; that is, towards the zenith! As this would be contrary to our universal, everyday experience, possibly the philosophic, certainly the unphilosophic, would wonder at it. Yet if we refer the direction of the vertical growth of plants to gravity, this is precisely what does take place: namely, the first start of the root is *with* the attraction of gravity towards the centre of the earth; the first start of the stem is *against* it, in the exactly opposite direction,—that is, towards the zenith. And *why* the root should obey, and the stem disobey, the otherwise universal law of gravity, would still puzzle our philosophy, as I said before.

But difficult as it would be to swallow the *fact*, that gravity should *cause* part of a plant to go with it and part against, this is only *half* of what we have to swallow. For, actually, the *whole* vertical growth of plants is against gravity; and to say that gravity *causes* that growth *against* itself, is as contradictory as to say that darkness *causes* light. But, in fact, gravity acts as much against the descent of the root *in earth*, as against the ascent of stem in air. Gravity is a fine word, and means weight. Attraction of gravitation is a fine term, and means the attraction of weight: and, loosely speaking, it may be said, that within this world the sole effect of gravity or weight is, that the heaviest things *have a tendency* to get lowest; that is, that (though we know not how or why) they are the most *drawn* towards the centre of the earth, and, consequently, that the lightest things *have a tendency* to get highest—that is, that, where there is perfect facility of movement in all directions, as in fluids, the lightest things are *pressed* farthest from the centre of the earth. But this *tendency* is constantly interfered with. If stones are put in a vessel half full of water, they, the heavier, are drawn by gravity, or weight, to the bottom, and they *press* the liquid water, the lighter, to the top of the vessel: and (though it is a false expression) the water may be said to ascend by gravity, as flame, or sparks, or smoke,—that is, heated air,—the lighter, may be said to ascend by gravity through the atmospheric air, the heavier.

But if stones are placed on the surface of the solid earth, though they are the heavier, gravity, or weight, has not the power to draw them through it. How then, is gravity, or weight, which has not the power to draw a stone, the heavier solid, through the earth, the lighter solid,—how is it to have the power to draw a root, the lighter solid, through the earth, the heavier solid?

There would, indeed, be nothing wonderful in the root, the heavier, descending by gravity, or weight, through the fluid air, the lighter. But that the root, the lighter, should descend by gravity, or weight, through the earth, the heavier, is as inexplicable and as contradictory as that the stem the heavier, should ascend by gravity, or weight, through the air, the lighter.

That light, not gravity, is the main conductor of the growth of the heads of plants is probable from the fact that, where trees stand close together, their chief growth is upwards, and their side-branches die; and as long as their stems are thus in the shade, they show no disposition to shoot out sideways again. But the moment such an over thick wood is over-thinned, the stems burst out sideways to the light which is admitted. And he who is most wedded to the extraordinary paradox that the leader owes its vertical direction to gravity, will, I think, scarcely assert that the same cause produces the horizontal growth of the branch.

Paradoxical as it may sound, if a side-branch of a tree descends from a height till it touches the ground, its growth all the time it is descending is rather upward than downward; that is, the new growth, or shoot, at the end of such a bough is generally slightly curved upwards by the action of light on the cellular structure of its upper side. Gravity, indeed, draws the whole branch down bodily, for light has no power to act through the dead bark; but light will so draw the new end up against gravity, that, when the branch comes to the ground, it will rest on a curved elbow, not on its end. This fight between gravity and light is the origin of very beautiful growth in many trees.

General growth of head towards light;

Were I to lay down a general rule about the direction of the growth of the greater part of plants, it would be, that the growth above is in whatever general growth of root to wherever it can find good soil.

direction it can find light; and that the growth below is in whatever direction it can find the best soil. I except the growth of the first tap-root of the seedling, and of its first gemmule, as long as this is below the earth, and consequently not exposed to light.

The fact shown us by Knight's most beautiful experiment, much as it says, says no more in favour of gravity, or weight, as the director of the growth of plants, than the fact which we see every day, that plants are *drawn* by light, says in favour of light.

Fools will rush in where angels fear to tread;

and we may have plenty of them to settle these questions for us nicely. But can the philosophic, or the unphilosophic, consider this first principle in physiology as settled, any more than any other first principle of it? The whole is doubt and darkness.

We are ignorant of how the sap is first imbibed. We are ignorant of what *causes* it to ascend. We are ignorant of where, or how, it is elaborated. We are ignorant of the office of the leaf. We are ignorant of the office of the pith. We are ignorant of what *causes* the stem to grow vertically upward. We are ignorant of what *causes* the branch to grow horizontally, or at any angle with the horizon, upward or downward. We are ignorant of what *causes* the tap-root to grow vertically downward. And we are ignorant of what *causes* the branch-root to grow horizontally, or at any angle with the horizon, upward or downward; or of what causes the branch-root to grow vertically upward. If the vertical upward growth of a root is doubted, I can show it now going on to any one who desires to see it. I have myself put the question to Nature, and I have her autograph answer to it in my possession.

In this case of roots growing upward, the *descensus* becomes an *ascensus*. What is called the ascending sap in the wood of the root, becomes a descending sap; and what is called the descending sap in the bark of the root, becomes an ascending sap. With submission to Sir Humphry Davy, what has gravity to do with all this?

That this our nineteenth century has infinitely more knowledge of vegetable physiology than any foregoing age I have not a doubt. But if any one imagines that we have arrived at a competent knowledge of the science, when a dozen questions of such vital importance as these are open, I think he only shows that he is ignorant of the depth of our ignorance.

CHAPTER IV.

MISCELLANEOUS.

Barked horse-chestnut at Esher.

In July, 1832, I observed a horse-chestnut tree near Esher, in the corner of a field adjoining Sandown turnpike-gate. It had been barked by cattle all round, I should suppose twenty or twenty-five years before, since the surface of the barked part was rotten, and might be picked off. Mr. King, steward to Mr. Spicer, to whom the tree belongs, said that he had recollected the tree in this state for eighteen years.

The head of the tree was in full foliage, and at the end of some branches, which had been cropped by cattle the previous year, had shot six or seven inches. The girth of the barked part of the stem was thirteen inches and seven-eighths. The girth below the barked part was twenty-two inches and a quarter, and above the barked part, twenty-nine inches. The tree had ceased to deposit new growth on the old scar, which I attribute to the rottenness of the surface of the scar, and to its having mouldered away from under the living bark. I think it probable that, if a new surface were veneered over the old scar, the stem would continue to deposit new growth on it from above. This tree is still alive (1844), having lived, possibly, nearly forty years in this state.

I imagine that the reason that this tree has continued to live is, that each year it has shot out new branches from below the scar. These branches have each year been eaten off by cattle; but they have elaborated and returned sufficient sap to nourish the root and to keep it alive. I imagine that, if these branches had been allowed to grow, they would have taken so much sap that it would have ceased to be forced up the old stem, and that the old stem would have died. So that, but for the annual outburst of shoots below the scar, the roots of the tree would die; and but for the annual browsing of these shoots, the head of the tree would die. Yet on this precarious tenure the tree has for so long held its existence. But the existence of this tree and of ringed branches proves to ocular demonstration that the sap goes up the heart-wood, since on the scar and on the rings no new wood or alburnum is deposited. It is true that the number of rings of what is called *sap-wood* or alburnum differ in different trees, and even parts of the rings of a tree may ripen sooner into heart-wood than other parts of the same rings, so that on the same transverse section of a tree there shall be more rings of sap-wood on one side than on the other. This may be observed in oaks; but on the scar of this tree no alburnum or sap-wood has been deposited for possibly nearly half a century.

I published this account of this tree in 1844. In 1849, Dr. Lindley writes as follows:—'Neither is it indispensable that bark should be present in order to allow the passage of sap downwards, as is proved by trees, whose bark has been accidentally destroyed, continuing to live for many years. In such cases *the supposition* is, that the falling sap passes laterally into the medullary plates, and descends by them until it gets into communication with those which end in bark, when the usual channel of descent is resumed.'

I take this *supposition* to be the Doctor's own particular supposition: and a supposition most difficult to swallow it is!

But the Doctor makes it unnecessarily so. Why make the sap hop, skip, and jump from one medullary plate to another? These plates all 'end in bark' where there is bark to end in, and are all continuous from end to end of the stem and branches, and, as I assert, of the roots also. But that 'the falling sap' should descend by these medullary plates is about as likely as that two meeting trains should pass one another on the same tramway, or that if a man's veins were destroyed his blood should flow *to* his heart through the arteries which are at the same time conducting it *from* his heart. And if the sap did descend at all below the scar, the tree would increase below the scar, which it does not, unless there is an outbreak of branches below the scar.

This is the gentleman who some pages before finds the heart-wood so '*filled* with secretions' that there is no room even for the upward sap to ascend through it. Yet now he makes it convey both upward and downward sap, for there can be no sap-wood under an old scar. But this voluminous compiler of other people's ideas, states all, however incompatible or contradictory one may be to the other, and uses either as convenient. Here he makes the growth in diameter to be the result of 'the falling sap.' A little before he adopts, confidently, the theory of Darwin and Du Petit-Thouars. In this there is no 'falling sap' allowed; but a downward growth 'of organic fibres descending from the leaf-buds.' If this theory were true, a common mind would shrink from the difficulty of passing these organic fibres through the medullary plates, even the first year: but it would require a Lindley to face the annually increasing difficulty; especially as neither the barked part of the stem, nor the part below it, is to increase in girthing by these excessive growths and deposits of organic fibres. Or, if the barked part is to increase it must be on the principle of one of De Candolle's *monocotyledonous endogens*; I like sesquipedalia!

Mr. Wallis brings forward a *fact* which is, perhaps, as complete a *stunner* as the Doctor's *theory*. He gives a portrait of a thorn which lived and grew for seven years after its stem was sawed across and divided from its roots: 'On examination, the lower part of its stem had remained of the size it was when sawed through;

whereas the upper part of the stem that had been so suspended in the air by its branches, had gained three inches in circumference.' Bravo! This is a famous fact for that numerous class of physiologists who, with Priestley and Liebig at their head, believe that trees elaborate their thick bulk from the thin air through the medium of their leaves. And it is a knock-me-down blow to those more commonplace grovellers who, with myself in their rear, fancy that roots may be necessary for the growth of timber. But I think that Mr. Wallis must have been mistaken. The effect he describes is precisely what we should expect if the stem were sawed quite *round*, but *not* quite *through*. Then the thorn would be in precisely the same situation as the barked horse-chestnut which I have just described. The sap would flow from the roots up that part of the heart-wood which was not sawed through, and in its descent through the bark would deposit new growth as low as the wound, but no lower. Below the wound no growth would occur; and the roots, unless the stem broke out below, being unfed, would, in the course of years, die. If this is not the true solution, if the fact stated by Mr. Wallis is a fact, then let us see it again. Nothing can be more easy than to repeat the experiment. Let those who, with Mr. Wallis and the great Liebig, consider the root as a mere pedestal for the mechanical support of the tree and not for the supply of its food, let them try this experiment on their wall-fruit trees. These trees are already nicely 'suspended in the air by their branches:' cut the stems below the heads and eradicate them. The trunkless heads on the wall, full of growth, leaves, and fruit, would be curious and beautiful objects! and the absence of the roots below would be a great convenience to the gardener!

We, indeed, see daily, in plashing thorn hedges, how *small* a quantity of wood and bark is necessary to form the connecting link between the head and the root, and permanently to preserve vitality. But if Mr. Wallis's facts are facts, we should see them every day; we should see the stems and branches of trees and underwood, when cut, continue to grow, and their roots die. But what we do see is the exact reverse of this.

Mr. Wallis alludes also to the fact, that trees when cut down will sometimes shoot out in the next summer. This has been always known, and always accounted for by the elder physiologists as the effect of what they called the *concrete* sap previously stored in the tree. But as trees separated from their roots are separated from the source of their sap, these shoots never live after the first summer.

Best time for transplanting with the ball of earth.

As the roots of trees grow in length through the earth, they are in perfect contact with it, and as they increase each year in girthing this contact is continued, and the pressure against the earth even increased. I imagine that this close contact of the roots with the earth is very essential for the absorption of moisture;

and that, when a ball of earth is taken up with a transplanted tree, the parts of the roots contained in the ball are infinitely more efficient for the supply of sap than five times their length of root not in perfect contact with the earth. But certain it is that, by taking a large ball of earth, with 'the tree-lifter,' I have transplanted tress of about twenty-five feet in height in *every* month in the year, without a single failure, and without the plant feeling its removal so much as a greenhouse plant does potting, that is without a single leaf drooping, even in the hottest days of June, July, and August, though the plant was unwatered, and with the same growth on the tree, in the next and following years, as on those in the plantation from which it was taken.

In 1846, I sent the following to the Hampshire paper, which appeared 13th June: 'Any one taking interest in vegetable physiology, who happens to be in the neighbourhood of Brookwood Park, is invited to inspect a tree transplanted on Wednesday, the 3rd instant. On that day the thermometer stood at 80° in the shade, and at 120° in the sun; and there was a parching east wind. This extraordinary heat continued for five days, and is now only beginning to abate. Yet although the tree is in full foliage, not a single leaf has flagged. The height of the tree is 27 ft., its girth 1 ft. 10 in. Every root is cut at the distance of about two feet from the tree. It can, therefore, have few ends of roots to feed it. That it is not supplied by the leaves, as

Liebig and others suppose, is clear, because the leaves of the branches which were cut off at the transplanting could not even supply themselves, but died before the transplanting was completed and dried immediately. If the leaves supplied the tree with sap, these branches should have remained green. One scorched specimen of them is attached to the tree. The tree stands in the right-hand hedge bordering the road going down from the Brookwood Lodge gate to the Dean, at the point where the cross-hedge falls on the road. The smaller trees on the sides of this road were planted from the nursery in January, 1834. The larger beech-trees among them were transplanted with "the tree-lifter" at different times since that year, chiefly, like the tree in question, in full leaf, and in the months of June, July, and August.' The tree transplanted in 1846 is growing well at the present writing (1853), and has grown well in all the intervening years.

I consider, however, that the worst time to transplant a tree is when it is shooting: the best time, as soon as possible after it has shot; that is, as soon as it has formed its winter-bud. This will differ in different trees. Some are fit to transplant in June, or even in May. The best months for transplanting the generality of English trees with the ball of earth, are July, August, and September; for, though the upward growth has then ceased, the growth in girthing, and the downward growth, that is, the elongation of the roots, are in the fullest tide.

From observation of the growth of the root in potted plants, and also of the seedlings of trees grown in water (one of which I have in its seventh year's growth, 1844), I am satisfied that the great downward growth of the root takes place immediately after the great upward growth of the head; that is, at the end of summer, during the autumn, and in early winter: and that the wounds of the roots of trees, transplanted immediately after they have made their upward shoot, begin to heal or cicatrise, or, as the gardeners say, *callous over* immediately. These *callouses* are a prolific source of new shoots for the root, which besides, from having been shortened, makes a profusion of lateral shoots that same season. These shoots become woody, and the root is consequently in a state to supply the great upward demand next spring.

Absurd notion that trees *deradicate* as well as defoliate.

It has been reserved for the nineteenth century to start the notion that trees *deradicate* as well as defoliate. Were a man to say that 'as stags shed their horns annually, in like manner they also shed their feet annually,' *risum teneatis?* Yet not a whit less laughable is the dictum that 'as trees shed their leaves annually, which are the extremities of their stems, they in like manner also shed the extremities of their roots annually.' I speak from recollection from a work on gardening by Mr. Mackintosh, and I think that he mentions Dr. Lindley as among the sapient holders of this brilliant notion.

The idea was probably originated by some observer of pot plants. In pots, from want of room, or from want of water, or from exposure to frost, the roots on the outsides of the balls of earth may die, while the parts inside the balls of earth may survive. But trees in good health and in good soils never lose a rootlet. On the contrary, the rootlets continue to grow each year for months after the stems have ceased to grow, and after they have shed their leaves ; possibly through the whole of a mild winter. In October, 1854, I scraped some mud out of a pond, the water of which was low ; I accidentally laid this mud in heaps on some horse-chestnut roots, which had been bared of earth by the tread of cattle. On the 26th of the following December I removed the heaps, and found them full of new roots, which were *apparently* in full shoot then ; many had silver ends six or eight inches long. This late growth of the root is, as I have said above, a strong reason for summer and autumn transplanting, where it can be done with a ball of earth, that is, without exposing the roots to drought.

We may convince ourselves by experiment that the downward is after the upward growth of trees. If notches are cut on the stem of a tree from the root to the setting on of the first branches, the new growth over the scars will be when the tree is ceasing to shoot. The upper notches will heal first, in the form of a horseshoe, with the heels downwards ; that is, the growth will be on the upper part and the sides of the notches, without any growth from the lower parts of the notches. This fact also strongly corroborates the opinion that the new growth in girthing is from the downward sap; for if it were a side-deposit from the upward sap, the lowest notches should heal first, and the healing would be from their lower sides. I have found that, if stems thus notched are inverted, the new growth comes only from the sides of the notches, and neither from the upper nor lower parts of them, which I am unable to account for. But the notches nearest the head are the first to heal, and those nearest the root the last to heal.

The growth in girthing descending from the head or any particular branch is general round the stem, though it is greater on the side proper to the branch;

The new growth on the notches will be free in proportion as they are in the line with large branches above them; and I imagine that, though the returning sap from branches deposits round the whole stem, it deposits most freely on the proper side of the branches; and the larger annual deposit found on the *outsides* of the outside trees of plantations, which has been attributed by Duhamel and Buffon to their having their largest roots on that side, is, I have no doubt, the result of their having their largest branches on that side. An exposed tree standing singly will throw out its roots equally all round it; but the new layers of wood round the stem will be much the largest on the leeward side, because the largest branches are on the leeward side; yet, if the upper part of one half of a stem is dead, the opposite living side will deposit round the whole living part below. And I imagine that it is thus that the windward roots of an exposed tree are nourished by the descending sap from its leeward branches.

Indeed, the downward stream of the growth in girthing may at will be mechanically stopped on one side of the stem, and projected to the other. This may be observed in the natural spiral carved work, formed by woodbine on the stems of coppice-wood; and doubtless, if instead of the woodbine wire were placed spirally up the stems, very regular and beautiful patterns might be produced. Wire fences, fixed on the stems of trees, destroy the circulation and kill the sides of the trees on which they are fixed.

and the growth may be mechanically and continually turned from one side of the stem to the other.

Again, as far as I have remarked, though young roots are round, the older ones greatly incline to the oval shape; and in all the transverse sections of roots which I have examined the *eccentricity* of the common point from which the (*medullary?*) rays diverge, and which is occasioned by the comparatively over-growth of the upper sides of the new annual rings, is very striking. I imagine that this is caused *mechanically*, and that it is the result of the growth in girthing of the roots meeting with less *mechanical* resistance from the earth on the upper sides.

As long as a branch root exists, it must, owing to its lateral growth in girthing, annually approach the surface of the ground, and, after that is reached, ascend above the surface. Suppose a root to run horizontally at the depth of one foot below the surface of the earth. Suppose this root to increase only $\frac{1}{4}$-in. in diameter

Lateral upward growth of the root.

each year,—in less than a century, that is in ninety-six years, it will be even with the surface, and in another century it will be one foot above the surface. Without contravening circumstances, this rising of the roots may be *seen* around all old trees.

In the growth in girthing of the roots, the earth above the roots is easily displaced, that on the sides with more difficulty, and the earth below roots with still greater difficulty. In proportion, then, as the surface against which the lower sides of roots grow is unyielding, each root has a tendency *to upheave itself bodily*, besides the rising at its upper surface from what is called *the growth by juxta-position;* and the whole mass of roots have a tendency to upheave the whole tree. In the case of roots growing on rock this upheaval must take place, or the roots must cease to grow on their lower sides.

In the observation above in regard to the *lateral upward* growth of roots, credit is only taken for *half* of the growth in diameter, that is for 1-8th of an inch growth, on the upper side of the root. The other 1-8th of an inch growth on the lower side is supposed to displace the earth downward. I think it, however, likely, in the generality of cases, that, owing to the resistance of the subsoil, the progress of the root upward is equal to nearly the *whole* of the growth in diameter; and that, in proportion as the deposit of new growth below the root decreases from mechanical pressure, the new deposit above the root increases. I have seen ash-trees growing on gravel with the roots all round them above the ground to an extent of double the length of the boughs.

Where cattle do not come, and where the surface is not liable to denudation, as turf or pavement, the ground may be observed to be raised about the roots of trees by this *lateral upward* growth of the roots. Where cattle do come the case may be altered. Cattle use trees as rubbing-posts, and as refuges from flies, the sun, wind, or rain. Under such circumstances, the ground, instead of being raised, is often worn into hollows around trees: for, the herbage being worn away, in drought the earth is blown away as dust, and cattle *paw* and cast it up with their feet to drive the flies from them; in wet weather the earth is carried away on the feet of the cattle. This, and the eternal disposition of roots to rise by lateral growth in girthing, bring them in contact with the feet of cattle, and they become what is called '*cattle-trod.*' This is a frequent cause of the death of large trees, or of their decay.

If there is this inherent natural tendency of roots to rise *above* the ground, it is easy to imagine the slaughter which the gardener's spade must commit among the roots of old fruit-trees. Indeed, these, and the roots of trees which are resorted to by cattle, may be said *to live in a perpetual state of destruction.*

I think it, however, probable that this martyrdom of the root may incline gross-growing trees to grow fruit instead of wood. Thus the grafting on a stock of minor growth, or the ringing of a branch, or the tying a ligature round it, or anything which checks the growth of a tree or branch, inclines it to fruit. I have been told by one whose word I trust as well as my eyes, that he once cut a standard pear-tree half down (that is, he cut half through the stem just above the root), because, though a gross-grower, the tree never bore fruit. He was accidentally called off his work, and neglected to finish it. The tree not only lived, but was ever after a profuse bearer. It is the beautiful and beneficent provision of our Creator that, in proportion to their age, decay, and approach to death, the vital energies of trees are converted to the production of seed, for the reproduction of their species in youth and vigour.

In the case of trees which are free growers but shy bearers, put them or parts of them out of health somehow. Dig half their roots to death—ring half their branches—or half bark their stems. For we must grow fruit, not timber, in our gardens and orchards. But do not dig *all* your trees to death, and then swear that they killed themselves by diving. And in the case of trees of minor growth, top-dress them, and break the surface over their roots charily.

Origin of spurs and the swell of the roots.

As the generality of roots do not leave the tender superficial seedling either in vertical or horizontal lines, but in lines forming angles with these, the lateral increase of these commencements of roots, as they become imbedded and embodied in the trunk, forms the projecting *spurs* of old trees, or what is called the 'swell of the roots.'

When trees which have spurs are felled by horizontal cutting, the annual growths of the spurs are not cut *directly across*, but diagonally, or slantwise: indeed, sometimes the cut approaches to being lengthwise with the grain. This makes each annual growth *appear* much larger than it really is, and elicits such expressions as 'How finely the tree was growing at last!' or, 'How finely it was growing on this or that side!' though, in reality, the growth may have been on the wane.

Aided by turgescence, the lateral growth in girthing of the roots takes place with a force *almost* resistless. It will upheave enormous weights, and may frequently be seen to rupture roots crossing the spurs. But the force must be *quite* resistless to perform the office assigned by the supporters of the tap-root. Let us suppose a first-rate oak of 30 ft. in girthing, and 100 ft. in height. Let us give this tree, according to the vulgar error, a tap-root equivalent to its stem.

This is the true Virgilian creed in regard to the tap-root of the æsculus, and may have been the vulgar creed in that respect for thousands of years before Virgil wrote:—

> Æsculus * imprimis, quæ quantum vertice ad auras
> Ætherias, tantum radice in Tartara tendit.

* Modern authorities say that æsculus is the beech; and, in reference to the *eating* of the beech mast, they derive the name from *esca*, as they do the name of *fagus* from φαγεῖν. But Ovid mentions the æsculus as distinct from the fagus (*Met.* x. 91). And

A tap-rooted tree *should* have no spurs or swell of the roots.

Such a tree, in the form of its stem and root together, should resemble two carrots placed head to head, or two cones with their bases one on the other. It should have no *spurs* whatever, or *swell of the roots;* but, on the contrary, should immediately *decrease* below the earth. We will say nothing of the mechanical difficulty of boring (with a *sponge*) through the solid deposits at the depth of 100 ft. from the surface in the longitudinal growth of the root, or of what the sponge, or the *one capillary stoma*, is to get there in the way of chemical nutriment; but to enable this monster carrot to increase laterally at these depths would require a force indeed resistless,—a force equal to that of igneous action,—a force sufficient to cleave the world asunder. In whatever light we view the idea of a tap-root, except for the seedling, it appears to me so preposterous that I think we may at least throw the *onus probandi* on the

Virgil mentions it as distinct from the quercus, and also from the castanea, in the beginning of the second book of the Georgics. Æsculus is probably the horse-chestnut. Was the Daunian and Apulian Asculum a corruption of æsculum? It was situated

> 'Qua violens obstrepit Aufidus,
> Et qua pauper aquæ Daunus agrestium
> Regnavit populorum,'

and Horace characterises Daunias 'latis æsculetis.' Does the horse-chestnut prevail in the woods neighbouring on modern Ascoli? Ovid, *Met.* i. 449, says that before Daphne was turned into a laurel, the victor at the Pythian games *Esculeæ* capiebat frondis honorem. A horse-chestnut bough would be a more conspicuous signal of honour than an oak or a beech bough. According to Theophrastus, quoted by Littleton and Hederic, πλατύφυλλος is the Greek equivalent for esculus. 'Broad-leaved' applies well to the horse-chestnut, but not at all to the oak or beech.

asserters of the positive, and say, 'If there be such a thing as a tap-root, find one, and show it to us.'

Nay, I am so easily contented, that I shall be satisfied of the existence of a tap-root if a large oak can be shown without the large spurs indicative of horizontal roots. Practically, some of the best growing trees that I have ever transplanted are oaks. I need not say that these had no tap-roots, or that if they had they would not have borne transplanting.

The largest growth in girthing on branches is on the sides on which they have the most spray or small twigs; so that branches which grow diagonally upward, having the greater quantity of spray on the outside, on account of the greater quantity of light, will also have their annual rings of wood largest on the outside from the descending sap of the spray depositing most freely on its own side.

Girthing of the stem dependent on the quantity of branches above it.

But the stem of a tree will be exactly like a river: its size will depend on the number and size of the branches which fall into it; and it will be seen to increase below and to decrease above the spot where each of its tributary branches joins it. It is beautifully ordained that no branch can grow above without depositing below strength to support itself.

Fir-trees, which are very regular in the size and position of their branches, are for this reason very regular in the tapering of their stems; but if the lower branches are cut, or killed by their neighbours, in the course of time the branchless part of the stem loses its tapering form. And the stem of any tree which has been long bare of branches shows like the Lower Nile, —unvarying in size, because without a tributary. It will appear to the eye as large above as below: for, as regards the horizontal girthing, the head deposits equally down the whole extent of the bare stem below it; that is, though the over-deposit of growth from over-large branches on their own side of the stem may tend to make the stem oval instead of round, this will make no difference to the comparative horizontal girthing of the tree at different heights. And if equals are annually added to unequals, though the original absolute inequality will for ever remain the same, the relative inequality will annually decrease; and the stems of trees which have been long branchless may be found of nearly the same girthing for 50 or 60 feet in height. If the yearling shoot is one inch in diameter, and the two-year-old shoot two inches in diameter, the girthing of the one will be double that of the other: but if each shoot increases annually one inch in diameter, the proportion of their difference alters the first year; that is, the girthing of the one, instead of being twice as large, is only one third larger than that of the other; and when the one girths 10 feet, the other will girth 10 feet 1 inch, which is in effect no difference at all.

There is a constant circulation of sap even in winter.

Against the theory of the one vernal ascent, and the one autumnal descent of the sap, and in favour of the constant circulation, or at least constant supply of sap, we must consider that boughs even of considerable thickness, cut off in the autumn, will become dried throughout before the spring. But what can account for the moisture of boughs, and even the most delicate spray, exposed at great heights in the air, but the constant supply of sap?

Indeed, if the first theory were correct, there is no reason why plants should not live through the winter out of the ground, and plants taken up in the autumn should grow as freely when again put in in the spring as if they had just been taken up. The contrary of this is the case; the roots of plants taken up in the autumn, as well as the plants themselves, unless they are '*laid by the heel*,' soon become dry. Why? Because the roots are deprived of the power of imbibing moisture.

I suppose there may be physiologists who think the roots of trees useless at all times, except to fix the trees; for if there is no circulation in the winter the roots are useless in the winter, and if, according to Liebig, trees derive their nutriment from their leaves in the summer, the roots are useless in the summer. I differ in both cases.

I have observed that, if the stem of a young tree grown in water is cut at the beginning of winter, the root immediately ceases to grow; doubtless, because the stem is necessary to return the sap to nourish the root. If this theory is true, if there is a winter circulation of sap, coppice-wood, hedges, and shrubs which are intended to shoot up again, should be cut at the end of winter,—not at the beginning of winter. If

If there is a winter circulation, coppice-wood should be cut at the *end* of winter.

they are cut at the beginning of winter, all circulation of the sap must be destroyed through the whole winter, till the plant can shoot out again in the spring; since the communication between the wood and the bark is annihilated: for, in winter, the buds form the points of junction between the upward current of the sap in the wood and the downward current in the bark. I imagine that this circulation and elaboration do go on in the winter; that in the early part of winter actual new growth of the root is often going on; and that during the whole of winter the new growth is solidifying and becoming woody.

I consider it a proof both of the existence and of the necessity of this winter circulation and elaboration of the sap, that shrubs which are headed at the beginning of winter are very liable to break out; they then suffer much from the frost. Besides this, the hoarded elaborated sap, which would be of infinite value for the spring outbreak, is wasted on this false start, not to mention the annihilation of any winter buds which may have been on the plants below where they were cut. When I have cut down sycamores in August, of about twenty years' growth, I have known them make this unnatural effort to relieve their roots from *suffocation*; and I have observed the leaves on the shoots which they have then thrown out green to the middle of the succeeding January. Plants which do not ripen their wood, and which are annually killed in parts by frost, such as fuchsias, verbenas, &c., should not be cut till the frost does come; they should then be cut *immediately*. This not only gives the last chance for the ripening of the roots, but if the plants are cut earlier they are very liable to break out, and then suffer from frost.

But, perhaps, the strongest proof of a winter circulation of sap is, that if boughs of evergreens are cut in the winter, and suspended to the boughs among which they grew, they die, while all remains green around them. Why is this, but because their brethren are supplied with sap from the parent root, while they are cut off from it? Yet, according to Liebig, branches in leaf should not only support themselves, but feed the tree which bears them through the hot months of continental summers. So far from this, however, being the case, they cannot even support themselves in the moist atmosphere of an English winter.

The laurel is among the latest in fading. In about three weeks, however, its leaves may be seen to turn paler, and may be felt less leather-like and more thin and paper-like. The dead leaves will then constantly play the second act of Gideon's fleece; that is, they will be found dry when their surrounding living sister leaves are condensing and bedewed to the utmost on both sides of each leaf. If the experiment is tried in an east wind and a clear sky, about the end of February or beginning of March, the fading and drying will be much more rapid; still more rapid in a room with a fire. *Nimium breves* are the branches then, as every Christmas shows us. But all this proves what I began with, that all parts of plants imbibe in proportion as they are exposed to moisture, and exhaust in proportion as they are exposed to draught.

Best time for felling timber. I do not believe that the sap ever ceases to circulate; but the tide is perhaps at its lowest ebb in January, and that is possibly the best month for felling timber. Timber which is felled at the high tide of sap and growth is extremely liable to fermentation and decay.

Roots should not be covered deeply. It is a dangerous experiment to cover up the roots of trees. Their chief duty appears to be to absorb moisture in the soil: but atmospheric aëration is necessary to them; and under the eternal agency of physical causes, acting probably on the peculiar structure of their cellular organisation, the roots of each tree grow at the level best adapted to them, and to the offices which they have to perform. This should not be interfered with. If dressing is laid on the roots, it should not be deep, or of a nature impermeable to air.

Trees which have had their roots deeply covered up languish and die, unless they throw out a new set of roots above the old ones which have been smothered. In this case, the whole tree may be considered as a huge *cutting* which has (*mirabile dictu!*) *struck*. But, generally, the new tier of roots is not strong enough to supply the exhaustion of the old head; and if death does not result, the head dies in and rots the stem from above, while the old roots do the same from below.

Roots are obliged to keep the surface, because the food of plants lies there, though many think that this food is imbibed from the atmoshere; if so, trees would not be injured by having their roots covered.

That a hill affords no more space for growth than its base would, an error.

I believe Sir Humphry Davy first remarked, *on the assumption that the upward and downward growth of plants is vertical*, that woods and crops growing on the side of a hill would derive no greater advantage from the additional space than if they grew on the horizontal surface of its base. But it must be recollected, that, as the plants on the side of a hill rise tier above tier, with the same light and aëration from above they have a greater side light and aëration. They are, in fact, placed head above head, like people in a race-stand, where, but for this arrangement, the *spectators* would have good opportunity for looking upward at the roof, but none for looking sideways at the race. But the merit of this principle is very apparent in the step-stands in green-houses; though, probably, the origin of these stands may be the greater facility they give to see and to water the plants. But if the plants stood on the area of the base of the stand, each would be shaded all round by its neighbours, and would receive light only from above. The base of what is called in Hampshire 'a hanger,' or a hanging wood, would not support as many trees with full heads as stand on the hill-side. Let us conceive these

Densas, umbrosa cacumina, fagos

to be sunk vertically downward from their beautiful gradations, till their roots shall stand on the base of the hanger. The long one-sided columns of green will be submerged, smothered, and killed below the one common level of the tops, and the plants will be deprived almost entirely of their organs of respiration and transpiration. But besides this greater space for the heads, there is also greater space for the roots of plants growing on the side of a hill, than if they had only the base of the hill-side to grow on. For roots, as has been shown (page 86), have the power of following the surface of the earth, be the inclination upward or downward what it will. And they do not grow solely vertically downward like the tap-root of a seedling. In reference to an entire hill, of a given base, this increase of surface or space for the roots will be not only directly as the height of the hill, but also directly as the steepness of its sides. Taking one side of a hill, if the side forms an angle of 45° with the horizon, its additional surface or space for roots, as compared with its base, will be as the diagonal is to the side of a square.

In the south of Europe and Madeira the steep hill-sides are terraced with stone walls, in order to arrest the soil, which would otherwise be washed down by rain. Here there would be a loss of superficies for the roots, and the hill-side would afford them no more space than the area of its base. But the vine stocks are *built in* horizontally through these stone walls, and their roots planted in the earth behind them which is within reach of atmospheric aëration. This doubles the extent of space available for the roots; it gives two sides of a square instead of one; and compared with the side of the hill if unterraced, space is gained instead of lost; it gives two sides of a square instead of a diagonal.

Effect of wind on trees.

We are not to expect that trees drawn up in the interior of sheltered plantations and transplanted to exposed situations will grow. If we could move a cube acre of ground, with a young tree, from a sheltered to an exposed situation, the plant would dwindle and decay. A tree grown in an exposed situation contrives by degrees to shelter itself; that is, it grows to leeward of itself. For the windward growth diverts the current of the wind, and *throws it up.* And we see, in exposed trees and woods, that they get taller by degrees from the windward to the leeward side. The chief injury which trees suffer from wind is while they are shooting. If the weather is calm while they are shooting, they will make a year's growth upward and to windward. But their general growth will be only upward and to leeward; not from being *bent* by the wind that way, but from all other growth being destroyed while the shoots are tender, and from the wind having a much greater power to break twigs which meet it than those which grow down the wind.*

* Wind does not increase cold to plants as it does to animals. Plants have no heat. Animals have a heat of 92°, and wind of a temperature under 92° deprives animals of the heat which would hang about their coats, or about the clothes of us poor

Effect of sea breeze mechanical, not chemical.

Plenty of examples of this sort of growth may be seen in the neighbourhood of the sea. This is from the mechanical force acquired by the wind in passing over the uninterrupted surface of the sea. It is common to attribute the blasted vegetation of trees in the neighbourhood of the sea to the saline or chemical qualities of the sea breeze. If it were so, the growth would not be hurt more on one side of the tree than the other. If it were so, trees would grow as luxuriantly on the south-west side, and on the top of Mount Edgecombe, as they do on the sheltered north-east side, for the chemical qualities of the atmosphere must be the same in each place. If it were so, we should not find the same sort of scarecrow growth on our inland bare plains and heaths as we do along our coasts. In a bare, open country, we have only to see on which side of a tree is the lowest and shortest growth of its head, to know where the south-west is. And if the stem of such a tree is cut across, the largest sides of the annual rings of wood will be found on the north-east side. If it is attempted, by pruning out the leeward growth, to give exposed trees straight leaders, or to force them to grow to windward, they will decay from want of head to return a sufficient nourishment to the root; though, if it is gradually done, trees may be very much *helped* on this principle. Firs being essentially single-leadered

unfledged Christians. The temperature therefore for plants may be taken *absolutely* from the thermometer, irrespective of wind. So that with the thermometer at 50°, and a strong wind, plants may have a spring day and we beasts a winter day.

trees, and not having the reproductive powers of deciduous trees, stand wind very badly. It is the common error to believe that they will stand exposure well, because they are found high up mountains. But this is only where they are sheltered by the mountain-*side;* and they will not bear well the exposure even of our low bare plains, still less of the *tops* of very moderate hills. I except the silver fir.

Trees may be often remarked whose growth has a *stratified* effect, with bare stems between the strata, or stages of growth. I think this may be from occasional accidental blights of growth from wind. I have never seen this sort of growth in sheltered situations.

PART III.

ARE SOILS ENRICHED, IMPOVERISHED, OR *POISONED* BY VEGETABLE GROWTH? THESE QUESTIONS INCLUDE EXCRETION FROM ROOTS; SOCIABILITY OF PLANTS; ACCUMULATION OF SOIL IN WOODS; GENERAL DENUDATION OF SOIL FROM WASH OF RAIN.

The food of plants is extracted from the soil. But if the plants are returned to the soil, no impoverishment takes place

I THINK that the food of plants is absorbed from the soil, not from the atmosphere; but that, if the remains of dead plants are restored to the soil from which they grew, owing to vegetable chemistry, independently of disintegration of rock, soils would become enriched, not impoverished. The two great causes of impoverishment of soils are, abstraction of vegetable crops by man or animals, and aqueous denudation, that is, the wash of rain. The food of plants is of two sorts, the *organic* or *combustible*, that part which can be consumed in burning; and the *inorganic* or *incombustible*, that part which remains as ashes after burning. Both parts are, in my opinion, absorbed by the roots from the soil; at least, what is absorbed *as food* from the atmosphere may be reckoned as nothing in comparison to what is absorbed by the roots from the soil. In reference to the *combustible* constituents of the food of plants, Liebig tells us that the presence of oxygen—consequently, of atmospheric air—is necessary for the generation of carbonic acid from the humus in the soil; and Priestley and Sénebier have shown that from carbonic acid plants assimilate their carbon (which, loosely speaking, the whole of the tree may be said to consist of), by decomposing the carbonic acid, and giving off the oxygen. I should consider this as the cause why roots keep within the reach of atmospheric aëration, since the main article of the food of trees is found in that district; though Liebig, and a host of modern physiologists, follow Priestley, Sénebier, and De Saussure in thinking that after the first infancy of the plant, that is, after the development of leaves, it is indebted to the atmosphere only for the supply of carbonic acid. But can we doubt that the chief growth of plants is from constituents absorbed from the soil, not from the atmosphere, when we see the perpetual difference of growth of the same plants in the different soils of the same parish; that is, in the same atmosphere?

Liebig supposes plants to assimilate their nitrogen by decomposing ammonia, stored in soils from rain water, manure, and humus, and giving off the hydrogen; their hydrogen, by decomposing water and giving off the oxygen. Carbon, nitrogen, hydrogen, and oxygen, with certain peculiar inorganic or incombustible matters, are the sole constituents of plants. Indeed, all organic existences, that is, the endless varieties of the animal and vegetable kingdoms, are composed of these four elements alone. These four elements are contained in carbonic acid, water, and ammonia. Throughout all organic nature, during life, combination from the constituents of these three goes on, and after death the decomposition of those combinations into the constituents of these three: that is, carbonic acid, water, and ammonia furnish the constituents from which, by combination, result all the exquisite living forms which we admire and love; and into these three those forms are by decomposition eventually resolved. Throughout the realms of vitality the actual living are the late dead freshly combined; and from the decomposition of one generation of plants and animals the recomposition of another generation results.*

In reference to the *incombustible* parts of the food of plants, all will agree that these ashes of plants are

* Pythagoras received doctrines very similar to these from Egypt and India. Ovid describes them thus:—

'Omnia mutantur; nihil interit . . . .
Hæc quoque non perstant quæ nos elementa vocamus . . . .
—— tamen omnia fiunt
Ex ipsis, et in ipsa cadunt . . . .
Nec species sua cuique manet, rerumque novatrix
Ex aliis alias reparat Natura figuras.
Nec perit in tanto quidquam (mihi credite) mundo:
Sed variat, faciemque novat; nascique vocatur,
Incipere esse aliud, quam quod fuit ante; morique,
Desinere illud idem.'

These are sublime doctrines as regards matter. So is the 'morte carent animæ,' as regards the soul. 'There is,' however, 'but one step from the sublime to the ridiculous,' and alas! that the profound philosophers who held these doctrines should have taken the one step beyond the transformation of matter to the ridiculous belief in the transmigration of souls.

absorbed from the soil, since they actually are soil. In trees their quantity, as compared with the combustible parts, is small; though during the life of the tree, in the chemical processes of decomposition, elaboration, and assimilation, their effects may be very great. The combustible or organic parts of trees, though they are not soil, are absorbed by the roots from the soil; that is, their constituents are elaborated or chemically prepared for the plant in the soil, and absorbed by the roots from the soil. But as neither animals nor plants bring anything into the world with them, so neither of them take anything away with them; and if their remains are restored to the soil, no impoverishment will take place.

Roots do not excrete.

I have no faith in the supposed excretion from the roots of substances unnecessary to the growth of the tree. If this were so, the roots would soon be surrounded with such substances, and would be incapable of absorbing nutriment. In chalk districts eternal woods are found composed of nothing but beech; in other soils, of nothing but oak. The oldest vineyards and the oldest hop-gardens are the best. And how many millions of acres are in this world covered with perpetual heath! In all these cases, if the roots excreted substances unfit for nourishing the plants, the whole soil would have become saturated with them. Land plants grown in water are always unhealthy. Under these circumstances, may not colouring matter, or other substances supposed by Macaire-Princep to be excretion, be the result of disease and decay or partial maceration of the roots. There is no discoloration of the water in which the seedlings of forest-trees are made to grow, while these are in health.

I imagine that trees, in absorbing by their roots the moisture with which they come in contact, give off the unnecessary parts of this by transpiration in the air. I do not perceive what should cause roots to transpire when surrounded by moisture; or if they do, they must return, like the dog to his vomit, and again absorb their own transpirations.

If it were owing to the poisonous excretions of the roots that the same crops cannot be taken year after year from the same land, this cause would apply equally to all lands; but there is an infinite variety in soils in this respect. And this infinite variety, and the infinite gradations in richness and productiveness of different soils, prove that the nutriment of plants comes from the soil, not from the air. Yet the very same physiologist who makes the plant imbibe its food from the air by its leaves, will force it to swallow poison from the soil with its roots! If plants feed through the medium of their leaves from the air, why manure the soil?

It is perhaps possible that the reason why each plant appears to have its favourite soil is that it finds there in the greatest abundance the particular inorganic or incombustible matters adapted to its peculiar constitution; that the reason why particular plants will *not* grow in particular soils is the absence of the particular inorganic matters adapted to their peculiar constitution; and that the reason why particular plants *cease* to grow on particular lands is their having taken up those peculiar inorganic constituents necessary to them, and these being together with the crop abstracted from the soil by man—not their having deposited a suicidal poison from their roots, and thus forming cases of vegetable '*felo de se.*'

The organs of absorption of the roots of wheat, beans, potatoes, turnips, or mangold wurzel, cabbage, and lucern, sainfoin, or the common grasses, probably differ as much as the internal and external structure of the roots and plants; and, besides, searching for their inorganic constituents at different levels in the soil, they may probably be only capable of taking up those adapted to their peculiar constitution.*

That the proper juices, the various peculiar acids, and the organic salts, found as carbonates in the ashes of plants, and formed by the combination of the alkaline bases, potash, soda, lime, magnesia, with the peculiar organic acids of plants, play an essential part in the functions and development of the different parts of

* We don't know whether roots have the power of selection or not; and, in reference to this all-important first principle of vegetable physiology, Liebig flatly contradicts himself. Page 92 he writes: 'All substances in solution in a soil are absorbed by the roots of plants, exactly as a sponge imbibes a liquid and all that it contains without selection.' Page 101 he writes: 'When roots find their more appropriate base in sufficient quantity, they will take up less of another.'

plants, cannot be doubted, though we are quite in the dark about it. And as regards the peculiar inorganic matters absorbed from the soil by particular plants, while the land is bearing one sort of crop it may be lying fallow, and collecting them, by disintegration, for another sort.

In this way *rotation* is of service in *man's* cropping; but in those farms or estates which God Almighty keeps in his own hands, where of all that is grown nothing is abstracted, vegetable growth, by its chemistry, enriches, not impoverishes, the soil.

Sociability of plants a fancy.

Akin to the question of excretion from the roots is that of the *sociability* of plants; and I have no more faith in the sociability of plants than in excretion from the roots. That particular plants grow best on particular soils, and in particular climates, is clear; though Nature has *not* grouped her flora or her fauna *solely* in reference to soil and climate—that is, in reference to the agreement or disagreement of the physiological constitutions peculiar to the plants or animals, with the physical conditions existent in each district of the globe.

Were it so, that is, were the same species of plants and animals *always* found under the same physical conditions, it might, with more reason, be argued (as has been argued by Lamarck) that vitality itself is the mere result of physical conditions—that the different *constitutions* of plants and animals are the result of different physical conditions—that the different *species* are mere changes of form and organisation resulting from different physical conditions—and that man himself is merely the final result of these manifold changes from the zoophyte upwards. But the Great Artificer authoritatively and absolutely contradicts all this by having, possibly from all time, and apparently *successively*, and at distinct times, and in comparatively modern times, created distinct existences or species, and kept these distinct species separate under precisely similar physical conditions. This is the case with animals, vegetables, birds, reptiles, insects, fish, shell animals, and zoophytes, whether any of all these named are terrestrial, or aquatic, or amphibious, or peculiar to fresh, brackish, or salt water. And not only is this so now, but apparently through an indefinite number of ages the terraqueous globe has been thus gradually and *in succession* stocked and restocked with species entirely distinct from those existing in this or in any two of the different periods. Indeed, supposing species to have originated from single stocks, the creation of existing species could not have been simultaneous. Had it been simultaneous, all animals, herbivorous or carnivorous, must have fasted or have destroyed whole species at a mouthful. Creation must have been successive as far as this. Plants must have multiplied before herbivorous animals were turned among them, and herbivorous animals before carnivorous animals were allowed to prey on them.

From the arctic to the tropical regions a wonderful variety of physical conditions exists, and an equally marvellous variety of species, and of physiological constitution, in all the classes named displays itself. The whole of the land and of the water, and even of the air, of these and of all the intermediate regions, are crammed full of organic existences. I will instance only some of the largest quadrupeds, herbivorous and carnivorous, on the extremes of cold and heat, because these could not exist in regions which were not replete with vegetable and animal life for their food. In the arctic regions we find the musk-ox, the reindeer, the huge polar bear, the wolf, the seal, the whale, &c.; in the tropics, the elephant, the rhinoceros, the camel, the giraffe, the lion, the hippopotamus, the shark, &c. But Nature is by no means content with this wonderful adaptation of her organic creation to differing physical conditions. Like the chicken-fancier, who keeps his fowl-yards separate, Nature seems purposely to have contrived *different* stations with *similar* physical conditions, in order to exhibit the profuseness of her creative power in cramming all full of animal and vegetable existences, with constitutions similar to those of similar but separate stations, but the species of each similar separate station differing entirely from the species of all other similar separate stations. These stations are in general kept separate by what Buffon called 'natural barriers.' Besides the difference of climate resulting from difference of latitude, difference of altitude and seas of water, or of sand, or of eternal snow, in general separate terrestrial districts. Continents, currents, difference of depth, saltness, freshness, or temperature of the water, separate aquatic districts. M. Alph. De Candolle, son of the great De Candolle, enumerates twenty-seven great *nations* of distinct indigenous aboriginal plants. That the plants and animals of such vast districts of stations as America and Australia should be different from those of every other part of the globe, from which they are so completely divided, does not strike one with so much astonishment as that there should be 'found one assemblage of species in China, another in the countries bordering the Black Sea, and a third in those surrounding the Mediterranean.' Here distance and prior occupancy seem to take the duties of natural barriers. But however small, and however comparatively modern the spot, if it be inclosed by natural barriers (as, for instance, St. Helena) it will apparently have a creation for itself.

So in the Galapagos islands, of which there are ten principal islands, under the line, 600 miles westward of America, of modern origin, judging from the fresh appearance of about 2,000 craters, Lyell says of them: 'Although each small island is not more than fifty or sixty miles apart, and most of them are in sight of each other, formed of precisely the same rock, rising nearly to an equal height, and placed under a similar climate, they are tenanted *each* by a different set of beings.' 'Of twenty-six different species of land birds found in the Galapagos archipelago, all, with the exception of one, are distinct from those inhabiting other parts of the globe; and in other archipelagoes a single island sometimes contains a species found in no other spot on the whole earth.' Wings themselves furnish no exception to the rule. The Creator hangs his cages containing distinct birds in distinct separate regions, though those distinct separate regions may have precisely the same physical conditions. Lyell quotes Darwin: 'The archipelago is a little world within itself. One is astonished at the amount of creative force displayed on so many small, barren, and rocky islands, and still more so at its diverse, yet analogous, action on points so near each other. I have said that the Galapagos archipelago might be called a satellite attached to America; but it should rather be called a group of satellites, physically similar, organically distinct, yet intimately related to each other, and all related in a marked, though much lesser, degree to the great American continent.'

But the plan of the Great Creator seems, in all time, and in all terrestrial space, to have gone on 'qualis ab incepto processerit'; and as he has subjected individuals to death by years, so he has made species mortal by geological change of physical conditions. Nay, he has made their actual local habitations mortal. It is not to man only that he has said 'dust thou art, and unto dust thou shalt return,' but to the hardest rock and to the hugest mountain of the hardest rock. He has throughout all time made *continents* to come and to go—to have a birth, life, death, and burial. He wields the mighty power of subterranean igneous action, in raising them into hypæthral existence; and, as if to show their nothingness in his hands, he redeposits them in the subaqueous regions by the quiet action of the rain-drop from heaven; or, at his will, he places his all-mighty finger on the mountain-top, and submerges it bodily below the wave. And he has always, as species died out, and probably continues, even at this moment, to originate single stocks of new plants and animals, which radiate from where their Creator first locates them as far round as his natural barriers will allow them. Every local change in physical geography works not only a local but a general change in climate and physical conditions over the whole globe. The admirable Lyell accounts most magnificently for the vast changes of the general temperature which have evidently taken place over the whole globe, and shows that these changes are and ever will be taking place. That is, directly as land increases in polar regions and decreases in equatorial regions, cold increases over the whole surface of the globe; and directly as land increases in equatorial regions and decreases in polar regions, heat increases over the whole surface of the globe.

Man, by his mental qualities—that is, by his arts, not by his physical constitution—overleaps all 'natural barriers.' His 'station' is the globe, and there must be wonderful changes in the 'physical conditions' of its entire surface ere man could be exterminated *by them.* Man might survive any number of continents, and his existence is apparently confided to no second causes, but hangs on the fiat of his Almighty Creator alone.

In reference to species bordering on distinct stations Lyell writes: 'In almost every district, especially if it be mountainous, there are a variety of species the limits of whose habitations are conterminous, some being unable to proceed further without encountering too much heat, others too much cold. Individuals which are thus on the borders of the regions proper to their respective species are like the outposts of hostile armies, ready to profit by every slight change of circumstances in their favour, and to advance upon the ground occupied by their neighbours and opponents. The proximity of distinct climates produced by the inequalities of the earth's surface, brings species possessing very different constitutions into such immediate contact that their naturalisations are very speedy whenever opportunities of advancing present themselves.' Now, these *opponents*, these outposts of *hostile armies*, 'possessing very *different constitutions*,' and natives of *distinct stations*, might, from their perpetual propinquity, be called by physiologists 'social plants;' as those of the same station are called whose perpetual propinquity is caused by *similarity of constitution*, or by any of the many other causes of propinquity.

This perpetual propinquity physiologists have attributed to the inclination of the plants each for the other, instead of both for the soil, or instead of both for the conditions of vegetable life existent at the spot: such as, besides those which have been alluded to, the degree of drought or humidity in the air and in the soil, the freshness or the brackishness of the moisture in the soil, the degree of light or shade, exposure, &c., &c. What is friendship but a name? And physiologists have been at the pains to furnish these vegetable friends with a name from a *dead* language (*plantæ sociales*), for fear their living disciples should not understand a name from their own language.

Notwithstanding this care, however, Lyell actually has mistaken the pysiological meaning of 'social plants:' he makes it to be plants of the same species which live together in *communities*, as heaths. But it means plants of different species or genera which live together in *amity*, as beech and holly. Physiologists even give us the *reason* of their affection; though it is but a cupboard-love, that which 'expedivit Psittaco suum *χαῖρε venter*.'

In reference to this, Richard writes:—'This unctuous matter was the product of a kind of excretion performed by the roots. To this matter, which, as we have said, is different in different species of plants, the sympathies and antipathies which certain plants have towards each other have been attributed. It is well known, in fact, that certain plants have, as it were, a kind of liking to each other, and constantly live together. These are named social plants.'

And again: 'Roots also excrete, *by their slender extremities*, certain fluids, which are injurious or useful to the plants which grow in their vicinity; and in this manner the likings and antipathies of certain plants may be accounted for.'

In any branch of science other than vegetable physiology it would be considered a mechanical difficulty to pass the two contrary currents of absorption and excretion through the same capillary tubes of the 'slender extremities,' to say nothing of the chemical difficulty of passing the food and poison through the same conduits. But what nonsense can be too nonsensical for vegetable physiologists!

De Candolle, Liebig, &c., believe that each *planta socialis* assists and is assisted by his fellow *planta socialis;* that there is, as Liebig expresses it, 'a mutual interchange of nutriment between the plants;' that each battens on his neighbour's excretions; and that each is relieved by his neighbour from his own, to himself, poisonous excretions, in which an all-wise Providence has thought fit to envelope the roots of every vegetable. When will these great theorists persuade practical farmers to sow social plants with their crops, or even not to eradicate these social intruders? Charlock has sworn an eternal friendship with turnips. The poppy and cornflower with corn-crops. But the thistle is the *désiré* among vegetables. The amiable qualities of this plant have made it a universal favourite. When we cut our coppice woods here periodically it will grow ten feet in one season, while the pigmy of the race makes love so diligently on our downs and sheep-walks as well nigh to smother the grass with its caresses. Practical farmers nip all this vegetable affection in the bud, and forbid their crops the society of any followers or strangers whatever; though, alas! how often, like the parents of Pyramus and Thisbe, 'vetuere quod non potuere vetare.' In this case the crops and the weeds ripen their seeds simultaneously (which is one constant cause of propinquity, or sociability, in cultivated annuals); and as they are threshed and re-sown by our own hands, their reunion is certainly not effected by any choice of theirs. But ask the farmer if these social plants benefit his crops, and ask the physiologist how *clean* crops and other *un*social plants clear their roots of their own poisonous excretions. But these are cases of ephemeral or annual illicit love in a state of civilisation, where it is notorious that the course of this passion never did run smooth. In a state of nature, where it does not stand upon consent of friends, the holly and the beech are a pair, and are supposed to have a mutual perennial affection one for the other. This is from the holly bearing *shade* better than other plants. Under very dense beech woods holly will grow even where the seedlings of the beech themselves cannot exist. '*Densas*' is Virgil's epithet for beeches, and they will grow nearer to each other, and produce a more intense shade, than perhaps any other tree in nature; so that sometimes the silver supports of the green canopy stand without a leaf to interfere with

Sociability of holly and beech owing to holly bearing shade better than other plants.

their beauty. Cold, smooth-barked trees, like beech, *drip* from condensation much more than others. Yet I know not why the pure water of heaven, when condensed by such an alembic, should not nourish rather than destroy the growth it falls on. If drip is poisonous, as is commonly believed, it should choke, not feed, Roget's circle of capillary stomata; and I cannot attribute the deleterious quality of the overgrowth of beech to anything but its greater density of shade. Physiologists, indeed, (if we include the poison of the drip) arm this beautiful gem of the chalk with a triple poisoning power. For, while they do not except it from the general power of poisoning itself, inherent, according to them, in the roots of all vegetables, they give the excretions of the roots of the beech the particular power to poison all other vegetation whatever, except the holly; while, *mirabile dictu!*, to the holly these poisonous excretions are wholesome food. But that the beautiful nakedness beneath the beech is not caused by its poisoning the ground, is apparent from the fact, that when the shade is removed, that is, when the beech woods are felled and the ground re-planted directly, all sorts of trees grow on it luxuriantly, even when the roots of the beech have not been grubbed. Yet, on the supposition that roots excrete, the ground must have been saturated with these excretions for centuries, perhaps for many thousands of years. This fact may be seen exemplified at this present moment (1853) in Lipping wood and West wood, in the neighbourhood of West Meon, in Hampshire. The forest between Meon and Proutesflod (Privet) bore the name of Westan wudu, as parts of it still bear the name of West wood, and doubtless acquired this name as being the west end of the Saxon Andredes weald, which succeeded to the Roman Anderida Silva, and the ancient British Andred. This forest extended, on the west, from the north and south of the vale of the Meons* to the coast east of the Roman Anderida, or Saxon Andredesceaster, whether this is taken as Pevensey in Sussex, or as Newenden in Kent. And Andredes *weald* still gives the name to a great part of Sussex and of Kent; and, singularly enough, it furnishes a European name to geological strata extending from Wardour, in Dorsetshire, to the chalk border of the Paris basin, Hanover, and the north of Germany. The self-sown trees of the woods in this neighbourhood are probably the lineal descendants of the trees of Westan-wudu, that is, of Anderida Silva; but the ancestors of these trees, for ages before Roman foot ever trod British ground, doubtless sheltered the Druidical worshipper of the Heavenly Host: and the ancestors of these trees, again, have probably held this ground ever since the

* The nameless stream which rises above East Meon flows through West Meon, Meon Stoke, and falls into the Southampton water near Mean. Were its banks inhabited by the Roman Meanvari and the Saxon Meonware? Andred signified *uninhabited.* Meon is the Hebrew and Phœnician word for *habitation* or village. Thus, Baal-meon is the habitation of Baal, or the Sun; Britannia, from two Phœnician words signifying the land of tin, included all the south of England.

cretaceous bed of the ocean was upheaved by the fiat of the Almighty, and transformed into chalk hill-tops; that is (though such huge *spaces* of *time* are as undatable as eternity), possibly from about the time that the Pyrenees and the Jura began to sprout, and to change from subaqueous, horizontal, alluvial flats into hypæthral precipices and mountain-ridges.

These *suppositions* are at least as probable as the generality of physiological *suppositions*, though that is not saying much for them. But

> I'll believe both:
> And what doth else want credit, come to me,
> And I'll be sworn 't is true;

and, believing both historically and physiologically, the ground may be supposed to have been accumulating poison for all vegetation save the holly, for myriads of years previous to the creation of man, instead of the poor centuries and thousands of years which I have mentioned. Yet nothing can be more flourishing than the mixed plantations where the beeches of West wood and Lipping wood stood; and the self-sown grass grows with extraordinary luxuriance where the beeches have been cut, and even under those which remain standing as far as the light which is let in sideways by the felling of the outside trees permits. So that neither drip nor excretion can have been the deleterious cause here, but simply extreme density of shade. The only larches I know in England more than half a century old, which have never shown a symptom of *foxy* blight, are a plantation in this neighbourhood, which succeeded immediately the felling of an old, perhaps primæval, beech wood, on a thin staple over chalk. On that part of the site of the old beech wood next the larch, a new self-sown beech wood, mixed with ash and oak, has sprung up, which is equally flourishing with the larch.

The soil of woods does not become poorer, but richer.

In fact the soil of woods is not impoverished by their luxuriant growth, notwithstanding the quantity of material taken from them by man, nor is it *poisoned* by excretions from the roots, even of beech trees; but, on the contrary, when woods are grubbed, the soil is much richer, either for the growth of trees, or of farm produce, than the surrounding ground. Probably the main cause of this is, that the roots protect the ground from aqueous denudation, and allow a greater accumulation of soil, formed by disintegration and by vegetable chemistry. At the risk of being hooted at, I will, however, suggest another possible cause.

Is there any other cause for this besides protection by the roots from aqueous denudation?

I think that there is an aërial denudation, as well as an aqueous denudation, and an aërial deposit, as well as an aqueous deposit; and that woods are not only free from the aërial denudation, but are favoured receptacles for the aërial deposit: and that these circumstances more than compensate woods even for the crops taken from them by man. A part of the products of vegetation may be said to be *stored* in the bodies of animals, and as carnivorous animals prey on the herbivorous, a part even in carnivorous animals; but the longest-lived animals and plants die, and their hardest parts decay; so that, on the average, the entire mass of a year's growth of all terrestrial vegetation may be considered as taken annually from below the surface, and annually deposited afresh above the surface. Much of the organic or combustible part of this growth goes into the atmosphere in the form of gases, to be returned again to the earth in rain. Of the inorganic or incombustible part, or what would be the ashes of animals and plants if burnt, much is washed into the earth by rain, much ploughed in as manure, much washed into the sea with other soil. But, doubtless, much of this extremely 'finely divided matter' is transported by wind: and although, in bulk, this aërial deposit is a joke to aqueous deposit, its fertilising qualities are great; and as woods and parts covered with strong vegetation catch a great quantity of it, by comparison, they are enriched, while surrounding lands are denuded, even supposing aqueous denudation and deposit the same in both. So, in dry windy weather, when cultivated lands are thoroughly cleansed, and broken finely for sowing, great quantities, even of *soil*, change places by wind, by which exposed spots are impoverished, and sheltered spots enriched.

This denudation of *soil* is visible; so is the deposit of leaves. There are spots on which leaves are never allowed to rest, and others which every year catch large quantities. Ponds fill up even in man's short existence, and must be emptied if they are intended to serve as ponds. This happens whether ponds have a run of a stream through them, or a run of rain into them, or no run at all into them. In the last case aërial deposit will fill them, chiefly with leaves. But in all cases aërial deposit forms a great item in filling ponds.

Aqueous denudation is universal, and is not confined only to the lines of torrents and rivers.

For the rest, my aërial fancy will be voted incredible, because, farther than this, it is invisible. Yet great effects come from causes which are not very visible; and some people would stare (and among them, perhaps,Professor Sedgwick), if you told them that the top of the same Hampshire hill is on one side moving to the German Ocean through the medium of the Thames, and on the other side to the English Channel by the Itchen. Yet from all sides of the tops of these hills, and from all sides of every height on the globe, there are *dry river beds*, down which soil flows whenever rain is heavy enough to *run:* and all the infinite ramifications of these *dry rivers*, or ravines, or gorges, or gullets, or combs, or chines, or bottoms, or vales, or dales, or deans, or lavants (qu. from *labens*), by whatever name they or any parts of them are locally called, all have descents graduated by water, and outlets to the running rivers (if not to the sea), without any abrupt junction of the lower ends of the dry valleys with the upper ends of the river valleys; and no drop of rain *runs* an inch on the surface of the earth without, as far as it goes, setting some soil forward on its road to the sea. And it won't run back again. No return tickets are given. It will wait there, and go on by the nex-*t-rain*. The very soil on which we tread, and which we cultivate, may be said to be on its road from the hill to the sea. This is no new doctrine. Lyell quotes Pythagoras for it through the medium of Ovid,

Eluvie mons est deductus in æquor.

Soil, which is the disintegration or detritus of rocks (I use the term rocks in the wide, geological sense), is in perpetual formation over the whole surface of the earth; and from the whole surface of the earth it is in perpetual movement, by the wash of the rain, to the bottom of the sea.

This paragraph was scarcely printed in the second edition of this book before I had an opportunity of *seeing* the *surface-water* flow from the two sides of Filmere Hill, as I have *supposed* the *soil* to do. Frost set in a day or two before Christmas 1853, and a great quantity of snow fell. In the night of Friday, the 6th of January 1854, a rapid thaw began, with heavy rain. I went from Rotherfield to Brookwood on Saturday. Owing to the hard frost, the ground absorbed no water. It stood on the high road through East Tisted more than a foot deep, being dammed up by the cross road. Thence it flowed into *the lavant* from Faringdon by Chawton and Maiden Lane to the east of Alton into the Wey and Thames. In following the high road from Tisted up the north-east side of Filmere Hill, I saw a continuous stream along the valley, from near Ashen Wood. Where the valley was crossed by the new enclosures of West Tisted

Common the water was ponded back into lakes till it flowed over the banks on which these fences stand. On descending the south-west side of Filmere Hill, I came to a stream below the 'Horse-shoes,' which continued along the Dean to Bramdean, and through *the lavant* between there and Cheriton to Tichborne and Southampton. The streams of these two usually dry trunk valleys were joined by very strong streams from their usually dry branch valleys.

In opposition to Professor Sedgwick's opinions, that '*Torrents and rivers act upon lines only*,' while vegetable growth and deposit are universal, the area of aqueous denudation, or the wash of rain water, which is carried off by rivers, is still more universal than the area of vegetation. The disintegration of the barest rocks, of the barest mountain-ridges, beyond the pale of vegetation, is washed by rain to the plant-clothed hill-side below. Nay, even from the mountain-top clad with eternal snow, the descent of this *en masse*, the avalanche, and the glacier, bring down débris with them to be disintegrated below. Indeed, glaciers bring their huge quota ready-ground for exportation; and if my admiration and reverence for the great master would allow me, I should say that Lyell made an error *in admitting* this vast error of the Professor's; that is (though Lyell controverts Sedgwick's opinions), in allowing the expression which I have marked to pass current. Nay, as regards the formation of valleys, I would actually impugn some favourite doctrines of the great Lyell himself, though the doing so I feel to be as audacious a sacrilege as if I were to attack an astronomical opinion of Newton's.

It is true that the *direct* action in waste and denudation of torrents and rivers is on lines only: and were it not for the atmospheric disintegration and the *lateral* wash of rain, this their direct action would only cut ravines and channels to the sea; that is, where a spring issues high up the rocky mountain-side it will cut a deep ravine, and the deeper it cuts, the more springs it will lay open. But what widens this ravine into a broad valley with gently sloping sides? The *lateral* wash of rain into the *longitudinal* valley.

And what forms the broad valley even where there is no river at the bottom? or within many miles? The *longitudinal* scooping power of the concentrated rush of rain which in no respect differs from that of the torrent, except in its being a hundredfold more powerful than the torrent. It is indeed intermittent: so is the real scooping force of the torrent; for torrents only really excavate when swollen by rain. A torrent swollen by rain to perhaps twenty times the volume of its usual spring water, and hurling fragments of rocks along of all sizes, is in point of excavating and destructive power as much more formidable than its usual self, as a shotted gun is more formidable than an unshotted gun. We never *see* the clear torrent set its rocky ammunition in movement, though the shape of this ammunition tells us how often, and for what distances, it has been projected. But when the torrent is turbid with the wash of rain, we can *hear* its huge cannon-balls rattling down, and grinding each other and their rocky bed and banks till what has started from the mountain's brow as a huge rock arrives at the sea in the form of pebbles, or of sand. For although, as the *flood of rain* subsides, *the flow of boulder-stones* ceases, this is only for a time: each rain sets them on a stage on their journey, as, in lower levels and gentler gradients, I have said of soil, and the more minute particles formed by disintegration and vegetable chemistry.

A very slight difference in hardness of surface, or thickness of vegetation, at the brow of the hill may concentrate the wash of rain into a stream; this forms a channel, which is fed by rain from its sides. And though all possible *natural* accidents of this sort might have been supposed to have taken place long ago in all but volcanic, or newly raised regions, Lyell, quoting Sir T. D. Lander's account of the great floods in Morayshire, August 1829, says: 'Some *new ravines* were formed on the sides of mountains where no streams had previously flowed,* and ancient river channels which had never been filled from time immemorial gave passage to a copious flood.'

And again, Lyell, in giving an account of the formation of *new ravines* by heavy rains on the 28th

* 'Quodque fuit campus, vallem decursus aquarum
Fecit.'

of August, 1826, in the White Mountains in New Hampshire, says: 'The natural excavations commenced generally in a trench a few yards in depth, and a few rods in width, and descended the mountains, widening and deepening till they became vast chasms.' This was the effect of *one* continuous heavy rain.

And in the present day we may see ravines begun by the accidental results of many operations of *man*. A hedge or ditch, a pathway, or waggon-way may commence the furrow on the mountain's brow, nay, even on downs which are covered with the closest greensward: and how many of our byeroads and lanes become ravines!

Indeed, as the long conduits whose gradients are laid by the wash of rain very generally become the roads or lines of traffic of man, so, *vice versâ*, the roads or lines of traffic of man, on or near declivities, very generally become conduits for the wash of rain. Hence our '*sunk lanes*' and '*sunk roads*.'

In fact, the moment roads or lanes leave the line of the valley, the moment they cross the hills from one valley to another, that moment they become more or less sunk roads. And this 'more or less' depends on the steepness of the hills. That is, the steeper the hill the deeper the sinking of the road. The reason is that roads on declivities are channels for the wash of rain, and they partake of the nature of narrow valleys or ravines. Without great care, this *sinking* of the road, as compared with the ground through which it goes, will continue even after the road is gravelled or stoned: because in proportion as the surface of declivities is hard and imporous, the wash of rain and its power accumulate. So that, although declivities whose surfaces are soft are from this reason more easily abraded,—by their porousness, which is generally a consequence of their softness, they are to a certain extent protected from denudation.

The deposit from the wash of rain on each side of all *level* roads is soon covered with growth; it then also catches the dust or deposit from the air, and, unless the pickaxe and shovel are constantly at work, the drainage of a road is soon choked up.

As long as the sea or a river acts on the foot of a cliff, it remains a precipice, for the undermining water acts more rapidly than disintegration and *wash*, and clears away all that falls. Abstract this power, and the cliff has a tendency to conform to the slope, that is, to the *wash* of the hill above it, both at its brow and at its foot. For what is washed down the hill and off its brow will lie at the foot of the cliff as a talus or shelving bank; and what was the mid cliff gradually becomes the sole cliff. But this will eventually disappear into one slope. If the top of the cliff is table-land, or slopes from it, the cliff will waste much more slowly by disintegration, and the action of the elements But supposing such a cliff to be all of the same material, I think the brow has a tendency to disappear most quickly, possibly from a freer access and action of rain water in disintegration, and possibly also from roots inserting themselves in crevices, and, by turgescence, detaching blocks bodily. Besides this, roots in decay generate carbonic acid, which is a great disintegrator. Independently of *porousness*, volcanic cones are unlikely to have ravines or gullies, since their shape tends to diffuse, instead of to concentrate, any run of water. In fact, the tendency of disintegration and the wash of rain would be to *form* cones out of single hills, and ridges out of chains of hills, with projecting spurs, each spur being itself a ridge, ending in a half-cone: and even these ridges are so studded with cones as to have a serrated or saw-like outline, and to have earned the modern Spanish, Portuguese, and Italian name of sierra, or serra, and to have originated the Latin expression of *per juga montium*; the very name of hills being taken from the *yoke*-like appearance of contiguous cones.

For this reason, though, as Sir Humphry Davy finely remarks, no work of mortal can be immortal, those works of man which approach nearest to immortality are cones,—the pyramid, the tumulus, and the cairn. Why do the *imber edax* and the *fuga temporum* pass with so light a touch over these? Because they begin with a form which others end in,—a form which is not deformed even by disintegration and the wash of rain.

In comparison to the broad waste from the wash of rain, the waste by the *direct* action of rivers may be reckoned as nothing; and even this waste by the direct action of rivers takes place, I might say, *entirely* when they are flooded by rain. The real main geological work of rivers is *indirect*; that is, the carrying off the traffic brought to them by the wash of rain: and they carry this mighty traffic for the entire terrestrial surface of the globe; at least, their channels do.

And the channels of most rivers would exist whether the rivers existed or not, as in the south you constantly see river beds dry, or almost dry, except when filled by the superficial run of rain, or the thawing of mountain snow; and the size of the channels of all torrents and rivers (except in alluvial parts, for a reason which will be given) is in proportion to the heavy floods of rain which occasionally rush through them, not to the comparatively small volume of spring water which always flows down them.

Lyell, quoting Mr. Everest, calculates that in the *rainy season* (four months) the Ganges discharges into the sea a weight of earth equal to fifty-six great Pyramids; and in the other eight months only the weight of four Pyramids. Now, if Professor Sedgwick will only grant us these four Pyramids for the snows of the Himalaya, we shall have an annual *superficial wash* of soil amounting in weight to sixty Pyramids. This is the ordinary and annual work resulting from the operation of ordinary and annual rains on one river basin. But how many extraordinary floods, the result of extraordinary rains, have passed *over* the Ganges!

Does Professor Sedgwick think that all this soil comes every year from the erosion of the banks of the Ganges, or even from the valley of the Ganges? If it did, the valley of the Ganges would soon be barer of soil than its declivities. The soil comes from a great part of all the tops, and from all the sides, of all the declivities of all the myriads of valleys ramifying from all the tributary valleys of the valley of the Ganges; and what has passed into the sea in the formation of the valley is a mere nothing to what has passed down the valley, and does now pass down it, from the denudation of these infinitely extended surfaces *by rain*. In the rainy season there is, perhaps, a body of *surface* water which flows down the vale to the sea in volume fifteen times as great as the spring water; and were every spring of the Ganges permanently dried up, the vale would still be flooded every year by a stream in volume only less by a fifteenth part than that which flows every rainy season now, and fourteen times greater than that which flows in the three hot months.

The following ('Principles') will show what has formerly come down, and what still does come down, the sides of the Himalaya *into* the valley of the Ganges: 'A very ancient subterranean town, apparently of Hindoo origin, was discovered in India in 1833, in digging the Doab canal. Its site is north of Saharunpore, near the town of Behat, and seventeen feet below the present surface of the country. More than 170 coins of silver and copper have already been found, and many articles in metal and earthenware. The overlying deposit consisted of about five feet of river sand, with a substratum about twelve feet thick of red alluvial clay. In the neighbourhood are several rivers and torrents which descend from the mountains charged with vast quantities of mud, sand, and shingle, and within the memory of persons now living the modern Behat has been threatened by an inundation which, after retreating, left the neighbouring country strewed over with a superficial covering of sand several feet thick. In sinking wells in the environs, masses of shingle and boulders have been reached resembling those now in the river channels of the same district, under a deposit of thirty feet of reddish loam. Captain Cautley, therefore, who directed the excavations, supposes that the matter discharged by torrents *has gradually raised the whole country skirting the base of the lower hills;* and that the ancient town, having been originally built in a hollow, was submerged by floods, and covered over with sediment, seventeen feet in thickness.'

Denudation by rain extends over the whole *space* of the earth. Its *pace* will be modified, hastened, retarded, or partially stopped, by a thousand such circumstances as comparative hardness of surface, porousness, levelness, vegetation, heaviness of rain, &c., &c. Over vast tracts denudation by rain is so slow as to be quite inappreciable. Still there is, perhaps, scarcely any place in nature where excessive, continued rain will not *run;* and this *run* will be discoloured. This discolouration will be from soil; and this soil will be deposited where the rain ceases to run.

Even where the overflow of a river *stands* and deposits, that deposit may be subject to denudation from local rain after the river, or rather the flood, has subsided; though the deposit of the river may be annually in excess, and accumulation of soil may result, as in all alluvial valleys.

But the chief thing which diminishes and retards the effect of the wash of rain is the very force of the cause. This force, speaking liberally, has thrown the whole surface of the earth into ridge and furrow, into these graduated vales sloping to the sea; so that all the broad superficial runs of this wash are *shortened*, and are made *lateral*, into these *longitudinal* channels. We shape our roads high in the middle on this principle, to throw the rain off on each side with the shortest possible run into the ditch or gutter, and to prevent a wash *along* the road, which would otherwise soon wash our artificial road away. Were it otherwise in nature, were there one plane descent from the tops of all the hills and mountains, the volume of the superficial wash of rain, increasing as it descended, would many times in the year desolate the whole of the lower parts of the hills and mountains; and the lower parts of the hill-sides would have less soil than the upper parts; they would be as bare of soil as the channels of torrents. The channels of torrents and rivers prevent this effect now; they are nature's ditches and gutters: so that, in this light, rivers may be regarded as a conservative, not a destructive power. But rivers are mere labourers, or accessories, in the affair. The wash of rain is the engineer which has laid down the gradients of this preventive surface-drainage over the entire area of the earth. *The source of the valley* is always much higher up than *the source of the river;* I mean, than the *spring* source of the river: for the snow source or glacier source, being both superficial sources, I consider the same as the rain source of the valley. The river has no power of making a valley above it; but a torrent of rain water has the power of scooping a valley below it. Even on Salisbury *plain*, which is comparatively flat and covered with the closest greensward, these dry valleys, or rather continuations of valleys, above the heads of rivers exist: and that the cause which caused them still works may be argued from the valleys being richer in soil than the tops or sides of the hills, owing to the gentle gradients of the bottoms of the valleys; for, other things the same, that is, in the same strata, with the same vegetation, &c., one universal result of the wash of rain is, that the degree of denudation of soil will be directly as steepness.

Some of these dry valleys have an almost imperceptible slope; and they might be perfectly level, and yet be regular channels for the wash of periodical heavy rains. For if water *can* get out at one end of a level channel, and *cannot* get out at the other end or at the sides, it not only *will*, but *must*, get out where it *can*—

> Unda impellitur undâ,
> Urgeturque prior venienti urgetque priorem ;

and it will carry with it much of the finer soil formed by disintegration and vegetation.

When these chalky downs are ploughed up, the brow of the hill shows light, and the soil darkens in descending. If this is *not* from the wash of rain, what is it from? If it *is*, the wash of rain must be considered as a very universal agent.

The lateral wash of rain acts constantly to fill valleys. The longitudinal scooping force of rain and the run of rivers constantly counteract this. But in this continual contest, by comparison with their adjacents, the '*lines*' of rivers and valleys are far from being denuded. On the contrary, they are the favoured receptacles, the permanent, rich reservoirs of soil. But, according to Professor Sedgwick, they ought to be the denuded parts.

Nothing can be more pellucid than our Hampshire streams, except in heavy rains; and to look at them we should not say that they carried much soil to the sea. But throw a dam across the valley, and form a pond. Deep mud is instantly deposited over the whole bottom of the pond, and a boggy delta where the stream enters. This is from the lateral wash of soil into the river in heavy rains. We have only to look at the complexion of the river to see this. And the river deposits the mud in the pond, which it would otherwise have carried down to the sea. I will instance the ponds at Alresford and at Warnford.

This universal portage of soil by rain, the *eternal* effect of *eternal* causes, which in huge spaces of *time* results in such vast geological changes, would to some be incredible were it invisible. But this I think may be made visible, *oculis fidelibus*. It may be *seen* wherever a fence runs horizontally along the side of a hill. A *natural terrace* is then formed, for aqueous denudation goes on below the fence, and in chalk countries the ground becomes white: and not only does aqueous denudation cease above the fence, but aqueous deposit takes place, and the good soil which was on its way to the valley is arrested. Even a slight dead wattle, if kept up, will produce this effect, and, though the hedge is dead, the ground on which it is placed will grow; and a gateway wrongly placed will often let considerable quantities of this collection of the best soil escape, which might otherwise have accumulated for what man might call for ever. So an injudicious, downhill waggon-way, across fields, will sometimes act as a channel, and, catching soil laterally, convey it away from its proper owner. If your neighbour's land lies below you on a steep hill-side, unless you wish to make him a present of your soil, pound it back on to your own land by a fence, and, when it accumulates against your own fence, cart it up the hill again.

The beautiful *artificial terracing* of the hill-side, which we see in southern mountain cultivation, originates in the necessity of catching the stream of soil from above, and preventing its farther descent to the valley below, down which it would be washed, whether it were a dry valley or a river valley.

In unusually heavy rains numbers of these terrace walls give way; and when a terrace goes which is high up the hill-side, a sort of earthen avalanche takes place, bearing crops, soil, and stone walls in succession to the vale below.

In these works man shows himself as a strong *conservative.* In alluding to a man as a *leveller*, our great geologist admirably remarks: 'By ploughing up thousands of square miles, and exposing a surface for part of the year to the action of the elements, we assist the abrading force of rain, and diminish the conservative effects of vegetation;' and by *fencing* these thousands of square miles, man acts as a very universal conservative.

The existence of upper valleys, or dry rivers of soil, proves that, were there no such things as rivers on the globe, the scooping power of rain would still give the same alternation of hills with valleys sloping to the sea which now obtains, and the same waste and denudation from lateral wash would still take place. The river only makes its own channel (which is much enlarged by rain floods), and in that channel assists in conveying away the denudation brought to it by rain, which would otherwise travel more slowly *along* the valley, and *out* of the valley, by the same force which brought it *into* the valley—rain.

I believe that in many cases where the country is composed of soft and porous materials, as chalk, the depth of the valleys and channels scooped out by rain lays open the springs, and forms the rivers, instead of the rivers forming the valleys. How many dry valleys are there sloping to the sea without having laid open a spring, and therefore without any stream? And what formed these valleys? How many lateral or branch dry valleys are there falling into the main longitudinal dry valley whose lower end joins the upper end of the river valley *without any step or inequality*? And how many lateral or branch dry valleys fall into the main river valley? And what formed these countless myriads of dry valleys?

These valleys exist even in volcanic countries, where the sea could not have formed them while the land was emerging; and the gradients of the river valleys and dry valleys, and the whole form of the ridges and furrows, of the entire surface-drainage of a volcanic region (say, of Madeira), are so precisely the same as those of any other mountainous district, that no eye can glance over the two and doubt for an instant that the same cause caused the form of the drainage of both.

In fact, rain, which we consider only as a productive power, is the destroyer, the dissolver, of continents. Subterranean igneous action, which we consider only as a destructive power, is the producer, the replacer, of continents. And the cause which caused the valleys is in as full operation at this moment as ever it was. Indeed, valleys only exist in the dissolution of hills; that is, in the gradual and eternal wash by rain of the existent earth into the sea.

> Resolutaque tellus
> In liquidas rorescit aquas,

and

> Tellus glomeratâ cogitur undâ,

are as true at this instant as in the time of Pythagoras; or as they have been and will be (shall I say?) evermore.

But in reference to the *marine theory* in general, that the action of *waves* on land slowly emerging from the deep should have a tendency to wash away soft parts and to leave hard parts, I can conceive; but to attribute the formation of our valleys to this cause, as Lyell does, is to suppose that the materials of all the valleys running from the tops of all the heights on the globe were originally softer than the materials of the intervening ridges; but in almost all cases we can *see* that this is not so, by the corresponding strata on the opposite sides of valleys.

In regard to *currents*: a current might decapitate a continent as it rose, supposing equal softness of materials, or it might scoop a horizontal groove of any size or depth, or (granting lines of hard intervening ridges) many grooves; but they must all be horizontal, and in one direction. No marine current could make a single channel sloping from a height to the sea; still less the myriads on myriads of dry upper valleys which ramify in all directions, from all river valleys through and to all sides of the tops of all elevations, whether high or low.

But, in fact, the action of the sea impedes the formation of valleys, instead of making them. The sea often beats back, in the form of a bar, what the operation of rain on rivers forces into it; and valleys often form land in the sea, instead of the sea forming valleys in the land. The delta of the Ganges stretches 220 miles into the sea, with a base of 200 miles. The Mississippi has pushed a delta fifty miles out into the sea, with an area of 14,000 square miles; yet so far is it from 'acting on lines' in *eroding* its banks, that at New Orleans it is less than half a mile wide: and it may be said to act on lines in *building* its banks, for it has *raised* its own banks *above* the land they pass through, and increases the area of its alluvial plain at the upper end and sides, as well as the depth of it.

These effects are owing to the increase of the delta; and the same cause has produced the same effects in the valley of the Nile: for the lengthening of the delta lengthens the channel of the river with it; but the sea (leaving out the effect of the tide) tends to keep the surface of the river always *at a dead level.* Now a river flowing for 200 miles, or even fifty miles, or one mile at a dead level, is very ill-calculated to discharge the floods poured into it in the rainy seasons from inclined channels. The consequence is ponding back, *the river rising on itself*, overflow, and deposit from the overflow: but the banks, catching the first and heaviest deposit, grow more quickly than the plain, and the last yard protruded *at* the level of the sea, by preventing the spreading of the water, tends to force it over the yard behind it, and to raise that yard *above* the level of the sea; and so every yard raises the yard behind it, in succession, to the highest point of the alluvial plain; though in any part, and particularly in the higher part, the growth of the plain *may* (from excess of the *longitudinal* over the *lateral* deposit) keep pace with the growth of the banks.

Thus it is that a river is enabled to build itself a channel 100 or 200 miles out to sea, the sides of which shall not only confine for three parts of the year the waters which built them, but these banks shall rise *high* above the level of the sea. This at first appears impossible. Yet every day, in every quarter of the globe, this impossibility is being performed.

And possibly banks thus built may *tend* to assume that slope or gradient lengthwise which, when flooded, would allow the water to escape sideways at the same instant, and at the same depth, everywhere. And this tendency to a gentle simultaneous overflow of the whole banks prevents their erosion and the enlargement of the channel in the flood season, and, I think, is the reason of the small size of the channels of rivers in alluvial plains compared with the *unduly* large size of the channels of their upper parts, or of torrents.

Lyell tells us that from the sea to the upper end of the alluvial plain of the Mississippi (800 miles, including the delta) there is a rise of three inches in a mile, amounting to 200 feet.* I imagine that this rise is the effect of the advance of the delta, and that, as the delta

* This was thus published in the second edition of this book in 1853. From Ellett's *Mississippi*, published in the same year, and which will be alluded to in the sequel, I find that the slope of the surface of the river is about three inches in the mile, but the slope of the land about eight; and I think that an approach to these two slopes may be universal in alluvial plains. The banks of an alluvial river are built by deposit from the overflow of its waters. Therefore the height of the banks above low water at any part is the difference between the high water and low water. This difference at the sea is nothing, therefore the banks die off to nothing at the sea; and I think that there must be a fixed rule for the height of alluvial banks according to the distance from the sea; in other words, that there must be a fixed certain rule for the increase of the rise of high water necessary to overflow alluvial banks directly as their increased distance from the sea. I have seen somewhere that at Mendes it required a rise of seven cubits to overflow the banks of the Nile, at Memphis fourteen, at Syene twenty-eight. The rule, however, would apply only where the banks and bed are completely alluvial. The deposits on the unalluvial valley must tend to die off to nothing at the upper end of the alluvial valley, unless the slope of the unalluvial valley is suddenly terminated by a ridge of rock. I only talk of *general tendencies*, and these will be perpetually influenced by accidental causes, both general and local: as the perpetual variation, general and local, of the quantity and suddenness of rain, or the mode in which flood-water escapes. For instance, in some places it may be obliged to return over the same ground to the river; or, in deltas, it may escape by lateral *forward* channels to the sea; in alluvial plains, by longitudinal channels parallel to the main channel and entering the river

continues to advance, the rise will increase. In this light, seas as well as rivers may be considered as filling valleys as well as excavating them.

The steeper the slope and the more rapid the stream, the straighter. It is in the alluvial part that the river winds, and shifts its course. But 'erosion of banks' and 'acting on lines' must not be claimed from these changes; for, besides that the river fills up the old course which it has left, in the change it only takes what it had before deposited, and had brought from a distance.

The Adriatic is filling up from its tributary valleys, and 100,000 years may see it an alluvial plain, the Po running through it, and falling into the Mediterranean. Lyell ('Principles,' page 272) says that the delta advances '*a mile* in a hundred years.' But, page 207, he says, 'It is calculated that the mean rate of advance of the delta of the Po on the Adriatic between the years 1200 and 1600' (before the embankments) 'was 25 yards or metres a year' (this is at the rate of *a mile and a half* nearly, that is, a mile and 740 yards, in a hundred years), 'whereas the mean annual gain from 1600 to 1804' (after embanking began) 'was 70

below; or it may return to the river by lateral *back* channels cut through the alluvial banks down to the low-water mark; or more than one of these causes may act in some places, or all of them. All such difference of circumstances, and all accidental alteration of such difference of circumstances, will cause infinite variation in the direction and degree of the gradients in large alluvial valleys.

metres.' This is at the rate of *four miles* in a hundred years. Let us suppose the Adriatic to be filled up. In this case, would the alluvial plain be a dead level, or would it slope? I think it would slope from the head of the Adriatic to the Mediterranean. If so, as the level of the sea would prevent a *fall*, it must *rise* from the Mediterranean to the present mouth of the Po. This rise, at eight inches a mile, would be considerable. Nor would it stop there. It would be continued up the course of the river to where its unalluvial bed is above this gradient. If this is true, those who have undertaken to embank the Po and the Adige are *longi laboris damnati*.

Indeed, in *principle*, it is impossible, under any circumstances, that strata deposited by ponding (which gives the longitudinal slope), or by moving water from materials held in suspension (which gives the lateral slope in alluvial plains) should be *absolutely* horizontal. They must die out: the first in the contrary, the second in the same direction as that in which the water moves; although to *almost* all *practical* intents they generally may be reckoned horizontal, that is, reckoning time and space *humanly*. *Geologically*, the effect of this slight difference between principle and practice, in such a basin as the Mississippi, may expose millions of men to the constant *chance* of death to themselves and destruction to their property from inundation, with the *certainty* that their *actual* estates are in process of becoming subterranean, and themselves and their works fossil. For if nature filled the Adriatic at a dead level, what signifies it to Ferrara? But give it a slope from Otranto to the foot of the Alps, and Ferrara must become subterranean.

I only talk of *general tendencies.* In such river basins as have been mentioned, these operations are on so vast a scale, they are spread over such vast *spaces* of *space* as well as of *time*, they are so liable to disorder from *particular accidents*, such as changes of the course of the river, extraordinary floods, landslips, earthquakes, subsidences, upheavals, partial destruction of deltas by the sea, &c., &c., that man, unable to *see* what *is* or what *has taken place*, can only speculate on what *must be*, or what *must have taken place.*

The sea *ends* every valley, but never yet *began* one; that is, where there is no delta, when the river and the tides have done their utmost, it is the sea which prevents the farther deepening of the estuary.

The force of the river, as it dies off in the sea, becomes as perfectly *horizontal* as the force of a marine current; and neither of them could form an inch of *sloping* valley. But where there are no currents to prevent it, the sea, by stopping the longitudinal rush of rivers, allows the deposit of deltas, and may thus be said to *prolong* valleys.

'Bottle off the sea,' and all estuaries and their valleys will be deepened; for the tidal part of each river will be a torrent on the brow of a mountain. In this case deltas and alluviums would disappear; but they would not 'go with a run,' or by the *direct* action of rivers. Rivers would only cut ravines through them; the rest must wait for disintegration and the wash of rain. The sea forms and preserves them now.

From the universal denudation by the wash of rain woods are, by comparison, free. But whether the catching of aërial deposit may have anything to do with it or not, or whether it is to be attributed only to the protection afforded by their roots against aqueous denudation, soil improves even in woods which are robbed by man.

All nature teems with carbonic acid,—earth, ocean, air. All soils contain it absorbed from the atmosphere, independently of rain and of that generated from organic remains; and it is not only contained in all *superficial* soils, independently of vegetable remains, but it is vomited forth in vast quantities from below the surface by springs of all countries, and especially of all volcanic countries, as is carbonic acid gas into the air by active volcanoes. Nay, beside this air carriage and water carriage, there is a vast land carriage of carbonic acid from the subterranean regions. In many places it exhales in a gaseous form through the earth, disintegrating granite, gneiss, limestone, &c., in quantities sufficient to extinguish a light or the life of animals; nay, even to destroy plants from excess of this their principal food. In these forms earth disgorges and restores what, by aqueous deposit, she may be said to have previously swallowed. All water contains it, fresh or salt, well, spring, or rain. Add to this the constant enormous supply generated chemically in the decay of animal and vegetable substances, and we have quite enough to account for the increase of vegetable matter formed by the action of vegetable chemistry on sap absorbed by the roots.

Suppose we take a mass of volcanic rock, in which there can be no vegetable or organic remains whatever, that we grind this into powder, and expose it to the air in an open case a foot in depth, whose lower part is sieve-like and equivalent to a porous subsoil. The powdered rock will be disintegrated by the action of the air and rain. It will also absorb carbonic acid and ammonia from the air and rain. An infinite variety of seeds will be brought to this soil by my pet aërial deposit. Very few at first will grow, on account of the small store of carbonic acid in the soil. But if they do not grow, or if they only partially grow, they will decay; and in decaying will so increase the stock of carbonic acid, that more plants will hereafter grow. Now, to prevent the effect of my aërial denudation, let us bury the heads as well as the roots of these plants below the soil. Every such plant, large or small, will, in decay, become a hoard of carbonic acid fixed in the soil for the food of future plants, in addition to the annual supply of carbonic acid and ammonia to the soil from the air and from rain. The carbonic acid of decaying vegetation will also help the rain in disintegrating our powdered rock. So that vegetation may be said to produce vegetation; and we may possibly see in this general tendency to the increase of vegetable remains a main cause of the formation of bogs: and perhaps aqueous denudation may be a necessary agent to prevent the *undue* increase of vegetable remains over the whole surface of the earth.

No necessity for rotation in nature's cropping.

And natural forests return to the soil all they take from it, and with interest; and Lyell should not talk of trees *dying out* from the soil having 'become exhausted for trees,' or of the necessity of *rotation* in nature's cropping. Rotation of crops is only necessary where man robs the soil of the produce he has raised, or raises plants by cultivation such as nature could not raise without cultivation. Plants in a state of nature stick to their appropriate *stations*, so long as the physical conditions of those stations remain unaltered. The doctrine of rotation is in direct contradiction to the doctrine of fixed stations for plants.

It is perhaps *probable* that were wheat sown every year on the same land, and ploughed in before ripening, the land would be enriched, not impoverished; that is, a great increase of carbonic acid would probably occur from vegetable chemistry, and a great increase of the inorganic constituents of plants from disintegration. In fact, although bearing wheat every year, the soil would become as rich as *maiden* soils always are. A process resembling this is what does go on in natural forests, in addition to the absence of denudation. In the case which Lyell mentions, of bogs formed 'by the fall of trees and the stagnation of water caused by their trunks and branches obstructing the free drainage of the atmospheric waters, and giving rise to a marsh,' and 'of mosses where the trees are all broken within two or three feet of the original surface, and where their trunks all lie in the same direction,' it is not the trees which are *dying out* on soils that have 'become exhausted for trees' which break or blow over in wind: but, on the contrary, the trees which break or blow over in wind are the rank product of a soil which suits them, and which grows them too close to have side-boughs, and consequently too tall for their girthing and for their circumscribed roots. Many countries have ceased to be covered with forests from many different causes; but the last cause I should assign would be the soil *becoming exhausted for trees.* The formation of bogs by the *over-luxuriance* of woods may be one of these causes. I think it possible that, in some cases, the irruption of peat into woods from the bursting of bogs above them may have violently overthrown the trees; in this case the trees should all lie down the stream: or that the drift from above, and quiet deposit of alluvial peat into woods, by suffocating the roots, may have killed the trees and caused their fall, instead of the fall of the trees causing the bog; but in this case it is unlikely that the stems should lie all one way, yet this is possible if the tract is only exposed to wind on one side.

PART IV.

PRUNING AND THINNING.

DOUBTLESS, in ornamental grounds, every variety of growth should be encouraged; and doubtless, every variety of growth can be attained by gradually and constantly cutting out all growth except in the direction required. To prove this, we have only to observe our wall-fruit trees, and the forms of animals, arches, &c., into which trees are cut.

For beauty we should have every variety of growth.

In trees, whether for beauty or profit, no attribute is more to be admired or desired than height. But it is probable that by nature, that is, without pruning, trees can never attain the maximum height of which they are capable if pruned.

Trees cannot attain their maximum height by nature—that is, without pruning.

In the shelter of timber-woods, from want of room for their roots, and from want of all side-boughs, trees in general grow weakly, and do not attain their maximum height in a minimum time, if ever. On the other hand, single trees, which have plenty of room for their roots, and even coppice-wood trees, from exposure, or from the quantity of light all round them, generally go more to side-branches than to height; but in sheltered situations, with good soils, I have no doubt that, by early and gradual pruning, single trees might be trained to much greater heights than we see at present.

The works of God can be improved by man.

When this was so stated in the first edition, an anonymous friend wrote, 'The works of God cannot be improved by man.' I differ. I think that the works of God *can* be improved by man. I think that God as much intended his works to be improved by man, as He intended us to improve ourselves. Are the glorious gifts of the Creator, the products of the farm, the kitchen-garden, and the flower-garden, not improved by man? Are our domestic animals not improved by man? Is the European man no better than the Bosjeman? If so, we give ourselves and children much pains for nothing. I believe that God has made man in general an instrument to perfect the terrestrial *treasures* of His creation, for man's own advantage; and that to some God has imparted the highest possible enjoyment in eliciting, *improving*, and displaying the *beauties* of His creation, quite distinct from any mercenary or selfish ends of their own.

It is true that one life will not *accomplish* much. But that we can do little is not a reason for doing less, or nothing at all. *One* year's pruning, by destroying competing leaders, may destroy what would for ever have vitiated the growth of the tree. Ten years' pruning might leave Nature a sketch to fill up, such as she could not have accomplished without the aid of man. Haply, also, others may take up the running when we succumb.

To grow valuable timber maximum head on maximum height of branchless stem.

To grow *valuable* timber, we should not only aim at a maximum height of branchless stem, but a maximum head on a maximum height of branchless stem; for in proportion to the quantity of head will be the quantity of its downward deposit, or increase of the girthing of the stem.

Rules for pruning for height.

For pruning trees to grow to their greatest possible height, the rules are simple, and they are applicable alike to the nursery plant and to the largest timber-tree: Keep a clear leader. Cut off all branches large enough to compete with the stem, or which grow parallel to it. Shrive the stem up one-third of its height. Cut all close to the stem. With the above exceptions, a tree cannot have too many branches, as the returning sap of each contributes to the growth in girthing of all that part of the stem which is below it, and to the growth of the root both in length and girthing. But pruning, like thinning a plantation, cannot be too gradual. It should be annual.

A well-placed but over-large branch should be curtailed where it turns up, or where it forks, or at the foot of a shoot. It is bad pruning to leave a dead stump with no growth beyond it, whose descending sap shall deposit over the scar.

In timber-woods and in plantations, the trees should stand close enough to discourage the growth of many side-boughs, or of any large ones. As the side-boughs are gradually and annually overgrown, and before they are actually killed, they should be removed with a common saw, set wide for the purpose; and the axe and cross-cut saw should gradually and annually thin the plants out to greater distances from each other. Timber may thus be reared *without a single disunited knot*; and if we suppose the side-boughs to be taken off each, where the stem is eighteen inches in girth, without a symptom even of a cross-grain, at a greater distance than three inches from the centre, the rest of the entire mass of timber will be without a vestige of a knot, or even of a cross-grain. If the plants are left too close, weak poles will be grown; if they are left too wide apart, too many and too large side-boughs will be developed.

Supposing perfect shelter and perfect room together, almost all trees will make a good fight on almost all soils: but it may be laid down as a general rule that, where shelter is given, room is not; that plantations are always planted too thick to grow, and are never thinned; that in plantations the nurse always over-lies the child. It is not meant here to object to planting plantations too thick to grow: they should be planted too thick to grow, and then thinned, taking the worst plant worst placed, and leaving the best plant best placed; regard being had to what is likely to suit the soil best, and what is intended to be grown permanently. In this way, not only is the ground *cropped*, not only is profit made by the thinning, and not only are unduly large side-boughs dis-

Plantations are and *should* be planted *too thick* to grow, and should be thinned every year.

couraged, but an immense choice for the permanent plants is gained, and the soil, instead of being exhausted by such cropping, is enriched by it, as has been argued.

No saying is more true than that 'fools may *plant*, but it requires a wise man to *rear* timber.' More than this, it requires a succession of wise men. It is extraordinary that those who by practice are annually convinced of the importance, nay, necessity, of thinning their turnips by hoeing, so often neglect this principle in their plantations. The principle should be practised from the beginning; but if it has been neglected ever so long, 'sapere aude, *incipe*.' Nothing has done so much harm to plantations as that, 'Oh, it is too late now!' It is never too late. It *can* never be too late. Can it be too late to begin cutting out dead rubbish? Can it do harm to take out what is doing harm? Can the wind be let into plantations by cutting out denuded poles without heads? Go into the plantation whose thinning has been put off till it is *too late*, and, I was going to say, *boldly*, but let us say *quietly*, cut out the dead and the dying, gross cases of rubbish, and then gradually and annually the worst plant worst placed, leaving and relieving the best plants best placed. He who perseveres on these principles will (besides eventually creating permanent fine plantations) very soon, in his present thinnings, be cutting boards instead of bavins.

No turnips without hoeing; no trees without thinning.

Never too late to thin.

Cut the worst plants worst placed; leave the best plants best placed.

Do not think it a matter of no import whether the dead and the dying are cut or left standing. The absence of the dead and the dying is of the greatest importance to the living. The space occupied by dead heads should be occupied by living limbs; and *attenuated*, dying, *waving* plants, from their *locomotive* power in wind, *whip* and denude their neighbours more than stouter plants can.

Exposure is no excuse for not thinning plantations. There is no reason, because the heads of trees are exposed to wind, that their roots should be exposed to robbery from their neighbours and starved by their own want of head, resulting from the *whipping* of their neighbours. The best plants, being the tallest, have always borne the exposure. You do not expose them by cutting out the worst plants from below them; but you relieve them from what denudes their sides and robs their roots. If this operation is trusted to a workman, he takes *the best plants to sell*, and leaves the weak ones to grow. These weak ones have always been overshadowed and so made *tender*, and when the large ones are withdrawn from above them, if they do not die, they do not grow, but remain hideous scarecrows; then thinning gets a bad name,—'the plantation has been spoilt *by letting the wind in.*'

Thinning and pruning should work together, and both should be gradual and annual. By rearing timber moderately close from the beginning, increasing the distance of the trees directly as their size, and thus depriving the sides and the lower parts of the stems of light, we not only encourage the heads to grow upward, but prevent the overgrowth of side-boughs. All side-boughs which are to be taken away, should be gradually sawed off before they die and while they are small, since the new annual growth over a wound is curved till it is wholly healed.

Pruning does not increase the aggregate quantity of wood made by a tree; but, by improving its location, increases the *measurable* timber.

All branches, though they may be said to rob that part of the stem which is above them, feed that part which is below them with their descending sap. The growth of branches which are gradually taken off from the side of a tree is transferred to its head, and the descending sap from this additional new growth of the head increases the bulk in girthing of the whole long stem, instead of being wasted on the increase of side-branches. If, indeed, too many side-branches are taken off at once, so that the diminished head cannot by extra growth elaborate the whole sap sent up by the root, the whole tree receives a check, not only in the increase of bulk in girthing, but in the growth of the root, on account of the diminished supply of elaborated descending sap. Thus bad pruning may *diminish* the quantity of timber grown; but I can by no means concur in Loudon's idea that pruning, by *increasing* the quantity of timber, deteriorates its quality. No pruning increases the quantity of wood made by a tree, but only alters the location of it; but the actual bulk of all the side-branches which are *gradually* taken off, or rather the bulk to which all those branches collectively would have attained, may be considered as laid on to that part of the stem which is above them, without detracting from the bulk of that part of the stem which is below them. This is the great merit which good pruning lays claim to.

Example. Suppose a nursery plant with two equal leaders; both are weak in comparison to the stem below, because each has only half the sap which ascends through the stem, and also each has only its own descending sap, while the descending sap of both deposits on the stem below. If one leader is taken off, new vigour is given to the other. The growth which would have been for ever divided, is for ever united. The two leaders are condensed in one, and the growth of the favoured one, in the bulk of its stem, is for ever doubled. But this twofold increase of the stem above the fork is no greater than the increase of the stem below the fork; and the increase of that stem is in no way altered by the pruning: it only receives from one stem the same quantity of descending sap which it would have received from two. Nor is the whole quantity of timber produced by the tree altered, though it is infinitely increased in value, by uniting in one long stem what would have been divided into the branches. Throughout all forest pruning the same principle reigns as has been exemplified in the case of the double-leadered nursery plant. Any argument to prove that the double increase of the single remaining leader deteriorates the quality of its wood, would also prove that the quality of the stem below the original fork is inferior to that of the two leaders, or that stems in general, which are the receptacles of the aggregate descending sap of the branches, are inferior in quality to the branches.

As an example of what *may* by accident happen to multiply leaders in the growth of forest-trees, I will instance what *does* happen, and what *must* happen, every year in the growth of the Paulonia. As in this climate it never ripens the wood of the current year to the end where the single leading bud is, the next year's shoot begins from *two* opposite side-buds; so that every single shoot *must* the next year be continued by a double shoot, unless this is remedied by pruning, that is, by cutting each shoot back to a vigorous bud, and pinching off its opposite rival.

Let us suppose the worst possible case against pruning. Suppose that, in consequence of neglect, it is necessary to take a large limb off at the centre of an otherwise branchless stem; that this makes so bad a flaw that the tree, when felled, must be cut and used in two lengths. Still, as long as it stands, as the root is uninjured and undiminished, the same supply of sap will be furnished. That sap will be elaborated in the head by the new growth which it will impart to it, and the girthing of the upper length of the stem will be increased by all the growth which would have been laid on the side-branch, while the increase in girthing of the lower length of the stem will not be diminished.

In such extreme cases, or when large branches have been cut off in consequence of being shattered by wind, their ends should be painted, and, if they crack, stopped with putty till the wound is healed over.

To rear first-rate timber, I think the whole surface of the ground should be canopied over with the heads. This canopy should, by gradual and annual pruning, be raised to the greatest possible height, and by gradual and annual thinning be supported by the fewest possible stems. I think mixed woods of coppice and timber bad; because if the trees are close enough to grow clean, even timber, they will destroy the coppice-wood; and if they are far enough apart to allow undergrowth, they will have large side-branches and irregular stems.

It is true that the growth of coppice-wood, by killing all side-branches, is the great natural pruner, and gives clean stems to a certain height; but as this is overdone in the youth of the plant, as soon as a coppice-wood or hedgerow-tree emancipates itself from the undergrowth it bursts forth hydra-headed, and becomes flat-topped. The judicious saw should remedy this.

That a branchless stem is a natural attribute of a tree is a mistake of De Candolle.

It is a great mistake of De Candolle, Richard, and other French writers, to lay down the branchless stem as a distinctive characteristic of a tree. All trees which grow singly on sheltered lawns, *if permitted*, have branches down to the ground, and from the lowest parts of their stems; and most beautiful objects they are!* Nay, if circumstances permit, trees will throw out branches to an indefinite extent *below* the ground on which they stand. This may be seen on the side of a chalk-pit, or any other bank sufficiently precipitous to prevent the browsing of cattle. The branchless stem is the result of injury from the hand of man, or beast, or neighbouring trees. The single exception to this rule is the Italian pine; and a most beautiful and a most picturesque object the branchless stem is! This Claude and Salvator, and all landscape painters, show us. And, as I have said, for *beauty* we should have every variety of growth; but if we desire *profit*, if we desire clean timber, we must not go to nature for it. Clean timber is no more a product of nature than a field full of clean wheat is. Nature's sole mode of pruning is killing the branches; and the timber of the Italian pine, the only branchless stem formed by nature, is more full of flaws and huge movable knots, than the timber of any other tree whatever. But in all cases except the Italian pine accident, not nature, produces the branchless stem.

* In artificial gardens and lawns in general, these boughs are prevented from lying on the ground on account of the convenience of mowing. In nature they are prevented from even reaching the ground by neighbouring growth or cattle; but where they are allowed to come down to the ground they will *layer* themselves in a circle round the tree and grow straight upright, so as even to smother their own parent. Many most beautiful and most curious instances of this may be seen at Wardour Castle, especially among the red cedar tribe.

Plant an oak in your kitchen garden, and clear it of all neighbouring growth; that is, shield the tree from accident, let nature alone, let the tree have perfect shelter, perfect soil, and perfect room. So far from growing with a branchless stem, its lower boughs shall on all sides, along the very ground, in length make a good race with its leader. What, then, are the natural, or rather accidental pruners? What pruners make the tall, clean stem valuable as timber? They are three in number—coppice-wood, cattle, or neighbouring trees. These are nature's journeymen-pruners, and most abominable bunglers they are. They follow their mistress's plan, and prune by killing the branches, which, till they rot off, are inclosed in the stem, and form disunited or movable knots. If accident may prune, why may not art? But if art and the saw are not allowed to do this pruning, they should at least assist, and cut off the boughs as they are killed by neighbouring trees. I only talk here in reference to the senseless clamour against pruning—of whether pruning is good or bad for the tree, and for the timber; not of whether it would *pay* or not. That must depend on a variety of circumstances—the price of labour, of the faggots, of the timber, &c., &c.

A living branch forms a crossgrain; when it dies, an *united* knot. What is afterwards inclosed, a

A branch, as long as it is alive, does not form a knot in timber, but only a cross-grain, that is, as the stem increases each year in girthing, it incloses each year a portion of the root of each of its branches; and the grain of these branches forms, of course, an angle more or less acute with the grain of the stem. But this cross-grain is *united* grain for grain; that is, growth for growth with the grain and growth of the stem. And if the tree is cut while the branch is alive, the branch forms no knot, but only a cross-grain. If the branch dies while the tree is alive, the cross-grain dries, and becomes an *united* knot. Afterwards the stem incloses, each year, a piece of *disunited* dead wood instead of living wood, which is united to it. This forms a *disunited* knot, instead of an *united* knot, in the timber; and as the dead wood is dry when it is inclosed, the living wood, when sawed up, dries from it. This forms a movable knot. The bark ceases to *run* when dead, and is frequently inclosed with the dead branch. This, and afterwards rottenness of the outside of branches, increase the disunion of knots from the timber. But, besides the flaw in the timber, the dead wood which is inclosed forms an impediment to the course of the sap, and a consequent distortion of the grain, as much as if a bolt of iron were passed into the tree.

disunited movable knot.

Now, the great objection to a cross-grain or to an *united* knot is, that it prevents the timber from *cleaving* and *working* well, as the carpenters say: but it does not *weaken* the timber, or render it more liable to break; but at every *disunited* knot the timber is *already broken*, besides the cross-grain.

De Candolle remarks that, as the girthing of the branch is at first extremely small, but increases annually, each year the stem incloses a larger circumference; and that part of the branch which is inclosed is in the form of a cone, its base at the bark, and diminishing inwardly towards the pith. The outer part of the branch is in the form of a cone, its base at the bark, diminishing outwardly. But no such internal cone exists except in appearance, that is, *in colour*, when a branch has died while the tree was alive: and doubtless De Candolle has been deceived by the appearance of knots formed by branches which died and dried before the tree was cut. When a branch dies while the tree is alive, it will indeed dry in and change colour in the form of a cone; because, as the continuation in the stem of its annual growths is not peculiar to the branch, but common to the whole tree, they do not dry in and change colour like the dead branch, but remain moist conduits for the upward sap to the head of the tree. But as long as the branch is alive, the medullary rays and longitudinal woody fibres of the new annual growth of it are prolonged, and run vertically down that part of the stem of the tree which is below the branch; so that it is only the grain of the centre part of the branch, that is, its *first* year's growth, which runs across the grain to the centre of the tree. It then in general joins the *second* year's growth of that part of the stem which is below it, and runs down the stem of the tree to the roots. The grain of every other year's growth of the branch annually turns down the stem of the tree, short of the centre of the tree, directly as the newness of its growth. The same or rather the reverse *appearance* may be observed above the branch, if a *living* branch and the stem are cut longitudinally where they join; that is, the grain of each year's growth of the branch *appears* to turn *up* the stem of the tree: for each annual downward growth of the branch meets the *corresponding* annual downward growth of the head of the tree, joins or anastomoses with it, and passes round the side of the branch down the stem. Thus, above, and below, and on the sides of the branch, each annual growth of the branch and of the stem is not two growths, but one growth, and it cannot be said where the growth of the branch ends and that of the stem begins; and the part of the branch within the stem is much more like the roots of a tree than a cone. When the tree is cut up in the saw-pit, if the saw does not strike the pith of the branch *exactly* lengthwise, if it cuts the branch *diagonally* lengthwise, the branch will form a double cone and taper both ways at once. If this double cone chance to be divided *across* the centre, the two parts of the same branch will on one board show as a cone tapering outwardly, and in another as a cone tapering inwardly. The double cone I allude to, will be easily seen by cutting a small branch across with a *long slant*. De Candolle's internal cone would only exist if the annual downward growth in girthing of the branch ceased when it arrived at the stem; but as this growth does not cease here, a branch unduly large in proportion to the head of the tree will form from its own deposit an excrescence below it where it joins the stem, equivalent to what are called 'the spurs,' or the 'swell of the roots' where they join the stem. De Candolle's observations would apply to roots as well as branches. But if we bisect them lengthwise, we may see with a glance that roots as well as branches stand on increasing, not decreasing bases, where they join the stem. A dead branch, or an undersized branch, overgrown by the head of the tree, will cause a hollow below it, from stopping the downward current from the head, which cannot turn sufficiently short to deposit immediately below the branch. This is often seen in beech-trees; and the groove is sometimes prolonged the whole extent of the stem.

Indeed, the *unity* of growth which must exist at the foot of each branch, with the part of the stem which is above it as well as below it, is apparent from the fact that, when branches are cut off at the distance of an inch or two from the stem, the descending sap of the bark of the stem will ascend the bark of these stumps, will well over between their dead upper bark and wood, annually increase their girthing, and cicatrise or heal over their ends, forming protuberances which will occasion a consequent distortion of the grain of the wood, and diversion of the current of sap. These protuberances will, indeed, in the course of time almost entirely disappear, because, if equals are annually added to unequals, in the course of time apparent, though not absolute equality will result; and this does take place in the annual deposit of new layers of wood and bark over the stem and these protuberances. But each of these protuberances creates a piece of dead, disunited wood, which is in general nearly, if not perfectly rotten. This system of pruning, as far as it goes, makes flaws in timber, and disunited knots, similar to the leaving dead branches on trees. These flaws are discovered only in the saw-pit, or by the searching augur, and the blame is laid on pruning generally; whereas pruning living branches close to the stem prevents the very evils which it is accused of creating. If a *dead* branch and the stem are cut longitudinally where they join, though the whole branch may have dried in in the form of a cone as far as the central pith of the tree, still there is a perfect unison of the dead wood of the branch with the living wood of the stem, and the junction of each new annual growth of the stem and the branch will be perfectly visible as long as the branch lived. But from where the branch died each annual growth of the stem will inclose a portion of a mere dead bolt without any junction with it; and this is one strong reason for not letting trees *prune themselves*, as it is called, that is, for cutting off the side-branches *before* they are killed by their neighbours, and for cutting them as close as possible to the stem; even then a protuberance of the thickness of the bark will be left, and where the bark is thick and dead, a part of this should be taken away with the branch.

Healing over an amputated branch.

When a living branch is cut off a vigorous tree close to the stem, new growth, both of wood and of bark, is gradually and annually deposited over the end of it. This new twin growth begins as the tree ceases to shoot. About June the bark may be observed to separate from the wood, and the granulations of this new growth may be seen between them; it proceeds in a semicircular form on the top and sides of the scar, till the growth from one side meets the growth from the other side at the lower part of the scar; the growth then proceeds towards the centre of the circle, and as the new annual growth both of wood and bark is deposited on the top as well as the sides of this circular *wave* of growth, the level of the top of the wave keeps pace with the level of the annually increasing girthing of the tree, and when the ring closes in the centre no indentation is left; and each succeeding year the new annual ring of wood and of bark is deposited over where the branch was, with as much regularity as on any other part of the stem, nor is any distortion of the grain of the wood or diversion of the current of the sap occasioned after the healing is completed. The end of the branch will die and dry in, possibly to the extent of the cross-grain occasioned by it, and a very slight and inconsiderable flaw will remain in the timber where the living wood is deposited on this dead surface; this flaw will be no greater than that occasioned by a small piece of bark being accidentally knocked off a tree.

The rapidity of the healing will be directly as the rapidity of the growth in girthing of the stem. Suppose the width of the new annual layer of wood to be a quarter of an inch, it would take twelve years to heal over the end of an amputated branch, whose diameter was six inches. During those twelve years, the grain of the new wood deposited over the end of the branch will be curled; after that, straight-grained wood will be annually deposited. These are reasons for preventing the undue growth of side-branches in bulk, and for finally taking them off while small.

On cutting across the part of a branch which I had rung, I found that in the course of twelve years the *outside* wood had died, and dried in only to the thickness of paper. From this I imagined that the case would be the same with the *ends* of amputated branches. I think it, however, probable that the reason why so slight a surface of the rung branch died and dried was, that the whole of the internal wood remained the conduit of the upward sap. In the case of amputated branches, the internal wood would cease to be a conduit of sap, and the whole probably dies and dries in as far as the cross-grain. This would occasion a knot to that extent; but it would be a knot united annually growth for growth with the stem-wood, and not like the detached knots which are formed by the inclosing in the stem of branches which have died.

The healing takes place over a dead branch which is cut off in the same manner as over a living one.

But if a dead branch is left till it becomes rotten where it joins the stem, as there is no firm surface for the deposit of new wood, the new growth curls round upon itself, and a hole remains in the stem of the tree. In this the water, running down the stem, lodges and saturates the parts. This, with the action of the oxygen of the air, continues the process of decay, which is communicated by contact to the heart-wood of the tree: and hollowness of the centre is almost always thus caused by rotten branches from above, not by rotten roots from below. This is the fruitful source of destruction to our timber-trees, to the life of which, otherwise, there is apparently no necessary limit. Very little care may avoid this chief cause of decay.

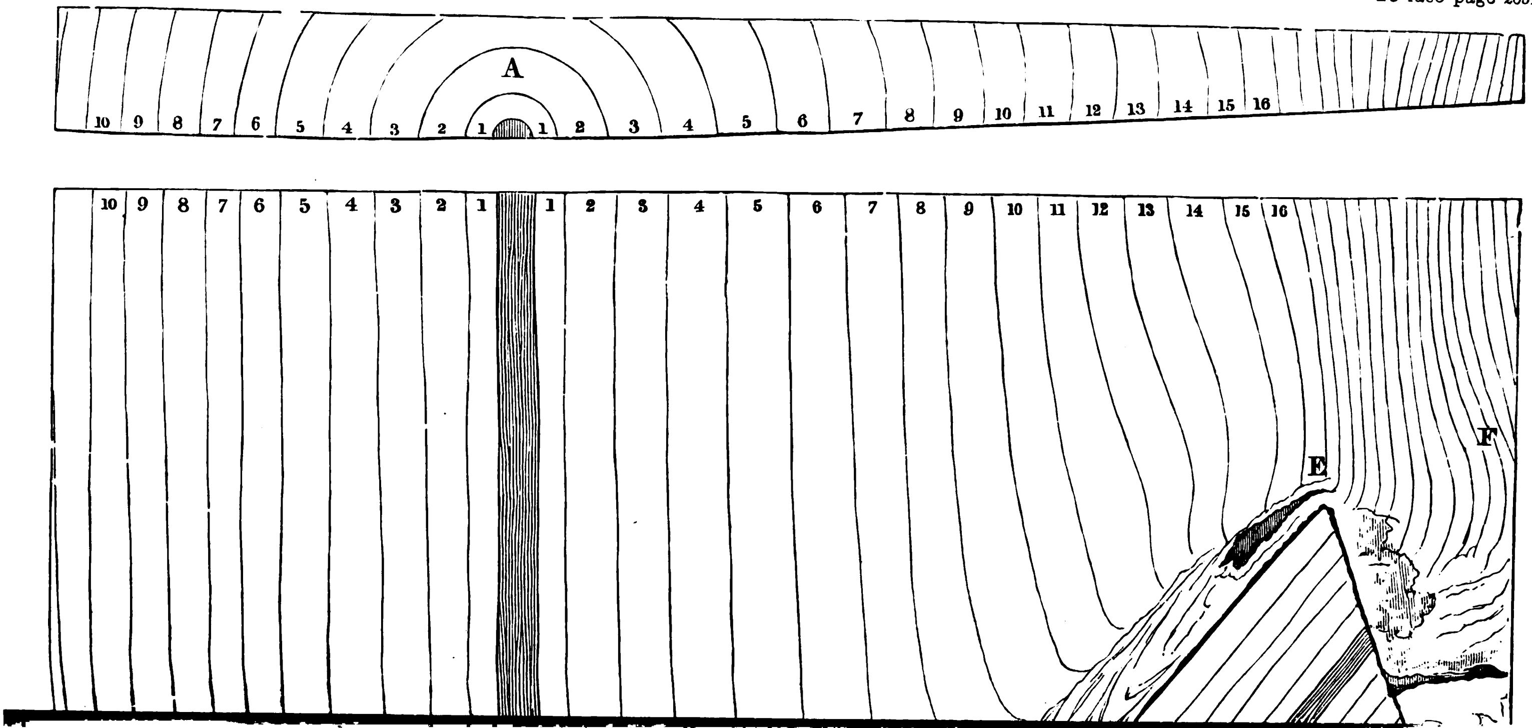
It is not meant to assert that there *is* no limit to the age, or height, or bulk which in a case of optimism trees may attain to; but at present we *know* of none. The whole appears to depend on circumstances; that is, even if we knew the maximum age, or height, or bulk which any particular sort of tree had ever attained to, it would not follow that under more favourable circumstances others might not have surpassed it.

Prejudice against pruning with a saw, a vulgar error.

The prejudice against pruning with a saw, or the idea of the necessity of afterwards cutting the wounds over with a sharp instrument, is a vulgar error. The new formation of wood and bark over an amputated branch is not from the cut wood, which dies, or from the lip of the bark, which also dies. It comes from springs far above these and independent of them. The

PLATE I.

To face page 205.



new deposit of wood and bark over the wounds of trees is the brimming over of the descending stream of growth *between* the wood and the bark, and has nothing to do with the dead wood of the wound, or the dead rim of bark which surrounds it. It would continue to grow over the dead wood if it were stuck full of nails or tenter-hooks. It does continue to grow over it even when it is rotten; and when the wood has quite mouldered away, the growth still continues, but, as there is no basis on which to deposit, it curls round on itself.

Since the publication of the first edition of this treatise, I have found an example which will throw light on what has been stated.

Plate I. is an engraving of a piece of a board planed down to a level with the centre of the pith of the stem, and of the pith of a branch. The board is from a Scotch fir cut in Brookwood Park, Hampshire. **Explanation of Plates I. and II.**

A is intended to represent the upper end of the board, which has been cut across; B, the lower end. Both of those cross-cut ends are turned so as to face the same way with the side of the board, which is cut lengthways, so that the correspondence of the pith and grain or growths of the ends with those of the side may be easily traced; and the numbering is intended to make this correspondence more clear. In the fir tribe generally, the latest annual shoot is surmounted by a circle of buds, or what is called a *whorl* of buds, around the leading bud. These whorls of buds become whorls of branches, and where the piths of these branches join the pith of the stem, they mark in the centre of the tree, indelibly and for ever, the highest point of each successive annual growth, from the first shoot of the seedling to the last shoot of the forest pine. C, the point where the pith of the branch joins the pith of the stem, marks also the highest point of the first annual growth of this board. The reason that this first growth is so small is, that it formed the taper top of the then leader of the tree. So the difference in the size of the main pith above and below the branch is caused by the junction of the tapering end of the pith of the older lower shoot with the broad-based beginning of the pith of the younger upper shoot.

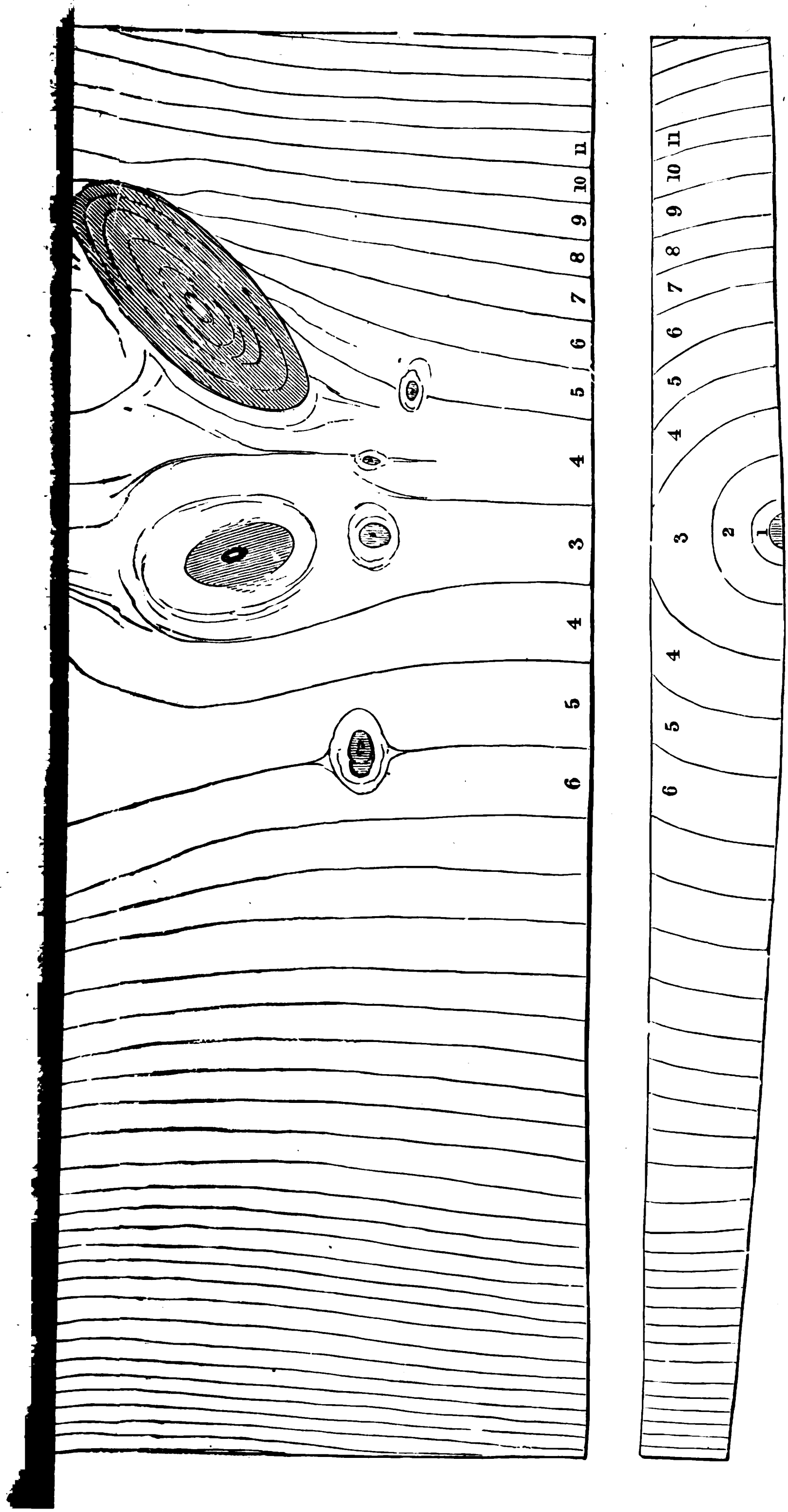
On the left side of the pith of the stem, where there is no branch, it may be seen that the first annual growth of the older shoot of the stem below the branch ceases at C, but the pith is prolonged through it upwards; also that the first annual growth of the younger shoot of the stem above the branch is continuous, and the same with the second annual growth of the older shoot of the stem below the branch: and, doubtless, the reason that the second growth below is so much larger than the first growth either above or below is, that it has received the deposit from the whorl of branches in addition to the growth from the leader, which did not accrue to either of these first annual growths. Each annual growth of the stem above the branch will be found continuous, and the same with the growth numbered one after it below the branch.

On the right of the pith, from C to D D, is the growth of the stem while the branch was alive: and five annual growths of the stem, both above and below the branch, may be seen to be united to the five annual growths of the branch; namely, the first five annual growths above the branch, and from the second to the sixth inclusive below it. So that the first annual growth of the branch is continuous and the same with the first annual growth of the younger shoot of the stem above the branch, but with the second annual growth of the older shoot of the stem below the branch; and each annual growth of the branch is continuous and the same with that growth of the stem above the branch which corresponds with it in number, but with the growth numbered one later below the branch.

Here, then, it may be *seen* that branches are attached to the stem by *increasing* bases, not, as might be imagined from De Candolle's statement, by *decreasing* bases; that is, each annual growth of the branch joins a corresponding annual growth of the stem, and as each annual growth of the stem, besides the deposit from the annual growth of the branch, receives a deposit from the head above it, each annual growth of the stem is larger than its corresponding annual growth of the branch. The width of each annual growth of the branch at the point of confluence, that is, where it joins and mingles with the stem, may be *seen* to open out as the river becomes wider as it receives each tributary; and each growth of the branch may be *seen* to be attached to the stem by an *increased*, not a *decreased* base. Besides this increase in bulk of each annual growth of the branch where it joins the stem, each growth in succession more deeply and more firmly imbeds and builds in all its predecessors in the stem of the tree. It is only such an arrangement as this which would support the enormous weight on the enormously long levers which wide-spreading branches offer.

When the growth of the stem had arrived at D D, the branch died; that is, when the central part of this board below the branch was six years old, and when the central part of the branch and of the board above it was five years old. From D D to E E, during a period of eleven years at the upper side of the branch, and of twenty-two years at the more projecting lower side of it, the dead branch has been gradually and annually inclosed by the growths of the stem, forming a *disunited* knot; that is, the branch may be *seen* to be disunited from these growths of the stem. At E E the growths of the stem curve over the cut end of the branch. After covering the cut end, the growths would have again become continuous and straight had the board been wide enough to show it. Indeed, in Plate II., which is the contrary side of the same board and branch, the growth has already become continuous.

If the branch had been cut off close to the stem at D D when it died, it would have healed over there,


11 10 9 8 7 6 5 4 3 4 5 6
11 10 9 8 7 6 5 4 3 2 1 4 5 6

and from D D to E E would have been solid clean timber instead of a disunited knot. So that there would have been no disunited knot at all, but only a cross-grain, formed by the living branch firmly united to the stem, and decreasing in size towards the centre of the tree, with a scar at the end D D, like that at E E. This scar would form no greater flaw in the timber, than one arising from a small piece of bark being knocked off the stem. If the branch had not died, the cross-grain would not only have been annually prolonged as long as the tree continued to grow, but would also have increased in bulk every year: for De Candolle's cone, whose apex is at the pith, and whose base at the bark of the stem, describes most accurately the form of the cross-grain occasioned by a living branch in the timber of a tree. If the branch when it died had not been cut off at E E, the existing disunited knot would have been prolonged; that is, from E E in the direction of F, as long as the tree grew, and the branch remained on it, a disunited knot would have been inclosed in exchange for the deposit of solid, clean, straight-grained timber. For that the grain becomes straight as soon as the scar is healed over, may be seen in Plate II.

The sixteenth annual growth above the branch is the last whose descent was checked by the dead branch. Its distance from the centre of the pith of the stem, measured at the upper edge of the board, is four inches one-sixteenth. The distance of the sixteenth annual growth from the centre of the pith below the branch, measured at the lower edge of the board, is only three inches five-sixteenths. I imagine that the dead branch acted like a ligature, and that, by checking the descent of the sap, it caused the swelling above it. Below the branch, the growths gradually and annually diminish after the sixth, which is the time of the death of the branch. I have no doubt that the reason of this is, that the branch was killed by the proximity of neighbouring trees, and that they at the same time killed, and afterwards continued to kill, an undue number of side-branches, which caused an unduly diminished return of descending sap, and, consequently, a diminished annual ring or growth of timber. So that, notwithstanding the gradual diminishing after the sixth growth, the first fifteen growths of this board are nearly three times the size of the next fifteen growths; and from after the fifteenth growth of this board, the tree was doubtless one of those denuded poles of which the growth of our *unthinned* plantations in general consists. But if room is given for these poles to increase the size of their heads, they will in the same proportion increase the size of their annual rings of timber.

The centre of the lower part of the tree was, doubtless, much older than any part of this board. I imagine that the sort of wavy cross-grain, which may be observed along the upper edge of all branches which are cut like this specimen, to be the result of the mechanical difficulty which the new growth has to raise and eject the bark from the acute angle formed by the upper side of the branch and the stem. The wavy cross-grain ceases when the bark ceases to be raised.

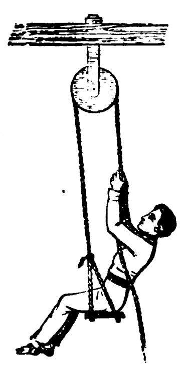
Plate II. shows the contrary side of the board.

Owing to the distance from the pith at which the saw has passed, the growths of the branch are not visible on this side, as they are in Plate I.

On the left, the fifth annual growth of the younger shoot of the stem above the branch, joins the beginning of the upper side of the branch; the sixth annual growth of the older shoot of the stem below the branch, joins the beginning of the lower side of the branch: these two growths are continuous and the same with each other; and, as in Plate I., they are continuous and the same with the last or outside growth of the branch. All the growths to the left of these are disunited with the branch; and, if the branch had been cut off at A when it died, they would have formed clean solid timber, like that outside the end of the branch, instead of inclosing a disunited knot. Each of the other annual growths of the stem above the branch will be found continuous, and the same with the growth numbered one after it below the branch.

In countries where it is the practice to shrive the hedge-row trees, their branchless stems are ascended by means of spikes at the side of the feet. In the East, palms are ascended to inoculate the flower and to gather the fruit, by placing the feet against the stem, and the back against a band which includes the stem and the climber.

In pruning old and long-neglected trees, the ladder should be placed perfectly upright against the stem of the tree, and tied fast to prevent its being knocked off by the falling branches, or broken by them. If the head of the tree is out of reach, a string may sometimes be swung over one of its boughs by a weight, or shot over with a blunt arrow, and by this a rope hauled over. Seated in a loop at one end of the rope, or with one end tied round the thigh, the hands on the opposite rope will acquire the mechanical advantage of a fixed and movable pulley; that is, the double rope doubles your power, or, in this case I should say, halves your exertion, and you may raise your *whole* weight with *half* the exertion required without a pulley. Let us call this *natural* pulley the pruner's



pulley. It is often useful to reach the head, or to remove dead wood or a detached branch on an otherwise branchless stem, or to make the pruner safe; and would make a good fire-escape.

In explanation I attach an engraving from Chambers's excellent educational course, 'Mechanics,' 1837. The writer, however, like others, has entirely mistaken the principle of this curious *mechanical paradox.* In mechanics, indeed, no axiom is more certain than that without *two* pulleys, that is, with the fixed pulley only, and without the movable pulley also, no mechanical advantage is gained. But in the pruner's pulley, even the single fixed pulley is dispensed with; yet, without *any apparent* mechanical advantage, *half is made equal to the whole.* This paradox disgusts the well-instructed engineer, even still more than it does the most brutally ignorant *man of plain common sense.* Truth, however, has a trick of being paradoxical, and this truth is as true as that you can blow hot and blow cold; and it was only the brutally ignorant satyr who denied that simple but paradoxical truth.

But in the pruner's pulley the man is, in fact, at once the movable pulley, the weight to be lifted, and the power that lifts; and the friction of the movable pulley is saved while its mechanical advantage is gained. Luckily, this is not a matter of opinion, but a matter of fact; and, practically, it may be proved by children or weak persons, who are unable to raise their weight on a single rope. Theoretically, it is in perfect accordance with what in mechanics is called 'the law of virtual velocities:' for, if you ascend fifty feet by a single rope, your hands pass over fifty feet of rope; and, for every foot your hands ascend, your body ascends a foot. But if you ascend fifty feet by a double rope, your hands pass over 100 feet of rope; and, for every foot your hands ascend, your body only ascends half a foot, and your hands descend again half a foot. Again, if you haul a *weight* up to a bough by a double rope fifty feet from the ground, fifty feet of rope will pass through your hands, and no mechanical advantage is gained. But if you haul *yourself* up fifty feet by a double rope, 100 feet of rope will pass through your hands; and as in ascending the whole space the hands will pass over *twice* the space of rope, so at any particular part of the ascent they will require only *half* the exertion.

The saw should have a loop to the handle, so as to hang on your arm while climbing. A rope or belt round the tree and your body, which you can lean back against while at work, adds infinitely to your power.

Best time for pruning. March and April are my two months for pruning trees which do not bleed. At that time branches have returned their downward sap for the nutriment of the root, and have scarcely yet begun to receive the new supply upward, and they will bequeath their annual share of this to the leader and other heirs of your choosing: and, the leaves being off, you can clearly see to which the talents should be entrusted.

I never dare touch sycamore or walnut except in summer. I have never known any tree bleed when in full leaf. I have known sycamore and walnut bleed when pruned at Christmas, which corroborates the idea of a winter circulation of sap; but perhaps we have an undue horror of bleeding from pruning. The southern vineyards are always pruned in the bleeding season: and the more freely they bleed, the better the sign. In England I once watched the bleeding of a vine, which was trained to the top of a high garden wall. The lowest wounds bled first, and it was about a week before the bleeding reached the upper wounds. By this time the lower wounds were ceasing to bleed. The sap which *ran* was as clear as water, but a thick sediment accrued over each wound, like soft-boiled white of egg, and *apparently* this stopped the bleeding, as the gluten of our blood will in our wounds, if it is not wiped off under the idea of *staunching* the wound. The resinous sap of fir-trees, instead of running off, remains adhering in white drops or streams; by which, I think, the pruner is in general more frightened than the tree is hurt. Supposing the branch to have been *de trop*, it would have used, or rather misused, *every year* a hundred times as much sap as for *once only* the tree loses by bleeding. I object to autumnal pruning, because the boughs are full of elaborated sap *due* to the root. These observations apply to pruning hardy forest-trees for *wood*, not to pruning for *fruit*.

Measurement and longevity of trees.

The largest *sound* tree I have ever measured is 'the grindstone oak' in the Holt Forest.* It is thirty-five feet in girthing at three feet from the ground. It is dead, and was apparently *lately* dead when I first saw it, since the bark was still on it: I think it has been originally a *pollard* (*polled* or headed); and the

* Unhappily burnt by a thoughtless boy some 5th of November, since this was published in 1844.

largest sound timber I have ever seen in England has been old pollards, allowed to grow up in our forest grounds after the pollard system had ceased. They were probably allowed to grow because, being many-headed, their timber was not valuable.

Such trees, I believe, continue to exist for centuries, perhaps for thousands of years; even after they are hollow. The old pollards which grace our forest-grounds and commons were probably headed as young trees, and their growth cut periodically, as our underwood is now, the browsing of the deer and cattle necessitating in such places this sort of aërial coppice-wood. Charcoal was generally used before coal; and I think that the old pollards and the black circles of earth about Rotherfield, in this neighbourhood, may both be remains of the charcoal-burners of the forest called by the Romans Anderida Silva, and by the Saxons Andredes Weald.

However the heads of these pollards may be lopped, every year of life adds one ring of new wood and bark to the girthing of the stem. The same takes place when the tree is perfectly hollow. The inside dead wood, being dry and imporous, prevents the bleeding or efflux of the sap. I have found the girthing of some of these relics of the olden time much greater than the girthing of any sound timber I have ever measured, though probably the pollards never girthed large as sound trees. Even when the circle is broken, and they stand like detached strips of bark, the new deposit of wood and bark takes place on their outside, while their inside is sloughing, or rotting off, and these detached strips gradually and annually progress outwards from where the centre of the tree was.

When old pollards are cut over, they throw out new branches most vigorously, which militates against the theory that all new branches are from original *latent* buds, and from the central pith; since *old* pollards may be found not only destitute of central pith, but the oldest part of whose stem-wood is possibly not ten years of age. So that the casuist might raise the question whether these, our supposed oldest trees, are not actually among our youngest; as the identity of the ship Argo was disputed when, from constant gradual repair in the temple where it was preserved, every part of the old ship had disappeared and had been replaced. Nay, since whilst a pollard is becoming hollow the internal decay surpasses the external growth, it may be said to become younger by age; and when decay and growth balance each other, that age, or the addition of years, makes it no older.

The great secret of large timber is, centuries of non-cutting down, good soil, room, and sheltered situation. These conditions rarely come together in cultivated countries, though they do sometimes in our old family places. The free growth and the enormous measurements of trees in the forests of uncultivated countries are more frequently to be attributed to the concurrence of the favourable conditions above stated, than to the peculiar attributes of the trees themselves. Such trees, when imported, and planted on the poor soils and exposed situations which are alone planted in cultivated countries, make moderate progress, and never reach any size.

As long as countries are in a state of nature, trees, being the original possessors, seize on the valleys and best soils, from which they actually exclude man and cultivation. But the case is reversed when man has cleared the best soils for cultivation. Trees are then seldom planted or suffered to grow except on soils so bad as not to pay for cultivation.

I have received the following marvellous measurements of some pinus Lambertianas on the Columbia, from an authority that I cannot doubt. At eight feet from the ground they were fifteen feet in diameter. The stems were branchless to two hundred and fifty feet from the ground, and were there thirteen feet in diameter. If the new annual ring of wood were a quarter of an inch wide, trees would attain this diameter in three hundred and sixty years; and, supposing them to have grown a foot a year in height, this would allow them eighty feet of head above the branchless stem.

These measurements, which I called marvellous in the first edition of this treatise, are (together with the guesses in reference to the age of the trees) rather put in the shade by the following. Adanson measured a baobab tree (Adansonia digitata) to be thirty feet in diameter, and gives it the astounding and patriarchal age of 5,150 years. This would be very slow growth; scarcely more than the twenty-ninth part of an inch for the width of each annual ring; that is, if the width of the annual ring were the twenty-ninth part of an inch, the tree would attain the diameter of thirty feet in 5,220 years, or in seventy years more than the supposed age of the tree.

The age of this identical baobab, then, at Noah's deluge, being short of 1,000 years, its diameter would be short of six feet, and its girthing perhaps seventeen feet; not an inconsiderable plant certainly, but small for so great an irrigation. Adanson's guess was made by cutting into the stem of the tree till the width of three hundred rings was measured. 'The average rate of growth of younger trees of the same species was then ascertained, and the calculation made according to a supposed mean rate of increase.' I quote from the admirable Lyell, who quotes the 'Biblioth. Univ.' on the longevity of trees.

If the general average width of the rings, which included the growth of the young trees, was only the twenty-ninth part of an inch, what was the average of the first three hundred rings, which were all old growths? Perhaps the fiftieth part of an inch. But Adanson should have given us these data, and his mode of calculation from them. I cannot help thinking that he may have made a slight mistake in these calculations; that he may have omitted to perceive that, although to make up the diameter of a tree from the rings on one side of its centre the width of these rings must be reckoned double, to ascertain the age of the tree the number of the rings on one side of its centre must be reckoned single. The width of a half diameter must be doubled to make a whole diameter; but when you count the years of a tree you must not double them to get at its age. 'There's ne'er a villain in all Denmark but he's an arrant rogue.' These three truisms seem equally profound and equally palpable. Yet I think that Adanson may have made the slip, and, with the tree standing, may have failed to perceive that the number of rings on the half diameter is the same as on the whole diameter; and that, having doubled their width in completing the *space* or diameter, he has also doubled their number in reckoning the *time* or the age of the tree. If so, the number of years he has given must be *halved*, and 2,575 years would be the age of the tree; a pretty good age too, since it would nearly take us back to the time of Romulus! Even for this age, however, the growth must have been slow; little more than the fifteenth part of an inch for the width of each annual ring. If the annual ring were one-eighth of an inch in width, the tree would attain the size of thirty feet in diameter in 1,440 years; if the ring were one-fourth of an inch in width, in 720 years.*

* The simplest view of the case is this:—The baobab is thirty feet in diameter—360 inches. If the width of the annual ring

I think that the baobab should be 'restored to its place in universal history,' because the next step taken by physiologists on this *datum* of Adanson's will be, that all trees of thirty feet in diameter are 5,150 years of age; and so, in proportion as the diameter of any tree exceeds or falls short of thirty feet, an age greater or less than 5,150 years will be assigned to it: from which it would result that a tree must grow 174 years, nearly two centuries, before it would attain one foot in diameter. Lyell, speaking of a submarine forest at Bournemouth, in Hampshire, says: 'Seventy-six rings of annual growth were counted in a transverse section of one of the buried trees, which was fourteen inches in diameter.' This, though exceedingly slow growth, is about three times the growth allowed by Adanson. But were the rings perfect on each half diameter? If not, the width of those wanting on the deficient half diameter must be added to the fourteen inches of growth. On the other hand, and in accordance with the rule above, we are told that De Candolle thinks that the Montezuma cypress (Taxodium sempervirens) at Mexico exceeds the age of the baobab,—exceeds these poor 5,150 years in age. And this opinion is quoted, with profound respect, by one of the most profound men of the day,—by Lyell. Did De Can-

were half an inch, the growth in diameter would be an inch, and the years and the inches of growth would be the same. That is, the baobab would be 360 years old; allow half this growth, which is perhaps fair, the tree would be 720 years old. (Third edition.)

dolle think this before Adanson gave us his measurements and guesses? I'll be bound that he did not. I'll be bound that the first philosopher has taken the second philosopher's calculations for granted; and, as the cypress exceeds the baobab in measurement, he concludes, naturally, that the cypress also exceeds the baobab in years. If the two philosophers are right, the two trees are slow growers. The measurement given of the cypress is 117 feet in girthing. This is about thirty-nine feet in diameter; and, supposing the annual ring to be the twenty-ninth part of an inch in width, which is the rate of growth assigned by Adanson to the baobab, the age of the cypress should be 6,786 years. So that 'the seedling began to vegetate' nearly a thousand years before the creation of man according to the Hebrew text of the Mosaic writings. If the width of the annual ring were one-eighth of an inch, the tree would attain the size of thirty-nine feet in diameter in 1872 years. If the width of the annual ring were one-fourth of an inch, the diameter of the tree would have been thirty-nine feet in 936 years; and the Montezuma cypress would have been about 400 years old at the conquest of Mexico. This is, perhaps, more likely than that it should be a thousand years old in the time of Adam. The growth in girthing of trees in decay—that is, with hollow trunks and pollard heads—is indeed very diminutive; and to give them the girthing which they attain to, any number of years may be allowed. So the growth in girthing of forest-trees will vary in the same tree, according to accidental circumstances. In Plate I. some of the early growths, when the plant had perfect room, exceed one-third of an inch in width. Some of the later growths, when its head had been crowded to death by neighbours, scarcely exceed one-tenth of an inch. But if one-fourth of an inch is allowed for the annual ring of growing trees—that is, of sound trunks with full heads—in 5,000 years they would attain a diameter of more than 200 feet, and a girthing of more than 600 feet. If this growth is halved, and one-eighth of an inch width is allowed for the annual ring, a diameter of more than 100 feet, and a girthing of more than 300 feet, would result in 5,000 years. We may ask, did the baobab grow in height for 5,000 years? If so, the Adansonia digitata is too modest a name. (I speak as regards the tree, not the man.) It should be christened the Adam-o-father-ia Skyscrapo-moonrakiana; though De Candolle, or any man well up in botany, would give the first semi-diameter of this name from the Latin, and the last *Græco fonte.*

I think it possible that oaks, which habitually make two shoots in the year, may make two (*annual?*) rings in the year: and this may be possible with many trees in the tropics. That trees of gigantic stature are not more frequently found in unappropriated forests is generally to be attributed to their want of room; that is, to their growing so close as to injure or kill one another. They cannot attain to first-rate growth without ages of contention and killing all their neighbours. In doing so, the growth of the survivors is not only delayed for centuries, but in general permanently marred. The axe *should* gradually and successively relieve them from their neighbours.

Of course, all side-growth is, from the position of its weight, more liable to break than upright or vertical growth. When a tree takes two leaders, from want of light and from want of room on the inside, the leaders grow from one another to the outside; and from their weight inclining to the outside, without anything to balance it on the inside, they are liable to split from one another. As each leader enlarges annually in girthing, the junction at their two bases progresses upwards, inclosing the bark of each between the two. This double stratum of thick bark is killed by mechanical pressure, perhaps for a foot or two in height, and rots to that extent. This prevents the deposit of any new wood on those parts of the inside of either leader, and consequently also prevents the perfect junction, or anastomosing, of the wood of the two leaders. Water lodges in the hollow at the fork; and a frost which is severe enough to freeze this water will rend apart the trunk of the sturdiest oak to a certainty. Besides this, the hollow at the fork becomes a leaf-trap, catches any dust which may be driven by wind, receives the moss and detritus of the bark which are washed down by rain, and forms a cupful of fine dark mould. Into this the tree itself often strikes roots, which descend between and through the rotten bark which I have mentioned to its very base. Then comes the miraculous force of turgescence, acting in the true line of cleavage of the tree, and the twin leaders are rent from each other to as great a certainty as the granite is split by the wetted bolt of wood. I can show a root thus formed on an elm after (as I believe) it had been the cause of splitting off half the tree. The root is still alive, though the soil in which it grew is gone. Whence comes the upward sap in the wood of this root? or of those denuded in the experiment which I have mentioned, p. 98?

Early and constant pruning will avoid the cause of these fruitful sources of decay in timber.

If the heads of trees are dying in, from accidental blight, or from the destruction of their leaves and shoots by a strong south-wester, or from frost, &c., in all cases they should be cut in, not only to where the boughs are alive, but to where they are vigorous, and, if possible, at the foot of a living twig or bud. If the dying boughs are left on the tree, the sap is *wasted* by going up the boughs, without the power of breaking out or returning, consequently the roots are starved; for the only power of return—that is, the only communication between the upward course of the sap in the wood, and the downward course in the bark—is a living leaf or bud.

If the dying boughs are cut off, the sap, which would have been uselessly expended in them, invigorates the present shoots, or bursts forth in the form of new shoots, and, in returning, contributes to the growth and nourishment of the roots. In such cases, trees are often killed by being left 'to see where they break out.' They should be cut immediately: 'Bis dat qui cito dat.' This *waste* of the sap in the stem and branches, without the power to return to the root, is the reason why trees which are barked round just above the earth *in general* die. If young trees are cut down just above the earth, they shoot out again freely and continue to grow; but if they are only barked, the sap going up the heart-wood diminishes the chance of an outbreak below, and without this outbreak the roots must die from a want of descending sap.

If a branch is not cut at the foot of a living twig, its end should be again cut off at the foot of the new shoot, in order that the descending sap of the new shoot may deposit wood and bark over the cut end. If long stump ends are allowed to remain, they rot before the new growth in diameter of their stock has inclosed and covered them.

Near natural ponds, where the whole soil holds, the presence of trees is beneficial, from their prevention of evaporation, and from the condensation which takes place in moist warm weather, particularly on smooth-barked trees; but near artificial ponds or dams no growth whatever should be allowed: roots are the great creators of leaks. Nor should they be allowed on masonry. Roots, by turgescence, will rend apart the strongest masonry, or lift any weight of stone. In Greece, Italy, and throughout the East, roots are the great dilapidators of the ruins of antiquity. We may observe the effect of a too sudden exposure to the opposite force of drought in the *warping* and rending asunder of the strongest woody organisations.

PART V.

THE PARK PINETUM.

I SHOULD like to say a word in recommendation of *park pinetums* as contrasted with flower-garden or lawn pinetums.

As the generality of pinuses grow by nature into magnificent and gigantic forest-trees, they should, I think, be planted in our *parks as well as in* our flower-gardens, shrubberies, and lawns. Lawn plants, it is true, produce an instant beautiful effect as *shrubs;* and as they have their boughs down to the ground, more shelter, and better soil, they surpass the young park plants in beauty. So the greenhouse or hothouse plant surpasses the lawn plant in beauty. But in how short a time does the greenhouse or lawn plant become *too big for its boots!* and the lawn plant must be cut down or mutilated, because it grows over this walk, or that flower-bed, or into this window or that door. Though exquisite when young, a few specimens will soon fill and overgrow the lawn pinetum. They have been planted as shrubs; they are misplaced as trees; and the greater the growth, the more we have to regret the position of our most favourite plants.

By comparison with the pleasure-ground pinetum, the park pinetum is *infinite* and *eternal;* that is, any number of plants may be grown, and the greater the number, and the greater their size, the greater the beauty and interest of the pinetum.

But short-lived mortals claim the short-lived pleasure of the present hour, and a very little present pretty effect is greatly preferred to any quantity of infinity and eternity. And I do not mean to condemn lawn pinetums; on the contrary, I think them inappreciably beautiful. What I plead for is, that we should *also* plant *posterity park pinetums.* If we do not live to see their beauty ourselves, we shall not die the sooner for having created it for those who succeed us on this earth. And must he be a liar who says he loves the neighbour whom he has *not* seen?

Pinuses should be planted out when from six inches to a foot high. I shall detail the method which I have followed, as my own labourer, in a small park pinetum since 1837.

Dig a pit five feet in diameter, but go no deeper than the good upper soil; throw the earth out; add and mix as much good soil as you can afford. Having to wheel it myself, I used to think three barrows of road-sand from the nearest ditch a quantum, and half-a-dozen barrows a liberal allowance. In replacing the earth, put the turf at the bottom, and form a flat, low eminence brimming over the pit on to the undug ground outside, so that, when the loose earth in the pit sinks to its former level, a raised outside *rim* remains. This rim prevents inundation from without, and facilitates irrigation from within. It also prevents cracks, in drought, between the old and the new ground. These cracks the roots have a difficulty in crossing. They admit the drought, and harbour mice, which will sometimes make a thoroughfare entirely round the plant. If the pit is dug deeper than the upper soil, the roots are enticed and entrapped in a cup whose sides are impervious to them.

The roots of the pot pinus should be carefully unwound; if not they can never escape from the circular growth which the pot has given them, and the plant will die a self-strangled Laocoon.

From mice, the small enemies which I have mentioned below the soil, to the reach of a horse, seven feet above the soil, the pot park pinus has many enemies to contend with, including colts, cows, calves, sheep, lambs, hares, and rabbits; and it must be confessed that a park pinetum comes under the head of 'the acquisition of pinuses under difficulties.'

The common horse-fence, seven feet high, besides being a great dis-sight, shuts your pet pot plant from your view, perhaps for a dozen years. Instead of this, I recommend a low inside sheep and game fence, and a low outside cattle fence. As the cheapest sheep fence, a rabbit-proof circular wattle fence round the pit may be used, or an hexagonal wood fence four bars in height with a wire game fence inside. And as the cheapest outside fence, an hexagonal single post and rail, two feet nine inches from the ground.

The best but dearest mode of fencing the pot pinus is by a circular sheep-fence of strong iron netting, three feet high round the pit. This fence may be lifted, and the interior ground kept hoed. The hoeings should be left on the surface, as future pabulum for the plant. There can be no better manure for plants than the remains of plants. If the hoeings are taken away, the soil is denuded at each hoeing. The best outside cattle-fence is eight iron hurdles, consisting only of an upper bar, two feet nine inches high, and an under bar close to the ground. Between these bars the sheep will pass to graze, so that no pasture is wasted, and no mowing necessary. When the plant is large enough, the inner fence may be taken away. The number of hurdles may be increased as the side-boughs increase; and these side-boughs may for ever be let down to the sheep-browsing line, instead of being kept up to the horse-browsing line. More than this in the way of side-boughs one must not attempt, or pretend to, in the park or pasture. As for boughs down to the ground, the very fence which protects them hides them, and is a still greater dis-sight than the absence of the boughs. The trees are planted as trees, and must not be looked at as shrubs. But they *will* be looked at as shrubs; and after having been at great pains and expense merely for the *preservation* of side-boughs, and when you can show larger and lower side-boughs than can be seen in any other pasture, you will *catch it* from the vulgar for the *destruction* of side-boughs. But the vulgar are here as unreasonable as usual, and might as well require the park pasture to be laid out in flower-beds, or decked with greenhouse plants.

On this system the pot pinus may be planted, when only an inch or two in height, in places exposed to cattle, and may be seen, pruned, cultivated, and *petted*, from first to last.

In deer-parks the pot pinus may be protected by a wire game-fence and numerous circles of slight rails, of which the plant is the common centre, about a foot from the ground and from one another. These fend off cattle by extangling their legs. The objections to this fence are, sometimes a broken leg to cattle, and always sacrifice of pasture.

The plants should be kept tied, by three strings, to the sheep-fence. When a large pinus requires steadying, or has been shaken by the wind, it should be made fast to the horse-rail, or iron hurdles, by three chains; the angles between the chains being equal each to each. The chains should be fastened with *S* hooks round boughs, with lead between the chains and the boughs, in order to keep the stem intact. They should run up as high as convenient, like the rigging of a mast. This for two reasons: first, the higher the ties, the greater the mechanical advantage in holding against a strong wind; secondly, if the part which you attempt to fix has any motion, it will be felt at the root inversely as taunt with their want of success philosophers who have attempted a labour, perhaps superhuman—to throw light on the hitherto impenetrable darkness which has enveloped the processes of vitality—to delineate the actually progressing operations of the hand of the Almighty in his noblest, most finished, most complicated works. It is the unthinking only who, becoming inured to the universally perpetual recurrence of the generation and growth of organic existences, take these most mysterious miracles as matters of course, and behold them with indifference.

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the distance of the ties from the root. Cords get tight in wet, and loose in drought.

An insignis raised in this way, planted in the autumn of 1837, is now (1853) above thirty-seven feet high. This is not much more than two feet each year; but the three last shoots measure together nine feet eight inches, and had the plant not been blown over when young, and its leader browsed by cattle, and afterwards broken by wind, I think its growth in height would have averaged a yard a year. The soil is clay on chalk (very unfavourable for pinuses), with a few barrows of road-sand mixed at the first planting.

I have never met with any observations on the length of time which the *grasses*, or leaves of cone-bearing trees, remain alive. Perhaps two whole years at the least; in many cases, much longer; and I should doubt if the Araucaria and Cryptomeria have any fixed *natural* period for shedding their leaves. The generality of English evergreens defoliate as regularly as other trees called deciduous; but evergreens retain their leaves about a whole year; deciduous trees, about half a year.

I wish I could persuade people to ornament the waysides. Vulgar-minded persons think they have done a clever thing, and that they have gained much when they have robbed the waste. But these waysides might be made with little trouble and no expense comparatively parks and gardens. I dare not ask the rich to plant avenues with 'the Tree-lifter' on common pastures. But let the landowner *not* cut down the hollies in hedgerows which border the waysides, and let anyone plant a few boxes (which no cattle will touch), and stick a red beech, or a Deodara, here and there amid protecting brambles. Gentle hands might dibble in plants of this sort near our villages, whose beauty might appropriate to the young and the landless miles of wayside garden ground. If this system were pursued through our beautiful island, what a garden of Eden we should make of it. And if in the attempt we occasionally get a blow from the ruthless or the thoughtless, is this different from our usual experience every day, every where, in every thing?

Concluding remarks.

I conclude by recommending the *practice* of transplanting with the ball of earth, without reference to the *theories* with which it has been supported. Indeed, with regard to them, I do not believe that in all vegetable physiology or agricultural chemistry there is one principle to be depended on. In fact, agricultural chemistry is a new light to us, for the first glimmerings of which we are indebted to our immortal Davy. I say this with the deepest veneration for the brilliant talents and undaunted perseverance of those who have devoted themselves, or who still do devote themselves, to sciences of the first importance to the existence of man and the honour of his Creator; and with a heartfelt disgust at those who, pluming themselves on their progress in lower but more certain science, presume to